

# **THE LONG-TERM EFFECTS OF HEDGE FUND ACTIVISM**

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## Abstract

We test the empirical validity of a claim that has been playing a central role in debates on corporate governance – the claim that interventions by activist shareholders, and in particular activist hedge funds, have an adverse effect on the long-term interests of companies and their shareholders. While this “myopic activists” claim has been regularly invoked and has had considerable influence, its supporters have thus far failed to back it up with evidence. This paper presents a comprehensive empirical investigation of this claim and finds that it is not supported by the data.

We study the universe of about 2,000 interventions by activist hedge funds during the period 1994-2007, examining a long time window of five years following the intervention. We find no evidence that interventions are followed by declines in operating performance in the long term; to the contrary, activist intervention are followed by improved operating performance during the five-year period following the intervention. These improvements in long-term performance, we find, are present also when focusing on the two subsets of activist interventions that are most resisted and criticized – first, interventions that lower or constrain long-term investments by enhancing leverage, beefing up shareholder payouts, or reducing investments and, second, adversarial interventions employing hostile tactics.

We also find no evidence that the initial positive stock price spike accompanying activist interventions fails to appreciate their long-term costs and therefore tends to be followed by negative abnormal returns in the long term; the data is consistent with the initial spike reflecting correctly the intervention’s long-term consequences. Similarly, we find no evidence for pump-and-dump patterns in which the exit of an activist is followed by abnormal long-term negative returns. Finally, we find no evidence for concerns that activist interventions during the years preceding the financial crisis rendered companies more vulnerable and that the targeted companies therefore were more adversely affected by the crisis.

Our findings that the considered claims and concerns are not supported by the data have significant implications for ongoing policy debates on corporate governance, corporate law, and capital markets regulation. Policymakers and institutional investors should not accept the validity of the frequent assertions that activist interventions are costly to firms and their long-term shareholders in the long term; they should reject the use of such claims as a basis for limiting the rights and involvement of shareholders.

Keywords: Corporate governance, short-termism, managerial myopia, long-term value, investor horizons, market efficiency, shareholder activism, hedge fund activism, shareholder rights, takeovers, proxy fights, takeover defenses, hedge funds, .

JEL Classification: D21, G12, G23, G32, G34, G35, G38, K22

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I. INTRODUCTION

This paper focuses on a claim that has been playing a central role in debates over shareholder activism and corporate governance. According to this “myopic activists” claim, activist shareholders with short investment horizon, especially activist hedge funds, push for actions that are profitable in the short term but are detrimental to the long term interests of companies and their long-term shareholders. The problem, it is claimed, results from the failure of short-term market prices to reflect the long term costs of actions sought by short-term activists. As a result, activists seeking a short term spike in a company’s stock price have an incentive to seek actions that would increase short-term prices at the expense of long-term performance, such as cutting excessively investments in long-term projects or the reserve funds available for such investments.

In this paper, we conduct a systematic empirical investigation of the myopic activists claim, focusing on interventions by activist hedge funds. Such funds have been playing an increasingly central role in the corporate governance landscape in general and shareholder activism in particular.<sup>1</sup> We find that the myopic activists claim is not supported by the data. Our findings have important policy implications for ongoing policy debates on corporate governance and the rights and role of shareholders.

The myopic activists claim has far been put forward by a wide range of prominent writers. Such concerns have been expressed by significant legal academics,<sup>2</sup> noted economists and business school professors,<sup>3</sup> prominent business columnists,<sup>4</sup> important business organizations,<sup>5</sup> and top corporate lawyers.<sup>6</sup>

Furthermore, those claims have been successful in influencing important public officials and policy makers. For example, Chancellor Leo Strine and Justice Jack Jacobs, two prominent

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<sup>1</sup> See, e.g., Marcel Kahan and Edward B. Rock, *Hedge Funds in Corporate Governance and Corporate Control*, 155(5) U. PA. L. REV. 1021 (2007).

<sup>2</sup> See, e.g., William W. Bratton and Michael L. Wachter, *The Case Against Shareholder Empowerment*, 158 U. PA. L. REV. 653–54 (2010).

<sup>3</sup> See, e.g., THE KAY REVIEW OF UK EQUITY MARKETS AND LONG-TERM DECISION MAKING, FINAL REPORT, 9 (2012); Justin Fox and Jay W. Lorsch, *The Big Idea-What Good Are Shareholders?*, 48 HARV. BUS. REV. 50, 51 (2012).

<sup>4</sup> See, e.g., Joe Nocera, *What is Business Waiting For?*, N.Y. TIMES, August 16, 2011; Andrew Sorokin, *‘Shareholder Democracy’ Can Mask Abuses*, N.Y. TIMES, February 25, 2013.

<sup>5</sup> See, e.g., The Aspen Institute, *Overcoming Short-Termism: A Call for a More Responsible Approach to Investment and Business Management*, at 2–3 (2009), available at [http://www.aspeninstitute.org/sites/default/files/content/docs/business%20and%20society%20program/overcome\\_short\\_state0909.pdf](http://www.aspeninstitute.org/sites/default/files/content/docs/business%20and%20society%20program/overcome_short_state0909.pdf).

<sup>6</sup> See, e.g., Martin Lipton and Steven A. Rosenblum, *A New System of Corporate Governance: The Quinquennial Election of Directors*, 58 U. CHI. L. REV. 187–88, 203, 210–12 (1991).

Delaware judges, have expressed strong concerns about short-sighted activism.<sup>7</sup> When serving as SEC chairman, William Donaldson expressed concerns about such activism.<sup>8</sup> And concerns about intervention by activists with short horizons persuaded the SEC to limit use of the proxy rule adopted in 2010 to shareholders that have held their shares for more than three years.<sup>9</sup>

The policy stakes are substantial. Invoking the long-term costs of activism has become a standard move in arguments for limiting the role, rights, and involvement of shareholder activists. In particular, such arguments have been used to support, for example, takeover defenses, impediments to shareholders' ability to replace directors, limitations on the rights of shareholders with short holding periods.

As one of us analyzes in detail in other work, the claim that activist interventions are detrimental to the long-term interests of shareholders and companies cannot be derived from theory even assuming the existence of inefficient capital markets and short activist horizons.<sup>10</sup> The claim is a factual proposition that can be empirically tested. However, those advancing the myopic activists claim have thus far failed to back their claims with any large sample empirical evidence. Some supporters of the claim seem to assume the validity of their claim, failing to acknowledge the empirically contestable nature of their claim and the need for evidence, while other supporters of the claim have offered their experience as evidence.<sup>11</sup>

At the same time, financial economists have produced significant empirical work on hedge fund activism.<sup>12</sup> There is evidence that the filing of 13D schedules – public disclosures of the purchase of a significant stake by an activist – are accompanied by significant positive stock price reactions as well as subsequent improvements in operating performance. However,

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<sup>7</sup> See Leo E. Strine, Jr., *One Fundamental Corporate Governance Question We Face: Can Corporations Be Managed for the Long Term Unless Their Powerful Electorates Also Act and Think Long Term?*, 66 *BUS. LAW.* 1, 26 (2010); Jack B. Jacobs, “*Patient Capital*”: *Can Delaware Corporate Law Help Revive It?*, 68 *WASH. & LEE L. REV.* 1645, 1649, 1657–63 (2011).

<sup>8</sup> See CFA Center for Finance Integrity & Business Roundtable Institute for Corporate Ethics, *Breaking the Short-Term Cycle: Discussion and Recommendations on How Corporate Leaders, Asset Managers, Investors and Analysts Can Refocus on Long-Term Value*, at 1, 3 (2006), available at [http://www.darden.virginia.edu/corporate-ethics/pdf/Short-termism\\_Report.pdf](http://www.darden.virginia.edu/corporate-ethics/pdf/Short-termism_Report.pdf). (statement of William Donaldson);

<sup>9</sup> See *Facilitating Shareholder Director Nominations*, Exchange Act Release Nos. 33-9136; 34-62764; IC-29384, 75 *Fed. Reg.* 56,668, 106–7 (Aug. 25, 2010).

<sup>10</sup> See Lucian A. Bebchuk, *The Myth that Insulating Boards Serves Long-Term Value*, 113 *COLUM. L. REV.* (forthcoming) (2013).

<sup>11</sup> See Wachtell, Lipton, Rosen & Katz Memorandum, *Bite the Apple; Poison the Apple; Paralyze the Company; Wreck the Economy*, REPROD. THE HARVARD LAW SCHOOL FORUM ON CORPORATE GOVERNANCE AND FINANCIAL REGULATION (Feb. 26, 2013) (Martin Lipton stating that his belief in the long-term costs imposed by activist campaigns is grounded in the “decades of experience” that he and his colleagues have accumulated while advising companies).

<sup>12</sup> For a review of this literature, see Alon Brav, Wei Jiang and Hyunseob Kim, *Hedge Fund Activism: A Review*, 4(3) *FOUNDATIONS AND TRENDS IN FINANCE* 185 (2009).

supporters of the myopic activist claims dismiss this evidence, taking the view that losses to shareholders and companies from activist interventions take place later on.

On their view, improved performance following activist interventions comes at the expense of sacrificing performance later on, and short-term positive stock reactions merely reflect inefficient market prices that are moved by the short-term changes and fail to reflect their long-term costs. Thus, one prominent supporter of the myopic activism claim has recently argued that the important question is “[f]or companies that are the subject of hedge fund activism and remain independent, what is the impact on their operational performance and stock price performance relative to the benchmark, not just in the short period after announcement of the activist interest, but after a 24-month period.”<sup>13</sup>

Data about companies’ operating performance and stock returns years following activist intervention is publicly available and easily accessible. Nonetheless, supporters of the myopic activists view have failed to back their view with empirical evidence or even to test empirically the validity of their view. In this paper, we seek to fill this void by providing the first comprehensive empirical investigation of the myopic activists claim. We find no support for this claim in the data.

Our study uses a dataset consisting of the full universe of approximately 2,000 interventions by activist hedge funds during the period 1994–2007. We identify for each activist effort the month (the intervention month) in which the activist initiative was first publicly disclosed (usually through the filing of a Schedule 13D). Using the data on operating performance and stock returns of public companies during the period 1991–2012, we track the operating performance and stock returns for companies during a long period – five years – following the intervention month. We also examine the three-year period that precedes activist interventions and that follows activists’ departure.

Starting with operating performance, we find that operating performance improves following activist interventions and there is no evidence that the improved performance comes at the expense of performance later on. During the third, fourth, and fifth year following the start of an activist intervention, operating performance tends to be better, not worse, than during the pre-intervention period. Thus, during the long, five-year time window that we examine, the declines in operating performance asserted by supporters of the myopic activism claim is not found in the data. We also find that activists tend to target companies that are underperforming relative to industry peers at the time of the intervention, not well-performing ones.

We then turn to stock returns following the initial stock price spike that is well-known to accompany activist interventions. We first find that, consistent with the results obtained with respect to pre-intervention operating performance, targets of activists have negative abnormal returns during the three years preceding the intervention. We then proceed to examine whether, as supporters of the myopic activism claim believe, the initial stock price reflects inefficient

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<sup>13</sup> See Wachtell, Lipton, Rosen & Katz Memorandum, *Bite the Apple; Poison the Apple; Paralyze the Company; Wreck the Economy*, REPROD. THE HARVARD LAW SCHOOL FORUM ON CORPORATE GOVERNANCE AND FINANCIAL REGULATION (Feb. 26, 2013).

market pricing that fails to reflect the long-term costs of the activist intervention and is thus followed by stock return underperformance in the long term.

In investigating the presence of negative abnormal returns during this period, we employ three standard methods used by financial economists for detecting stock return underperformance. In particular, the study examines: first, whether the returns to targeted companies were systematically lower than what would be expected given standard asset pricing models; second, whether the returns to targeted companies were lower than those of “matched” firms that are similar in terms of size and book to market; and, third, whether a portfolio based on taking positions in activism targets and holding them for five years underperforms relative to its risk characteristics. Using each of these methods, we find no evidence of the asserted reversal of fortune during the five-year period following the intervention. The long-term underperformance asserted by supporters of the myopic activism claim, and the resulting losses to long-term shareholders resulting from activist interventions, are not found in the data.

We also analyze whether activists cash out their stakes before negative stock returns occur and impose losses on remaining long-term shareholders. Because activist hedge funds have been documented to deliver adequate returns to their own investors, such a pattern is a necessary condition for long-term shareholders being made worse off by activist interventions. We therefore examine whether targets of activist hedge funds experience negative abnormal returns in the three years after an activist discloses that its holdings fell below the 5% threshold that subjects investors to significant disclosure requirements. Again using the three standard methods for detecting the existence of abnormal stock returns, we find no evidence that long-term shareholders experience negative stock returns during the three years following the partial or full cashing out of an activist’s stake.

We next turn to examine the two subsets of activist interventions that are most resisted and criticized – first, interventions that lower or constrain long-term investments by enhancing leverage, beefing up shareholder payouts, or reducing investments and, second, adversarial interventions employing hostile tactics. In both cases, interventions are followed by improvements in operating performance during the five-year period following the intervention, and no evidence is found for the adverse long-term effects asserted by opponents.

Finally, we examine whether activist interventions render targeted companies more vulnerable to economic shocks. In particular, we examine whether companies targeted by activist interventions during the years preceding the financial crisis were hit more in the subsequent crisis. We find no evidence that pre-crisis interventions by activists were associated with greater declines in operating performance or higher incidence of financial distress during the crisis.

Our findings that the data does not support the claims and empirical predictions of those holding the myopic activism view have significant implications for ongoing policy debates. Going forward, policymakers and institutional investors should not accept the validity of assertions that interventions by hedge funds are followed by long-term adverse consequences for companies and their long-term shareholders. The use of such claims as a basis for limiting shareholder rights and involvement should be rejected.

The remainder of this paper is organized as follows. Part II describes our dataset and the universe of 2,000 activist interventions that we study. Part III analyzes operating performance. Part IV analyzes long-term stock returns. Part V focuses on the two subsets of activist interventions that are especially resisted and criticized by critics. Part VI investigates whether activist interventions during the years preceding the financial crisis were followed by increased vulnerability to the subsequent crisis. Part VII concludes.

## II. THE UNIVERSE OF HEDGE FUND ACTIVISM

We build in this paper on the dataset, covering the period from 2001 to 2006, used in the first comprehensive study of hedge-fund activism published by two of us along with Frank Partnoy and Randall Thomas.<sup>14</sup> This dataset was also used by the same authors in subsequent work.<sup>15</sup> Two of us, with Hyunseob Kim, extended the data to include 2007 in a subsequent study,<sup>16</sup> and presented an updated sample covering the period from 1994 through 2007 in a more recent article focusing on the effects of activism on plant productivity and capital reallocation.<sup>17</sup> The three of us, working with Robert Jackson, have recently used this dataset to study pre-disclosure accumulations of stock by hedge fund activists.<sup>18</sup> Thus, this database has proven fruitful for previous analysis of several issues, and in this paper we extend it to study the long-term effects of hedge fund activism.

The dataset includes information drawn from disclosures required to be filed under Section 13(d), which are typically made on the SEC's Schedule 13D.<sup>19</sup> To begin, the dataset was constructed by first identifying all of the investors that filed Schedule 13Ds between 1994 and 2007. Then, based on the names and descriptions of the filers required to be disclosed under Item 2 of Schedule 13D,<sup>20</sup> filer types such as banks, insurance companies, mutual funds, and other non-activist investors were excluded from our sample. In addition, based on the description of

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<sup>14</sup> Alon Brav, Wei Jiang, Frank Partnoy and Randall Thomas, *Hedge Fund Activism, Corporate Governance, and Firm Performance*, 63 J. FIN. 1729 (2008).

<sup>15</sup> Alon Brav, Wei Jiang, Frank Partnoy and Randall Thomas, *The Returns to Hedge Fund Activism*, 64 FIN. ANALY. J. 45 (2008).

<sup>16</sup> Alon Brav, Wei Jiang & Hyunseob Kim, *Hedge Fund Activism: A Review*, 4(3) FOUNDATIONS AND TRENDS IN FINANCE 185 (2009).

<sup>17</sup> Alon Brav, Wei Jiang & Hyunseob Kim, *The Real Effects of Hedge Fund Activism: Productivity, Asset Allocation, and Product Market Concentration* (May 23, 2013), available at [http://papers.ssrn.com/sol3/papers.cfm?abstract\\_id=2022904](http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2022904).

<sup>18</sup> Lucian Bebchuk, Alon Brav, Robert Jackson and Wei Jiang, *Pre-Disclosure Accumulations by Activist Investors: Evidence and Policy*, 39 J. CORP. L. (forthcoming) (2013).

<sup>19</sup> See SECURITIES AND EXCHANGE COMMISSION, FORM OF SCHEDULE 13D, 17 C.F.R. § 240.13d (2010).

<sup>20</sup> See *id.* at Item 2 (requiring a description of the “name[,] principal business[, and] address of [the] principal office” of the filer).



the purpose of the investment required to be included in Item 4,<sup>21</sup> events where the purpose of the investor is to be involved in a bankruptcy or reorganization due to financial distress, the purpose of the filer is to engage in merger or acquisition-related risk arbitrage, or the security in which the investment is made is not a common share were also excluded.

In addition, extensive news searches were conducted using the hedge fund and company names drawn from Schedule 13D. These searches allow for the inclusion in the dataset of additional information not available in the Schedule 13Ds, such as the hedge fund's motive and the target company's response. Due to these searches, the dataset includes instances in which hedge funds maintained an activist position in a large public company but owned less than 5% of the company's stock (and, thus, were not required to file a Schedule 13D).<sup>22</sup>

In this paper, we use this dataset to provide the first systematic evidence on the long-term effects of hedge fund activism. To this end, we supplement the dataset of activist filings with data on operating performance and stock returns of the companies targeted by activist interventions. We use standard sources – COMPUSTAT for operating performance data and CRSP for stock return data. This enables us to study the long-term effects of activist interventions on both operating performance and shareholder wealth.

In particular, we seek to study long-term results during the five years following the activist intervention. We have data on the operating performance and stock returns of public companies through the end of 2012. Thus, because 2007 is the last year for which we have data on interventions, we have data on the stock return and operating performance of public companies during the five years following each of the activist events in our dataset. In the analysis below, we track each company for up to five years and for as long as it remains public within that period.<sup>23</sup>

Table 1 below provides summary data on 1,886 Schedule 13D filings by activist hedge funds during the period 1994-2007. As Table 1 shows, there has been an increase in the frequency of activist hedge funds filings over time. Furthermore, the dataset includes a

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<sup>21</sup> See *id.* at Item 4 (requiring investors to disclose the “[p]urpose of [the t]ransaction,” including, *inter alia*, any plans relating to the acquisition of additional stock or a corporate event such as a merger or acquisition).

<sup>22</sup> Because of the significant amount of capital required to own 5% or more of the stock of a large public company, relying exclusively on Schedule 13D filings might exclude cases in which outside investors maintained significant holdings of stock. Thus, our sample includes 42 events in which the activist hedge fund did not file a Schedule 13D because it held less than 5% of the stock of the target company. For further discussion, see Alon Brav, Wei Jiang, Frank Partnoy & Randall Thomas, *Hedge Fund Activism, Corporate Governance, and Firm Performance*, 63 J. FIN. 1729, 1739 (2008). For a more detailed description of the procedure for assembling the dataset, see Alon Brav, Wei Jiang and Hyunseob Kim, *Hedge Fund Activism: A Review*, 4(3) FOUNDATIONS AND TRENDS IN FINANCE 185, 193-5 (2009).

<sup>23</sup> As is the case for public companies in general, many of the companies that are targets of activist interventions stop being independent public companies over time. In particular, within five years, targets of activist intervention have “attrition” rates of about 47%, which is similar to the 44% five-year attrition rate of public companies in general.

significant number of filings in nearly every year, with more than 100 filings in every year except for four throughout our fourteen-year sample. Furthermore, except for the first two years, 1994 and 1995, the dataset includes more than 80 filings for each year in our study.

**Table 1: Incidence of 13D Filings by Activist Hedge Funds**

Year	Number of 13D Filings by Hedge Fund Activists	Year	Number of 13D Filings by Hedge Fund Activists
1994	9	2001	88
1995	31	2002	126
1996	86	2003	117
1997	198	2004	139
1998	152	2005	213
1999	111	2006	248
2000	110	2007	258
Total, 1994-2000	697	Total, 2001-2007	1189

The dataset described in this Section has two features that make it especially useful for the study of our subject. First, it is comprehensive and includes all hedge fund activist interventions during a substantial period of time, thus avoiding the questions that could arise if one were to use a sample or otherwise select a subset of interventions. Second, with nearly 2,000 interventions, the dataset provides us with a large number of observations that facilitates statistical testing.

### III. OPERATING PERFORMANCE

#### *A. Measures of performance*

We pay special attention to Tobin's Q and ROA which are very commonly used measures in financial economics. Tobin's Q, named after Noble-prize-winner James Tobin, is a measure that reflects the effectiveness with which a company turns a given book value into market value accrued to investors.<sup>24</sup> Tobin's Q, often referred to as Q for simplicity, has been extensively used in studies that seek to measure the efficiency of governance arrangements, ownership structures, or investor protection rules.<sup>25</sup>

<sup>24</sup> Tobin's Q is measured as the ratio of market value of equity and book value of debt to the book value of equity and book value of debt.

<sup>25</sup> See, e.g., Randall Morck, Andrei Shleifer, and Robert Vishny, *Management Ownership and Market Valuation: An Empirical Analysis*, 20 J. FINANC. ECON. 293 (1988); David Yermack, *Higher Market Valuation for Firms with a Small Board of Directors*, 40 J. FINANC. ECON. 185 (1996); Robert Daines, *Does Delaware Law Improve Firm Value?* 62 J. FINANC. ECON. 559 (2001).

ROA refers to return on assets as the ratio of earnings before interest, taxes, depreciation and amortization to the book value of assets. ROA reflects the earning power of a business and thus the effectiveness with which the firm uses assets of a given book value to generate earnings for investors. Like Q, ROA has been significantly used by financial economists as a metric for operating performance.<sup>26</sup>

Industries differ significantly in their levels of Q and ROA. For example, old economy firms have lower Q levels, and higher ROA levels, than new economy firms. In examining the quality of a firm’s governance or management, it is natural to compare it to its industry peers. Accordingly, it is common to look at a firm’s industry-adjusted level of Q or ROA – that is, the difference between the firm’s level and the industry’s mean or median level. A positive level of industry-adjusted Q or ROA indicates that the firm outperforms its industry peers on this dimension, and, conversely, a negative level indicates under-performance.

*B. Operating Performance Following Activist Interventions*

We begin by looking at the operating performance of firms that experienced activist intervention at different points in time relative to the time of the intervention. In particular, we examine operating performance in the five-year period following the intervention.

Table 2 below reports the levels of Q and ROA at such different points in time. The column labeled t refers to performance in the year of the intervention. Columns labeled t+1, t+2, and so forth represent years after the intervention. We initially report just raw figures that are not adjusted for the industry. For each year, we report the average and the median level of the metric across our sample.<sup>27,28</sup>

**Table 2: Operating Performance Pre- and Post-Intervention – No Industry Adjustment**

<b>Panel A: ROA</b>						
	t: Event Year	t+1	t+2	t+3	t+4	t+5
Average	0.022	0.034	0.038	0.048	0.049	0.046
std. err.	0.006	0.005	0.006	0.006	0.007	0.010
Median	0.069	0.075	0.073	0.083	0.091	0.089
Observations	1584	1363	1187	1055	926	694
<b>Panel B: Q</b>						
	t: Event Year	t+1	t+2	t+3	t+4	t+5
Average	2.074	2.010	2.033	2.086	2.128	2.190
std. err.	0.057	0.059	0.066	0.071	0.077	0.092
Median	1.373	1.332	1.316	1.363	1.347	1.412
Observations	1611	1384	1206	1076	942	710

<sup>26</sup> See, e.g., Paul Gompers, Joy Ishii, and Andrew Metrick, *Corporate Governance and Equity Prices*, 118 QUART. J. ECON. 107 (2003); Lucian Bebchuk, Alma Cohen and Charles C.Y. Wang, *Learning and the Disappearing Association between Governance and Returns*, 108 J. FINANC. ECON. 323 (2013).

<sup>27</sup> As is standard, when calculating averages, we pre-winsorize the sample at the 1% and 99% extremes.

<sup>28</sup> The number of hedge fund activism observations in table 2 is lower than in Table 1, since some target firms are not covered by COMPUSTAT for certain years.

Table 2 indicates that, on average, ROA is higher in each of the five years following the intervention year than in the year of the intervention. Furthermore, we do not see the patterns feared by those advancing the myopic activists claim – that is, an initial spike in operating performance followed by a decline to below intervention-year levels. Indeed, while average ROA is higher in each of the two years following the intervention than in the intervention year, it is higher still in each of the subsequent three years. The average ROA is more than double the average ROA in the year of intervention in each of the years t+3, t+4, and t+5.

Table 2 also shows that, focusing on average Q as a metric of operating performance, the operating performance at t+3, t+4, and t+5 is better, on average, than at the intervention year. While average Q declines somewhat in the first two years following the intervention year, average Q exceeds its event year level at t+3, t+4, and t+5 and reaches its highest level at t+5.

As noted earlier, researchers commonly base their analysis not on “raw” levels of Q and ROA but rather on industry-adjusted levels. Performance is best assessed in comparison to the company’s industry peers. Accordingly, we present in Table 3 below an analysis based on industry-adjusted levels. We identify for each company the firms with the same SIC three-digit industry classification. We define the industry-adjusted level of Q and ROA as equal to the difference between the raw Q or raw ROA level and the industry average Q or ROA.<sup>29,30</sup>

Table 3 below presents the evolution of average industry-adjusted ROA and Q over time among the targets of hedge fund activists. As before, we report levels for the intervention year and each of the five years following the intervention year.

**Table 3: Industry-Adjusted Operating Performance Pre- and Post-Intervention**

<b>Panel A: Industry-adjusted ROA, with benchmark = industry average</b>						
	t: Event year	t+1	t+2	t+3	t+4	t+5
Average	-0.028	-0.013	-0.010	-0.004	-0.005	-0.009
std. err.	0.005	0.005	0.005	0.006	0.006	0.009
Median	-0.005	-0.002	0.001	0.000	0.005	0.002
Skew	-3.394	-1.660	-1.553	-2.701	-2.790	-7.144
Observations	1584	1363	1187	1055	926	694
<b>Panel B: Industry-adjusted Q, with benchmark = industry average</b>						
	t: Event year	t+1	t+2	t+3	t+4	t+5
Average	-0.440	-0.396	-0.325	-0.311	-0.192	-0.097
std. err.	0.054	0.053	0.058	0.059	0.065	0.084
Median	-0.643	-0.497	-0.439	-0.490	-0.415	-0.398
Skew	3.689	3.587	3.351	3.576	3.321	3.399
Observations	1611	1384	1206	1076	942	710

<sup>29</sup> Industry average levels differ somewhat from industry median levels because both ROA and Q are skewed. ROA is significantly skewed to the left and, as a result, the average is lower than the median. Q is significantly skewed to the right and, as a result, the benchmark average is higher than the median.

<sup>30</sup> The Industry averages are calculated after pre-winsorizing the firm Q and ROA. In an earlier version of this paper, we calculated the industry averages Q and ROA before pre-winsorization. The two results are qualitatively similar.

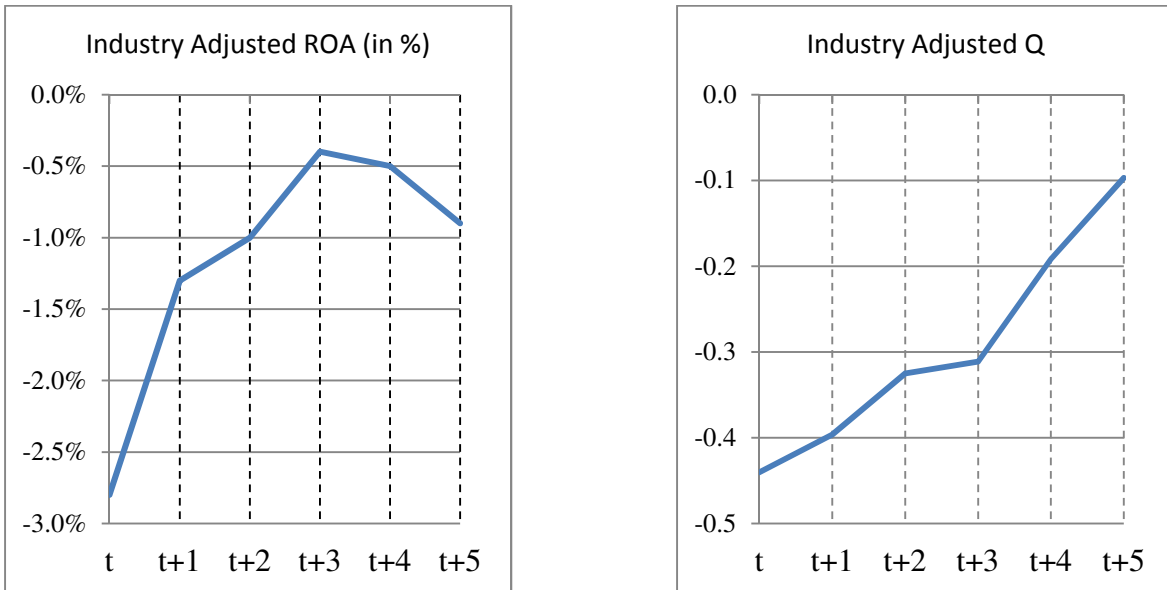
Table 3 indicates that targets of activist interventions tend to under-perform at the time of the intervention. In the year of intervention, both the average industry-adjusted ROA and the industry-adjusted Q are negative.<sup>31</sup>

Furthermore, and most importantly for the purposes of our inquiry in this paper, Table 3 displays clear patterns of improved operating performance relative to industry peers during the five years following activist interventions. As Panel A shows, the average industry-adjusted ROA increases over time during the five-year period following the intervention year. Indeed, average industry-adjusted ROA is higher in each of the five years following the intervention than in the year of intervention. Furthermore, the increase is of significant magnitude and closes most of the under-performance relative to industry peers at the time of the intervention.

Panel B displays a similar pattern with respect to average industry-adjusted Q. The average industry-adjusted Q increases over time during the five-year period following the intervention year. Furthermore, average industry-adjusted Q is higher in each of the five years following the intervention than in the year of intervention, and the increase during the five years is of significant magnitude relative to the under-performance at the time of the intervention. Last, as with the change in ROA, the increase in Q closes most of the under-performance relative to industry peers at the time of the intervention.

Finally, Figure 1 displays graphically the results presented in Table 3. In particular, the Figure plots the evolution of industry-adjusted ROA and industry-adjusted Q. The graphs vividly display the increasing patterns of Q and ROA during the years following the intervention.

**Figure 1: The Evolution of ROA and Q Over Time**



<sup>31</sup> In addition, note that the median industry-adjusted ROA and the median industry-adjusted Q are also both negative.

*C. Regression Analysis*

We now turn to a regression analysis of the evolution of ROA and Q over time. This analysis enables us to control for other factors that might be relevant and to assess the statistical significance of our results.

Table 4 below displays the results of four regressions. In column (1) and (2), we run a regression in which the dependent variable is ROA. The adjustment for industry performance is made by including industry (or firm) fixed effects. In both regressions we include as explanatory variables dummy variables representing the year of intervention as well as each of the subsequent five years.

As controls, we use in both regressions the company's market value and age, year fixed effects to account for time trends in the values of ROA and Q and the impact of macroeconomic factors, and dummy variables for each of the three years preceding the intervention year. In regression (1) we include industry fixed effects. As a result, the coefficients on the key variables  $t, t+1, \dots, t+5$ , should be interpreted as a "difference-in-difference." It is as if we first take a difference of each firm-year ROA against the average level of all firms in the same year and the sample average of all firms in the same industry. We then estimate the difference between the industry-and-year-adjusted ROA of firms targeted in the current and next five years and that of the non-target firms while holding constant company size and age.

In regression (2) we include a dummy for each firm, running a firm fixed effect regression, to account for time-invariant factors unique to each firm. Under such a specification, the coefficients on the key variables,  $t, t+1, \dots, t+5$ , should be interpreted as the excess performance of a target firm, during years  $t$  to  $t+5$ , over its own all-time average and adjusted for market-wide conditions (due to the year fixed effects). Firm fixed effects automatically subsume industry fixed effects.

In columns (3) and (4) we run regressions that are identical to those in (1) and (2) respectively except that the dependent variable is now Q rather than ROA. Thus, regression (3) includes industry fixed effects and regression (4) includes firm fixed effects.

**Table 4: Evolution of ROA and Q over Time**

Dependent Variable	(1) ROA	(2) ROA	(3) Q	(4) Q
t: Event year	-0.007 (-1.363)	-0.013*** (-3.036)	-0.346*** (-6.357)	-0.016 (-0.341)
t+1	0.010** (2.075)	0.002 (0.543)	-0.271*** (-4.996)	0.066 (1.296)
t+2	0.016*** (3.223)	0.007 (1.510)	-0.175*** (-2.951)	0.178*** (3.342)
t+3	0.020*** (3.678)	0.010* (1.886)	-0.065 (-1.014)	0.255*** (4.444)
t+4	0.013** (2.112)	0.007 (1.415)	0.029 (0.433)	0.304*** (5.083)
t+5	0.008 (0.954)	0.002 (0.348)	0.118 (1.385)	0.338*** (4.520)
ln(MV)	0.036*** (90.124)	0.043*** (39.843)	0.242*** (65.065)	0.840*** (76.917)
Ln(Age)	0.021*** (28.254)	0.009*** (5.653)	-0.275*** (-40.435)	-0.345*** (-22.375)
Year FE	Y	Y	Y	Y
SIC 3 FE	Y		Y	
Firm FE		Y		Y
Pre-event dummies (t-1,t-2,t-3)	Y	Y	Y	Y
Observations	126,742	126,742	129,966	129,966
R-squared	0.202	0.702	0.191	0.626
F-Tests:		F Tests:		
[t+3] – t	12.9	13.8	[t+3] - t	11.1
p-val	0.0%	0.0%	p-val	0.0%
[t+4] – t	6.2	10.78	[t+4] - t	18.5
p-val	1.3%	0.1%	p-val	0.0%
[t+5] – t	2.3	3.9	[t+5] - t	20.9
p-val	12.6%	5.0%	p-val	0.0%

The results of the regressions are consistent with the view that targets of activist interventions tend to under-perform at the time of the intervention. The coefficient of the event year is negative in both of the ROA regressions (1) and (2), and is statistically significant at the 99% confidence in regression (2), indicating that target firms were performing significantly below their own “normal” levels at the time of intervention. Similarly, the coefficient of the event year t is negative in both of the Q regressions (3) and (4) and is statistically significant at the 99% level in regression (3), indicating that target firms command valuations (relative to their book values) that were considerably below their industry peers at the time of intervention

Most importantly for the purposes of our inquiry in this paper, there is no evidence for the post-intervention decline in operating performance feared by those making the myopic activist claims. Indeed, the results are consistent with the view that operating performance

improves steadily during the years following the intervention relative to the intervention time. Indeed, in each of the four regressions, each of the coefficients for the dummy variables representing the years  $t+1$ ,  $t+2$ ,  $t+3$ ,  $t+4$ , and  $t+5$  is higher than the coefficient for the event year.

Because holders of the myopic activists focus on declines in operating performance, we pay special attention to the coefficients for  $t+3$ ,  $t+4$ , and  $t+5$ . In particular, for each of the four regressions, we conduct F-tests for the difference between each of these coefficients and the event year coefficient. In each of the tests, each of the  $t+3$ ,  $t+4$ , and  $t+5$  coefficients is higher than the event year coefficient and the difference is statistically significant at the 90% confidence level in eleven of the twelve F-tests we conduct and, indeed, significant at the 99% confidence level in nine of these tests.

Finally, looking at the coefficients for the pre-intervention years used as controls (not tabulated), we note that, in three of the four regressions, these coefficients decline from  $t-3$  to the event year  $t$ . Indeed, F-tests conducted for these three regressions indicate that the difference between the event year coefficient and the  $t-3$  coefficient is negative and significant at a confidence level of 95% or higher. This suggests that the operating performance of the target of activist intervention was trending in a negative direction during the period preceding the intervention and that the intervention was followed by a reversal of this trend.

#### *D. Summary*

##### *1. A Clear Pattern*

The analysis above establishes a clear pattern. To begin, activists do not generally target well-performing companies. Targets of activism tend to be companies whose operating performance was below industry peers or their own historical levels at the time of intervention. Indeed, at the time of the intervention the targets seem to be in a negative trend with operating performance declining during the three years preceding the intervention.

Furthermore, during the five years following the intervention, we find no evidence for the concerns that activists push companies to increase short-term earnings at the expense of operating performance in subsequent years. To the contrary, we find that the activist intervention is followed by systematic improvement in operating performance relative to industry peers during this five-year period. Indeed, operating performance exceeds the intervention year level in each of the years three, four, and five following the intervention; these improvements are both statistically significant and economically meaningful.

Thus, the evidence is inconsistent with the view that activists push for and produce short-term increases in earnings at the expense of worse operating performance later on. Activist interventions are followed in the long-term by improved, not worse, operating performance.

##### *2. Causality vs. Stock Picking*



In response to the above findings, critics of hedge fund activism might argue that the identified association between activist intervention and subsequent improvements in operating performance does not by itself demonstrate a causal link. It could merely reflect the activists' tendency to choose targets whose operating performance is expected to increase in any event. Under such a scenario, the improvement in long-term performance experienced by targets reflect the activist's "stock picking" ability rather than the activist's impact on the company's operating performance.

We would like to stress at the outset that accepting that activist interventions are followed by improvements in operating performance, and merely questioning whether activists should "get credit" for these improvement, would already concede that the long-term consequence of activism provide no basis for calls to limit the influence of activism and to insulate boards from such influence. Such calls have been premised on the claim that activist interventions are followed by (and bring about) declines in long-term operating performance. To the extent that interventions are followed by improvements in operating performance, there is no reason to seek to limit the influence of the activists regardless of how much credit they should be getting for the improvements. Stock pickers who successful bet on future improvements might not deserve a medal, but certainly do not warrant criticism and opposition.

However, there are reasons to believe that the identified improvements in operating performance are at least partly due to the activist interventions. To begin, the activist themselves clearly believe that their activities contribute to the subsequent improvements in operating performance. Activist activities involve significant costs, and activists would thus have strong incentive to avoid bearing them if they believed that the improvements in performance would ensue even without them engaging in such activities. They would just buy a stake, avoid intervening in how the company is run, and passively capture the benefits of the improved performance expected to take place.

Furthermore, the view that the interventions contribute to the subsequent improvements is consistent with the finding in earlier work co-authored by the two of us that such improvements do not take place after outside blockholders pursuing a passive strategy announce the purchase of a block of shares, but occur after a subset of the blockholders switch from passive to activist stance.<sup>32</sup> This finding is also consistent with the view that the patterns we identify above are at least partly a product of the activists' work and not merely a reflection of their foresight in choosing targets.

In any event, causality issues in corporate governance and finance are notoriously difficult to resolve confidently and are not a main focus of this paper. Our chief interest is in investigating empirically whether the long-standing claim that activist interventions are followed by declines in long-term operating performance are backed by the evidence. Because this claim has been the basis for calls to limit the rights and involvement of activist investors, determining

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<sup>32</sup> See Alon Bray, Wei Jiang and Hyunseob Kim, *The Real Effects of Hedge Fund Activism: Productivity, Asset Allocation, and Industry Concentration*, NBER WORKING PAPER No. w17517 (2011).

its validity is important for the ongoing policy debate. Our results provide a clear answer: this long-standing claim is not supported by the data.

#### IV. STOCK RETURNS

We now turn to examine the long-term returns to the shareholders of companies targeted by hedge fund activists. Opponents of activism believe that the initially positive stock market reaction to activist interventions represent inefficient, myopic market pricing that fails to reflect the subsequent negative returns that are experienced by long-term shareholders and make such shareholders worse off. Such opponents also hold the view that, while activists might benefit from capturing positive stock price returns prior to their departure, negative long-term stock returns follow this exit, leaving long-term shareholders holding the bag. In this Part, we subject these claims to an empirical test.

Section A begins by confirming the short-term pattern conceded by opponents of activism, i.e., the positive abnormal stock returns that such shareholders enjoy during the 40-day period surrounding the filing of Schedule 13D by an activist hedge fund. Section B investigates empirically the claim that these initial gains are wiped out by significant negative returns in subsequent years. Section C examines empirically the claims that negative long-term returns follow the departure of activists. Finally, Section D concludes.

##### *A. Short-Term Returns*

We begin by examining in our sample the stock price movements that accompany the announcement of the activist campaign. That is, we document the initial stock price spike that activism opponents argue to be reversed in the long term.

The initial spike we confirm below has been extensively documented by prior work. This pattern was first documented in an empirical study co-authored by two of us,<sup>33</sup> as well as in a study conducted by April Klein and Emanuel Zur.<sup>34</sup> These initial findings were corroborated by three subsequent studies—by Nicole Boyson and Robert Mooradian,<sup>35</sup> Christopher P. Clifford,<sup>36</sup> and Robin Greenwood and Michael Schor.<sup>37</sup> Each of these studies found that 13D filings by activists were accompanied by positive stock market reactions.<sup>38</sup>

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<sup>33</sup> See Alon Brav, Wei Jiang, Frank Partnoy, and Randall Thomas, *Hedge Fund Activism, Corporate Governance, and Firm Performance*, 63(4) J. FIN. 1729, 1741–45 (2008).

<sup>34</sup> April Klein and Emanuel Zur, *Entrepreneurial Shareholder Activism: Hedge Funds and Other Private Investors*, 64 J. FIN., 187 (2009).

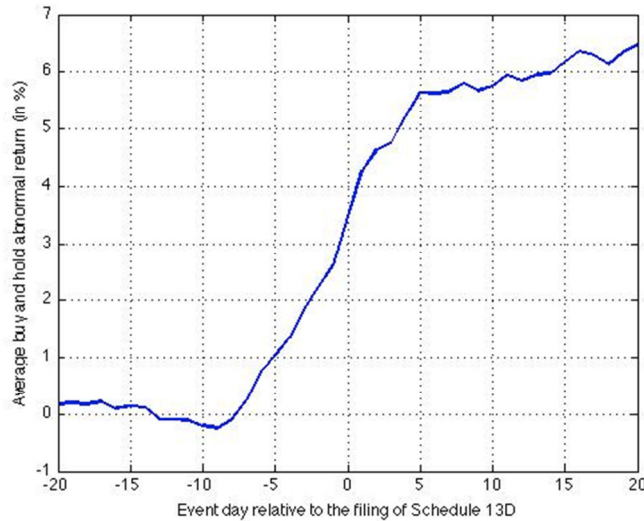
<sup>35</sup> Nicole M. Boyson and Robert M. Mooradian, *Intense Hedge Fund Activists*, WORKING PAPER (2009), available at [ssrn.com/paper=1492641](http://ssrn.com/paper=1492641).

<sup>36</sup> Christopher P. Clifford, *Value Creation or Destruction? Hedge Funds as Shareholder Activists*, 14 J. CORP. FIN. (2008).

<sup>37</sup> Robin Greenwood and Michael Schor, *Investor Activism and Takeovers*, 92 J. FINANC. ECON., 362–75 (2009).

Although our focus is on long-term results, we begin by confirming this effect in our extended sample. Figure 2 below describes the average abnormal buy-and-hold returns in a 40-day window surrounding the filing of a Schedule 13D. This period begins twenty days before an activist hedge fund files a schedule through twenty days afterwards.

**Figure 2: short-term stock returns around 13d filings**



As the Figure shows, the average abnormal returns observed during the 20-day period before and after an investor files a Schedule 13D are approximately 6%, a magnitude consistent with the findings in prior work. The initial market reaction to the announcement of an activist stake views this development as “good news.” And this market reaction is consistent with the view that hedge fund activists provide benefits to, rather than impose costs on, the targets of their campaigns.<sup>39</sup>

Opponents of activism do not contest the evidence that activist interventions are accompanied by positive short-term stock returns but rather dismiss its significance. Lipton, for example, argues that the important question is, “[f]or companies that are the subject of hedge fund activism and remain independent, what is the impact on ... stock price performance relative to the benchmark, not just in the short period after announcement of the activist interest, but after a 24-month period.”<sup>40</sup>

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<sup>38</sup> Similarly, Marco Becht, Julian Franks, Colin Mayer, and Stefano Rossi studied activist engagement by the Hermes U.K. Focus Fund and found that positive and significant abnormal short-term returns (about 5% in the seven-day event window) accompanied the announcement of changes produced by such engagement. See Marco Becht, Julian R. Franks, Colin Mayer, and Stefano Rossi, *Returns to Shareholder Activism: Evidence from a Clinical Study of the Hermes U.K. Focus Fund*, 23(3) REV. FIN. ST., 3093 (2009).

<sup>39</sup> See, e.g., Alon Brav, Wei Jiang and Hyunseob Kim, *Hedge Fund Activism: A Review*, 4(3) FOUNDATIONS AND TRENDS IN FINANCE 185 (2009).

<sup>40</sup> *Id.*

Opponents of activism believe that (i) the elevated stock price levels following 13D filings represent inefficient market pricing that fails to perceive the expected long-term costs of the intervention, (ii) as a result, the initial spike is expected to be followed in the long term by negative abnormal stock returns, and (iii) these negative returns are so large that they wipe out the initial spike and make long-term shareholders worse off. To the assessment of this claim we will now turn.

### *B. Subsequent Reversal?*

Clearly, the above proposition has empirical implications that make it testable using publicly available data. Surprisingly, however, opponents of activism have not tried to test this key proposition empirically or to commission or encourage such testing by others. Nor have those holding this view provided any empirical support for the reversal and long-term underperformance they assert.

Although opponents of activism holding this view have failed to provide empirical evidence backing it, some have stressed that it is strongly confirmed by their experiences. Martin Lipton, for example, recently wrote that his short-termism concerns are based on “the decades of [his and his] firm’s experience in advising corporations.”<sup>41</sup> In our view, however, arguments and policy decisions should not be based on reported individual experience and felt intuitions concerning long-term returns. Data on stock returns is generally publicly available and accessible and it makes it possible to subject claims about returns to a rigorous and objective test.

Below we engage in such testing. We examine returns to the shareholders of targets of activist interventions in the five years following the initial stock price spike accompanying the intervention. We look for evidence of the asserted long-term reversal that is believed to make long-term shareholders worse off.

In investigating the presence of negative abnormal long-term returns, we employ three standard approaches used by financial economists for detecting underperformance relative to the risks involved. First, in Subsection 1, we examine whether the returns to targeted companies were systematically lower during the considered five-year period than what would be expected given standard asset pricing models. Second, in Subsection 2, we examine whether the long-term returns to targeted companies were lower than those of “matched” firms—that is, firms that are similar in terms of size and book to market. Third, in Subsection 3, using a portfolio approach, we examine whether a portfolio that took a position in each targeted company after the 13D announcement window—and retained this position for the subsequent five years—underperformed relative to its risk characteristics.

Using each of these methods, we look for evidence of the asserted long-term underperformance of companies that were the targets of activist interventions. As we discuss

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<sup>41</sup> See Wachtell, Lipton Memorandum, *Bite the Apple; Poison the Apple; Paralyze the Company; Wreck the Economy*, REPROD. THE HARVARD LAW SCHOOL FORUM ON CORPORATE GOVERNANCE AND FINANCIAL REGULATION (Feb. 26, 2013).

below, we find no evidence for the existence of the asserted long-term negative returns in the data.

### 1. Individual Firm Regressions

We first examine stock returns for each individual firm. Of course, to identify whether stock returns are abnormally low or high, one needs a benchmark of comparison. Such benchmarks of comparison are provided by the Capital Asset Pricing Model,<sup>42</sup> and the Fama-French-Carhart asset-pricing model.<sup>43</sup> These models provide a prediction of the return that “normally” would be expected for a given security during a given period and thus enable identifying “abnormal” returns.

In particular, using the Capital Asset Pricing Model, the standard procedure is to estimate an “alpha,” the average excess return that is not explained by comovement with the market.<sup>44</sup> Similarly, using the Fama-French-Carhart four-factor model, the standard procedure is to estimate an “alpha,” the average excess return that is not explained by the four market-wide factors identified in seminal works by Fama and French and by Carhart.<sup>45</sup>

For each of the firms that were the targets of activist interventions, we estimate a monthly alpha, or abnormal return, for the three years prior to month of the intervention. In addition, we estimate monthly alphas for the three years following the month of the intervention and the five years following the month of the intervention.<sup>46</sup> To the extent that firms delist from the sample we incorporate into the performance measurements in this section information on delisting returns from the Center for Research in Security Pricing (CRSP).

Table 5 below provides results concerning the alphas we have calculated. For each of the periods, we provide both the median and average alpha for all the firms in our sample. We also

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<sup>42</sup> See RICHARD A. BREALEY, STEWART C. MYERS & FRANKLIN ALLEN, *PRINCIPLES OF CORPORATE FINANCE*, (9th ed., McGraw-Hill, 2007).

<sup>43</sup> See Eugene F. Fama and Kenneth R. French, *Common Risk Factors in the Returns on Stocks and Bonds*, 33 J. FINANC. ECON. 3 (1993); Mark M. Carhart, *On Persistence in Mutual Fund Performance*, 52 J. FIN. 57 (1997).

<sup>44</sup> Specifically, we estimate for each firm  $i$  an alpha using the regression:

$$r_{it} - r_f = \alpha_i + \beta_{i1}RMRF_t + \varepsilon_{it}.$$

<sup>45</sup> Specifically, we estimate for each firm  $i$  an alpha using the regression:

$$r_{it} - r_f = \alpha_i + \beta_{i1}RMRF_t + \beta_{i2}SMB_t + \beta_{i3}HML_t + \beta_{i4}MOM_t + \varepsilon_{it}.$$

We obtain the factor returns and monthly risk-free rates from Ken French’s web site at Dartmouth College.

<sup>46</sup> We do such estimation for all firms that have a minimum of 24 monthly returns following the intervention (i.e., all firms that remained public for at least 24 months following the month of the intervention) so that there is a significant number of monthly returns on which a regression can be based. We note that, for the few events in our sample in which the hedge fund did not file a Schedule 13D, we use the month in which the activism was made public via news searches as the monthly intervention.

indicate the statistical significance of our results; however, as is now well-known in the financial economics literature,<sup>47</sup> the standard error of the average of the estimated alphas understates the unobserved variability in performance, and the reported t-stats should thus be treated as merely suggestive.

**Table 5: Firm-level estimates of abnormal performance using market pricing models**

<b>Panel A: CAPM alphas</b>			
	Holding period		
	[-36,-1]	[+1,+36]	[+1,+60]
Median	-0.19	0.53	0.65
Average	-0.09	0.50	0.37
Stdev	2.69	3.06	2.66
t-stat	1.33	6.06	5.23
Observations	1563	1386	1397
<b>Panel B: Four-factor alphas</b>			
	Holding period		
	[-36,-1]	[+1,+36]	[+1,+60]
Median	-0.35	0.29	0.40
Average	-0.21	0.32	0.16
Stdev	2.91	3.38	2.94
t-stat	2.92	3.52	2.07
Observations	1563	1386	1397

The first column in Table 5 provides our results concerning stock returns during the three-year period preceding the intervention. Using both pricing models, we find an alpha during this period that is negative and economically meaningful. These results, like those obtained concerning operating performance in Part V, are consistent with the view that hedge fund activists target under-performing companies.

The second and third columns provide results concerning stock returns during the three-year period following intervention and the five-year period following intervention. The average of the estimated alpha is positive and statistically significant when we use both the CAPM model and the Fama-French-Carhart four-factor model. The results provide no support for the negative returns during these periods hypothesized by opponents of activism.

## 2. Buy and Hold Abnormal Returns

<sup>47</sup> See, e.g., Eugene Fama, *Market Efficiency, Long-Term Returns, and Behavioral Finance*, 49 J. FINANC. ECON. 283 (1998).

In the preceding analysis we have focused on regression intercepts as estimates of monthly abnormal performance subsequent to activists’ intervention. We now report average buy and hold abnormal return as an alternative measure of abnormal performance.<sup>48</sup>

Specifically, for each event, we compute the buy and hold return over a predetermined holding period after the intervention net of a benchmark return that is meant to capture the event firm’s expected return. In particular, for each event firm, we use information on its pre-event market capitalization and book-to-market to match it to one of the Fama and French 25 size and book-to-market value-weight portfolios.

Since the target firm’s market capitalization and book to market ratio change over the subsequent holding period we allow the benchmark portfolio to change by using the new firm attributes in every subsequent year. In those cases in which a target firm is missing a book to market ratio in a given year we impute the value from the previous year and if, missing, two years earlier. Finally, if a target firm delists prior to the chosen investment horizon we reinvest the proceeds in the market portfolio (the Fama and French value weight portfolio, “RM”) and similarly reinvest the benchmark return to that point in the market as well.

The results are reported in Table 6 below. The Table provides both equal- and value-weight average buy and hold abnormal return beginning in the first month post intervention. As in the preceding subsection, we report results over three- and five-year holding periods following the month of the intervention.

**Table 6: Buy and hold abnormal return subsequent to hedge fund intervention**

Window	Average abnormal return		StDev	Observations
	Equal-weight	Value-weight		
[+1,+36]	7.24	3.46	5.30	1480
[+1,+60]	1.82	8.67	4.67	1449

Consistent with the regression-based evidence presented earlier the evidence indicates that the buy and hold abnormal returns are positive at both the three-year and five-year holding periods. We also report the cross-sectional standard deviation of abnormal returns in the column marked ‘StDev’. Not one of the averages that we report in the table would lead to a rejection of the null of no abnormal return.<sup>49</sup> Overall, the evidence in Table 6 is consistent with the view that there is no evidence of long-term underperformance of the stocks of target firms.

### 3. Portfolio Analysis

<sup>48</sup> See Brad M. Barber and John D. Lyon, *Detecting Long-Run Abnormal Stock Returns: The Empirical Power and Specification of Test-Statistics*, 43 J.FINANC. ECON. 341 (1997).

<sup>49</sup> It is well known that long-horizon abnormal returns are likely to be positively correlated. As a result, our use of the cross-sectional standard deviation assuming independence underestimates the true standard error.

Next, we turn to calendar-time portfolio regressions in which event firms are grouped into a portfolio that is traded in calendar-time and we estimate the portfolio's abnormal performance. For example, we form a [-36, -1] portfolio beginning in January 1994 by buying all firms that will be targeted by an activist hedge fund in three years' time and the firms are held until the month preceding the intervention before selling. Similarly, we form a [+1, +36] portfolio by buying all firms that were targeted by a hedge fund one month earlier and these firms are held for three years before selling. We form portfolios with both equal and value-weighting of firms' returns.

For each holding period and weighting scheme we estimate a regression of the resulting portfolio excess returns on the Fama-French RMRF, SMB, and HML factors and the momentum factor, MOM. Because the number of events in our sample shows a steady increase over the sample period we estimate the regression coefficients using weighted least squares using the number of events firms in a given calendar month as weights.<sup>50</sup>

As in the preceding estimation we focus on the regression intercept, the portfolio's alpha, as evidence for possible mean reversion in prices. Clearly, the portfolio in the pre-event window does not represent a tradable strategy. It is presented for an ex post analysis of the stock return patterns of the companies in the pre-targeting period.

Table 7 below provides the regression results. "Alpha" is the estimate of the portfolio intercept. "Beta" is the factor loading on the market excess return (the Fama and French RMRF). "SMB," "HML," and "MOM" are the estimates of factor loadings on the Fama-French size and book-to-market factors, and the Carhart momentum factor, respectively. We report t-statistics below the respective point estimates. "R2" is the adjusted R2 from the regressions and "N" is the number of monthly portfolio return observations. Panel A reports the results of equal-weighted portfolios and Panel B reports the results of value-weighted portfolios. We set a minimum of ten firms per month for all portfolios.

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<sup>50</sup> In our setting, such an approach is especially warranted because the number of observations fluctuates considerably during the years we consider. We also ran the tests of Table 7 without using such weight least squares and again did not find any evidence for negative and statistically significant abnormal returns during the five years following the initial spike.



**Table 7: Calendar-time portfolio regressions**

Panel A: Equal-weight four-factor model							
Window	Alpha	Beta	SMB	HML	MOM	N	R2
[-36,-1]	-0.23 (1.50)	0.93 (21.91)	0.85 (19.19)	0.34 (6.09)	-0.24 (7.72)	193	86.5%
[+1,+36]	0.25 (1.22)	0.87 (18.79)	0.87 (15.12)	0.25 (4.06)	-0.28 (7.80)	192	83.0%
[+1,+60]	0.24 (1.37)	0.90 (23.03)	0.81 (16.02)	0.27 (5.12)	-0.25 (8.36)	216	86.0%
Panel B: Value-weight four-factor model							
Window	Alpha	Beta	SMB	HML	MOM	N	R2
[-36,-1]	-1.16 (8.50)	1.08 (28.62)	0.60 (14.76)	0.29 (5.80)	-0.16 (5.71)	193	88.6%
[+1,+36]	0.16 (0.97)	0.98 (26.26)	0.54 (11.67)	0.25 (4.99)	-0.01 (0.47)	192	85.0%
[+1,+60]	-0.03 (0.18)	0.98 (28.88)	0.40 (9.08)	0.25 (5.49)	-0.00 (0.03)	216	85.0%

The results in Table 7 indicate that, in both Panel A and Panel B, the returns to activists targets are highly correlated with the SMB and HML factors, reflecting that targeted companies co-move with the returns of small value firms. Hence, by accounting for size and book to market, we are able to control for a significant part of the average return earned by target firms and thus increase the power of our tests to detect possible underperformance in the post-intervention period.

We examine post-intervention returns using four specifications – we look at both equal-weighted and value-weighted portfolios and for each we examine both a 3-year period and a five-year period. The alpha we obtained is positive and economically meaningful in three specifications and is negative but economically insignificant in the fourth specification. In all four specifications, the alpha coefficient is not statistically significant. We conclude that this testing approach also finds no evidence for the asserted long-term underperformance of activism targets.

#### 4. Summary

Overall, analyzing the publicly available data on stock returns provides no support for the myopic activists claim that activist intervention makes shareholders of target companies worse off in the long term. The emerging picture is that, taking a fully long-term perspective, the market does not fail to appreciate the long-term consequences of activism as insulation advocates fear it does. Rather, the stock appreciation accompanying activists' initial announcement reflects the market's correct anticipation of the intervention's effect, and the initial positive stock reaction is not reversed in the long term. The significant long-term losses to shareholders of activist targets on which insulation advocates have been resting their case on are not found in the data.

*C. Pump and Dump?*

*1. The Question*

We now turn to examining long-term returns following the decisions of activist hedge funds to start liquidating their holdings in the targets. Opponents of activism claim that, while such funds benefit from stock price appreciation during the period in which they hold a sizable stake, fortunes change once they dump their holdings. Following such departure, it is claimed, negative returns eventually follow and make long-term shareholders worse off.

There is evidence that investors in activist hedge funds have been making significant positive returns. A study in which two of us participated found that activist investors capture positive abnormal returns between the month prior to the Schedule 13D filing date and their exit date,<sup>51</sup> and a subsequent study by Boyson and Mooradian reached a similar conclusion.<sup>52</sup> Furthermore, another study in which two of us participated documented that activist hedge funds have outperformed the returns of equity-oriented hedge funds of similar size and age.<sup>53</sup>

Opponents of activism do not dispute that activist hedge funds and their investors benefit from activism. Rather, they base their opposition to activism on an alleged divergence between the payoffs of activist funds and the other shareholders in the companies that the activists target; the former are supposed to benefit at the expense of the latter. For such divergence to exist, activist hedge funds must succeed in getting out before the stock prices decline and impose losses on other shareholders. This view implies that activist targets experience negative abnormal returns in the years following activists' departure.

We should note that such negative returns are a necessary but not a sufficient condition for long-term shareholders bearing losses while activist hedge funds capture positive returns. To the extent that activist hedge funds bail out before such negative returns take place, (i) the returns to the long-term shareholders of the targets of activists' funds must be lower than (ii) the returns to the activists' hedge funds themselves. However, (i) might still be positive albeit smaller than (ii). Thus, negative returns in the aftermath of activists' departure is a necessary – but not a sufficient condition -- for the validity of the myopic activists claim.

The existence of the hypothesized negative returns provides another proposition that clearly can and should have been empirically tested using publicly available data. Still, opponents of activism have thus far failed to provide any empirical evidence on the subject. We therefore conduct such a test below.

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<sup>51</sup> See Alon Brav, Wei Jiang, Frank Partnoy, and Randall Thomas, *Hedge Fund Activism, Corporate Governance, and Firm Performance*, 63(4) J. FIN. 1729, 1741–45 (2008).

<sup>52</sup> Nicole M. Boyson and Robert M. Mooradian, *Intense Hedge Fund Activists*, WORKING PAPER (2009), available at [ssrn.com/paper=1492641](http://ssrn.com/paper=1492641).

<sup>53</sup> Alon Brav, Wei Jiang, Frank Partnoy and Randall S. Thomas, *The Returns to Hedge Fund Activism*, 64(6) FIN. ANAL. J. 45 (2008).

In particular, we focus on stock returns in the three years that follow an activists’ filing of a disclosure statement (an amendment to the Schedule 13D) indicating that the activist’s holding has fallen below the 5% threshold that subjects investors to significant disclosure requirements. We refer to such partial liquidation of activist stakes as “departures,” and we study what long-term returns follow during the three years following such departures.

## 2. Individual Firm Regressions

We first examine stock returns for each individual firm following the methodology used in Section IV.B.1 for studying stock returns for each individual firm following activist arrivals. As was done earlier, we examine stock returns both compared to the benchmark provided by the Capital Asset Pricing Model and the benchmark provided by the Fama-French-Carhart asset-pricing model. For each of the firms that were the targets of activist interventions, we estimate an alpha, or average abnormal return, for the three years following the month of the activist’s departure.<sup>54</sup>

Table 8 below provides results concerning the alphas we have calculated. For each of the benchmarks, we provide both the median and average alpha for all the firms in our sample. We also indicate the statistical significance of our results, but we remind the reader that the standard error of the average of the estimated alphas understates the unobserved variability in performance and the reported t-stats should thus be treated as merely suggestive.<sup>55</sup>

**Table 8: Firm-level estimates of abnormal performance using market pricing models**

Panel A: CAPM alphas	
Holding period	[+1,+36]
Median	0.75
Average	0.78
Stdev	3.35
t-stat	6.34
Observations	735
Panel B: Four-factor alphas	
Holding period	[+1,+36]
Median	0.63
Average	0.57
Stdev	3.41
t-stat	4.52
Observations	735

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<sup>54</sup> Similar to what was done in Section IV.B.1, we do such estimation for all firms that have a minimum of 24 monthly returns following the departure so that there is a significant number of monthly returns on which a regression can be based.

<sup>55</sup> See Eugene Fama, *Market Efficiency, Long-Term Returns, and Behavioral Finance*, 49 J. FINANC. ECON. 283 (1998).

The results reported in Table 8 indicate that, during the three-year period following activist departures, estimated alphas are positive and statistically significant. This is the case both for the CAPM model and the Fama-French-Carhart four-factor model. Thus, the data provides no support for the pump-and-dump patterns feared by holders of the myopic activists view.

### 3. Buy and Hold Results

As we did in Section IV.B.2, we next consider buy and hold abnormal return as an alternative measure of abnormal performance. We follow the same methodology as we used in producing the results displayed in Table 6. In particular, we compute the buy and hold return over a three-year period after the activist’s departure net of a benchmark portfolio, with the return and the benchmark portfolio computed and identified in the ways described in Section IV.B.2.

The results are reported in Table 9 below. As was done in Table 6, we report the results of both equal weight portfolios in which the results of all targets get an equal weight, and the results of value-weight portfolios in which the results of targets are value-weighted.

**Table 9: Buy and hold abnormal return subsequent to hedge fund exit**

Window	Average abnormal return		StDev	Observations
	Equal-weight	Value-weight		
[1,+36]	25.78	19.83	14.97	768

Consistent with the results based on individual firm regressions presented earlier, the results in Table 9 indicate that buy and hold abnormal returns are positive. This is the case both when using equal weights and when value-weighting. Thus, the results in Table 6 are consistent with the conclusion that pump-and-dump concerns are not supported by the data.

### 4. Portfolio Analysis

Finally, as we did in Section IV.B.3, we turn to calendar-time portfolio regressions in which event firms are grouped into a portfolio that is traded in calendar-time and we estimate the portfolio’s abnormal performance. In particular, we form portfolios by buying all firms that were targeted by a hedge fund one month after the departures of the activist and hold them for three years selling. We form portfolios with both equal and value-weighting of firms’ returns, and we estimate abnormal returns following the methodology described in Section IV.B.3.

Table 10 below provides the results. As before, “alpha” is the estimate of the portfolio intercept. Panel A reports the results of equal-weighted portfolios and Panel B reports the results of value-weighted portfolios.

**Table 10: Calendar-time portfolio regression estimates subsequent to the hedge fund exit**

Panel A: Equal-weight four-factor model							
Window	Alpha	Beta	SMB	HML	MOM	N	R2
[1,+36]	0.39 (2.20)	1.01 (25.51)	0.89 (16.05)	0.23 (4.13)	-0.20 (6.35)	200	88.7%
Panel B: Value-weight four-factor model							
Window	Alpha	Beta	SMB	HML	MOM	N	R2
[1,+36]	0.22 (1.18)	1.01 (24.00)	0.54 (9.14)	-0.02 (0.25)	0.12 (3.59)	200	82.2%

The results reported in Table 10 indicate that pump-and-dump patterns are not found in the data. During the three years subsequent to activists’ exit, the equal weight portfolio has a monthly alpha that is positive and statistically significant (0.39%, t-stat = 2.20), and the value weight portfolio has a monthly alpha that is positive and statistically insignificant (0.22%, t-stat = 1.18). Thus, like the other two methodologies used earlier, the portfolio approach again fails to find any evidence in support for the pump-and-dump concerns.

#### D. Summary

Using the three standard methods for detecting abnormal returns we have used – individual firm regressions based on capital asset pricing models, comparison of buy-and-hold returns with returns on similar firms, and a long-term portfolio analysis – we have found no evidence for the pump-and-dump view. Following the month of partial cashing out by the activist, there is no evidence for negative abnormal returns in the subsequent three years. Indeed, returns in this period are positive, though not always statistically significant, in many specifications.

To the extent that targets earn some positive abnormal returns during this three-year period following the month of partial liquidation, it might be asked why activists would sell some of their initial stake at this point. The answer might be that the above-market returns are too small to enable the activists to provide their own investors, after taking out the significant fees charged by hedge fund activists, with adequate returns; or the excess return is too modest to justify the cost of lack of a diversification. So the activists choose to move some of their capital elsewhere.

In any event, analyzing fully the departure choice of activists is beyond the scope of this paper. Our chief interest in this section is in testing empirically the validity of the pump-and-dump claim that negative long-term effects follow, making long-term shareholders worse off, after activists cash out their stakes. Using three standard methods for detecting such negative abnormal returns, we find no support of this claim in the data on stock returns during the three years following activist departures.

V. ACTIVIST INTERVENTIONS THAT ARE ESPECIALLY RESISTED

Our analysis in the preceding Parts III and IV has focused on the full universe of activist interventions. In this Part, we focus on important subsets of activist interventions – those that companies and opponents of activist interventions seem to be especially concerned about and focused on. We investigate whether these subsets of interventions exhibit the long-term declines in company performance feared by opponents of hedge fund activism.

Section A focuses on the subsets of interventions that lead directly or indirectly to reduction in the pool of resources available for the firm’s long-term investments because they are followed by substantial reductions in capital investments, substantial increases in leverage, or substantial rises in payout distributions to shareholders. Section B focuses on interventions that are openly adversarial and hostile. In both cases, we find that, during the five years following the intervention, operating performance does not decline, as opponents of activism fear, but rather improves.

*A. Leverage-Enhancing, Payout-Increasing, and Investment-Reducing Interventions*

Opponents of hedge fund activism and holders of the myopic activists view focus on, and express concerns about, activist intervention that seem or produce changes that seek to enhance shareholder value at the expense of the resources available to the company’s long-term investments. They are thus especially concerned about activism that leads to increase in leverage or higher payouts to shareholders, both of which could leave the firm with fewer resources for future investments, or to direct reductions in capital investments.

Opponents view such strategies as involving a “sacrificing the future for a quick buck.”<sup>56</sup> Commenting on the attempt by activist David Einhorn to persuade Apple to distribute some of its large cash holdings, for example, one prominent opponent viewed it as a “clarion example” of activism that can be expected to have an adverse effect on the long-term interests of Apple and its long-horizon shareholders.<sup>57</sup>

There is no good theoretical basis, however, for presuming that activist-initiated reductions in investments or expenditures are likely to be value-reducing in the long term.<sup>58</sup> An agency problem that has received a great deal of attention from both financial economists and corporate law scholars concerns management’s tendency to avoid distributing excess cash or

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<sup>56</sup> See Wachtell, Lipton, Rosen & Katz Memorandum, *Important Questions about Activist hedge funds*, REPROD. THE HARVARD LAW SCHOOL FORUM ON CORPORATE GOVERNANCE AND FINANCIAL REGULATION (March 9, 2013).

<sup>57</sup> See Wachtell, Lipton, Rosen & Katz Memorandum, *Bite the Apple; Poison the Apple; Paralyze the Company; Wreck the Economy*, REPROD. THE HARVARD LAW SCHOOL FORUM ON CORPORATE GOVERNANCE AND FINANCIAL REGULATION (Feb. 26, 2013).

<sup>58</sup> See Lucian A. Bebchuk, *The Myth that Insulating Boards Serves Long-Term Value*, 113 COLUM. L. Rev. (forthcoming) (2013), Section III.B.4.

assets to shareholders.<sup>59</sup> When a company has excessive cash holdings or investment levels, management might refrain from taking actions that would reduce the size of the empire under its control. Thus, opponents of hedge fund activism overlook that reducing levels of cash holdings and investments might move companies closer to, rather than away from, the levels that are optimal for the long term.

At a minimum, the asserted long-term costs of activism that results in increased leveraged, higher shareholder payouts, or reduced investment cannot be derived theoretically from the very nature of such interventions. It is an empirical proposition that should be backed by evidence. In this Section we therefore turn to testing this proposition.

To this end, we identify a subset of “investment-limiting” activist interventions that are followed by a substantially increased leverage, higher payouts, or reduced investment by the end of year  $t$ ,  $t+1$ , or  $t+2$ . We focus on changes of this nature that take place by the end of year  $t+2$  because we focus on the long-term effect of short-term actions and because changes taking place by the end of year  $t+2$  are more likely to be related to the intervention than changes taking place later on.

In particular, we classify an activist event as “investment-limiting” if any of the following is true: (i) the increase in leverage from the base year to any of the examined years falls within the top 5% of leverage increases among all public companies in that year;<sup>60</sup> (ii) the increase in payout yield (including dividends and share buybacks) from the base year to any of the examined years falls within the top 5% of payout increases among all public companies in that year;<sup>61</sup> and (iii) the increase in capital expenditure and R&D from the base year to any of the examined years falls within the bottom 5% of all firms in that year (hence decrease in investment in large magnitude).<sup>62</sup> By “base year” we refer to the year-end before targeting, that is, year  $t-1$ .

Using the above definition, we find that 19% of the activist interventions fall within the subset of investment-limiting interventions.<sup>63</sup> These interventions are ones that are especially resisted by, and underlay many of the concerns of, opponents of hedge fund activism. We

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<sup>59</sup> See Sanford J. Grossman and Oliver D. Hart, *Corporate Financial Structure and Managerial Incentives*, in *THE ECONOMICS OF INFORMATION AND UNCERTAINTY* 107–40 (John J. McCall ed., 1982); Oliver Hart and John Moore, *Debt and Seniority: An Analysis of the Role of Hard Claims in Constraining Management*, 85 *AM. ECON. REV.* 567, 568–69 (1995); Michael C. Jensen, *Agency Costs of Free Cash Flow, Corporate Finance, and Takeovers*, 76 *AM. ECON. REV.* 323, 323 (1986).

<sup>60</sup> Change in leverage is calculated as  $(Debt_t - Debt_b)/(Debt_b + Equity_b)$ , all using book value. The subscript  $b$  stands for “base year” while the subscript  $r$  stand for the “report year” extending from the event year  $t$  through event year  $t+2$ . 6.3% of the events fall into the top 5% by this criterion.

<sup>61</sup> Change in payout yield is calculated as  $[(Dividend_r + Repurchase_r) - (Dividend_b + Repurchase_b)]/MV_b$ . 9.2% of the events fall into the top 5% by this criterion.

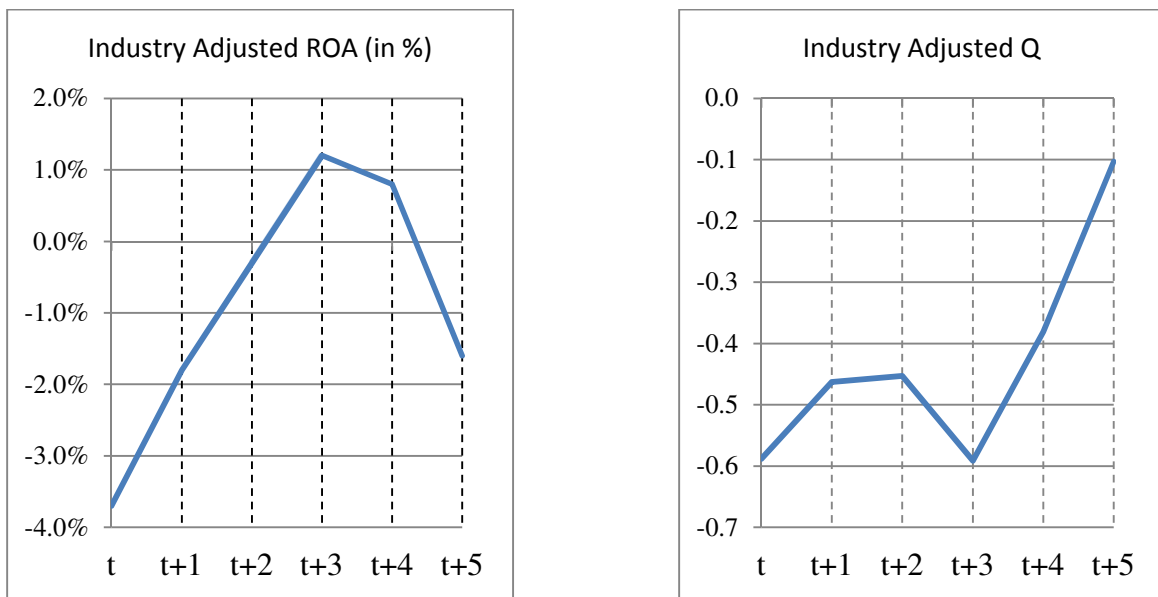
<sup>62</sup> Change in investment is calculated as  $[(Capex_r + R\&D_r) - (Capex_b + R\&D_b)]/Assets_b$ . Missing R&D values are imputed as zeros. 5.9% of the events fall into the bottom 5% by this criterion.

<sup>63</sup> Of the interventions classified as allegedly myopic, about one quarter of them are classified into that set of interventions based on two or more of the criteria (i)-(iii) defined in the preceding paragraph.

therefore investigate whether the claimed adverse effect on long-term performance is present for these interventions.

To set the stage for this examination, we first report the evolution of average industry-adjusted ROA and average industry-adjusted Q during the five years following the activist intervention for this restricted subsample. Similar to what we did in Figure 1 for the whole sample of activist interventions, Figure 3 displays the evolution of industry-adjusted ROA and industry-adjusted Q for the set of investment-limiting interventions.

**Figure 3: The Evolution of ROA and Q Over Time for the “Investment-Limiting” Subsample**



As Figure 3 shows, within the set of investment-limiting interventions, average industry-adjusted ROA and average industry-adjusted Q trend upwards during the five years following the interventions. Indeed, the level of average industry-adjusted ROA and average industry-adjusted Q are higher in each of the five years following the intervention than in the intervention year.

Next, we repeat the regression analysis of the evolution of ROA and Q over time we conducted in Section III.C, but this time we include as event observations only allegedly myopic interventions and not the universe of all activist interventions.

In particular, we re-run the regressions reported in Table 4 but this time focus only on investment-limiting interventions. Table 11 displays our results. As in Table 4, columns (1) and (2) report regressions in which the dependent variable is ROA, and columns (3) and (4) report regressions in which the dependent variable is Q. Industry adjustment is made through the inclusion of industry fixed effects or the finer firm fixed effects. In all regressions, we include as explanatory variables dummy variables representing the year of intervention as well as each of the subsequent five years. We also include as control variables the same controls as those used in



the corresponding regression in Table 4. Among other things, regressions (1) and (3) include industry fixed effects and regressions (2) and (4) include firm fixed effects.<sup>64</sup>

**Table 11: The Evolution of ROA and Q over Time – “Investment-Limiting” Interventions**

Dependent Variable	(1) ROA	(2) ROA	(3) Q	(4) Q		
t: Event year	-0.023* (-1.769)	-0.003 (-0.289)	-0.479*** (-4.024)	-0.305*** (-2.606)		
t+1	-0.005 (-0.432)	0.013 (1.280)	-0.322** (-2.351)	-0.028 (-0.189)		
t+2	0.010 (0.692)	0.028** (2.429)	-0.320** (-2.324)	-0.054 (-0.392)		
t+3	0.026* (1.927)	0.034** (2.483)	-0.361*** (-2.976)	-0.063 (-0.486)		
t+4	0.015 (1.082)	0.022 (1.586)	-0.219* (-1.807)	0.065 (0.483)		
t+5	-0.004 (-0.220)	0.011 (0.722)	0.111 (0.508)	0.352* (1.767)		
ln(MV)	0.036*** (90.544)	0.043*** (40.122)	0.244*** (65.740)	0.838*** (77.028)		
Ln(Age)	0.021*** (28.406)	0.009*** (5.624)	-0.275*** (-40.476)	-0.341*** (-22.114)		
Year FE	Y	Y	Y	Y		
SIC 3 FE	Y		Y			
Firm FE		Y		Y		
Pre-event dummies (t-1,t-2,t-3)	Y	Y	Y	Y		
Observations	126,742	126,742	129,966	129,966		
R-squared	0.202	0.702	0.190	0.625		
	F-Tests:		F Tests:			
	[t+3] – t	6.9	5.5	[t+3] – t	0.5	2.4
	p-val	0.9%	1.9%	p-val	48.6%	12.5%
	[t+4] – t	4.0	2.5	[t+4] – t	2.4	5.3
	p-val	4.5%	11.6%	p-val	12.5%	2.1%
	[t+5] – t	0.8	0.7	[t+5] – t	5.6	9.1
	p-val	36.8%	40.5%	p-val	1.8%	0.3%

As Table 11 indicates, we find no evidence that investment-limiting interventions have an adverse long-term effect on operating performance. Rather, the results are consistent with the view that operating performance improves during the years following the intervention relative to

<sup>64</sup> In addition, as in Table 4, controls used include the company’s market value and age, year fixed effects to account for time trends in the values of ROA and Q, and dummy variables for each of the three years preceding the intervention year.

the intervention time. Indeed, *in each of the four regressions, each of the coefficients for the dummy variables representing the years t+1, t+2, t+3, t+4, and t+5 is higher than the coefficient for the event year.*

Furthermore, focusing on operating performance in years 3, 4, and 5 after the intervention, we conduct F-tests for the difference between each of these coefficients and the event year coefficient, doing so for each of the four regressions. We find that not only is the coefficient of years t+3, t+4, and t+5 higher than the event year coefficient in all regressions but that the difference is statistically significant in many of the F-tests that we conduct. In particular, in regressions (1), ROA in year t+3 and t+4 are significantly higher than the level in the year of intervention at confidence level of 99% and 95% respectively; in regression (2), ROA in year t+3 is significantly higher than the level in the year of intervention at the 95% confidence level; in regression (3), Q in year t+5 is significantly higher than the level in the year of intervention at the 95% confidence level; and in regression (4), Q in year t+4 and Q in year t+5 are significantly higher than the level in the year of intervention at the 95% and 99% confidence level, respectively.

We conclude that the data does not support the serious concerns that opponents of hedge fund activism express concerning interventions that produce substantial enhancement of leverage, increase in shareholder payout, or reductions in investments. We do not find any evidence that such interventions produce long-term declines in operating performance thereby “sacrificing the future for a quick buck.” To the contrary, such interventions tend to produce improvements in operating performance during the five-year period following them.

### *B. Adversarial Interventions*

We now turn to another subset of activist interventions that deserve special attention -- interventions that employ adversarial tactics. Hedge fund activists use such tactics when companies are or expected to be resistant to their suggested course of action and thus conclude that adversarial tactics would be necessary (though might not always be sufficient) to move the company in the direction they deem desirable. Adversarial interventions seek to pressure boards to move in a direction they would otherwise oppose. They are especially resisted and criticized as costly and disruptive by those opposed to hedge fund activism.

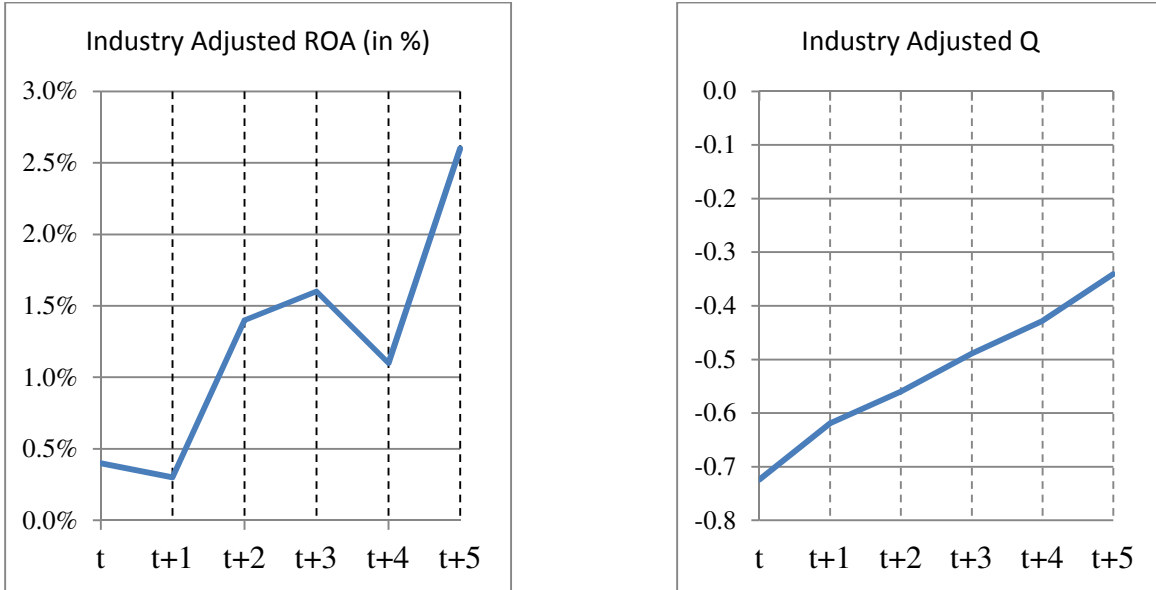
We classify activist interventions as “adversarial” when the initial or amended 13D filing by the activist threatens or opens the door to a proxy contest, a lawsuit, or public campaigns involving confrontation.<sup>65</sup> While our classification procedure might miss events that were hostile behind closed-doors, it should avoid type-II errors, that is, treating as adversarial a non-adversarial initiative. Our set of adversarial interventions accounts for 21.6% of the universe of all interventions in our regression analysis.

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<sup>65</sup> The definition of adversarial intervention we use is identical to the definition of a hostile interventions in Alon Brav, Wei Jiang, Frank Partnoy & Randall Thomas, *Hedge Fund Activism, Corporate Governance, and Firm Performance*, 63 J. FIN. 1729 (2008).

Below we investigate whether the alleged adverse effect on long-term performance is present in the subset of interventions that are adversarial. As in Section V.A, we first plot in Figure 4 the evolution of average industry-adjusted ROA and average industry-adjusted Q during the five years following the activist intervention for this restricted subsample.

**Figure 4: The Evolution of ROA and Q over Time -- “Adversarial” Interventions**



The picture emerging out of Figure 4 with respect to the set of adversarial interventions is similar to the one emerging out of Figure 3 with respect to investment-limiting interventions. As Figure 4 shows, within the set of investment-limiting interventions, average industry-adjusted ROA and average industry-adjusted Q trend upwards during the five years following the interventions. Furthermore, the level of average industry-adjusted ROA and average industry-adjusted Q are higher in each of the five years following the intervention than in the intervention year.

Next, we repeat the regression analysis of the evolution of ROA and Q over time we conducted in the preceding Section V.A, but this time we focus not on investment-limiting interventions but on adversarial interventions.

Table 12 reports our results. As in Table 11, columns (1) and (2) report regressions in which the dependent variable is ROA; columns (3) and (4) report regressions in which the dependent variable is Q; regressions (1) and (3) include industry fixed effects; and regressions (2) and (4) include firm fixed effects. In addition, as in Table 4, controls used include the company’s market value and age, year fixed effects to account for time trends in the values of ROA and Q, and dummy variables for each of the three years preceding the intervention year.

**Table 12: The Evolution of ROA and Q over Time – Adversarial Interventions**

Dependent Variable	(1) ROA	(2) ROA	(3) Q	(4) Q	
t: Event year	0.014** (2.034)	-0.008 (-1.215)	-0.563*** (-7.183)	-0.127* (-1.707)	
t+1	0.019** (2.138)	-0.001 (-0.106)	-0.354*** (-3.957)	0.161* (1.847)	
t+2	0.030*** (3.308)	0.015* (1.868)	-0.310*** (-3.217)	0.230** (2.388)	
t+3	0.031*** (3.200)	0.018** (1.963)	-0.255** (-2.555)	0.244** (2.251)	
t+4	0.023** (2.057)	0.013 (1.252)	-0.162 (-1.536)	0.314*** (3.026)	
t+5	0.029** (2.146)	0.020* (1.916)	-0.022 (-0.123)	0.461*** (3.192)	
ln(MV)	0.036*** (90.638)	0.043*** (40.036)	0.244*** (65.567)	0.839*** (77.071)	
Ln(Age)	0.021*** (28.186)	0.009*** (5.727)	-0.274*** (-40.233)	-0.341*** (-22.087)	
Year FE	Y	Y	Y	Y	
SIC 3 FE	Y		Y		
Firm FE		Y		Y	
Pre-event dummies (t-1,t-2,t-3)	Y	Y	Y	Y	
Observations	126,742	126,742	129,966	129,966	
R-squared	0.202	0.702	0.190	0.625	
F-Tests:		F Tests:			
[t+3] – t	1.9	6.2	[t+3] - t	5.9	9.7
p-val	16.8%	1.3%	p-val	1.5%	0.2%
[t+4] – t	0.5	3.3	[t+4] - t	9.4	14.6
p-val	50.1%	6.8%	p-val	0.2%	0.0%
[t+5] – t	0.9	5.7	[t+5] - t	7.9	14.4
p-val	34.7%	1.7%	p-val	0.5%	0.0%

As Table 12 indicates, we find no evidence that adversarial interventions have a negative long-term effect on operating performance. Rather, the results are consistent with the view that operating performance improves during the years following the intervention relative to the intervention time. Indeed, *in each of the four regressions, each of the coefficients for the dummy variables representing the years t+1, t+2, t+3, t+4, and t+5 is higher than the coefficient for the event year.*

Furthermore, focusing on operating performance in years 3, 4, and 5 after the intervention, we conduct F-tests for the difference between each of these coefficients and the event year coefficient for each of the regressions (1) – (4). We find that not only is the

coefficient of each of the years t+3, t+4, and t+5 higher than the event year coefficient in all regressions, the difference is statistically significant in nine out of the twelve F-Tests we conduct. Indeed, the difference is statistically significant at the 99% confidence level in five out of these nine F-tests.

In the Q regression ((3) and (4)), target firms' Q values are significantly higher at the 99% confidence level in each of the years t+4, t+5 than in the intervention year using either industry or firm fixed effects, and at the 95% confidence level for t+3. And target firms' ROA values are significantly higher at the 95% confidence level in each of the years t+3, t+5 than in the intervention year using firm fixed effects. We conclude that the alleged adverse effect on long-term performance is not found when one focuses on adversarial interventions, either. We do not find any evidence that such interventions are followed by declines in operating performance in the long term. To the contrary, such interventions tend to produce improvements in operating performance during the five-year period following them.

## VI. INCREASED VULNERABILITY TO ECONOMIC SHOCKS?

When discussing the results presented thus far with lawyers and investment bankers that counsel boards, some have suggested that, even if activist interventions benefit investors on an expected value basis, activist interventions might be troubling to the extent that they increase downside risk by making companies more vulnerable (say, by increasing leverage or shareholder payouts) in the event of an adverse economic shock. We are not persuaded that this line of reasoning justifies an opposition to hedge fund activism (and even less so rules that insulate boards from such activism). We note that most shareholders of public companies hold diversified portfolios and that our stock return analysis reaches its conclusions adjusting for risk using standard methodologies for doing so. In any event, however, the analysis in this Part examines empirically whether activist interventions during the years preceding the financial crisis made targeted firms more vulnerable to the downturn when the crisis came. We find no evidence that this is the case.

We divide our analysis into two parts. Section A examines whether targeted firms suffered during the financial crisis more severe declines in operating performance than firms not targeted by activism. Section B compares these two groups of firms in terms of the likelihood of financial distress or delisting during the crisis.

### *A. Operating Performance during the Crisis*

We conduct our analysis in this Part on all public firms that were reported in Compustat in 2006-2007. The dependent variables are changes in ROA and Q from before to after the crisis. The levels before the crisis are averaged over the two years 2006-2007 for each firm; and the post crisis levels are averaged over the two years 2008-2009. The dependent variables  $\Delta$ ROA and  $\Delta$ Q are the differences (Post minus Before).

The key independent variable “Targeted during 2004-2007” is a dummy variable equal to 1 if the firm was targeted by any activist hedge fund during the period of 2004-2007. About 8.58% of the firms in existence at the end of 2007 fall into this category.

Control variables include the logarithm of individual firms’ market cap and age averaged over the two years 2006-2007. We use different specifications that include or exclude industry fixed effects. Note that firm fixed effects are not feasible because the data are constructed as one cross section.

Table 13 reports the results of our regressions. Regressions (1) and (2) focus on changes in ROA, and regressions (3) and (4) focus on changes in Q. Regressions (1) and (3) do not use industry fixed effects, and regressions (2) and (4) use such fixed effects.

**Table 13: Changes in Operating Performance during the Crisis**

Dependent Variable	(1) ΔROA	(2) ΔROA	(3) ΔQ	(4) ΔQ
Targeted during 2004-2007	0.021*** (2.959)	0.018** (2.312)	0.110 (1.404)	0.186** (2.177)
Ln(Market Cap 2006-2007)	0.004** (2.202)	0.006*** (2.882)	-0.071*** (-5.683)	-0.079*** (-5.508)
Ln(Firm age 2006-2007)	-0.000 (-0.168)	0.000 (0.014)	0.155*** (6.300)	0.154*** (5.671)
Industry fixed effects	N	Y	N	Y
R-squared	4,473	4,473	4,626	4,626
Observations	0.003	0.041	0.016	0.096

The results of Table 13 provide no support for the concern that firms targeted by activism fared worse – that is, experienced a more significant decline in performance -- during the financial crisis. The coefficient of the variable “targeted during 2004-2007” is positive and statistically significant in the regressions for change in Q at the 5% level when including industry fixed effect (but only marginal significant when excluding industry fixed effect), and is positive and significant (at the 1% and 5% levels) in the change in ROA regressions (with or without industry fixed effects respectively). Thus, the evidence does not support the view that activism during the years preceding the financial crisis made the targeted firms more fragile and vulnerable to downturns and economic shocks and thus hurt more by the crisis.

### *B. Financial Distress and Delisting during the Crisis*

The analysis of Section A did not control for firms that disappeared from the Compustat database during the financial crisis due to financial distress. Thus, the question arises whether

targeted firms had higher odds of financial distress during the crisis than firms that were not targeted. Before proceeding, we note that summary statistics for the full universe of activist interventions do not reveal higher odds of financial distress than for non-targeted public companies; the five-year cumulative probability of distress-related delisting (bankruptcy, insolvency and liquidation) is the same among targets of activist interventions as in the universe of public companies in general (both are about 5%). In this Section, however, we focus on the narrower set of financial distress cases occurring during the financial crisis.<sup>66</sup>

To examine this question, we use a logit model to assess how the propensity of delisting due to insolvency, defined as bankruptcy or delisting due to insolvency, is related to the status of being targeted during 2004-2007. In our sample as a whole, the incidence of delisting during 2008-2009 due to bankruptcy/insolvency/liquidation is 2.48%. As before, we use market capitalization and firm age as controls, and we use both specifications that include and do not include industry fixed effects.

Table 14 reports the results of our logit regressions. Regression (1) does not include industry fixed effects while in regression (2) we do use such fixed effects.<sup>67</sup>

**Table 14: Incidence of Delisting during the 2008-2009 Crisis**

Dependent Variable	(1) Insolvency Delisting	(2) Insolvency Delisting
Targeted during 2004-2007	0.288 (0.629)	0.229 (0.556)
(marginal probability)	0.85%	0.20%
ln(Market Cap 2006-2007)	-0.473*** (-4.962)	-0.357*** (-4.295)
Ln(Firm age 2006-2007)	-0.247** (-2.114)	-0.257** (-2.344)
Industry fixed effects	Y	N
Observations	2,208	4,627
Pseudo R-squared	0.093	0.049

<sup>66</sup> Although we examine this issue for completeness of analysis, we would like to note that, in our view, a finding of increased vulnerability in some states of the world would not justify opposition to hedge fund activism. As we noted earlier, most shareholders of public companies hold diversified portfolios and what matters for them is impact of activism on an expected value risk-adjusted basis.

<sup>67</sup> The number of observations in the first column in Table 14 is lower when we include the industry fixed effect since an industry fixed effect can only be identified for industries in which there was at least one delisting event.

The results in Table 14 do not provide support for the concern that pre-crisis activism produced an increased in the odds of delisting due to bankruptcy or insolvency during the crisis. The magnitude (as indicated by the marginal probability) of the effects is small, and the t-statistics are not significant. Thus, an examination of the data from the recent financial crisis also provides no basis for a negative assessment of the effects of hedge fund activism.

## VII. CONCLUSION

This paper has investigated empirically the claim that interventions by activist hedge funds have an adverse effect on the long-term interests of companies and their shareholders. While this claim has been regularly invoked and has had considerable influence, its supporters have thus far failed to back it up with evidence or even to subject it to an empirical test. This paper provides a comprehensive investigation of this claim and finds that it is not supported by the data.

We study the universe of about 2,000 interventions by activist hedge funds during the period 1994-2007, examining a long time window of five years following the intervention. We find no evidence that interventions are followed by declines in operating performance in the long term; to the contrary, activist intervention are followed by improved operating performance during the five-year period following the intervention. These improvements in long-term performance, we find, are present also when focusing on the two subsets of activist interventions that are most resisted and criticized – first, interventions that lower or constrain long-term investments by enhancing leverage, beefing up shareholder payouts, or reducing investments and, second, adversarial interventions employing hostile tactics.

We also find no evidence that the initial positive stock price spike accompanying activist interventions fails to appreciate their long-term costs and therefore tends to be followed by negative abnormal returns in the long term; the data is consistent with the initial spike reflecting correctly the intervention's long-term consequences. Similarly, we find no evidence for pump-and-dump patterns in which the exit of an activist is followed by abnormal long term negative returns. Finally, we find no evidence for concerns that activist interventions during the years preceding the financial crisis rendered companies more vulnerable and that the targeted companies therefore were more adversely affected by the crisis.

Our findings that the considered claims and concerns are not supported by the data have significant implications for ongoing policy debates. Policymakers and institutional investors should not accept the validity of the frequent assertions that activist interventions are costly to firms and their long-term shareholders in the long term. They should reject the use of such claims as a basis for limiting the rights and involvement of shareholders.