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THE LONG-TERM EFFECTS OF HEDGE FUND ACTIVISM

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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 hedge funds have a detrimental effect on the long-term interests of companies and their shareholders. We subject this claim to a comprehensive empirical investigation, examining a long time window of five years following activist interventions, and we find that the claim is not supported by the data.

We find no evidence that activist interventions, including those types of interventions that are most resisted and criticized, are followed by short-term gains in performance that come at the expense of long-term performance. We also find no evidence that the initial positive stock price spike accompanying activist interventions tends to be followed by negative abnormal returns in the long term. To the contrary, 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.

Our findings have significant implications for ongoing policy debates. Policymakers and institutional investors should not accept the validity of the assertions that activist interventions are costly to firms and their shareholders in the long term; such claims do not provide a valid basis for limiting the rights, powers, and involvement of shareholders.

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INTRODUCTION

Hedge fund activism is now a key aspect of the corporate landscape. Activists have been engaging with and influencing major American companies, and the media has been increasingly referring to the current era as the "golden age of activist investing."[1] The increase in hedge fund activism, however, has been meeting with intense opposition from public companies and their advisers, creating a heated debate.[2] Are hedge fund activists catalysts of beneficial changes that legal rules and corporate arrangements should facilitate? Or are such activists short-term opportunists that are detrimental to long-term value creation and that legal rules and corporate arrangements should discourage? This Article aims to advance this debate by putting forward empirical evidence that resolves some of the key underlying disagreements. Our findings have important policy implications for ongoing policy debates on activism and the rights and role of shareholders.

We focus on the "myopic-activists" claim that has been playing a central role in debates over shareholder activism and the legal rules and policies shaping it. According to this claim, which we describe in detail in Part I, activist shareholders with short investment horizons, 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.[3] The problem, it is claimed, results from the failure of short-term performance figures and short-term stock 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 excessively cutting investments in long-term projects or in the reserve funds available for such investments.

The myopic-activists claim has been put forward by a wide range of prominent writers. Such concerns have been expressed by significant legal academics, noted economists and business-school professors, prominent business columnists, important business organizations, business leaders, and top corporate lawyers.[4] Furthermore, those claims have been successful in influencing important public officials and policy makers. For example, Leo

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1. For recent media observations referring to the current era as the golden age of activist investing, see, e.g., Ken Squire, A Golden Age for Activist Investing, Barron's (Feb. 16, 2009), http://online.barrons.com/news/articles/SB123457667407886821 (on file with the *Columbia Law Review*); Nathan Vardi, The Golden Age of Activist Investing, Forbes (Aug. 6, 2013, 8:25 AM), http://www.forbes.com/sites/nathanvardi/2013/08/06/the-golden-age-of-activist-investing/ (on file with the *Columbia Law Review*).

2. See, e.g., infra notes 23–26, 29–31 and accompanying text (discussing writings questioning whether activist investors are beneficial for corporations and their shareholders).

3. See, e.g., infra notes 23–26and accompanying text (discussing works suggesting activist investors harm long-term interests of companies and their shareholders).

4. For references to such writings, see infra note 22.

Strine Jr. and Jack Jacobs, two prominent Delaware judges, have expressed strong concerns about short-sighted interventions by activists.[5] 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.[6]

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 activist shareholders.[7] In particular, such arguments have been used to support, for example, allocating power to directors rather than shareholders, using board classification to insulate directors from shareholders, impeding shareholders' ability to replace directors, limiting the rights of shareholders with short holding periods, tightening the disclosure obligations regarding the disclosure of stock accumulations by hedge fund activists, and corporate boards taking on an adversarial approach toward activists.[8]

Even assuming that capital markets are informationally inefficient and activists have short investment horizons, the claim that activist interventions are detrimental to the long-term interests of shareholders and companies does not necessarily follow as a matter of theory.[9] The claim is thus a factual proposition that can be empirically tested. However, those advancing the myopic-activists claim have thus far failed to back their claims with large-sample empirical evidence, relying instead on their (or others') impressions and experience in advising corporations.[10]

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5. See Jack B. Jacobs, "Patient Capital": Can Delaware Corporate Law Help Revive It?, 68 Wash. & Lee L. Rev. 1645, 1649, 1657–63 (2011) (expressing concerns about "decline . . . of patient capital and the substitution, in its place, of impatient capital, driven by parallel pressures from investors . . . to generate short-term profits."); 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, 7–9, 26 (2010) [hereinafter Strine, Fundamental Question] ("[T]here is a danger that activist shareholders will make proposals motivated by interests other than maximizing the long-term, sustainable profitability of the corporation.").

6. See Facilitating Shareholder Director Nominations, 75 Fed. Reg. 56,668, 56,697–99 (Sept. 16, 2010) [hereinafter Director Nominations] (discussing rationale behind adopting three-year holding requirement).

7. For a broad range of writings making such moves, see infra notes 109–132.

8. For a discussion of, and references to, such arguments, see infra Part VII.

9. See Lucian A. Bebchuk, The Myth that Insulating Boards Serves Long-Term Value, 113 Colum. L. Rev. 1637, 1660–76 (2013) [hereinafter Bebchuk, Myth] (analyzing conceptual structure of myopic-activists claim and showing myopic-activists claim does not follow from assuming existence of inefficient capital markets and short activist horizons).

10. See, e.g., Martin Lipton, Wachtell, Lipton, Rosen & Katz, Bite the Apple; Poison the Apple; Paralyze the Company; Wreck the Economy, Harvard Law Sch. Forum on Corporate Governance & Fin. Regulation (Feb. 26, 2013, 9:22 AM), http://blogs.law.harvard.edu/corpgov/2013/02/26/bite-the-apple-poison-the-apple-paralyze-the-company-wreck-the-economy/ [hereinafter Wachtell Memorandum, Bite the Apple] (on file with the *Columbia Law Review*) (Martin Lipton stating that short-termism concerns are based "on the decades of [his and his] firm's experience in advising corporations" without suggesting any empirical backing for his belief).

In this Article, we conduct a systematic empirical investigation of the myopic-activists claim, focusing on interventions by activist hedge funds. We find that the myopic-activists claim is not supported by the data.

Prior to our work, financial economists had already put forward evidence that Schedule 13D filings—public disclosures of the purchase of a significant stake by an activist—are accompanied by significant positive stock-price reactions as well as followed by subsequent improvements in operating performance.[11] However, supporters of the myopic-activists claim have dismissed this evidence, asserting that improvements in operating performance are short-lived and come with the cost of subsequent declines in performance and, furthermore that short-term positive stock reactions to disclosures of an activist stake merely reflect inefficient market prices that fail to reflect the costs of the long-term declines in performance. Thus, in a widely circulated memorandum of the law firm Wachtell, Lipton, Rosen & Katz, Martin Lipton, a prominent supporter of the myopic-activists claim, 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," and challenged those supporting activism to study this important question.[12]

In this Article, we meet this challenge. Going beyond the 24-month period, we study how operational performance and stock performance relative to the benchmark evolve during the five-year period following activist interventions. We find that the empirical evidence does not support the predictions and assertions of supporters of the myopic-activists claim.

After Part I discusses the myopic-activists claim we investigate and the substantial policy stakes involved, Part II describes our dataset and the universe of about 2,000 activist interventions that we study. Our study uses a dataset consisting of the full universe of approximately 2,000 interventions by activist hedge funds during the period from 1994 to 2007. For each activist effort we identify the "intervention time" in which the activist initiative was first publicly disclosed (usually through the filing of a Schedule 13D). We track the operating performance and stock returns for companies during a long period—five years—following the intervention time. We also examine the three-year period that precedes activist interventions and the three-year period that follows activists' departures.

Part III focuses on operating performance. We find that activists tend to target companies that are underperforming relative to industry peers at the time of the intervention, not well-performing ones. Most importantly, there is no evidence that activist interventions produce short-term improvements in

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11. Studies documenting such positive abnormal returns are cited in infra notes 17, 74-77. For a review of some of these studies, see generally Alon Brav, Wei Jiang & Hyunseob Kim, Hedge Fund Activism: A Review, 4 Found. & Trends Fin. 185 (2009) [hereinafter Brav et al., Hedge Fund Activism: A Review].

12. Wachtell Memorandum, Bite the Apple, supra note 10.

performance at the expense of long-term performance. During the long, five-year window that we examine, the declines in operating performance asserted by supporters of the myopic-activists claim are not found in the data. Indeed, while lack of long-term declines in performance is sufficient for rejecting the myopic-activists claim, we find evidence, especially when assessing performance using the standard measure of Tobin's Q, that performance is higher three, four, and five years after the year of intervention than at the time of intervention.

Part IV then turns to stock returns following the initial stock price spike that is well known to accompany activist interventions. We first document 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-activists claim believe, the initial spike in stock price reflects inefficient 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. Using each of the three standard methods used by financial economists for detecting stock-return underperformance, we find no evidence of the asserted reversal of fortune during the five-year period following the intervention. The long-term underperformance predicted by the myopic-activists claim, and the resulting losses to long-term shareholders, are not found in the data.

Part IV also analyzes whether activists cash out their stakes before negative stock returns occur and impose losses on remaining long-term shareholders. In particular, we 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 abnormal stock returns, we find no evidence that long-term shareholders experience negative stock returns during the three years following an activist's departure.

Part V next turns to analyze the two subsets of activist interventions that are most resisted and criticized. One subset consists of interventions that lower or constrain long-term investments by enhancing leverage, beefing up shareholder payouts, or reducing investments. The other subset consists of adversarial interventions employing hostile tactics. In both cases, the long-term declines in performance asserted by opponents are not found in the data.

Part VI examines whether activist interventions render targeted companies more vulnerable to economic shocks. In particular, we examine whether companies targeted by activist interventions during the three 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 financial crisis.

Part VII discusses the significant implications that our findings have on policy debates. Going forward, policymakers and institutional investors should not accept the validity of assertions that interventions by activist hedge funds are followed by long-term adverse consequences for companies and their long-term shareholders. Furthermore, Part VII discusses several ongoing debates in which the myopic-activists claim has been playing a key role and that should thus be informed by our findings. The rejection of the myopic-activists claim should weigh against arguments for limiting the rights and involvement of shareholders in general or activist shareholders in particular by using staggered boards, avoiding reforms of corporate elections, and tightening the disclosure rules governing stock accumulations by activist investors. Furthermore, corporate boards should not take the generally adversarial attitude toward activist interventions that is urged by key legal advisers.

Since early versions of this study started circulating, it has already had a significant effect on the ongoing debate on hedge fund activism. About fifty articles discussing the study have been published by, among others, the *Wall Street Journal*, the *New York Times*, the *Economist*, *Harvard Business Review*, *Time Magazine*, Bloomberg, Reuters, *Fortune*, *Forbes*, and *Barron's*.[13] Still, our study has also attracted significant resistance from opponents of activism, and senior partners of Wachtell Lipton, including founding partner Martin Lipton, issued several detailed memoranda criticizing our study and calling for a reliance on the "depth of real-world experience" of business leaders rather than on any empirical studies.[14] Because our study focuses on the precise question that Wachtell Lipton challenged researchers to study, we view its current opposition to empirical studies of the subject as unwarranted.

In responses that we issued to Wachtell Lipton's critiques,[15] and in the course of this Article, we explain that our study addresses the methodological

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13. See Lucian Arye Bebchuk, Harvard L. Sch., http://www.law.harvard.edu/faculty/bebchuk (on file with the *Columbia Law Review*) (last visited Jan. 29, 2015) (providing links to over forty media pieces discussing this study, including pieces in publications listed above).

14. For three such memoranda criticizing our work issued by Wachtell Lipton, see Martin Lipton, Wachtell, Lipton, Rosen & Katz, The Bebchuk Syllogism, Harvard Law Sch. Forum on Corporate Governance & Fin. Regulation (Aug. 26, 2013, 12:32 PM), http://blogs.law.harvard.edu/corpgov/2013/08/26/the-bebchuk-syllogism/ [hereinafter Wachtell Memorandum, The Bebchuk Syllogism] (on file with the *Columbia Law Review*); Martin Lipton & Steven A. Rosenblum, Wachtell, Lipton, Rosen & Katz, Do Activist Hedge Funds Really Create Long Term Value?, Harvard Law Sch. Forum on Corporate Governance & Fin. Regulation (July 22, 2014, 3:55 PM), http://blogs.law.harvard.edu/corpgov/2014/07/22/do-activist-hedge-funds-really-create-long-term-value/ (on file with the *Columbia Law Review*); Martin Lipton, Wachtell, Lipton, Rosen & Katz, Empiricism and Experience; Activism and Short-Termism; the Real World of Business, Harvard Law Sch. Forum on Corporate Governance & Fin. Regulation (Oct. 28, 2013, 9:40 AM), http://blogs.law.harvard.edu/corpgov/2013/10/28/empiricism-and-experience-activism-and-short-termism-the-real-world-of-business/ [hereinafter Wachtell Memorandum, Empiricism and Experience] (on file with the *Columbia Law Review*).

15. For posts that we issued in response to each of the three Wachtell Lipton critiques of our work, see Lucian Bebchuk, Alon Brav & Wei Jiang, Don't Run Away from the Evidence: A Reply to Wachtell Lipton, Harvard Law Sch. Forum on Corporate Governance & Fin. Regulation (Sept.

criticism raised in these critiques and that empirical evidence provides a superior tool for assessing the myopic-activists claim than anecdotes or self-reported impressions of business leaders.[16] Below we seek to contribute to the literature by providing empirical evidence that could inform the ongoing debate and a foundation on which subsequent empirical work can build.

I. THE MYOPIC-ACTIVISTS CLAIM

This Part discusses the myopic-activists claim that this Article aims to test empirically. Part I.A describes the claim and its conceptual structure. Part I.B highlights the need for testing the empirical validity of the claim.

A. *The Claim*

Hedge fund activists might seek a wide range of actions in the strategy and management of a company. They might propose, for example, divesting assets, changing investment or payout levels, altering the capital structure, or replacing the CEO.[17] In recent cases that received some attention, for example, activist investors David Einhorn and Carl Icahn urged Apple to increase distributions to shareholders,[18] and hedge fund Elliott Management urged Hess to undergo major structural changes.[19] Because developing an operational change often requires first acquiring a substantial amount of company-specific

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17, 2013, 9:00 AM), http://blogs.law.harvard.edu/corpgov/2013/09/17/dont-run-away-from-the-evidence-a-reply-to-wachtell-lipton/ (on file with the *Columbia Law Review*); Lucian Bebchuk, Alon Brav & Wei Jiang, Still Running Away from the Evidence: A Reply to Wachtell Lipton's Review of Empirical Work, Harvard Law Sch. Forum on Corporate Governance & Fin. Regulation (Mar. 5, 2014, 9:02 AM), http://blogs.law.harvard.edu/corpgov/2014/03/05/still-running-away-from-the-evidence-a-reply-to-wachtell-liptons-review-of-empirical-work/ [hereinafter Bebchuk et al., Still Running Away from the Evidence] (on file with the *Columbia Law Review*); Lucian Bebchuk, Wachtell Keeps Running Away from the Evidence, Harvard Law Sch. Forum on Corporate Governance & Fin. Regulation (July 28, 2014, 9:15 AM), http://blogs.law.harvard.edu/corpgov/2014/07/28/wachtell-keeps-running-away-from-the-evidence/#more-64978 (on file with the *Columbia Law Review*).

16. See infra notes 35–37, 52, 56, 65, 67, 70, 78, 91, 95, 97–98 and accompanying text (addressing Lipton's critiques of hedge fund activism and this project).

17. For discussions of the range of operational changes sought by activists, see Alon Brav, Wei Jiang, Frank Partnoy & Randall Thomas, Hedge Fund Activism, Corporate Governance, and Firm Performance, 63 J. Fin. 1729, 1741–45 (2008) [hereinafter Brav et al., Hedge Fund Activism] (describing and classifying motives behind hedge fund activism).

18. For discussions of the activist intervention in the Apple case, see Steven M. Davidoff, Why Einhorn's Win May Be Apple's Gain, N.Y. Times: Dealbook (Feb. 26, 2013, 10:02 AM), http://dealbook.nytimes.com/2013/02/26/why-einhorns-win-may-be-apples-gain/ (on file with the *Columbia Law Review*); Michael J. De La Merced, Icahn Ends Call for Apple Stock Buyback, N.Y. Times: Dealbook (Feb. 10, 2014, 10:19 AM), http://dealbook.nytimes.com/2014/02/10/icahn-backs-off-apple-buyback-proposal/ (on file with the *Columbia Law Review*).

19. See, e.g., Elliott Management Calls for Board Shake-Up at Hess, N.Y. Times: Dealbook (Jan. 29, 2013, 8:38 AM), http://dealbook.nytimes.com/2013/01/29/elliott-management-calls-for-board-shake-up-at-hess/ (on file with the *Columbia Law Review*) (noting Elliott has announced wide-ranging strategy for Hess, which includes selling off pieces of business and spinning off assets).

information, activists often hold a significant stake in the company and hope to benefit from the appreciation in the value of the stake that would result from implementing the change.[20] In addition to seeking such "operational" changes, hedge fund activists often seek governance changes in how the company is run or personnel changes in its leadership.[21]

Critics of such activist interventions have long put forward the myopic-activists claim that the actions being sought are overall (or on average) value decreasing in the long term even when they are profitable in the short term. Such concerns have been expressed by a broad range of prominent authors, including legal academics, economists and business school professors, business columnists, business leaders, business organizations, and corporate lawyers.[22]

Then-Chancellor Strine described the essence of the myopic-activists claim advanced by critics as follows: "[I]n corporate polities, unlike nation-states, the citizenry can easily depart and not 'eat their own cooking.' As a result, there is a danger that activist stockholders will make proposals motivated by interests other than maximizing the long-term, sustainable profitability of the corporation."[23]

In a similar account of the claim, Harvard Business School professor and former Medtronic CEO William George stated that the essential problem is that activists' "real goal is a short-term bump in the stock price. They lobby publicly for significant structural changes, hoping to drive up the share price

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20. See generally Marcel Kahan & Edward B. Rock, Hedge Funds in Corporate Governance and Corporate Control, 155 U. Pa. L. Rev. 1021, 1069–70, 1088–89 (2007) ("[I]ncentives for a fund to engage in activism depend on its stake in a portfolio company.").

21. See Brav et al., Hedge Fund Activism, supra note 17, at 1741–44, 1753–55, 1757–60 (discussing changes sought by hedge fund activists).

22. For writings expressing such concerns by such a broad range of authors, see, e.g., William W. Bratton & Michael L. Wachter, The Case Against Shareholder Empowerment, 158 U. Pa. L. Rev. 653, 653–54, 657–59 (2010); Martin Lipton & Steven A. Rosenblum, A New System of Corporate Governance: The Quinquennial Election of Directors, 58 U. Chi. L. Rev. 187, 187–88, 203, 210–12 (1991) [hereinafter Lipton & Rosenblum, Quinquennial Election]; Justin Fox & Jay W. Lorsch, The Big Idea—What Good Are Shareholders?, Harv. Bus. Rev. (July 2012) (on file with the *Columbia Law Review*), available at https://hbr.org/2012/07/what-good-are-shareholders; John Kay, The Kay Review of UK Equity Markets and Long-Term Decision Making, Final Report 9 (2012) (on file with the *Columbia Law Review*); Aspen Inst., Overcoming Short-Termism: A Call for a More Responsible Approach to Investment and Business Management 2–3 (Sept. 9, 2009) http://www.aspeninstitute.org/sites/default/files/content/docs/business%20and%20society%20program/overcome_short_state0909.pdf (on file with the *Columbia Law Review*); Joe Nocera, Op-Ed, What Is Business Waiting For?, N.Y. Times (Aug. 15, 2011), http://www.nytimes.com/2011/08/16/opinion/nocera-what-is-business-waiting-for.html? (on file with the *Columbia Law Review*); Andrew Ross Sorkin, 'Shareholder Democracy' Can Mask Abuses, N.Y. Times: Dealbook (Feb. 25, 2013, 9:30 PM), http://dealbook.nytimes.com/2013/02/25/shareholder-democracy-can-mask-abuses/ (on file with the *Columbia Law Review*).

23. Strine, Fundamental Question, supra note 5, at 8.

and book quick profits. Then they bail out, leaving corporate management to clean up the mess."[24]

Critics of hedge fund activism also express concerns about certain types of changes that might be induced by myopic activists. They worry, for example, that myopic activists will pressure companies to make cuts in "research and development expenses, capital expenditures, market development, and new business ventures, simply because they promise to pay off only in the long term."[25] They also argue that activist investors use their power "to sway and bully management to . . . meet the quarterly targets and disgorge cash in extra dividends or stock buy backs in lieu of investing in long-term growth."[26]

The myopic-activists claim that is the focus of this Article should be distinguished from another claim that opponents of activism make. According to what might be referred to as the counterproductive-accountability claim,[27] the fear of shareholder intervention (or even removal by shareholders) in the event that management fails to deliver good short-run outcomes leads management itself to initiate and take myopic actions—actions that are profitable in the short term but detrimental in the long term. This counterproductive-accountability claim, and the empirical evidence against it, are discussed in detail in another paper by one of us.[28] In this Article, however, we focus exclusively on the myopic-activists claim.

The impact that supporters of the myopic-activists claim have had is, in our view, at least partly due to the alleged gravity of the concerns that some of them have raised. Some opponents, for example, have argued that shareholder activists "are preying on American corporations to create short-term increases in the market price of their stock at the expense of long-term value"[29] and that

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24. Bill George, Activists Seek Short-Term Gain, Not Long-Term Value, N.Y. Times: Dealbook (Aug. 26, 2013, 10:56 AM), http://dealbook.nytimes.com/2013/08/26/activists-seek-short-term-gain-not-long-term-value/ (on file with the *Columbia Law Review*).

25. Lipton & Rosenblum, Quinquennial Election, supra note 22, at 210.

26. Ira M. Millstein, Re-Examining Board Priorities in an Era of Activism, N.Y. Times: Dealbook (Mar. 8, 2013, 3:52 PM), http://dealbook.nytimes.com/2013/03/08/re-examining-board-priorities-in-an-era-of-activism/ (on file with the *Columbia Law Review*).

27. Bebchuk, Myth, supra note 9, at 1676–78 (defining counterproductive-accountability claim and distinguishing it from myopic-activists claim).

28. Id. at 1676–86 (discussing conceptual structure of, and lack of empirical support for, counterproductive accountability claim). A subsequent study by Nickolay Gantchev, Oleg Gredil and Chotibhak Jotikasthira provides empirical evidence that, by increasing the threat of activism vis-à-vis firms similar to the targets of activist interventions, the disclosures are accompanied by positive abnormal returns to such similar firms. See Gantchev et al., Governance Under the Gun: Spillover Effects of Hedge Fund Activism 26–28, 49 tbl. 7 (Jan. 2015) (unpublished manuscript), available at http://ssrn.com/abstract=2356544 (on file with the *Columbia Law Review*) (showing announcements of activist stakes are accompanied by positive abnormal returns to companies similar to target).

29. Martin Lipton, Wachtell, Lipton, Rosen & Katz, Important Questions About Activist Hedge Funds, Harvard Law Sch. Forum on Corporate Governance & Fin. Regulation (Mar. 9, 2013, 10:10 AM), http://blogs.law.harvard.edu/corpgov/2013/03/09/important-questions-about-activist-hedge-funds [hereinafter Wachtell Memorandum, Important Questions] (on file with the *Columbia Law Review*).

pressure from short-term activists "is directly responsible for the short-termist fixation that led to the [2008–2009] financial crises."[30] The gravity of asserted concerns has registered with prominent Delaware judges; then-Justice Jacobs, for example, has accepted that the influence of short-term activists "has created a national problem that needs to be fixed."[31]

The significance of the myopic-activists claim is also due to its wide-ranging implications. As noted in the Introduction, and as will be discussed in detail in Part VII, the myopic-activists claim has been playing a critical role in attempts to limit the rights and involvement of shareholders in many contexts. Therefore, an empirical resolution of the validity of this claim would have substantial implications for various significant policy debates.

B. *The Need for Evidence*

Supporters of the myopic-activists claim believe that stock market prices are sometimes informationally inefficient and are thus set at levels that do not represent the best estimate of long-term share value that can be derived from all available public information.[32] These supporters also stress that activist investors commonly have short horizons.[33] As one of us has shown in prior work, however, the myopic-activists claim does not follow from assuming that capital markets are often inefficient and that activists often have short investment horizons.[34] To be sure, with inefficient market pricing and short investor horizons, it is theoretically possible that activists might, in some cases,

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30. Martin Lipton, Theodore N. Mirvis & Jay W. Lorsch, The Proposed "Shareholder Bill of Rights Act of 2009," Harvard Law Sch. Forum on Corporate Governance & Fin. Regulation (May 12, 2009, 4:56 PM), http://blogs.law.harvard.edu/corpgov/2009/05/12/the-proposed-%E2%80%9Cshareholder-bill-of-rights-act-of-2009%E2%80%9D/ (on file with the *Columbia Law Review*); see also Lynne L. Dallas, Short-Termism, the Financial Crisis, and Corporate Governance, 37 J. Corp. L. 265, 268 (2012) (arguing short-termism contributed to recent financial crisis).

31. Jacobs, supra note 5, at 1657. Similarly, Chief Justice Strine (then Vice Chancellor Strine) accepted that the influence of short-term activists contributed to excessive risk-taking in the run-up to the financial crisis. See Leo E. Strine, Jr., Why Excessive Risk-Taking Is Not Unexpected, N.Y. Times: Dealbook (Oct. 5, 2009, 1:30 PM), http://dealbook.nytimes.com/2009/10/05/dealbook-dialogue-leo-strine/ (on file with the *Columbia Law Review*) ("[T]o the extent that the [2008 financial] crisis is related to the relationship between stockholders and boards, the real concern seems to be that boards were warmly receptive to investor calls for them to pursue high returns through activities involving great risk and high leverage.").

32. See, e.g., Bratton & Wachter, supra note 22, at 691–94 (stating financial markets are not efficient and surveying related literature); Lipton & Rosenblum, Quinquennial Election, supra note 22, at 208–10 (arguing stock market is generally inefficient by referring to economic literature accepting stock market can and does misprice stocks).

33. See, e.g., Strine, Fundamental Question, supra note 5, at 8–11 ("[M]any activist investors hold their stock for a very short period of time . . . . What is even more disturbing than hedge fund turnover is the gerbil-like trading activity of the mutual fund industry . . . ." (footnote omitted)).

34. Bebchuk, Myth, supra note 9, at 1660–76 (analyzing implications of assuming capital markets are often inefficient and activists often have short investment horizons).

want companies to act in ways that are not value maximizing in the long term. However, it is far from clear how often such cases arise. Furthermore, such cases might be outweighed by cases in which activists have a clear interest in seeking actions that are positive both in the short term and the long term.

Thus, the myopic-activists claim is, at best, a contestable proposition that might or might not be valid and should be supported by evidence. However, rather than backing up the myopic-activists claim with a study of the financial performance and stock prices of companies several years after an activist intervention, opponents of activism have stressed that their belief in the myopic-activists claim is strongly confirmed by their own experience or the experience of corporate leaders; Martin Lipton, for example, wrote that his short-termism concerns are based on "the decades of [his] firm's experience in advising corporations."[35] Indeed, in a memorandum responding to this Article, Wachtell Lipton urged reliance on the "depth of real-world experience" of corporate leaders rather than on empirical evidence.[36] Similarly, some other critics of this Article faulted us for questioning the views of "wise people, with loads of practical experience" and their "collective judgment that activist interventions are detrimental," and argued that "policymakers should weight the experience and expertise of knowledgeable people rather than tortured statistics."[37]

In our view, however, arguments and policy decisions should not be based on anecdotes, reported individual experience and felt intuitions concerning long-term outcomes. Advocates of reliance on the reported impressions of corporate leaders would surely oppose policymakers' relying on claims by leaders of activist hedge funds that activist interventions are beneficial if these claims were based solely on the leaders' professed experience. Furthermore, relying on self-reported impressions is especially unwarranted for a claim that is clearly testable using objective and available data.

The myopic-activists claim asserts propositions concerning the financial performance and stock returns of public firms. Data about such financial performance and stock returns are available and widely used by financial economists. Using such data enables subjecting claims about financial performance and stock returns to a rigorous and objective test.

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35. Wachtell Memorandum, Bite the Apple, supra note 10.

36. Wachtell Memorandum, The Bebchuk Syllogism, supra note 14. In a subsequent memorandum, Wachtell Lipton attempted to argue that, although it did not rely on empirical evidence in advancing the myopic-activists claim, there are in fact twenty-seven studies listed in the memorandum that support this claim. Wachtell Memorandum, Empiricism and Experience, supra note 14. An analysis of these twenty-seven studies that we conducted, however, found that none of them provides evidence that is inconsistent with our findings. See Bebchuk et al., Still Running Away from the Evidence, supra note 15 (conducting this analysis).

37. See Yvan Allaire & François Dauphin, Inst. for Governance of Private & Pub. Orgs., "Activist" Hedge Funds: Creators of Lasting Wealth? What Do the Empirical Studies Really Say? 4, 17 (2014), available at http://igopp.org/wp-content/uploads/2014/07/IGOPP_Article_Template2014_Activism_EN_v6.pdf [hereinafter Allaire & Dauphin, Lasting Wealth?] (on file with the *Columbia Law Review*) (criticizing our study).

Even if some business leaders genuinely believe in the validity of the myopic-activists claim, policymakers and institutional investors should accept the claim as valid only if it is supported by the data. An empirical examination is thus essential for assessing the myopic-activists claim. We provide such an examination below.

II. THE UNIVERSE OF HEDGE FUND ACTIVISM

Our empirical examination of the myopic-activists claim in this Article builds 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.[38] This dataset was also used by the same authors in subsequent work.[39] Two of us, with Hyunseob Kim, extended the data to include 2007 in a subsequent study[40] 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.[41] The three of us, working with Robert Jackson, have recently used this dataset to study predisclosure accumulations of stock by hedge fund activists.[42] Thus, this database has proven fruitful for previous analyses of several issues, and in this Article we extend the use of this database 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.[43] 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,[44] filer types such as banks, insurance companies, mutual funds, and other nonactivist investors were excluded from our sample. In addition,

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38. Brav et al., Hedge Fund Activism, supra note 17, at 1736–39 (discussing data used).

39. Alon Brav, Wei Jiang, Frank Partnoy & Randall S. Thomas, The Returns to Hedge Fund Activism, 64 Fin. Analysts J. 45, 46–47 (2008) [hereinafter Brav et al., The Returns to Hedge Fund Activism] (discussing data used).

40. Brav et al., Hedge Fund Activism: A Review, supra note 11, at 196 (discussing data used).

41. Alon Brav, Wei Jiang & Hyunseob Kim, The Real Effects of Hedge Fund Activism: Productivity, Asset Allocation, and Industry Concentration 5–7 (May 23, 2013) (unpublished manuscript), available at http://www.columbia.edu/~wj2006/HF_RealEffects.pdf [hereinafter Brav et al., The Real Effects of Hedge Fund Activism] (on file with the *Columbia Law Review*) (discussing data used).

42. Lucian A. Bebchuk, Alon Brav, Robert J. Jackson Jr. & Wei Jiang, Pre-Disclosure Accumulations by Activist Investors: Evidence and Policy, 39 J. Corp. L. 1, 7–14 (2013) [hereinafter Bebchuk et al., Pre-Disclosure Accumulations] (discussing data used).

43. See SEC, Form of Schedule 13D, 17 C.F.R. § 240.13d-101 (2014) (requiring investors to file with SEC within ten days of acquiring more than 5% of any class of securities of a publicly traded company if they have interest in influencing company's management under section 13(d) of 1934 Securities Exchange Act).

44. See id. (requiring description of "name[,] principal business[,] [and] address of principal office" of filer).

based on the description of the purpose of the investment required to be included in Item 4,[45] 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, the dataset includes the results of extensive news searches, 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.[46] 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).[47]

In this Article, we use this dataset of activist interventions to provide the first systematic evidence on the long-term effects of hedge fund activism.[48] 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 Center for Research in Security Prices (CRSP) for stock return data. This enables us to study the long-term effects of activist interventions on both operating performance and shareholder wealth.

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45. See id. (requiring investors to disclose "[p]urpose of [t]ransaction," including, inter alia, any plans relating to acquisition of additional stock or corporate event such as merger or acquisition).

46. The researchers putting together the dataset conducted extensive news searches in Factiva using the hedge fund and target company names as key words, plus a general search using various combinations of "hedge fund" and "activism" as key words. They further checked the completeness of the news search using the Thomson Financial Form 13F database. For a detailed description of the construction of this database, see Brav et al., Hedge Fund Activism, supra note 17, at 1736–39.

47. 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 forty-two 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 of this issue, see Brav et al., Hedge Fund Activism, supra note 17, at 1738–39. For a more detailed description of the procedure for assembling this dataset, see Brav et al., Hedge Fund Activism: A Review, supra note 11, at 193–95.

48. While putting together a dataset such as the one we use requires significant work, other teams of researchers who wish to redo or refine our analysis can do so following the description of the construction of the dataset in Brav et al., Hedge Fund Activism, supra note 17, at 1736–39. Indeed, various teams of researchers have already put together, and used in their empirical work, large datasets of activist interventions. For such studies issued recently, see, e.g., Aslan and Kumar, The Product Market Effects of Hedge Fund Activism, Working Paper, January 2014, available at https://editorialexpress.com/cgi-bin/conference/download.cgi?paper_id=311&db_name=AFA2015 at ___ (describing authors' dataset of activist interventions); Gantchev et al., supra note 28 at ___ (same); Krishnan et al., Top Hedge Funds and Shareholder Activism, Vanderbilt University Law School Law & Economics Working Paper 15-9, available at www.ssrn.com/abstract-2589992 at ___ (same).

In particular, we seek to study long-term results during the five years following the activist intervention. We use 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.[49]

Table 1 below provides summary data on 2,040 Schedule 13D filings by activist hedge funds during the period from 1994 to 2007. As Table 1 shows, there has been an increase in the frequency of activist hedge fund filings over time. Furthermore, except for the first two years, 1994 and 1995, the dataset includes more than ninety filings for each year in the fourteen-year period of 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	10	2001	96
1995	37	2002	134
1996	99	2003	127
1997	212	2004	148
1998	161	2005	237
1999	118	2006	269
2000	120	2007	272
Total 1994–2000	757	Total 2001–2007	1,283

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 over 2,000 interventions in the dataset, the large number of observations facilitates statistical testing.

III. OPERATING PERFORMANCE

This Part presents our findings concerning the operating performance of firms targeted by activists during the five-year period following the activist intervention. Part III.A describes the standard metrics of operating

49. The 2013 version of this Article was based on a dataset that did not include 2012 Compustat data, which were not available when this dataset was put together. Thus, the dataset that we now analyze includes data, which were initially missing, on the operating performance of 2007 targets in their fifth year of operation after the intervention.

performance, Q and ROA, used in our study. Part III.B provides summary statistics; in particular, it shows that the industry-adjusted Q and ROA of target firms are on average higher during each of the five years following the intervention than at the intervention time. Part III.C presents a regression analysis of the evolution of operating performance during the five-year period following the intervention. Part III.D extends the regression analysis to control for levels of past performance. Finally, Part III.E discusses the interpretation of our findings; in particular, we explain why the clear pattern of post-intervention improvements in long-term operating performance identified in this Part is unlikely to be driven by firms that are acquired or otherwise delisted before the end of five years, or mere stock picking by hedge fund activists.

A. *Metrics of Performance*

The metric of operating performance to which we pay closest attention is Tobin's Q. Named after Nobel Laureate James Tobin, Tobin's Q is the metric most commonly used by financial economists for studying the effectiveness with which firms operate and serve their shareholders, and numerous peer-reviewed studies have used this metric for assessing the efficiency of governance arrangements, ownership structures, or investor protection rules.[50] Tobin's Q, often referred to as "Q" for simplicity, is designed to reflect a company's success in turning a given book value of assets into market value accrued to investors.[51] The design of Q enables it to reflect the aggregate effects through all channels that a given arrangement, structure, or event has on the value accruing to investors.

We also use ROA throughout as another metric for operating performance. ROA refers to return on assets—the ratio of earnings before interest, taxes, depreciation, and amortization to the book value of assets—and it has been significantly used by financial economists as a metric for operating performance.[52] ROA reflects the earning power of a business and thus the

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50. For studies that use Q for analyzing the efficiency of governance arrangements, ownership structures, or investor protection rules, see, e.g., Robert Daines, Does Delaware Law Improve Firm Value?, 62 J. Fin. Econ. 525, 527 (2001); Paul Gompers, Joy Ishii & Andrew Metrick, Corporate Governance and Equity Prices, 118 Q. J. Econ. 107, 109–10 (2003) [hereinafter Gompers et al., Corporate Governance and Equity Prices]; Larry H. P. Lang & René M. Stulz, Tobin's q, Corporate Diversification, and Firm Performance, 102 J. Pol. Econ. 1248, 1250 (1994); John J. McConnell & Henri Servaes, Additional Evidence on Equity Ownership and Corporate Value, 27 J. Fin. Econ. 595, 596 (1990); Randall Morck, Andrei Shleifer & Robert W. Vishny, Management Ownership and Market Valuation: An Empirical Analysis, 20 J. Fin. Econ. 293, 294 (1988); David Yermack, Higher Market Valuation for Firms with a Small Board of Directors, 40 J. Fin. Econ. 185, 186–87 (1996).

51. 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. For a discussion of Tobin's Q and its definition, see Gary Smith, Tobin's q, *in* 8 The New Palgrave Dictionary of Economics 316, 316–17 (Steven N. Durlauf & Lawrence E. Blume eds., 2d ed. 2008).

52. For studies that use ROA as a metric of operating performance, see, e.g., Gompers et al., Corporate Governance and Equity Prices, supra note 50; Lucian A. Bebchuk, Alma Cohen &

effectiveness with which the firm uses assets of a given book value to generate earnings for investors. We note that activist interventions could improve performance and thereby shareholder value in ways other than through increasing the earnings of assets in place—such as through changing the company's mix of assets or investments. Therefore, of the two metrics we use, Q is probably the one that is most informative about a firm's performance and prospects.[53]

Because industries differ significantly in their levels of Q and ROA, financial economists commonly 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.[54] 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 underperformance.

B. *Operating Performance Following Activist Interventions*

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

Charles C.Y. Wang, Learning and the Disappearing Association Between Governance and Returns, 108 J. Fin. Econ. 323, 341 (2013).

53 A memorandum issued by Wachtell criticizes the analysis of this section on grounds that Tobin's Q and ROA are imperfect metrics for measuring operating performance. Wachtell Memorandum, The Bebchuk Syllogism, supra note 14.While no metric of operating performance is viewed by financial economists as perfect, we chose these two methods because their use as operating performance metrics is standard among financial economists working on corporate governance issues. Indeed, Wachtell does not advocate any particular alternative metric or argue that we failed to make the best possible choices in a world with imperfect metrics for operating performance.

Interestingly, Wachtell seems to have no problem with studies that rely on Tobin's Q in reaching conclusions Wachtell favors; it commended as "impressive empirical work" a recent study by Cremers et al. that relies on Tobin's Q to argue that staggered boards are desirable. See Martin Lipton, Wachtell, Lipton, Rosen & Katz, New Empirical Studies Support Director-Centric Governance, (Dec. 8, 2013), available at www.wlrk.com/webdocs/wlrknew/WLRKMemos/WLRK/WLRK.22995.13.pdf (on file with the Columbia Law Review) (commending study by Cremers et al.); K.J. Martijn Cremers, Lubomir P. Litov & Simone M. Sepe, Staggered Boards and Firm Value, Revisited 32–34 (July 2014) (unpublished manuscript), available at http://ssrn.com/abstract=2364165 (on file with the Columbia Law Review) (using Tobin's Q to assess desirability of staggered boards).

In criticizing the use of Q, Wachtell notes an unpublished paper by Philip Dybvig and Mitch Warachka. See Philip H. Dybvig & Mitch Warachka, Tobin's q Does Not Measure Firm Performance: Theory, Empirics, and Alternatives 3 (Jan. 7, 2015) (unpublished manuscript), available at http://ssrn.com/abstract=1562444 (on file with the Columbia Law Review) (criticizing standard use of Tobin's Q). These authors discuss potential imperfections in the use of Tobin's Q and suggest two alternative metrics of operating performance that, to the best of our knowledge, have not yet been used by any other empirical study that has been published or made available on SSRN since the Dybvig-Warachka paper was first placed on SSRN in 2010.

54. For a well-known study using industry-adjusted performance, see Gompers et al., Corporate Governance and Equity Prices, supra note 50, at 126.

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. We note that Q is highly right skewed, which results in average Q exceeding median Q, and that ROA is highly left skewed, which results in average ROA below median ROA.

TABLE 2: OPERATING PERFORMANCE OVER TIME—NO INDUSTRY ADJUSTMENT

This Table reports the levels of Q and ROA of target companies from the targeting (*t*) to five years afterwards (*t*+5). Both variables are constructed using data from Compustat. Panel A reports the average, the standard error, the median, and the number of observations for the Q of target firms at each point of time. Q is defined as the sum of the market value of equity and book value of debt (including both short-term and long-term debt), scaled by the sum of the book value of equity and book value of debt. Panel B reports the same summary statistics for ROA, where ROA is defined as a firm's EBITDA (earnings before interests, taxes, depreciation, and amortization) scaled by the average value of the firm's assets in the current and previous year. For both Q and ROA, if the value for the lagged assets is missing, the denominator becomes the current year assets. Both variables are recorded at the end of the company's fiscal year and are winsorized at the 1% extreme in the full Compustat sample.[55]

Panel A: Q

	t: Event Year	*t+1*	*t+2*	*t+3*	*t+4*	*t+5*
Average	2.075	2.011	2.035	2.087	2.130	2.150
Standard Error	0.057	0.058	0.065	0.071	0.077	0.082
Median	1.374	1.333	1.317	1.363	1.347	1.412
Observations	1,611	1,384	1,206	1,076	942	831

Panel B: ROA

	t: Event Year	*t+1*	*t+2*	*t+3*	*t+4*	*t+5*
Average	0.026	0.035	0.039	0.051	0.053	0.057
Standard Error	0.005	0.005	0.006	0.006	0.007	0.007
Median	0.070	0.075	0.073	0.084	0.091	0.091
Observations	1,584	1,363	1,187	1,055	926	815

The evidence in Table 2 does not support 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. Panel A shows that, focusing on average Q as a metric of operating performance, average Q exceeds its event year level at *(t+3)*, *(t+4)*, and *(t+5)* and reaches its

55. As is standard, in order to reduce the influence of outliers, our analysis of operating performance winsorizes—that is, limits extreme values in—operating performance results. We winsorize at the 1% and 99% extremes, using the full sample of all Compustat firms from 1991 to 2012 to define extremes.

highest level at *(t+5)*.[56] Panel B in turn indicates that average ROA also exceeds its event year level at *(t+3)*, *(t+4)*, and *(t+5)* and reaches its highest level at *(t+5)*.

Note that, like peer companies of similar size and performance, many of the target firms stop being public companies during the five-year period that we examine, and data about their operating performance are no longer available on Compustat after their delisting. In particular, within five years, targets of activist interventions have "attrition" rates of about 49%, with most of the disappearances from Compustat due to acquisitions. When we compare the target firms to peer companies matched by size and performance, we find that the matched firms also have a high attrition rate of 42% within five years; most disappearances from Compustat are again due to acquisitions. While we focus on the operating performance of the companies that remain public and for which data on Compustat are available, we explain in Part III.E.2 that doing so is unlikely to result in an overstatement of targets' operating performance following the intervention.

As noted in Part III.A, 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.[57] After identifying for each company the firms with the same SIC three-digit industry classification (SIC3), 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.[58] Table 3 below presents the evolution of average industry-adjusted Q and ROA 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 indicates that targets of activist interventions tend to underperform at the time of the intervention. In the year of intervention, both the average

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56. In Table 2, the pattern of improvement is sharper when one examines averages rather than medians. Wachtell Lipton incorrectly criticizes us for failing to stress the difference in Table 2 between results using means and results using medians. Wachtell Memorandum, The Bebchuk Syllogism, supra note 14 (noting "averages can be skewed by extreme results"). However, Table 2 merely presents summary statistics of "raw" levels, and we do not stress or rely on any of its results for our conclusions. As we explain below, the standard approach by financial economists is to control by industry. In our subsequent Table 3, which presents summary statistics using industry-adjusted levels, the results are in fact similar using both means and medians; in both cases, industry-adjusted operating performance, measured by either Tobin's Q or ROA, is higher in each of the five years following the intervention year than during the intervention year. Furthermore, and most importantly, our conclusions are primarily based on a regression analysis, not on the summary statistics of Tables 2 and 3, and our regression analysis (see infra Tables 4 and 5) uses standard methods for avoiding excessive influence of outlier observations.

57. See, e.g., Gompers et al., Corporate Governance and Equity Prices, supra note 50, at 129, 140 (using industry-adjusted Tobin's Q).

58. Industry average levels differ somewhat from industry median levels because both Q and ROA are skewed. Because Q is significantly skewed to the right, industry average tends to be higher than the median. Because ROA is significantly skewed to the left, industry average tends to be lower than the median.

industry-adjusted Q and the average industry-adjusted ROA are negative.[59] Furthermore, and most importantly for the purposes of our inquiry in this Article, Table 3 displays clear patterns of improved operating performance relative to industry peers during the five years following activist interventions.

TABLE 3: INDUSTRY-ADJUSTED OPERATING PERFORMANCE OVER TIME

This table reports the industry-adjusted Q and ROA of target companies from the year of targeting *(t)* to five years afterwards *(t+5)*. Each performance measure, industry-adjusted Q or ROA, is defined as its value in excess of the average value of all firms from the same SIC three-digit industry classification. When using three-digit industry classification results in fewer than five firms, we use two-digit SIC classification or, if using two-digit SIC industry classification also provides fewer than five firms, one-digit industry classification.

Panel A: Industry-Adjusted Q, with Industry Average as Benchmark

	t: Event Year	*t+1*	*t+2*	*t+3*	*t+4*	*t+5*
Average	-0.469	-0.414	-0.335	-0.279	-0.194	-0.137
Standard Error	0.053	0.053	0.059	0.062	0.066	0.075
Median	-0.661	-0.526	-0.471	-0.492	-0.425	-0.399
Observations	1,611	1,384	1,206	1,076	942	831

Panel B: Industry-Adjusted ROA, with Industry Average as Benchmark

	t: Event Year	*t+1*	*t+2*	*t+3*	*t+4*	*t+5*
Average	-0.025	-0.013	-0.009	-0.002	-0.004	-0.004
Standard Error	0.005	0.005	0.005	0.005	0.006	0.006
Median	-0.004	-0.002	0.000	0.000	0.005	0.002
Observations	1,584	1,363	1,187	1,055	926	815

As Panel A shows, 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 underperformance at the time of the intervention.

Panel B displays a similar pattern with respect to average industry-adjusted ROA. 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 closes most of the underperformance 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 in the five years following the intervention. The graphs

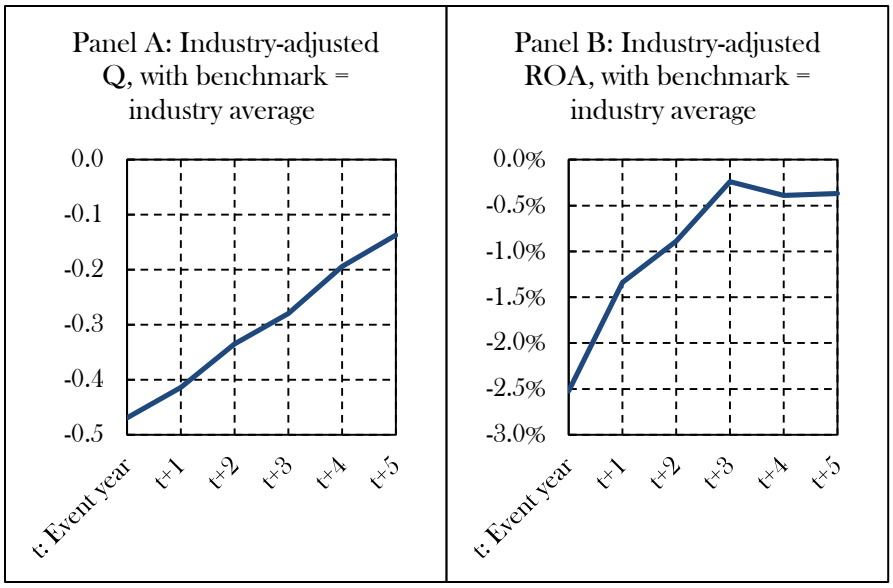
59. In addition, note that the median industry-adjusted Q and the median industry-adjusted ROA are also both negative.

vividly display the increasing patterns of Q and ROA during the years following the intervention.

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.

FIGURE 1: Q AND ROA OVER TIME



1. *Baseline Specifications.* — Panel A of Table 4 below displays the results of four regressions. In columns (1) and (2), we run a regression in which the dependent variable is Q. The adjustment for industry performance is made by including industry- (or firm-) fixed effects (FE). In both regressions we include as explanatory variables dummy variables representing the year of intervention as well as each of the subsequent five years.[60] In the regressions

60. The regressions of Table 4 use over 130,000 observations because we have an observation for each combination of a Compustat firm and one of the years from 1994 (the first year in which interventions in our dataset start) and 2012 (five years after the last year in which such interventions take place). Seeming unfamiliar with how such a regression analysis is conducted, Allaire and Dauphin attack the large number of observations in our Table 4 as suspiciously "far-fetched" and "staggering." See Allaire & Dauphin, Lasting Wealth, supra note 37, at 11 (criticizing large numbers of observations). Allaire and Dauphin also criticize other practices followed by us that are standard in empirical work in financial economics.

reported in Table 4, and all subsequent regressions, we cluster the standard errors at the firm level unless otherwise noted.[61]

As controls, we use in both regressions the company's market value and age,[62] year-fixed effects to account for time trends in the values of 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)*, . . . , *(t+5)* should be interpreted as a "difference-in-difference." It is as if we take a difference of each firm-year Q against the average level of all firms in the same year and also against the average level of all firms in the same industry over all years. We then estimate the difference between the industry-and-year-adjusted Q of firms targeted in the current and next five years and that of the nontarget 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)*, . . . , *(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 ROA rather than Q. Thus, regression (3) includes industry-fixed effects and regression (4) includes firm-fixed effects.

The results of the regressions are consistent with the view that targets of activist interventions tend to underperform at the time of the intervention. The coefficient of the event year is negative in each of the four regressions, and is statistically significant at the significance level of 5% or stronger in three of these four regressions. These results are consistent with target firms performing below their own "normal" levels at the time of intervention.

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61. For a widely cited article recommending such clustering, see Mitchell A. Petersen, Estimating Standard Errors in Finance Panel Data Sets: Comparing Approaches, 22 Rev. Fin. Stud. 435 (2009).

62. For studies suggesting that performance is related to company age, see Rajshree Agarwal & Michael Gort, Firm Product Life Cycles and Firm Survival, 92 Am. Econ. Rev. (Papers & Proc.) 184, 190 (2002); Steven Klepper, Entry, Exit, Growth, and Innovation over the Product Life Cycle, 86 Am. Econ. Rev. 562, 562–63 (1996). Company size is a standard control. We include age and size, but not characteristics that are a function of management choice such as leverage or capital expenses, because these are the policies that activists might seek to change and thus we should not make inferences premised on their being constant.

TABLE 4: EVOLUTION OF ROA AND Q OVER TIME

Panel A of Table 4 reports the results (coefficients and t-statistics in the parentheses) from linear regressions where the dependent variables are Q (columns (1) and (2)) and ROA (columns (3) and (4)), as defined in Table 2. The sample includes all firm-year observations from Compustat from 1991 to 2012. The independent variables of key interest are dummy variables, *(t+j), (j = 0, 1, . . . , 5)*, which are equal to one if a firm was targeted by activist hedge funds in *j* years prior to the current year. Year *(t)* is the year of intervention. Control variables include pre-event dummies *(t–j), (j = 1, 2, 3)*, which are equal to one if a firm is targeted by activist hedge funds *(j)* years going forward; "ln(MV)," which is the logarithm of a firm's market capitalization at a given year-end, and "ln(Age)," which is the logarithm of the number of years since the firm's first appearance in the merged CRSP/Compustat database. All regressions include yearly dummies. Columns (1) and (3) further include SIC3 industry-fixed effects, while columns (2) and (4) include firm-fixed effects. Panel B of Table 4 reports the F-statistics and the associated p-value from two sets of F-tests: One set tests for the equality of the coefficients of *(t+j) (j = 3, 4,* and *5)* and those of *t* (the event year), and the second set tests for the equality of the coefficients of year *(t+j) (j = 3, 4,* and *5)* and those of year *(t–1)* (the year preceding the event year). All standard errors adjust for heteroskedasticity as well as clustering at the firm level. Finally, *, **, and *** indicate statistical significance of the coefficients at the 10%, 5%, and 1% significance levels, respectively.

Panel A: Regressions

	(1)	(2)	(3)	(4)
Dependent Variable	Q	Q	ROA	ROA
t: Event Year	-0.3425***	-0.0273	-0.0104**	-0.0136***
	(-6.34)	(-0.45)	(-2.34)	(-3.00)
t+1	0.2604***	0.0645	0.0030	-0.0032
	(-4.91)	(1.00)	(0.69)	(-0.72)
t+2	-0.1792***	0.1563**	0.0088*	0.0007
	(-3.07)	(2.37)	(1.89)	(0.14)
t+3	-0.0578	0.2395***	0.0148***	0.0054
	(-0.87)	(3.45)	(3.00)	(1.05)
t+4	0.0362	0.2826***	0.0101*	0.0051
	(0.49)	(3.91)	(1.89)	(0.95)
t+5	0.0804	0.3015***	0.0086	0.0048
	(1.02)	(4.14)	(1.53)	(0.89)
ln(MV)	0.2469***	0.8534***	0.0346***	0.0452***
	(31.72)	(51.35)	(51.05)	(41.71)
ln(Age)	-0.3198***	-0.4566***	0.0193***	0.0074***
	(-20.81)	(-17.04)	(16.08)	(3.86)
Year FE	Y	Y	Y	Y
SIC3 FE	Y		Y	
Firm FE		Y		Y
Pre-Event Dummies *(t–1, t–2, t–3)*	Y	Y	Y	Y
Observations	133,562	133,562	130,077	130,077
R-Squared	0.19	0.63	0.27	0.76

TABLE 4: EVOLUTION OF ROA AND Q OVER TIME (CONT.)

Panel B: F-Tests

F-Tests	(1) Q	(2) Q	(3) ROA	(4) ROA
Relative to *t*				
(t+3) vs. (t)	0.29***	0.27***	0.025***	0.019***
F-stat	15.93	14.30	22.18	12.89
p-val	0.00%	0.02%	0.00%	0.03%
(t+4) vs. (t)	0.38***	0.31***	0.021***	0.019***
F-stat	23.65	15.68	12.61	11.46
p-val	0.00%	0.01%	0.04%	0.07%
(t+5) vs. (t)	0.42***	0.33***	0.019***	0.018***
F-stat	26.87	16.21	9.91	10.54
p-val	0.00%	0.01%	0.16%	0.12%
Relative to *(t–1)*				
(t+3) vs. (t–1)	0.33***	0.36***	0.014**	0.011**
F-stat	19.48	23.55	6.46	4.43
p-val	0.00%	0.00%	1.11%	3.52%
(t+4) vs. (t–1)	0.42***	0.40***	0.009	0.011**
F-stat	26.15	23.92	2.49	3.91
p-val	0.00%	0.00%	11.50%	4.80%
(t+5) vs. (t–1)	0.47***	0.42***	0.008	0.011*
F-stat	29.18	24.85	1.68	3.61
p-val	0.00%	0.00%	19.50%	5.74%

Most importantly for the purposes of our inquiry in this Article, there is no evidence for the post-intervention decline in operating performance feared by those making the myopic-activists claim. 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, in each of the regressions, the coefficients for the dummy variables representing the years keep trending up, relative to the coefficient of the time of intervention, during the five-year period we examine, consistent with the view that operating performance improves through the end of this period relative to the performance level at the time of intervention.

Because the myopic-activists claim we investigate focuses on long-term changes in operating performance, we pay special attention to the coefficients for *(t+3)*, *(t+4)*, and *(t+5)*—that is, three, four and five years after the year of interventions. In particular, for each of the four regressions, we conduct two sets of F-tests: One for the difference between each of these coefficients and the coefficient of the event year *t*, and one for the difference between each of these coefficients and the coefficient of the year *(t–1)* that precedes the intervention year. Because both Q and ROA are recorded at the end of the year,

the time in which metrics for year *t* are measured comes after, and the time in which metrics for year *(t–1)* are measured comes before, the exact time in which the occurrence of the intervention is disclosed. We therefore make comparisons both relative to *(t)* and *(t–1)* performance metrics.

Panel B indicates that, in the twelve F-tests we conduct for the two Q regressions, each of the *(t+3)*, *(t+4)*, and *(t+5)* coefficients is higher than the event-year coefficient and the positive difference increases from years three to five.[63] Furthermore, the positive difference is statistically significant at the 1% significance level in each of the twelve F-tests that we conduct. Thus, firm valuation is not pulled down by declining performance in years three to five following the intervention but is rather significantly higher than during the time of the intervention.

Turning to the twelve F-tests we conduct for the two ROA regressions, Panel B indicates that each of the *(t+3)*, *(t+4)*, and *(t+5)* coefficients is higher than the event year coefficient and that the positive difference is statistically significant in nine out of the twelve F-tests we conduct, with significance at the 1% significance level in six out of these F-tests. We note, however, that the positive changes in ROA are less economically significant than the positive changes in Q. One explanation for the difference might be that some of the identified long-term improvements in firm valuation come from channels other than increasing the earning on existing assets.

Thus, whether the comparison is made to the end of the intervention year or the preceding year, and whether using Q or ROA, the results of Table 4 are inconsistent with the view that activist interventions are associated with short-term gains during the first two years that cannibalize performance in subsequent years. The evidence does not support the myopic-activists claim that activist interventions are, during the five-year window following intervention, followed by long-term declines in performance.

Finally, looking at the coefficients for the pre-intervention years used as controls (not tabulated), we find 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 significance level of 5% or stronger. This suggests that the operating performance of the target of an 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.

2. *Using High-Dimensional Fixed Effects*. — For robustness purposes, we re-run the regressions reported in Table 4 substituting the industry and year fixed effects with the higher dimensional fixed effects, and further add firm-fixed effects to the specifications in regressions (2) and (4). The use of year-fixed effects in the regressions of Table 4 enables netting out the average Q or

63. Consistent with Q at the end of the year of the intervention already reflecting the expectations for subsequent improvements in operating performance, the comparison to the *(t–1)* coefficients yields higher positive differences than the comparison to the *t* coefficients.

ROA that is observed in a given year measured across all industries, and the use of industry-fixed effects enables netting out an industry average effect measured across all sample years. By replacing the two types of fixed effects with ones that are unique to each industry and year combination, using fixed effects that are unique to each industry and year combination, we enable accounting for the Q (or ROA) that is observed for a given industry in a given year. By further adding firm fixed effects we analyze the dynamics of performance that adjust for the "normal" level of each firm benchmarked against industry peers in a given year. This estimation procedure, which allows multiple high-dimensional fixed effects, follows the one put forward recently by Guimarães and Portugal.[64]

Panel A of Table 5 below reports our results. Columns (1) and (3) of Table 5 report the results of regressions that use a fixed effect for all observations that belong to a given SIC three-digit industry (SIC3) *and* are in the same year. There are 5,869 SIC3 × year dummy variables for this specification. Columns (2) and (4) further add firm-fixed effects (Firm FE), introducing 22,067 additional dummy variable representing unique firms that ever existed during the sample level. The inclusion of many layers of fixed effects is expected to reduce the power of our tests to detect abnormal performance, and results obtained using this procedure should therefore be assessed in light of this higher hurdle for finding statistical significance.

The specification used in columns (1) and (3) compares targeted firms to control firms in the same year belonging to the same SIC3 industry; and the corresponding F-tests in Panel B of Table 5 test whether the same industry-year benchmark-adjusted improvement in performance during years three to five following intervention (relative to the year *(t)* or the year *(t–1)*) is statistically significant. The specification used in columns (2) and (4) provides one more layer of differencing against a firm's own normal level (i.e., all-time average); and the corresponding F-tests in Panel B of Table 5 test whether the within-firm improvement in years three to five is significant after adjusting for the same industry-year benchmark.

64. Paulo Guimarães & Pedro Portugal, A Simple Feasible Procedure to Fit Models with High-Dimensional Fixed Effects, 10 Stata J. 628, 628–40 (2010) (using simple feasible procedure to fit models with high-dimensional fixed effects). For another recent empirical paper using this procedure, see Todd A. Gormley & David A. Matsa, Common Errors: How to (and Not to) Control for Unobserved Heterogeneity, 27 Rev. Fin. Stud. 617, 646–51 (2014).

TABLE 5: EVOLUTION OF Q AND ROA OVER TIME–USING HIGH-DIMENSIONAL FIXED EFFECTS

Panels A and B of Table 5 follows the same specifications as in Table 4 except replacing year- and industry-fixed effects with the high-dimensional industry (SIC3) × (Year FE). Columns (2) and (4) further add firm-fixed effects. As in Table 4, Panel A reports the results of the regressions and Panel B reports the results of F-tests. All standard errors adjust for heteroskedasticity as well as clustering at the firm level. Finally, *, **, and *** indicate statistical significance of the coefficients at the 10%, 5%, and 1% significance level, respectively.

Panel A: Regressions

	(1)	(2)	(3)	(4)
Dependent Variable	Q	Q	ROA	ROA
t: Event year	-0.3390***	0.0015	-0.0101**	-0.0133***
	(-5.98)	(0.02)	(-2.17)	(-2.84)
t+1	-0.2538***	0.0982	0.0030	-0.0029
	(-4.51)	(1.48)	(0.66)	(-0.63)
t+2	-0.1505**	0.2165***	0.0060	-0.0016
	(-2.40)	(3.10)	(1.22)	(-0.31)
t+3	-0.0764	0.2567***	0.0109**	0.0015
	(-1.10)	(3.56)	(2.09)	(0.29)
t+4	0.0223	0.2974***	0.0062	0.0014
	(0.29)	(3.94)	(1.10)	(0.25)
t+5	0.0815	0.3331***	0.0047	0.0013
	(0.96)	(4.30)	(0.77)	(0.24)
ln(MV)	0.2348***	0.8390***	0.0347***	0.0464***
	(28.94)	(46.51)	(49.12)	(38.91)
ln(Age)	-0.3051***	-0.3628***	0.0189***	0.0065***
	(-19.08)	(-12.67)	(15.24)	(3.17)
SIC3 * Year FE	Y	Y	Y	Y
Firm FE		Y		Y
Pre-event dummies (*t–1*, *t–2*, *t–3*)	Y	Y	Y	Y
Observations	130,077	130,077	133,562	133,562
R-squared	0.31	0.78	0.23	0.65

TABLE 5: EVOLUTION OF Q AND ROA OVER TIME–USING HIGH-DIMENSIONAL FIXED EFFECTS (CONT.)

Panel B: F-Tests

F-Tests:	(1) Q	(2) Q	(3) ROA	(4) ROA
Relative to *t*				
(t+3) vs. (t)	0.26***	0.26***	0.021***	0.015***
F-stat	12.74	12.29	13.80	7.07
p-val	0.00%	0.00%	0.00%	0.80%
(t+4) vs. (t)	0.36***	0.30***	0.016***	0.015**
F-stat	19.78	13.12	7.11	6.34
p-val	0.00%	0.00%	0.80%	1.20%
(t+5) vs. (t)	0.42***	0.33***	0.015**	0.015**
F-stat	22.87	14.20	5.25	6.15
p-val	0.00%	0.00%	2.20%	1.30%
Relative to *(t–1)*				
(t+3) vs. (t–1)	0.30***	0.35***	0.009	0.007
F-stat	14.87	20.25	2.15	1.48
p-val	0.00%	0.00%	14.30%	22.40%
(t+4) vs. (t–1)	0.40***	0.39***	0.004	0.007
F-stat	21.50	20.65	0.39	1.31
p-val	0.00%	0.00%	53.40%	25.20%
(t+5) vs. (t–1)	0.46***	0.43***	0.002	0.007
F-stat	24.67	22.18	0.13	1.30
p-val	0.00%	0.00%	71.50%	25.40%

The results in Table 5 are similar in magnitude and statistical significance to those of Table 4, suggesting that Table 4's findings are robust to the inclusion of high-dimensional-fixed effects. All regressions indicate positive changes in Q and ROA post intervention. For Q regressions, the coefficients of years three, four, and five remain higher than both the coefficient of year *(t)* and the coefficient of year *(t–1)*, and the positive differences are economically meaningful and statistically significant at the 1% significance level in all 12 F-tests we conduct. For ROA regressions, these differences in coefficients are positive in all F-tests and significant in six out of the twelve F-tests we conduct. Thus, the results of Table 5 overall reinforce the conclusions of Table 4 that the asserted adverse effect on long-term performance is not supported by the data.

D. *Controlling for Past Performance*

As we have seen, target firms are underperforming at the time of intervention, and it might be suggested that post-intervention improvements are driven by initial underperformance that provides room for improvement or

facilitates reversion toward the mean.[65] In this section we therefore examine whether our results are robust to controlling for prior underperformance. To this end, we extend the regressions reported in Tables 4 and 5 to control for past performance.

In particular, for each of the regressions in Tables 4 and 5 used to assess performance improvement during a given period since the time of intervention employing an F-test, we add a lagged performance variable where the time lag matches the given period. More formally, when we analyze the performance improvement in year *(t+j)* relative to *(t)* (or *(t–1)*), we add the performance *(j)* years (or *(j+1)* years) ago into the regression. For example, in order to test the difference between Q at *(t+3)* and Q at *(t)*, we add the Q value of the same firm three years ago to the regression as an additional control variable. Thus, the estimated improvement in the performance in three years post-intervention controls for the performance of both target and control firms three years prior to the point of assessment.

We further vary the specifications of the regressions by different combinations of industry, firm, and year fixed effects using the various specifications employed in Tables 4 and 5. Table 6 below reports the results. In addition to the added lagged performance variable, columns (1) and (4) use industry- and year-fixed effects following the specifications in columns (1) and (3) of Table 4; columns (2) and (5) use firm- and year-fixed effects following the specifications in columns (2) and (4) of Table 4; and columns (3) and (6) use high-dimensional-fixed effects that are unique to each industry and year combination following the specifications of columns (1) and (3) of Table 5. Because each F-test is derived from a unique regression, we do not report the numerous regressions we run but only the results from the F-tests. For each F-test, we report the difference between the coefficients on *(t+j)* and *(t)* (or (between *(t+j)* and *(t–1)*), the F-statistics, and the associated p-value.

The results in Table 6 indicate that the results reported by Tables 4 and 5 are robust to controlling for performance at the time of intervention. For the Q regressions, the coefficients of *(t+3)*, *(t+4)*, and *(t+5)* are higher than both the coefficient of *t* and the coefficient of *(t–1)*, and the differences are positive and statistically significant at the 1% significance in all the eighteen F-tests that are reported in Table 6. For the ROA regressions, these differences are positive in all the eighteen F-tests that we conduct and are statistically significant in ten out of these eighteen F-tests at the 5% significance level and in one other F test at the 10% significance level. Thus, the results reported in Table 6 indicate that the conclusions reached in the preceding section are robust to control for past performance.

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65. See John C. Coffee, Jr. & Darius Palia, The Impact of Hedge Fund Activism: Evidence and Implications 5–6 (European Corporate Governance Inst., Working Paper No. 266, 2014), available at http://ssrn.com/abstract=2496518 (on file with the *Columbia Law Review*) (discussing usefulness of taking into account initial underperformance of activists' targets). Wachtell Lipton also expresses concern that our work does not sufficiently take into account that the targets of activism tend to be underperforming at the time of intervention. Wachtell Memorandum, The Bebchuk Syllogism, supra note 14.

TABLE 6: CONTROLLING FOR PAST PERFORMANCE

This Table reports the results of the F-tests for the differences between the coefficients on *(t+j), (j =3, 4, 5)* and the coefficients on year *(t)* or year *(t–1)* (as the case may be) from regressions that extend those reported in Table 4 and Table 5 to control for past performance. Each F-test is from a unique regression. In each regression, the dependent variable (Q or ROA), the key independent variables, *(t), (t+1), . . . (t+5)*, and the control variables ln(MV) and ln(Age) are the same as in Table 4. In addition, we add *one* lagged performance variable (Q or ROA) in year *(t–j)* (or *(t–j–1)*), in a regression that tests the difference between *(t+j)* and *(t)* (or *(t–1)*). For example, in order to test the difference of *(t+3)* vs. *(t)* for Q, we add Q*(t–3)* (i.e., the Q value of the same firm three years ago) to the regression as an additional control variable. We further vary the specifications of the regression by a different combination of industry, firm, and year fixed effects, as reported at the bottom on the table. For economy of space, the regression coefficients are suppressed. For each F-test conducted, we only report, the difference of the coefficients, the F-statistics, and the associated p-value. All standard errors adjust for heteroskedasticity as well as clustering at the firm level. Finally, *, **, and *** indicate statistical significance of the coefficients at the 10%, 5%, and 1% significance level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
F-Tests	Q	Q	Q	ROA	ROA	ROA
Relative to *t*						
(t+3) vs. (t)	0.24***	0.20**	0.21***	0.033***	0.030***	0.021***
F-stat	7.77	5.11	7.77	27.28	18.86	15.14
p-val	0.53%	2.38%	0.53%	0.00%	0.00%	0.01%
(t+4) vs. (t)	0.32***	0.30***	0.23***	0.033***	0.029***	0.019***
F-stat	11.85	9.21	7.52	24.11	16.20	11.60
p-val	0.06%	0.24%	0.61%	0.00%	0.01%	0.07%
(t+5) vs. (t)	0.37***	0.38***	0.24***	0.03***	0.026***	0.021***
F-stat	14.35	13.04	7.41	19.63	12.81	11.98
p-val	0.02%	0.03%	0.65%	0.00%	0.03%	0.05%
Relative to (*t*–1)						
(t+3) vs. (t–1)	0.35***	0.34***	0.29***	0.012*	0.011	0.009
F-stat	15.32	13.61	12.46	3.17	2.15	2.51
p-val	0.01%	0.02%	0.04%	7.50%	14.30%	11.30%
(t+4) vs. (t–1)	0.40***	0.41***	0.30***	0.012	0.009	0.012**
F-stat	18.82	17.35	13.00	2.52	1.37	4.10
p-val	0.00%	0.00%	0.03%	11.30%	24.10%	4.29%
(t+5) vs. (t–1)	0.44***	0.43***	0.34***	0.011	0.008	0.009
F-stat	21.99	18.12	15.54	2.24	1.00	2.00
p-val	0.00%	0.00%	0.01%	13.50%	31.90%	15.70%
SIC3 FE	Y			Y		
Firm FE			Y			Y
Year FE	Y		Y	Y		Y
SIC3 × Year FE		Y			Y	

The potential issue of "mean reversion" might be most relevant when a target firm was at bottom performance levels among industry peers. To address this issue, we ran another set of regressions. In particular, we re-ran the regressions of Table 6 replacing the lagged performance control with a dummy variable indicating whether the lagged performance was at the bottom quartile in the industry-year group.[66] The (untabulated) results are either very similar to or stronger than those reported in Table 6, and they thus further confirm that our findings are robust to controlling for past performance.

E. *Interpreting Our Findings*

1. *A Clear Pattern*. — The analysis of the preceding sections of this Part 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 and also their own historical levels at the time of intervention. Moreover, 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 supporting concerns that activist interventions are followed by short-term gains that come at the expense of subsequent long-term declines in operating performance. Examining both Q and ROA, and conducting comparisons both to the end of the year following the intervention and the end of the year preceding it, the feared adverse effect on long-term performance is not found in the data. Indeed, in each of the years three, four, and five following the intervention, we find improvements that are statistically significant. Thus, overall, the evidence on firm performance does not support the myopic-activists claim.

2. *Adverse Effect on the Post-Acquisition Performance of Acquired Firms*? — As is the case with peer companies, a significant percentage of targeted firms are no longer public by the end of the five-year period, having been acquired or otherwise delisted, and are thus no longer part of the Compustat dataset of public company data. Because our results indicate that targets' operating performance improves for as long as they remain public, it might still be argued that activism has an adverse effect on targets that stop being public during the five-year period, that this effect occurs after these targets are no longer public and thus is not detected by our analysis, and that this adverse effect is sufficiently large to make the effects of activism overall negative in the aggregate.[67]

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66. We classify the industry at the SIC three-digit level if there are at least eight firms in the industry during the year—so that the quartile is well defined—and use the SIC two-digit classification if the SIC three-digit level includes less than eight firms.

67. Wachtell Lipton criticizes our study for focusing on the operating performance of companies that remain independent. Wachtell Memorandum, The Bebchuk Syllogism, supra note 14. While we address this issue below, it is surprising to have this criticism come from Wachtell Lipton given that in an earlier memo, the firm's founding partner stated that the important

However, there is no reason to expect that the operating performance of targets that are acquired will be more likely to decline rather than improve post-acquisition. Indeed, acquisitions can often be expected to be motivated by the acquirer's expectation that it will be able to improve the performance of the purchased assets through synergies or otherwise. To the extent that this is the case, it can be expected that the performance of assets of activism targets that are acquired will tend to improve, rather than decline, after the targets are acquired and stop having their operating performance reported on Compustat.

Furthermore, as explained below, this concern is directly addressed by a recent empirical study, co-authored by two of us and Hyunseob Kim, that tracks the operating performance of activism targets after they are acquired.[68] That study uses U.S. Census Bureau's longitudinal databases of manufacturing businesses to study activism at targets engaged in manufacturing. A key attribute of the Census data is that the Census continues to record data on manufacturing assets previously belonging to a public company even after the company stops being public due to an acquisition or otherwise.[69] The study is therefore able to assess directly, for targets in the manufacturing sector, what changes in operating performance took place in targets that stopped being public.

The study documents that plants belonging to targets that eventually drop from the Compustat database perform better than those plants whose firms are still covered by Compustat. Thus, to the extent that the targets out the manufacturing sector exhibit a similar pattern, the evidence provided by the study indicates that focusing on target firms that remain public should not be expected to result in an overstatement – and indeed might well generate an understatement – of the post-intervention performance of targets.

3. *Stock Picking*? — Finally, critics of hedge fund activism might argue that the identified association between activist interventions 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.[70] Under such a scenario, the improvement in long-term performance experienced by targets reflects the activists' "stock picking" ability rather than the activists' impact on the company's operating performance.

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question to study is, "[f]or companies that are the subject of hedge fund activism and remain independent" 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." Wachtell Memorandum, Bite the Apple, supra note 10.

68. See Brav et al., The Real Effects of Hedge Fund Activism, supra note 41 (describing empirical study of activism targets in manufacturing sector that tracks operating performance of their assets over time even if companies are acquired).

69. Id. at 5–6 (discussing advantages of census data).

70. See Wachtell Memorandum, The Bebchuk Syllogism, supra note 14 ("[F]avorable results would arise . . . whenever managements of the target companies pursue value-enhancing strategies.").

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 improvements, would already concede that the long-term consequences 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 limit the influence of activists regardless of how much credit they should be getting for these improvements. Stock pickers who successfully bet on future improvements might not deserve a medal, but they do not warrant opposition and resistance.

However, there are at least three reasons to believe that the identified improvements in operating performance are at least partly due to the activist interventions. First, activist engagements involve significant costs, and activist investors would have strong incentives to avoid bearing them if they believed that the improvements in performance would ensue in any event, even without engaging with target companies. In such a case, these investors would just buy a stake, avoid any intervention, and capture the benefits of the improved performance expected to take place without incurring costs. Thus, activists' willingness to bear the significant costs of engagement likely reflects their judgment that their activities contribute to the subsequent improvements in operating performance.

Second, as will be discussed in Part V, improvements in operating performance follow activist interventions not just in our dataset as a whole but also in the subset of activist interventions that employ adversarial tactics. Such tactics are used when activists expect companies to resist the activists' suggested course of action. This finding is in tension with the view that the improvements in operating performance following activist interventions are due to corporate actions that incumbents would choose to take even without any intervention.

Furthermore, the view that the interventions contribute to subsequent improvements is consistent with the finding in earlier work co-authored by two of us (together with Hyunseob Kim) that such improvements do not take place after outside blockholders pursuing a passive strategy announce the purchase of a block of shares, but do occur when blockholders switch from passive to activist stance.[71] 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.

We therefore conclude that the identified improvements in performance should be expected to be at least partly due to the activist intervention. Of course, causality issues in corporate governance and finance are notoriously

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71. See Brav et al., The Real Effects of Hedge Fund Activism, supra note 41, at 27–29 (reporting such findings).

difficult to resolve with absolute confidence,[72] and we do not aim at precise identification of the extent to which the improvements are due to activist interventions. Our chief interest in this Article is in investigating empirically whether the long-standing and influential claim that activist interventions are followed by declines in long-term operating performance is backed by the evidence. 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. As discussed in Part I.A, opponents of hedge fund activism believe that the initially positive stock-market reaction to activist interventions represents 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. On this view, while activists might benefit from capturing positive stock-price returns prior to their departure, the negative long-term stock returns that follow their exit leave long-term shareholders holding the bag. In this Part, we subject these claims to an empirical test.

Part IV.A begins by examining the abnormal stock returns that such shareholders experience during the forty-day period surrounding the filing of Schedule 13D by an activist hedge fund. Part IV.B investigates empirically whether these initial gains are wiped out by significant negative returns in subsequent years. Part IV.C examines empirically the long-term returns that follow the departure of activists. Finally, Part IV.D concludes.

A. *Short-Term Returns*

We begin by examining the stock price movements that accompany the announcement of an activist campaign in our dataset. We thus 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,[73] as well as in a study conducted by April Klein and Emanuel Zur.[74] These initial findings were corroborated by three subsequent

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72. See, e.g., Alma Cohen & Charles C.Y. Wang, How Do Staggered Boards Affect Shareholder Value? Evidence from a Natural Experiment, 110 J. Fin. Econ. 627, 628–29 (2013) (stressing difficulties involved in resolving questions of causality).

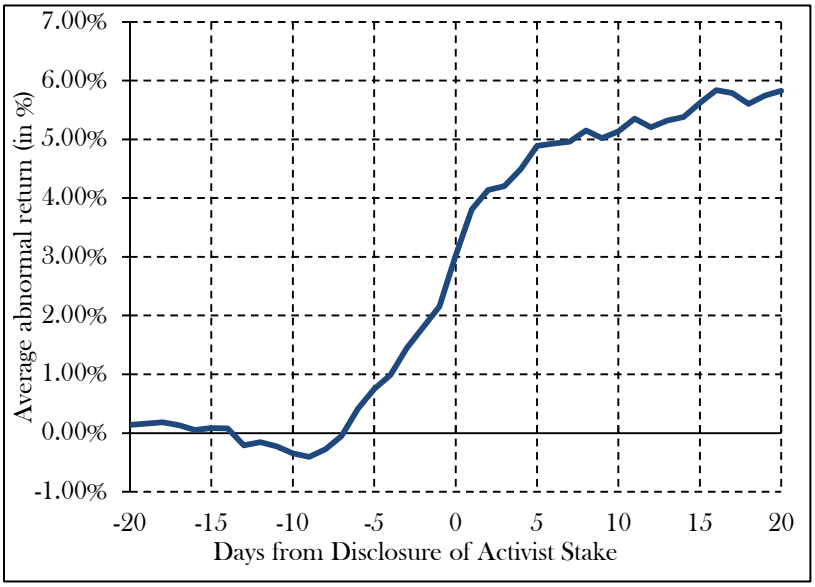
73. See Brav et al., Hedge Fund Activism, supra note 17, at 1755–57 (finding positive abnormal returns for twenty-day event windows around filing of Schedule 13D).

74. April Klein & Emanuel Zur, Entrepreneurial Shareholder Activism: Hedge Funds and Other Private Investors, 64 J. Fin. 187, 207–11, 225–26 (2009) (finding positive abnormal stock returns during thirty-day event windows surrounding initial Schedule 13D filings).

studies.[75] Furthermore, a recent study documented that disclosures of activist interventions are accompanied by positive abnormal stock returns in more than 20 stock markets outside the United States.[76]

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 forty-day window surrounding the filing of a Schedule 13D. This period begins twenty days before an activist hedge fund files a Schedule 13D through twenty days afterwards.

FIGURE 2: SHORT-TERM STOCK RETURNS AROUND 13D FILINGS



As the Figure shows, the average abnormal returns observed during the twenty-day period before and after an investor files a Schedule 13D are

75. For subsequent studies confirming the positive stock price reactions to 13D filings, see Christopher P. Clifford, Value Creation or Destruction? Hedge Funds as Shareholder Activists, 14 J. Corp. Fin. 323, 328–33 (2008); Robin Greenwood & Michael Schor, Investor Activism and Takeovers, 92 J. Fin. Econ. 362, 362–75 (2009); Nicole M. Boyson & Robert M. Mooradian, Intense Hedge Fund Activists 21–30 (Oct. 12, 2009) (unpublished manuscript), available at http://ssrn.com/abstract=1492641 (on file with the *Columbia Law Review*).

A related study examined activist engagement by the Hermes U.K. Focus Fund and found that positive and significant abnormal short-term returns (about 5% in a seven-day event window) accompanied the announcement of changes produced by such engagement. See Marco Becht, Julian Franks, Colin Mayer & Stefano Rossi, Returns to Shareholder Activism: Evidence from a Clinical Study of the Hermes UK Focus Fund, 22 Rev. Fin. St. 3093, 3113–17 (2009).

76. See Becht et al., The Returns to Hedge Fund Activism: An International Study 55 (European Corporate Governance Inst., Working Paper No. 402/2014, 2015) available at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2376271 (on file with the *Columbia Law Review*).

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 positive market reaction to the appearance of a hedge fund activist is consistent with the view that activists provide benefits to, rather than impose costs on, the targets of their campaigns.[77]

Opponents of activism do not contest the clear evidence that activist interventions are accompanied by positive short-term stock returns but rather dismiss its significance. Martin Lipton, for example, 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 . . . 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."[78]

For hedge fund activism to reduce the wealth of shareholders in the long term, it must be the case 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. We will now turn to empirically assessing these propositions.

B. *Subsequent Reversal?*

Clearly, the above proposition has empirical implications that make it testable using publicly available data. 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 Part IV.B.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 Part IV.B.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 Part IV.B.3, using a portfolio approach, we examine whether a portfolio that took a position in each targeted company after the 13D announcement

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77. A recent study confirms that the significant positive returns accompanying Schedule 13D announcement continue after our sample period ends in 2007. See Krishnan et al., supra note 48, at 3 ("the announcement period abnormal stock price returns from hedge fund activism are consistently and robustly high from 2008 through 2014").

78. Wachtell Memorandum, Bite the Apple, supra note 10.

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 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,[79] and the Fama-French-Carhart asset-pricing model.[80] Each of these standard models provides a framework that enables 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 co-movement with the market.[81] 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.[82]

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.[83] 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 7 below provides results concerning the alphas we have calculated. For each of the periods, we provide both the median and average alpha for all

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79. For an account of the Capital Asset Pricing Model, see Richard A. Brealey, Stewart C. Myers & Franklin Allen, Principles of Corporate Finance 197–205 (11th ed. 2014).

80. For the classic studies introducing this model, see Mark M. Carhart, On Persistence in Mutual Fund Performance, 52 J. Fin. 57 (1997); Eugene F. Fama & Kenneth R. French, Common Risk Factors in the Returns on Stocks and Bonds, 33 J. Fin. Econ. 3 (1993).

81. Specifically, we estimate for each firm *(i)* an alpha using the regression:

$r_{it} - r_{f,t} = \alpha_i + \beta_{i1} RMRF_t + \varepsilon_{it}$.

82. See sources cited supra note 80 (introducing this model). Specifically, we estimate for each firm *(i)* an alpha using the regression:

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

We obtain the factor returns and monthly risk-free rates from Ken French's website at the Tuck School of Business at Dartmouth College, see Kenneth R. French, Data Library, Tuck at Dartmouth, http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html (on file with the *Columbia Law Review*) (last visited Mar. 28, 2015).

83. We do such estimation for all firms that have a minimum of twenty-four monthly returns following the intervention (i.e., all firms that remained public for at least twenty-four 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 month of intervention.

the firms in our sample. We also indicate the statistical significance of our results; however, as is now well known in the financial-economics literature, 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.[84]

TABLE 7: FIRM-LEVEL ESTIMATES OF ABNORMAL RETURNS SUBSEQUENT TO HEDGE FUND INTERVENTION—USING MARKET-PRICING MODELS

This Table reports statistics on abnormal returns to target firms subsequent to hedge fund activism. For each firm targeted by a hedge fund activist, we estimate a monthly alpha for three distinct event periods. The first event period extends from three years prior to the month of the intervention through the month prior to the intervention; the second and third event periods both begin in the month following the month of the intervention through either three or five years following the month of the intervention. For the latter two-event periods, we require a minimum of twenty-four monthly returns following the intervention. Panel A presents average, median, standard deviation, t-statistic, and number of estimated firm alphas for the CAPM regressions for each of the three-event periods. Panel B presents these statistics for the regressions based on the Fama-French-Carhart four-factor model.

Panel A: CAPM Alphas

	Holding Period (in Months)		
	[-36,-1]	[+1,+36]	[+1,+60]
Median	-0.25	0.49	0.65
Average	-0.17	0.52	0.44
Standard Deviation	2.73	2.99	2.62
t-stat	-2.42	6.13	6.11
Observations	1478	1264	1294

Panel B: Four-Factor Alphas

	Holding Period (in Months)		
	[-36,-1]	[+1,+36]	[+1,+60]
Median	-0.40	0.25	0.40
Average	-0.28	0.33	0.23
Standard Deviation	2.90	3.31	2.91
t-stat	-3.65	3.55	2.81
Observations	1478	1264	1294

The first column in Table 7 provides our results concerning stock returns during the three-year period preceding the intervention. Using both the CAPM pricing model and the Fama-French-Carhart four-factor pricing model, we find an alpha during this period that is negative and economically meaningful. The monthly abnormal return has a median of -0.25% and an average of -0.17% in the first pricing model and has a median of -0.40% and an average of -0.28% in

84. For a discussion of this problem, see, e.g., Eugene F. Fama, Market Efficiency, Long-Term Returns, and Behavioral Finance, 49 J. Fin. Econ. 283, 295–96 (1998) [hereinafter Fama, Market Efficiency].

the second pricing model. These results, like those concerning operating performance obtained in Part III, are consistent with the view that hedge fund activists target underperforming companies.

The second and third columns provide results concerning stock returns during the three- and five- year period following the 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 thus fail to provide evidence for the negative returns during these periods hypothesized by opponents of hedge fund activism.

2. *Buy-and-Hold Abnormal Returns*. — 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.[85]

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 twenty-five Fama and French 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 8 below. The Table provides both equal- and value-weight average buy-and-hold abnormal return during a long holding period 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.

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85. For a well-known study using such an approach, see generally Brad M. Barber & John D. Lyon, Detecting Long-Run Abnormal Stock Returns: The Empirical Power and Specification of Test Statistics, 43 J. Fin. Econ. 341 (1997).

TABLE 8: BUY-AND-HOLD ABNORMAL RETURNS SUBSEQUENT TO HEDGE FUND INTERVENTION

This Table reports statistics on buy-and-hold abnormal returns to target firms subsequent to hedge fund activism. For each firm targeted by a hedge fund activist, we compute the buy-and-hold return beginning in the first month post-intervention extending through either three or five years afterwards. For each firm, we use information on its pre-event market capitalization and book-to-market to match it to one of the twenty-five Fama-French size and book-to-market value-weight portfolios. To allow for time variation in expected returns, we allow the benchmark portfolio to change by using the new firm attributes in every subsequent event 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. If a target firm delists prior to the end of 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. For each event window, we report both equal-weight and value-weight average abnormal returns, the standard deviation of abnormal returns, and the number of observations.

Window	Average Abnormal Return		St. Dev.	Observations
	Equal-Weight	Value-Weight		
[+1,+36]	7.17	2.58	4.97	1605
[+1,+60]	-0.29	5.81	4.43	1605

Consistent with the regression-based evidence presented earlier, the evidence indicates that the value-weighted buy-and-hold abnormal returns are positive at the three-year holding period (2.58% over a thirty-six-month period) and the five-year holding period (5.81% over a sixty-month period), and that equal-weighted buy-and-hold abnormal returns are positive at the three-year holding period (7.17% over a thirty-six-month period) and are practically zero at the five-year holding period (-0.29% over a sixty-month period). While we find positive returns in three specifications, these positive returns are not statistically significant.[86] Overall, the findings of Table 8 do not support the view that activist interventions are followed by abnormal negative long-term returns, and thus stock return underperformance, for the target's shareholders.

3. *Portfolio Analysis*. — Next, we turn to "calendar-time portfolio regressions" in which event firms are grouped into a portfolio whose abnormal portfolio over time is estimated. 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 that 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 that are held for three years before selling. We form portfolios with both equal- and value-weighting of firms' returns.

86. We report the cross-sectional standard deviation of abnormal returns in the column marked "St. Dev." However, because long-horizon abnormal returns are likely to be positively correlated, our use of the cross-sectional standard deviation assuming independence underestimates the true standard error. This factor further reinforces the conclusion that the positive returns in three specifications are not statistically significant.

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 with the number of events firms in a given calendar month as weights.[87]

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 9 below provides the regression results. Panel A reports the results of equal-weighted portfolios, and Panel B reports the results of value-weighted portfolios. "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. "R-squared" is the adjusted R^2 from the regressions and "N" is the number of monthly portfolio return observations. We set a minimum of ten firms per month for all portfolios.

The results in Table 9 indicate that, in both Panel A and Panel B, the returns to activist targets are highly correlated with the SMB and HML factors, reflecting the fact that targeted companies co-move with the returns of small firms (firms that are relatively small in size) and value firms (firms with a relatively high book-to-market value ratio). 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.

With respect to the thirty-six months preceding the intervention, the results reported in Table 9 are consistent with earlier results reported in Table 7 above. Targeted firms underperform in the three-year period prior to the arrival of activist hedge funds. The monthly abnormal return (alpha) from the equal weight results in Panel A is similar to the average of the individual target firm alphas reported in Table 7, Panel B.

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87. In our setting, such an approach is especially warranted because the number of observations fluctuates considerably during the years we consider. We also ran our tests without using such weighted 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 9: ABNORMAL RETURNS SUBSEQUENT TO HEDGE FUND INTERVENTIONS—USING CALENDAR-TIME PORTFOLIOS

This Table reports statistics on abnormal returns to target firms subsequent to hedge fund activism from calendar-time portfolio regressions. The portfolio holding 'window' indicates the holding period in months relative to the month of the hedge fund intervention. For example, the portfolio with the holding period [+1, +36] continually adds target firms that have had an activist event in the preceding month and holds these firms through three years after their respective activism event. The regression follows the specification provided in footnote 82. Panel A provides the results based on equal-weight portfolio regressions, whereas Panel B provides results based on value-weight portfolio regressions. "Alpha" is the estimate of the regression intercept from the four-factor model. "Beta" is the loading on the market excess return; SMB and HML are the estimates of portfolio factor loadings on the Fama-French size and book-to-market factors; MOM is the portfolio factor loading on the Carhart momentum factor. R-squared is the adjusted R^2 from the regressions. Finally, N is the number of monthly observations. We estimate the regression coefficients employing weighted least squares and using the number of events firms in a given calendar month as weights. We set a minimum of ten firms per month for all portfolios. Finally, *, **, and *** indicate statistical significance of the coefficients at the 10%, 5%, and 1% significance levels, respectively.

Panel A: Equal-Weight Four-Factor Model

Window	Alpha	Beta	SMB	HML	MOM	N	R-squared
[-36,-1]	-0.29*	0.96***	0.87***	0.32***	-0.25***	167	87.20%
	(1.71)	(20.52)	(18.10)	(5.31)	(7.30)		
[+1,+36]	0.24	0.90***	0.88***	0.26***	-0.27***	189	84.54%
	(1.20)	(19.72)	(15.30)	(4.14)	(7.45)		
[+1,+60]	0.21	0.92***	0.82***	0.25***	-0.25***	213	87.90%
	(1.29)	(24.71)	(16.66)	(5.12)	(8.55)		

Panel B: Value-Weight Four-Factor Model

Window	Alpha	Beta	SMB	HML	MOM	N	R-squared
[-36,-1]	-1.13***	1.09***	0.59***	0.28***	-0.17***	167	89.35%
	(7.86)	(27.01)	(14.04)	(5.30)	(5.70)		
[+1,+36]	0.17	0.98***	0.53***	0.26***	-0.02	189	86.41%
	(1.10)	(27.22)	(11.7)	(5.33)	(0.73)		
[+1,+60]	-0.03	0.98***	0.40***	0.25***	-0.01	213	86.19%
	(0.23)	(29.61)	(9.09)	(5.64)	(0.30)		

With respect to the thirty-six-month and sixty-month periods following the intervention, we examine post-intervention returns using four specifications. We looked at both equal-weighted and value-weighted portfolios, and for each we examined both a three-year period and a five-year period. The alpha we obtained is positive and economically meaningful in three specifications (ranging from 0.17% a month to 0.24% a month) and is negative but economically insignificant (0.03% a month) in the fourth specification. In all four specifications, however, the alpha coefficient is not statistically significant.

Thus, this testing approach also finds no evidence for the asserted long-term underperformance of activism targets.

4. *Summary*. — Overall, the analysis of stock returns carried out in this Part provides no support for the 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 asserted by the myopic-activists claim 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. In particular, we examine below whether negative long-term returns follow such departures 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,[88] and a subsequent study by Boyson and Mooradian reached a similar conclusion.[89] 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.[90]

Opponents of activism do not dispute that activist hedge funds and their investors benefit from activism. Rather, they assert that, while "[a]ctivist hedge funds are reportedly outperforming many other asset classes," the value they capture is "appropriated from fellow stockholders with longer-term investment horizons."[91] Such divergence in the returns to activists and long-term shareholders can be expected only if activist hedge funds succeed in getting out before the stock prices decline. This pump-and-dump 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 to bear losses while activist hedge funds capture positive returns. If activist hedge funds were to bail out

88. See Brav et al., Hedge Fund Activism, supra note 17, at 1760 (reporting such gains).

89. Boyson & Mooradian, supra note 75, at 25–30 (finding abnormally high returns to hedge funds engaged in intense activism).

90. Brav et al., The Returns to Hedge Fund Activism, supra note 39, at 54–56 (documenting outperformance of activist hedge funds during January 2005–June 2007 period).

91. Wachtell Memorandum, Important Questions, supra note 29.

before such negative returns take place, this would imply that (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, in this case, although (i) would be lower than (ii), (i) might still be positive.

The existence of the hypothesized negative returns provides another proposition that clearly can and should have been empirically tested, using publicly available data, by supporters of the myopic-activists claim. We conduct such a test below.

In particular, we focus on stock returns in the three years that follow an activist's 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."[92] We study the long-term returns 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 Part IV.B.1 for studying stock returns for each individual firm following activist arrivals. As was done in Part IV.B.1, 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 firm that was the target of activist interventions, we estimate an alpha, or average abnormal return, for the three years following the month of the activist's departure.[93]

Table 10 below provides results concerning the alphas that we 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 abnormal performance and the reported t-stats should thus be treated as merely suggestive.[94]

The results reported in Table 10 indicate that during the three-year period following activists' departures, estimated alphas are positive and statistically significant at the 1% significance level. This is the case both for the CAPM model and the Fama-French-Carhart four-factor model. Thus, the data provide no support for the pump-and-dump patterns feared by holders of the myopic-activists view.

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92. We note that the time difference between the initial 13D filing and the departure date in our database of activist interventions has a median of 539 days (about 1.5 years) and an average of 811 days (over two years).

93. Similar to what was done in Section V.B.1, we make such an estimation for all firms that have a minimum of twenty-four monthly returns following the departure so that there is a significant number of monthly returns on which a regression can be based.

94. See Eugene Fama, Market Efficiency, supra note 84, at 294–96 (stating "failure to account for the cross-correlation of event firm returns during long post-event periods can affect inferences").

TABLE 10: FIRM-LEVEL ESTIMATES OF ABNORMAL RETURNS SUBSEQUENT TO HEDGE FUND DEPARTURE—USING MARKET-PRICING MODELS

This Table reports statistics on abnormal returns to target firms subsequent to the departure of hedge fund activists. For each firm targeted by a hedge fund activist with an identified exit date, we estimate a monthly alpha extending from the month of the activist's departure through three years afterwards. We require a minimum of twenty-four monthly returns. Panel A presents average, median, standard deviation, t-statistic, and number of estimated firm alphas based on the CAPM regressions, and Panel B presents these statistics for the regressions based on the Fama-French-Carhart four-factor model.

Panel A: CAPM Alphas	
Holding Period	[+1,+36]
Median	0.77
Average	0.77
Standard Deviation	3.24
t-stat	6.39
Observations	722

Panel B: Four-Factor Alphas	
Holding Period	[+1,+36]
Median	0.64
Average	0.53
Standard Deviation	3.24
t-stat	4.40
Observations	722

3. *Buy-and-Hold Results*. — As we did in Part IV.B.2, we next consider buy-and-hold abnormal return as an alternative measure of abnormal performance. We follow the same methodology used to produce the results displayed in Table 8. In particular, we compute the buy-and-hold return over a three-year period after the activists' departure net of a benchmark portfolio, with the return and the benchmark portfolio computed and identified in the ways described in Part IV.B.2.

The results are reported in Table 11 below. As in Table 8, 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. Consistent with the results based on individual-firm regressions presented in Table 10, the results in Table 11 indicate that buy-and-hold abnormal returns are positive. This is the case both when using equal weighting and when using value weighting. Thus, the results in Table 11 are consistent with the conclusion that pump-and-dump concerns are not supported by the data.

TABLE 11: BUY-AND-HOLD ABNORMAL RETURNS SUBSEQUENT TO HEDGE FUND EXIT

This Table reports statistics on buy-and-hold abnormal returns to target firms subsequent to the departure of hedge fund activists. For each firm targeted by a hedge fund activist with an identified exit date, we compute the buy-and-hold return beginning in the first month post-departure extending through three years afterwards. For each event firm, we use information on its pre-departure market capitalization and book-to-market to match it to one of the Fama-French 25 size and book-to-market value-weight portfolios. To allow for time variation in expected returns, we allow the benchmark portfolio to change by using the new firm attributes in every subsequent event 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. If a target firm delists prior to the three-year 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. We report both equal-weight and value-weight average buy-and-hold abnormal returns, the standard deviation of abnormal returns, and number of observations.

Window	Average Abnormal Return		St. Dev.	Observations
	Equal-Weight	Value-Weight		
[+1,+36]	19.06	14.9	11.73	952

4. *Portfolio Analysis*. — Finally, as we did in Part 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 departure of the activist and hold them for three years before selling. We form portfolios with both equal and value-weighting of firms' returns, and we estimate abnormal returns following the methodology described in Part IV.B.3.

Table 12 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.

The results reported in Table 12 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 at the 5% level (0.37% over the thirty-six-month period, t-stat = 2.16), and the value-weight portfolio has a monthly alpha that is positive and statistically significant at the 10% level (0.31% over the 36-month period, t-stat = 1.81). Thus, like the other two methodologies used earlier, the portfolio approach again fails to find any evidence in support of the pump-and-dump concerns.

TABLE 12: CALENDAR-TIME PORTFOLIO REGRESSIONS OF ABNORMAL RETURNS SUBSEQUENT TO HEDGE FUND EXIT

This Table reports statistics on abnormal returns to target firms subsequent to hedge fund activists' departure from calendar-time portfolio regressions. The portfolio holding "window" indicates the holding period in months relative to the month of the departure by the activist. The regression takes the form specified in footnote 82. Panel A provides the results based on an equal-weight portfolio regression, whereas Panel B provides results based on a value-weight portfolio regression. "Alpha" is the estimate of the regression intercept from the four-factor model. "Beta" is the loading on the market excess return. SMB and HML are the estimates of portfolio factor loadings on the Fama-French size and book-to-market factors. MOM is the portfolio factor loading on the Carhart momentum factor, R-squared is the adjusted R^2 from the regressions, and N is the number of monthly observations. We estimate the regression coefficients employing weighted least squares using the number of events firms in a given calendar month as weights. We set a minimum of ten firms per month for all portfolios. Finally, *, **, and *** indicate statistical significance of the coefficients at the 10%, 5%, and 1% significance levels, respectively.

Panel A: Equal-Weight Four-Factor Model

Window	Alpha	Beta	SMB	HML	MOM	N	R-squared
[+1,+36]	0.37**	1.03***	0.91***	0.20***	-0.22***	211	89.74%
	(2.16)	(27.17)	(17.08)	(3.85)	(7.18)		

Panel B: Value-Weight Four-Factor Model

Window	Alpha	Beta	SMB	HML	MOM	N	R-squared
[+1,+36]	0.31*	0.97***	0.55***	0.05	0.11***	211	84.03%
	(1.81)	(25.78)	(10.35)	(0.97)	(3.76)		

5. *Summary*. — Using each of the three standard methods for detecting abnormal returns—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 activists, 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, one might ask 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 that the excess return is too modest to justify the costs associated with the lack of diversification. So the activists choose to move some of their capital elsewhere.

In any event, analyzing fully the exit strategy of activists is beyond the scope of this Article. Our chief interest in this Part is to test empirically the validity of the pump-and-dump claim that negative long-term returns follow activists' departures. 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 such departure.

V. ACTIVIST INTERVENTIONS THAT ARE ESPECIALLY RESISTED

Our analysis in Parts III and IV focuses 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.

Part V.A focuses on the subsets of interventions that are followed by substantial reductions in capital investments, substantial increases in leverage, or substantial rises in payout distributions to shareholders—changes that directly or indirectly reduce the pool of resources available for the firm's long-term investments. Part V.B focuses on interventions that are openly adversarial and hostile. In both cases, we find no evidence for the asserted adverse effect on long-term performance.

A. *Investment-Limiting Interventions*

Opponents of hedge fund activism and holders of the myopic-activists view focus on and express concerns about activist interventions that might bring about a reduction in the company's long-term investments and the resources available to finance them.[95] Opponents are thus especially concerned about activism that leads to an 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.[96]

Opponents view such strategies as "sacrificing the future for a quick buck."[97] 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 call for effective action" against activism that can be expected to have an adverse effect on the long-term interests of Apple and its long-horizon shareholders.[98]

There is no good theoretical basis, however, for presuming that activist-initiated reductions in investments are value-reducing in the long term. Both financial economists and corporate-law scholars have long recognized management's tendency to avoid distributing excess cash or assets to

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95. See, e.g., Lipton & Rosenblum, Quinquennial Election, supra note 22, at 210 (arguing shareholders pressure companies to make cuts in "research and development expenses, capital expenditures, market development, and new business ventures, simply because they promise to pay off only in the long term").

96. See, e.g., Millstein, supra note 26 (arguing activist investors use their power "to sway and bully management to . . . meet the quarterly targets and disgorge cash in extra dividends or stock buy backs in lieu of investing in long-term growth").

97. Wachtell Memorandum, Important Questions, supra note 29.

98. Wachtell Memorandum, Bite the Apple, supra note 10.

shareholders.[99] Even if 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 or the freedom to pursue projects without the discipline generated by having to raise outside financing. Thus, opponents of hedge fund activism overlook that reducing cash holdings and investments might actually 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 result 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;[100] (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;[101] or (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

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99. See Sanford J. Grossman & Oliver D. Hart, Corporate Financial Structure and Managerial Incentives, *in* The Economics of Information and Uncertainty 107, 109 (John J. McCall ed., 1982), available at http://www.nber.org/chapters/c4434.pdf (on file with the *Columbia Law Review*) (explaining managers have interest in increasing resources under their firm's control); Oliver Hart & John Moore, Debt and Seniority: An Analysis of the Role of Hard Claims in Constraining Management, 85 Am. Econ. Rev. 567, 568–69 (1995) (analyzing management's preference for empire building and free cash flow); Michael C. Jensen, Agency Costs of Free Cash Flow, Corporate Finance, and Takeovers, 76 Am. Econ. Rev. 323, 323 (1986) (explaining debt might be necessary to motivate management because payouts to shareholders reduce resources under managers' control).

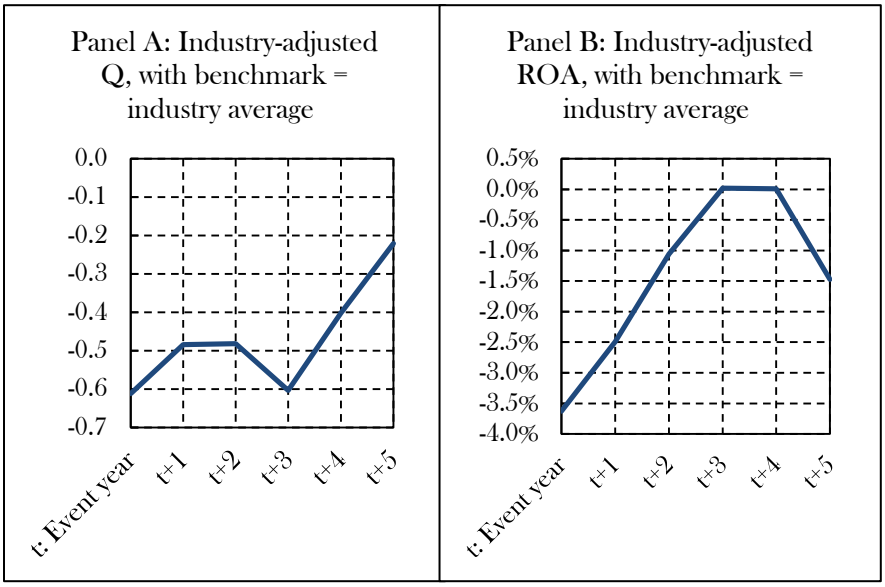
100. Change in leverage is calculated as $(Debt_r - Debt_b)/(Debt_b + Equity_b)$, all using book value. The subscript *(b)* stands for "base year" while the subscript *(r)* stands for the "report year" extending from the event year *(t)* through event year *(t+2)*. By this criterion, 6.3% of the events fall into the top 5%.

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

(hence decrease in investment in large magnitude).[102] 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.[103] To investigate whether the claimed adverse effect on long-term performance is present for these interventions, we begin by reporting the evolution of average industry-adjusted Q and average industry-adjusted ROA during the five years following the activist intervention for this restricted subsample. Similar to what Figure 1 presents for the whole sample of activist interventions, Figure 3 displays the evolution of industry-adjusted Q and industry-adjusted ROA for the set of investment-limiting interventions.

FIGURE 3: Q AND ROA OVER TIME FOR THE "INVESTMENT-LIMITING" SUBSAMPLE



102. 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. By this criterion, 5.9% of the events fall into the bottom 5%.

103. 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.

Coffee and Palia argue that the significant number of investment-limiting engagements that we identify raises concerns that activism is associated with reduced levels of long-term investments. Coffee & Palia, supra note 65, at 61–64. However, there is a large body of work in financial economics suggesting that managers have a tendency to invest excessively and decreases in investments might thus move targets toward, rather than away from, optimal investment levels. Bebchuk, Myth, supra note 9, at 1665–66.

As Figure 3 shows, within the set of investment-limiting interventions, average industry-adjusted Q and average industry-adjusted ROA trend upwards during the five years following the interventions. Indeed, the levels 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 Q and ROA over time that we conducted in Part III.C, but this time we focus exclusively on investment-limiting interventions. In particular, we include as event observations only investment-limiting interventions and not the universe of all activist interventions; we re-do the regressions and accompanying F-tests as reported in Table 4. Table 13 below displays our results.

As in Table 4, columns (1) and (2) report regressions in which the dependent variable is Q, and columns (3) and (4) report regressions in which the dependent variable is ROA. We control for "normal" levels by including 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.

As Table 13 indicates, we find no evidence that investment-limiting interventions are associated with adverse long-term declines in operating performance. Indeed, in the F-tests we conduct for the two Q regressions, each of the *(t*+3*)*, *(t*+4*)*, and *(t*+5*)* coefficients is higher than either the event year coefficient or the year *(t–1)* coefficient and the positive differences are statistically significant in four out of the twelve F-tests. Similarly, in the F-tests for the ROA regressions, each of the *(t*+3*)*, *(t*+4*)*, and *(t*+5*)* coefficients is higher than either the coefficient of year *(t)* or the coefficient of year *(t–1)*, and the differences are statistically significant at a significance level of 5% or stronger in six out of the twelve F-tests.

Finally, we examine whether improvements in operating performance during post-intervention years that targets of investment-limiting interventions enjoy tend to be smaller than those experienced by other targets of activist interventions. To explore this question, we run regressions (not tabulated) that follow the specifications of the baseline regressions in Table 4 with the addition of interaction terms for the dummy variables of *(t)*, *(t+1)*, *(t+2)*, *(t+3)*, *(t+4)*, and *(t+5)* with a dummy variable for "investment-limiting" interventions. The coefficients of these interaction terms indicate how the "investment limiting" interventions differ from other activist interventions. We then conduct joint F-tests to examine whether the post-intervention improvements in operating performances are different between the investment-limiting subsample and the complement subsample of other interventions. These results indicate, at the 10% significance level, that the two subsamples are not statistically different in terms of the magnitude of the post-intervention improvements in operating performance.

TABLE 13: THE EVOLUTION OF Q AND ROA OVER TIME FOR THE "INVESTMENT-LIMITING" SUBSAMPLE

This Table follows the same specifications as in Table 4, except that the "target firms" include only the targets in the "investment-limiting" subsample. An event is classified 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, with change in leverage calculated as $(Debt_r - Debt_b)/(Debt_b + Equity_b)$, all using book value and the subscript b standing for "base year" or the year-end prior to targeting while the subscript r stands for the "report year" extending from the event year *(t)* through event year *(t+2)*; (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, with change in payout yield calculated as $[(Dividend_r + Repurchase_r) - (Dividend_b + Repurchase_b)]/MV_b$; or (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), with change in investment calculated as $[(Capex_r + R\&D_r) - (Capex_b + R\&D_b)]/Assets_b$ and missing R&D values are imputed as zeros. As in Table 4, all standard errors adjust for heteroskedasticity as well as clustering at the firm level, and *, **, and *** indicate statistical significance of the coefficients at the 10%, 5%, and 1% significance levels, respectively.

Panel A: Regressions

	(1)	(2)	(3)	(4)
Dependent Variable	Q	Q	ROA	ROA
t: Event Year	-0.4711***	-0.2941*	-0.0259**	-0.0036
	(-4.00)	(-1.87)	(-2.24)	(-0.29)
t+1	-0.3196**	-0.0239	-0.0109	0.0094
	(-2.35)	(-0.13)	(-0.99)	(0.82)
t+2	-0.3792***	-0.1150	0.0046	0.0219*
	(-3.00)	(-0.69)	(0.37)	(1.70)
t+3	-0.3570***	-0.0752	0.0173	0.0263**
	(-2.96)	(-0.46)	(1.32)	(2.00)
t+4	-0.2136*	0.0522	0.0088	0.0156
	(-1.78)	(0.31)	(0.64)	(1.15)
t+5	-0.0168	0.2601	-0.0074	0.0062
	(-0.09)	(1.25)	(-0.50)	(0.47)
ln(MV)	0.2488***	0.8513***	0.0345***	0.0453***
	(32.05)	(51.42)	(51.17)	(41.86)
ln(Age)	-0.3204***	-0.4527***	0.0194***	0.0074***
	(-20.86)	(-16.86)	(16.21)	(3.84)
Year FE	Y	Y	Y	Y
SIC3 FE	Y		Y	
Firm FE		Y		Y
Pre-Event Dummies *(t–1, t–2, t–3)*	Y	Y	Y	Y
Observations	133,562	133,562	130,077	130,077
R-Squared	0.19	0.63	0.27	0.76

TABLE 13: THE EVOLUTION OF Q AND ROA OVER TIME FOR THE "INVESTMENT-LIMITING" SUBSAMPLE (CONT.)

Panel B: F-Tests

F-Tests:	(1) Q	(2) Q	(3) ROA	(4) ROA
Relative to *t*				
(t+3) vs. (t)	0.11	0.22	0.043***	0.030**
F-stat	0.57	2.04	9.35	4.41
p-val	45.20%	15.30%	0.20%	3.60%
(t+4) vs. (t)	0.26	0.35**	0.035**	0.019
F-stat	2.69	4.62	4.83	1.46
p-val	10.10%	3.20%	2.80%	22.80%
(t+5) vs. (t)	0.45**	0.55***	0.018	0.010
F-stat	4.74	7.21	1.22	0.38
p-val	2.90%	0.70%	27.00%	53.70%
Relative to *(t–1)*				
(t+3) vs. (t–1)	0.00	0.12	0.047***	0.036**
F-stat	0.00	0.42	9.56	5.70
p-val	98.20%	51.60%	0.20%	1.70%
(t+4) vs. (t–1)	0.14	0.25	0.038**	0.025
F-stat	0.65	1.72	5.99	2.61
p-val	42.00%	19.00%	1.40%	10.60%
(t+5) vs. (t–1)	0.34	0.46**	0.022	0.016
F-stat	2.31	4.00	1.90	1.10
p-val	12.90%	4.60%	16.80%	29.50%

We conclude that the data do not support the concerns expressed regarding investment-limiting interventions.[104] We find no evidence that such interventions produce long-term declines in operating performance and thereby involve "sacrificing the future for a quick buck."[105]

B. *Adversarial Interventions*

We now turn to another subset of activist interventions that deserve special attention—interventions that employ adversarial tactics. Hedge fund activists can be expected to use such tactics when they view companies as likely to be resistant to the direction suggested by them and therefore deem adversarial tactics as necessary to move the company in this direction. Such interventions, however, could be viewed by opponents as especially costly and disruptive.

104. See supra notes 96–98 and accompanying text (discussing works critical of hedge fund activism).

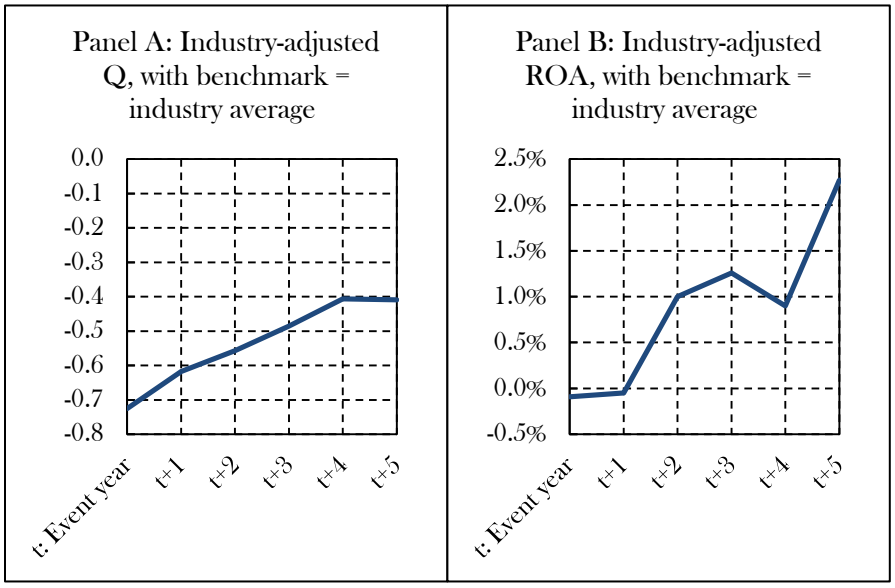
105. Wachtell Memorandum, Important Questions, supra note 29.

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.[106] 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 nonadversarial initiative. Our set of adversarial interventions accounts for 21.6% of the universe of all interventions in our regression analysis.

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

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 Q and average industry-adjusted ROA trend upwards during the five years following the interventions. Furthermore, the level of average industry-adjusted Q and average industry-adjusted ROA are higher in each of the five years following the intervention than in the intervention year.

FIGURE 4: Q AND ROA OVER TIME—"ADVERSARIAL" INTERVENTIONS



106. For an earlier article co-authored by two of us that uses this definition of adversarial intervention, see Brav et al., Hedge Fund Activism, supra note 17, at 1737–39 (formulating methodology for classifying hedge funds as activist).

Next, we repeat again the regression analysis of the evolution of Q and ROA over time we conducted in the preceding Part V.A, but this time we focus on adversarial interventions. Table 14 below reports our results. As in Table 13, columns (1) and (2) report regressions in which the dependent variable is Q; columns (3) and (4) report regressions in which the dependent variable is ROA; regressions (1) and (3) include industry-fixed effects; regressions (2) and (4) include firm-fixed effects; and, as in Table 4, controls include the company's market value and age, year-fixed effects to account for time trends, and dummy variables for each of the three years preceding the intervention year.

As Table 14 indicates, we find no evidence that adversarial interventions are followed by negative long-term effects on operating performance. 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, in the F-tests we conduct for the two Q regressions, each of the *(t+3)*, *(t+4)*, and *(t+5)* coefficients is higher than either the coefficient for year *t* or the coefficient for year *(t–1)*, and the positive differences, which increase from year three to five, are statistically significant in all twelve F-tests at the 1% significance level. As to the F-tests for the ROA regressions, the corresponding differences are positive in eleven out of twelve F-tests (and zero in the remaining F-test) and the positive differences are statistically significant in five out of these eleven F-tests.

Finally, we examine whether improvements in operating performance that targets of adversarial interventions enjoy during the years following the intervention tend to be smaller than those experienced by other targets of activist interventions. To this end, we run regressions (not tabulated) that follow the specifications of Table 4 adding interaction terms for the dummy variables of *(t)*, *(t+1)*, *(t+2)*, *(t+3)*, *(t+4)*, and *(t+5)* with a dummy variable for "adversarial" interventions. We then conduct joint F-tests to examine whether the post-intervention improvements in operating performances are different between the adversarial subsample and the complement subsample of other targets. We find that the two subsamples are not statistically different in terms of the magnitude of the post-intervention improvements in operating performance at the 10% significance level.

We conclude that the alleged adverse effect on long-term performance is not found when one focuses on adversarial interventions, either. The evidence does not support concerns that adversarial interventions are followed by long-term declines in performance.

TABLE 14: THE EVOLUTION OF ROA AND Q OVER TIME—ADVERSARIAL INTERVENTIONS

This Table follows the same specification as Table 4 except that the "target firms" include only targets that belong to the "adversarial" subsample of all targets. An event is classified as an "adversarial intervention" if the activist adopts tactics that are openly confrontational, including threats or actual proxy contests, lawsuits, and hostile takeovers, as well as shareholder proposals and public campaigns that involve confrontation (e.g., campaigns to oust CEOs). As in Table 4, all standard errors adjust for heteroskedasticity as well as clustering at the firm level, and *, **, and *** indicate statistical significance of the coefficients at the 90%, 95%, and 99% level, respectively.

Panel A: Regressions

	(1)	(2)	(3)	(4)
Dependent Variable	Q	Q	ROA	ROA
t: Event Year	-0.5516***	-0.1376	0.0106	-0.0073
	(-6.94)	(-1.44)	(1.61)	(-0.89)
t+1	-0.3424***	0.1463	0.0145*	-0.0002
	(-3.79)	(1.29)	(1.81)	(-0.03)
t+2	-0.2960***	0.2104*	0.0241***	0.0126
	(-3.09)	(1.78)	(2.80)	(1.20)
t+3	-0.2420**	0.2213*	0.0249***	0.0160
	(-2.43)	(1.77)	(2.72)	(1.34)
t+4	-0.1451	0.2854**	0.0186*	0.0113
	(-1.37)	(2.42)	(1.71)	(0.87)
t+5	-0.0853	0.3454**	0.0331***	0.0285**
	(-0.58)	(2.47)	(2.82)	(2.16)
ln(MV)	0.2485***	0.8524***	0.0346***	0.0453***
	(31.98)	(51.43)	(51.22)	(41.74)
ln(Age)	-0.3189***	-0.4522***	0.0193***	0.0076***
	(-20.76)	(-16.87)	(16.09)	(3.91)
Year FE	Y	Y	Y	Y
SIC3 FE	Y		Y	
Firm FE		Y		Y
Pre-Event Dummies (*t–1, t–2, t–3*)	Y	Y	Y	Y
Observations	133,562	133,562	130,077	130,077
R-Squared	0.19	0.63	0.27	0.76

TABLE 14: THE EVOLUTION OF ROA AND Q OVER TIME—ADVERSARIAL INTERVENTIONS (CONT.)

Panel B: F-Tests

F-Tests:	(1) Q	(2) Q	(3) ROA	(4) ROA
Relative to *t*				
(t+3) vs. t	0.31***	0.36***	0.014	0.023**
F-stat	8.68	9.92	2.58	5.69
p-val	0.30%	0.20%	10.90%	1.70%
(t+4) vs. t	0.41***	0.42***	0.008	0.019*
F-stat	12.66	12.85	0.59	2.80
p-val	0.00%	0.00%	44.20%	9.50%
(t+5) vs. t	0.47***	0.48***	0.022*	0.036***
F-stat	9.62	11.23	3.73	8.93
p-val	0.20%	0.10%	5.30%	0.30%
Relative to *(t –1)*				
(t+3) vs. (t–1)	0.33***	0.44***	0.005	0.014
F-stat	10.81	14.03	0.25	1.88
p-val	0.10%	0.00%	61.70%	17.10%
(t+4) vs. (t–1)	0.43***	0.50***	-0.002	0.009
F-stat	16.63	19.39	0.02	0.68
p-val	0.00%	0.00%	88.40%	40.80%
(t+5) vs. (t–1)	0.49***	0.56***	0.013	0.027**
F-stat	12.60	17.51	1.23	4.95
p-val	0.00%	0.00%	26.70%	2.60%

VI. INCREASED VULNERABILITY TO ECONOMIC SHOCKS?

It might be suggested that, even if activist interventions benefit investors on an expected-value basis, activist interventions might be troubling to the extent that they increase risks by making companies more vulnerable (say, by increasing leverage or decreasing cash or other liquid reserves) in the event of an adverse economic shock.[107] We are not persuaded that this line of reasoning could justify 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, the analysis in this Part examines empirically whether activist interventions during the years preceding the financial crisis made targeted firms

107. This argument was raised, for example, by corporate lawyers participating in a Harvard Law School event in which our findings were discussed.

more vulnerable to the downturn when the crisis came. We find no evidence that this is the case.

The financial crisis provides a good setting for testing impact on the vulnerability of companies both because the negative shock was of considerable magnitude and the shock was exogenous to any individual firm (i.e., not caused by the actions of any given firm). We divide our analysis of the crisis period into two parts. Part VI.A examines whether targeted firms suffered more severe declines in operating performance during the financial crisis than firms not targeted by activism. Part VI.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 section 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 ΔROA and ΔQ are the differences between after and before the crisis.

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

The results of Table 15 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 in all four regressions, and it is significant at the 5% significance level in one of the Q regressions and one of the ROA regressions. 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 that those firms were thus hurt more by the crisis.

TABLE 15: CHANGES IN OPERATING PERFORMANCE DURING THE FINANCIAL CRISIS

This Table reports coefficients (and t-statistics in parentheses) of linear regressions where the dependent variables are differences in firm performance (Q in columns (1) and (2) and ROA in columns (3) and (4)) between the average over the 2008–2009 period and that over the 2006–2007 period. 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. Control variables "ln(MV)" and "ln(Age)" are defined in the same way as in Table 4. Columns (1) and (3) do not use industry-fixed effects, and Column (2) and (4) use such fixed effects. All standard errors adjust for heteroskedasticity as well as clustering at the firm level. Finally, *, **, and *** indicate statistical significance of the coefficients at the 90%, 95%, and 99% level, respectively.

	(1)	(2)	(3)	(4)
Dependent Variable	ΔQ	ΔQ	ΔROA	ΔROA
Targeted During 2004–2007	0.112	0.188**	0.013**	0.010
	(1.43)	(2.20)	(2.14)	(1.59)
ln(Market Cap 2006–2007)	-0.072***	-0.080***	0.000	0.002
	(5.79)	(5.61)	(0.16)	(1.55)
ln(Firm Age 2006–2007)	0.154***	0.154***	-0.001	0.000
	(6.28)	(5.64)	(0.56)	(0.18)
Industry FE	N	Y	N	Y
Observations	4,626	4,626	4,473	4,473
R-Squared	0.02	0.10	0.00	0.10

B. *Financial Distress and Delisting During the Crisis*

The analysis of Part VI.A does not incorporate 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 nontargeted 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.[108]

108. 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 the impact of activism on an expected-value risk-adjusted basis.

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, or liquidation is 2.48%. As before, we use standard controls for market capitalization and firm age as controls, and we use both specifications that include and do not include industry-fixed effects. In the regression with fixed effects, we use the conditional logit model that accommodates fixed effects.

Table 16 below reports the results of our logit regressions. Regression (1) includes industry-fixed effects, while in regression (2) we do not use such fixed effects.

The results in Table 16 do not provide support for the concern that pre-crisis activism produced a significant increase in the odds of delisting due to bankruptcy or insolvency during the crisis. The coefficient of "Targeted During 2004–2007" is positive but far from being statistically significant in both regressions. 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.

TABLE 16: INCIDENCE OF DELISTING DURING THE FINANCIAL CRISIS

This Table reports coefficients (and t-statistics in parentheses) of logit regressions where the dependent variable is "Insolvency delisting," a dummy variable equal to 1 if a firm was delisted from Compustat due to distress-related reasons during the 2008–2009 period. Control variables are "ln(Market Cap 2006–2007)," defined as the logarithm of a firm's market capitalization averaged over the 2006–2007 period; and "ln(Firm Age 2006–2007)" defined as the logarithm of the number of years since a firm's first appearance in the merged CRSP/Compustat database. Column (1) adopts the conditional logit regression with industry-fixed effects, and column (2) adopts the logit model without industry-fixed effects. All standard errors adjust for clustering at the firm level. Finally, *, **, and *** indicate statistical significance of the coefficients at the 10%, 5%, and 1% significance level, respectively.

	(1)	(2)
Dependent Variable	Insolvency Delisting	Insolvency Delisting
Targeted During 2004–2007	0.288	0.229
	(0.54)	(0.55)
(Marginal Probability)	0.95%	0.21%
ln(Market Cap 2006–2007)	-0.473	-0.357***
	(1.37)	(4.24)
ln(Firm Age 2006–2007)	-0.247	-0.257**
	(1.14)	(2.35)
Industry FE	Y	N
Observations	2,208	4,627
Pseudo R-Squared	0.09	0.05

VII. POLICY IMPLICATIONS

Our findings have implications for a significant number of ongoing policy debates. As stressed in the Introduction, the myopic-activists claim has been a key argument for opposing reforms that would strengthen the rights, power, and involvement of activist shareholders and for supporting changes that would limit such rights, power, and involvement. Below we illustrate the substantial policy stakes by discussing a number of significant corporate-law debates.

A. *Balance of Power Between Shareholders and Boards*

We begin with the implications of the myopic-activists claim for the desirable balance of power between shareholders and boards in public companies. This claim has been used to argue against any shifts in this balance in favor of shareholders. For example, when Senator Charles Schumer suggested federal legislation that would have substantially expanded shareholder rights in a number of ways (the Shareholder Bill of Rights Act of 2009),[109] opponents invoked the myopic-activists claim. They argued that the increase in shareholder rights "would fuel the very stockholder-generated short-termist pressure that, in the view of many observers, contributed significantly to the financial and economic crises we face today."[110]

Such arguments have succeeded in influencing how prominent Delaware judges view the optimal balance of power between shareholders and directors. In an essay on the virtues of "patient capital," then-Justice Jacobs expresses his concern about "legal developments that empower shareholders to force corporate boards and managements to be more responsive to their immediate agendas,"[111] and he opines that the combination of increased shareholder power with shareholder willingness to use it has created a serious national problem.[112] Similarly, in an essay on the fundamentals of corporate governance, then-Vice Chancellor Strine expressed concern about the increasing empowerment of shareholders.[113] He states that "undifferentiated

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109. E.g., Press Release, Senator Chuck Schumer, Schumer, Cantwell Announce 'Shareholder Bill of Rights' to Impose Greater Accountability on Corporate America (May 19, 2009), available at http://votesmart.org/public-statement/427143/schumer-cantwell-announce-shareholder-bill-of-rights-to-impose-greater-accountability-on-corporate-america (on file with the *Columbia Law Review*) (describing proposed legislation's objectives and major components). Senator Schumer's proposed bill intended to implement, by federal mandate, a series of measures to strengthen shareholder rights, including facilitating stockholders' proxy access, ending staggered boards at all companies, and requiring that all directors receive a majority of votes cast to be elected. Shareholder Bill of Rights Act of 2009, S. 1074, 111th Cong. (2009) (proposing changes to Securities Exchange Act of 1934).

110. Lipton, Lorsch & Mirvis, supra note 30.

111. Jacobs, supra note 5, at 1652.

112. Id. at 1657.

113. Leo E. Strine, Jr., Toward Common Sense and Common Ground? Reflections on the Shared Interests of Managers and Labor in a More Rational System of Corporate Governance, 33 J. Corp. L. 1, 7 (2007) (expressing concern stockholder empowerment "does not empower end-user investors so much as it empowers intermediaries").

empowerment of these so-called stockholders may disproportionately strengthen the hand of activist institutions that have short-term or non-financial objectives that are at odds with the interests of individual index fund investors."[114]

Thus, our findings address key concerns that have been used to justify an allocation of power that favors directors over shareholders. By addressing these concerns, our findings weaken the case for such an allocation.

B. *Staggered Boards*

The extent to which directors are accountable to and influenced by shareholders depends on whether the board is staggered. When the board is staggered, directors are elected to three-year terms, and two-thirds of the directors do not come up for reelection in any year. By contrast, in companies with boards that are not staggered, all directors come up for reelection each year. Thus, having a staggered board provides a significant impediment to hedge fund activism and, conversely, having annual elections for all directors facilitates the influence of such investors.[115]

Annual elections for all directors are widely viewed as a best governance practice by shareholders and their advisers, and shareholder proposals calling for board declassification have been receiving massive support from shareholders.[116] Supporters of staggered boards, however, have been using the myopic-activists claim to defend staggered boards. In 2012, Wachtell Lipton issued strongly worded memoranda criticizing the submission of board-declassification proposals and the resulting large-scale dismantling of staggered boards.[117] It argued that the dismantling of staggered boards "would exacerbate

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114. Id.

115. For an analysis of how staggered boards insulate directors from removal, see Lucian Arye Bebchuk, John C. Coates IV & Guhan Subramanian, The Powerful Antitakeover Force of Staggered Boards: Theory, Evidence, and Policy, 54 Stan. L. Rev. 887, 900–39 (2002).

116. See Lucian Bebchuk, Scott Hirst & June Rhee, Towards the Declassification of S&P 500 Boards, 3 Harv. Bus. L. Rev. 157, 162–64, 167–73 (2013) (discussing data on shareholder support of declassification proposals from 2012 Shareholder Rights Project); see also Shareholder Rights Project Report for the 2012 and 2013 Proxy Seasons 5–6, 16–17, available at http://srp.law.harvard.edu/releases/SRP-2012-2013-Annual-Report-Final.pdf (on file with the *Columbia Law Review*) (providing additional information on shareholder support of declassification proposals)).

117. Martin Lipton & Daniel A. Neff, Wachtell, Lipton, Rosen & Katz, Harvard's Shareholder Rights Project Is Still Wrong, Harvard Law Sch. Forum on Corporate Governance & Fin. Regulation (Nov. 30, 2012, 8:55 AM), http://blogs.law.harvard.edu/corpgov/2012/11/30/harvards-shareholder-rights-project-is-still-wrong (on file with the *Columbia Law Review*) (characterizing work contributing to eliminating staggered boards as an "exercise in corporate deconstruction . . . detrimental to the economy and society at large"); Martin Lipton, Theodore N. Mirvis, Wachtell, Lipton, Rosen & Katz, Harvard's Shareholder Rights Project Is Wrong, Harvard Law Sch. Forum on Corporate Governance & Fin. Regulation (March 23, 2012, 10:38 AM), http://blogs.law.harvard.edu/corpgov/2012/03/23/harvards-shareholder-rights-project-is-wrong [hereinafter Lipton & Mirvis, Harvard's Shareholder Rights Project Is Wrong] (on file with the *Columbia Law Review*) (criticizing Shareholder Rights Project work contributing to dismantling staggered boards as "unwise," "unwarranted," and "inappropriate").

the short-term pressures under which American companies are forced to operate."[118]

Thus, our findings address key concerns that have been raised to defend board classification and to insulate directors from shareholders. By addressing these concerns, our findings weaken the case for using staggered boards and weigh in favor of having annual elections for all directors.

C. *Reforms of Corporate Elections*

The myopic-activists claim also has implications for the rules governing elections. These rules determine the extent to which it is difficult for shareholders to replace directors, and they influence the extent to which boards are attentive to the preferences of shareholders in general and activist investors in particular. Opponents of reforms that strengthen shareholder power to replace directors have argued that such a reform "perversely incentivizes directors to generate immediate returns at the cost of future growth, at the expense of the corporation and its shareholders (and the economy as a whole)."[119]

To illustrate the use of the myopic-activists claim in opposing any invigoration of the shareholder franchise, consider the debate over providing shareholders with "proxy access"—that is, the power to place some director candidates on the corporate ballot.[120] Not surprisingly, companies, corporate advisers, and management groups have invoked the myopic-activists claim to oppose proxy access altogether or to argue for substantial restrictions of its use.[121]

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118. Lipton & Mirvis, Harvard's Shareholder Rights Project Is Wrong, supra note 117.

119. Martin Lipton & William Savitt, The Many Myths of Lucian Bebchuk, 93 Va. L. Rev. 733, 747 (2007).

120. For opposing views on the question of proxy access, compare Martin Lipton & Steven A. Rosenblum, Election Contests in the Company's Proxy: An Idea Whose Time Has Not Come, 59 Bus. Law. 67, 78–79 (2003) [hereinafter Lipton & Rosenblum, Election Contests], with Lucian Arye Bebchuk, The Case for Shareholder Access to the Ballot, 59 Bus. Law. 43, 43–46 (2003).

121. See, e.g., Letter from Alexander M. Cutler, Chair, Corporate Leadership Initiative, Bus. Roundtable, to Elizabeth Murphy, Sec'y, SEC 14–17 (Aug. 17, 2009), available at http://www.sec.gov/comments/s7-10-09/s71009-267.pdf (on file with the *Columbia Law Review*) (arguing proposed proxy access rules will promote unhealthy emphasis on short-termism and encourage election of "special interest" directors); Letter from David T. Hirschmann, President & Chief Exec. Officer, Ctr. for Capital Mkts. Competitiveness, U.S. Chamber of Commerce, to Elizabeth Murphy, Sec'y, SEC 3–4 (Jan. 19, 2010), available at http://www.sec.gov/comments/s7-10-09/s71009-618.pdf (on file with the *Columbia Law Review*) (same); Letter from Samuel J. Palmisano, Chairman, President & Chief Exec. Officer, IBM Corp., to Mary L. Schapiro, Chairman, SEC 1–2 (Aug. 20, 2010), available at http://www.sec.gov/comments/s7-10-09/s71009-692.pdf (on file with the *Columbia Law Review*) (urging SEC to reject proxy access rule because it may be used by certain shareholders to leverage single concern such as payment of dividends); Letter from Gloria Santona, Exec. Vice President, Gen. Counsel & Sec'y, McDonald's Corp., to Elizabeth M. Murphy, Sec'y, SEC 4 (Aug. 24, 2009), available at http://www.sec.gov/comments/s7-10-09/s71009-504.pdf (on file with the *Columbia Law Review*) (expressing concern proxy access rule will be used by short-term holders for their own gain); see also Lipton & Rosenblum, Election Contests, supra note 120, at 78–79 (invoking short-termism

Thus, our findings that the myopic-activists claim is not supported by the data should inform the ongoing debate on the shareholder franchise and the reform of corporate elections. These findings weigh in favor of reforms that provide shareholders with the power to place director candidates on the corporate ballot, and undermine a key objection to an effective shareholder franchise.

D. *Limiting Rights of Shareholders with Short Holding Periods*

A standard feature of corporate arrangements and rules is that they provide shareholders with the same rights per share regardless of when the shareholders came to own their shares; when *A* buys *B*'s shares, *A* usually steps into *B*'s shoes and obtains the same rights that *B* had. Opponents of activism, however, have been attracted to arrangements that weaken the powers and rights of shareholders who have held their shares for shorter periods. Because such arrangements could be designed to decrease the voting power of activists and to provide a disproportionately large voting power to insiders, they can further insulate directors from allegedly myopic activists.

The increase in investor activism in recent years has led to increased interest in and calls for such arrangements. In recent years, Justin Fox and Jay Lorsch suggested adopting "a sliding scale on which voting power increases with length of ownership" or "restrict[ing] voting in corporate elections of any kind to those who have owned their shares for at least [one] year";[122] then-Chancellor Strine expressed support for the principle that "[s]tockholders who [p]ropose [l]ong-[l]asting [c]orporate [g]overnance [c]hanges [s]hould [h]ave a [s]ubstantial, [l]ong-[t]erm [i]nterest that [g]ives [t]hem a [m]otive to [w]ant the [c]orporation to [p]rosper";[123] the SEC limited the use of the proxy-access rule it adopted (later invalidated on procedural grounds by the D.C. Circuit[124]) to shareholders with a substantial holding period of at least three years,[125] responding to many comments filed with the SEC in support of such a requirement;[126] and the Generation Foundation, an arm of Generation Investment Management, commissioned consulting company Mercer to study the possibility of encouraging shareholders to hold shares for long periods (and

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claims to oppose proxy access and arguing many activist investors have competing interests that may conflict with best interests of public corporations).

122. Fox & Lorsch, supra note 22, at 56–57.

123. Strine, Fundamental Question, supra note 5, at 7.

124. Bus. Roundtable v. SEC, 647 F.3d 1144, 1148 (D.C. Cir. 2011).

125. See supra note 6 and accompanying text (discussing SEC's attempted adoption of proxy access rule).

126. See, e.g., Director Nominations, supra note 6, at 56,697–98 (discussing commenters who supported increasing duration of minimum holding period to ensure use of rule was limited to long-term shareholders); see also supra note 121 and accompanying text (listing proponents of longer-term proxy access rule).

thereby have a long-term focus) through "loyalty rewards" of extra dividends, warrants, and additional voting rights.[127]

Our findings eliminate a key motivation for proposals to limit the rights of short-term shareholders. Concerns that hedge fund activism is detrimental to the long-term interest of companies and shareholders are not supported by the data.

E. *Disclosure of Stock Accumulations by Activist Investors*

The myopic-activists claim also has clear implications for the ongoing debate on the disclosure requirements governing predisclosure stock accumulations by activist hedge funds. Seeking to discourage such activism, opponents have been urging the SEC to tighten the existing disclosure requirements.[128] And the SEC has announced that it will reexamine these existing disclosure requirements.

In prior work by the three of us with Robert Jackson, we have pointed out that any such examination should take into account the reduction in activist engagements that can be expected to result from such tightening.[129] Whether such a reduction would be detrimental or beneficial depends, in turn, on the validity of the myopic-activists claim. Opponents of activism have claimed that negative long-term consequences justify tightening of disclosure requirements.[130]

Thus, our findings that hedge fund activism is associated with beneficial long-term consequences should weigh against a tightening of disclosure rules that would discourage and reduce the incidence of such activism. These findings should inform any SEC examination of the subject.

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127. Barry B. Burr, Mercer Seeks Long-Term Shareholder Rewards Program from Corporations, Pensions & Investments (Dec. 6, 2012, 12:53 PM), http://www.pionline.com/article/20121206/DAILYREG/121209930 (on file with the *Columbia Law Review*).

128. See Letter from Wachtell, Lipton, Rosen & Katz to Elizabeth M. Murphy, Secretary, SEC 3–7 (Mar. 7, 2011), http://www.sec.gov/rules/petitions/2011/petn4-624.pdf (on file with the *Columbia Law Review*) (urging SEC to shorten 13D reporting deadlines); see also Adam O. Emmerich, Theodore N. Mirvis, Eric S. Robinson & William Savitt, Fair Markets and Fair Disclosure: Some Thoughts on the Law and Economics of Blockholder Disclosure, and the Use and Abuse of Shareholder Power, 3 Harv. Bus. L. Rev. 135, 137–140 (2013) (same).

129. See Lucian A. Bebchuk & Robert J. Jackson, Jr., The Law and Economics of Blockholder Disclosure, 2 Harv. Bus. L. Rev. 39, 47–51 (2012) (explaining tightening rules can be expected to reduce incidence of activist engagements); see also Bebchuk et al., Pre-Disclosure Accumulations, supra note 42, at 17–19 (analyzing how tightening disclosure requirements can be expected to discourage hedge fund activism).

130. See, e.g., Martin Lipton, Steven A. Rosenblum, & Sabastian V. Niles, Wachtell, Lipton, Rosen & Katz, Current Thoughts About Activism, Revisited, Harvard Law Sch. Forum on Corporate Governance & Fin. Regulation (Apr. 8, 2013, 9:19 AM) http://blogs.law.harvard.edu/corpgov/2014/04/08/current-thoughts-about-activism-revisited/ (on file with the *Columbia Law Review*) (arguing short-termism concerns justify such tightening).

F. *Boards' Dealings with Activists*

Finally, the myopic-activists claim has implications for how boards should engage with activist investors. To the extent that the actions sought by hedge fund activists tend to be detrimental to long-term value, boards would be justified in viewing the emergence of an activist as an unwelcome development, engaging with the activist in an adversarial fashion, and approaching the activist's proposals with substantial skepticism. Conversely, to the extent that activist interventions are associated with long-term benefits to companies and their shareholders, such an adversarial and defensive attitude would not serve the interests of the company.

Invoking the myopic-activists claim, influential corporate advisors have indeed advised boards facing an activist intervention to view it as an activist "attack."[131] Boards have thus been encouraged to work to "forestall an attack" and be prepared to "defend vigorously."[132]

Our findings should inform how corporate directors view and engage with activists. Corporate boards facing an activist should not "circle the wagons" and should reject advisors' suggestions that the board keep in mind that it has "no duty to discuss" with the activist and "no duty to respond."[133] To the contrary, boards should keep in mind that activist interventions are on average associated with beneficial outcomes in the long term. Rather than taking an adversarial position, boards should be open to the activist's ideas and to discussing them with the activist. A board's constructively engaging with the activist, rather than defending vigorously against it, could well serve the long-term interests of the company and its shareholders.

CONCLUSION

This Article 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. Although this testable claim has been influential and regularly invoked, its supporters have thus far failed to back it up with empirical evidence. This Article provides a comprehensive investigation of this claim and finds that it is not supported by the data.

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131. For example, a recent blueprint for dealing with activists issued by Wachtell Lipton uses the term "attack" and variations on it about twenty times. See Martin Lipton & Sabastian V. Niles, Wachtell, Lipton, Rosen & Katz, Dealing with Activist Hedge Funds, Harvard Law Sch. Forum on Corporate Governance & Fin. Regulation (Nov. 21, 2013, 12:24 PM) http://blogs.law.harvard.edu/corpgov/2013/11/21/dealing-with-activist-hedge-funds-2/ [hereinafter Wachtell Memorandum, Dealing with Activist Hedge Funds] (on file with the *Columbia Law Review*). For another memorandum by a prominent law firm referring to activist engagement as "attacks," see Simpson Thacher & Bartlett LLP, Shareholder Activism in M&A Transactions, Harvard Law Sch. Forum on Corporate Governance & Fin. Regulation (Feb. 26, 2014, 09:06 AM) http://blogs.law.harvard.edu/corpgov/2014/02/26/shareholder-activism-in-ma-transactions/ (on file with the *Columbia Law Review*).

132. Wachtell Memorandum, Dealing with Activist Hedge Funds, supra note 131.

133. Id.

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, including the investment-limiting and adversarial interventions that are especially resisted by opponents, are followed in the long term by declines in operating performance. Indeed, we find evidence that such interventions are followed by long-term improvements, rather than declines, in performance.

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 are 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. Among other things, these findings undermine a key claim that has been invoked in support of (i) weakening shareholders' powers vis-à-vis directors, (ii) using board classification to insulate directors from shareholders and impeding shareholders' ability to replace directors, (iii) opposing any invigoration of the shareholder franchise, (iv) limiting the rights of shareholders with short holding periods, (v) tightening the disclosure rules governing stock accumulations by hedge fund activists, and (vi) boards following a "circle the wagons" approach to adversarial interventions. Our findings thus have implications for each of these ongoing debates.

Going forward, policymakers and institutional investors should not accept the validity of the frequent assertions that activist interventions are costly to firms and their shareholders in the long term. Both public officials and investors should reject any use of such claims as a basis for limiting the powers, rights, and involvement of shareholders.

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