european corporate governance institute

Long-Term Economic Consequences of Hedge Fund Activist Interventions

Finance Working Paper N° 577/2018
December 2018

Ed deHaan
Foster School of Business, University of Washington
David F. Larcker
Graduate School of Business, Stanford University, Rock Center for Corporate Governance and ECGI
Charles McClure
Booth School of Business, University of Chicago

© Ed deHaan, David F. Larcker and Charles McClure 2018. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

This paper can be downloaded without charge from:
https://doi.org/10.67929/ecgi-fin-577-2018

european corporate governance institute

ECGI Working Paper Series in Finance

Long-Term Economic Consequences of Hedge Fund Activist Interventions

Working Paper N° 577/2018
December 2018

Ed deHaan
David F. Larcker
Charles McClure

We thank Ian Gow, Jon Karpoff, and Eric So for helpful advice and Alon Brav for kindly sharing data on hedge fund activism.

© Ed deHaan, David F. Larcker and Charles McClure 2018. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

Abstract

Abstract: We examine the long-term effects of interventions by activist hedge funds. Research documents positive equal-weighted long-term returns and operating performance improvements following activist interventions, and typically conclude that activism is beneficial. We extend the literature in two ways. First, we find that equal-weighted long-term returns are driven by the smallest 20% of firms, with an average market value of $22 million. The larger 80% of firms experience insignificant negative long-term returns. On a value-weighted basis, which likely best gauges the effects on shareholder wealth and the economy, we find that pre- to post-activism long-term returns insignificantly differ from zero. For operating performance, we find that prior results are a manifestation of abnormal trends in pre-activism performance. Using an appropriately matched sample, we find no evidence of abnormal post-activism performance improvements. Overall, our results do not strongly support the hypothesis that activist interventions drive long-term benefits for the typical shareholder.

JEL Classification: G34; G38; G14; M41; M48

Ed deHaan

Foster School of Business, University of Washington

David F. Larcker

Graduate School of Business, Stanford University, Rock Center for Corporate Governance

Charles McClure

Booth School of Business, University of Chicago

ROCK CENTER FOR CORPORATE GOVERNANCE
STANFORD UNIVERSITY

ROCK CENTER *for* CORPORATE GOVERNANCE

WORKING PAPER SERIES NO. 236

Long-Term Economic Consequences of Hedge Fund Activist Interventions

Ed deHaan
Foster School of Business
University of Washington

David Larcker
Graduate School of Business, Stanford University
Rock Center for Corporate Governance

Charles McClure
Booth School of Business
University of Chicago

December 31, 2018

Working papers are in draft form. This working paper is distributed for purposes of comment and discussion only.
It may not be reproduced without permission of the copyright holder(s).
Copyright © 2018 by the Board of Trustees of the Leland Stanford Junior University. All rights reserved.
Stanford Graduate School of Business • Knight Management Center • 655 Knight Way • Stanford, CA 94305
Email: gsb_corpgovernance@stanford.edu

Electronic copy available at: https://ssrn.com/abstract=3260095

Long-term economic consequences of hedge fund activist interventions

Ed deHaan

Foster School of Business
University of Washington

David Larcker

Graduate School of Business, Stanford University
Rock Center for Corporate Governance

Charles McClure

Booth School of Business
University of Chicago

December 31, 2018

Abstract: We examine the long-term effects of interventions by activist hedge funds. Research documents positive equal-weighted long-term returns and operating performance improvements following activist interventions, and typically conclude that activism is beneficial. We extend the literature in two ways. First, we find that equal-weighted long-term returns are driven by the smallest 20% of firms, with an average market value of $22 million. The larger 80% of firms experience insignificant negative long-term returns. On a value-weighted basis, which likely best gauges the effects on shareholder wealth and the economy, we find that pre- to post-activism long-term returns insignificantly differ from zero. For operating performance, we find that prior results are a manifestation of abnormal trends in pre-activism performance. Using an appropriately matched sample, we find no evidence of abnormal post-activism performance improvements. Overall, our results do not strongly support the hypothesis that activist interventions drive long-term benefits for the typical shareholder, nor do we find evidence of shareholder harm.

JEL Classification: G34; G38; G14; M41; M48

We thank Ian Gow, Jon Karpoff, and Eric So for helpful advice and Alon Brav for kindly sharing data on hedge fund activism. We gratefully acknowledge the support of the Stanford Rock Center for Corporate Governance, the Centers & Initiatives for Research, Curriculum and Learning Experiences (CIRCLE), the University of Washington's Foster School of Business, the FMC Faculty Research Fund, and the University of Chicago's Booth School of Business.

Electronic copy available at: https://ssrn.com/abstract=3260095

1. Introduction

The economic consequences of activist hedge fund interventions are widely debated. Proponents assert that companies with engaged shareholders are more likely to succeed because these shareholders mitigate natural agency problems. They also claim that shareholder activists are an important component of the disciplining role played by the market for corporate control. By contrast, opponents allege that hedge fund activism is either an uninformed distraction or a mechansim for some investors to "take the money and run." In its extreme form, activism is claimed to weaken companies by contributing to managerial myopia.

The debate is illustrated by the dialogue between Lucian Bebchuk, a Harvard University law professor, and Martin Lipton, an attorney at the law firm Wachtell, Lipton, Rosen & Katz. Lipton asserts that interference by hedge fund activists has "very serious adverse effects on the companies, their long-term shareholders, and the American economy. To avoid becoming a target, companies seek to maximize current earnings at the expense of sound balance sheets, capital investment, research and development, and job growth" (Lipton 2013). By contrast, Bebchuk (2013) cites academic findings that hedge fund activism leads to improved operating performance and returns, and argues that concerns about myopic activists "should be rejected as a basis for limiting the rights and powers of public-company shareholders" (Bebchuk 2013).

Debate over hedge fund activism is not limited to academics, but is also common among regulators and managers. As an example of its broad interest, in 2017, the *The Wall Street Journal* published an average of more than one article per day mentioning activism. The Brokaw Act, introduced in the U.S. Senate in 2016, attempts to limit activists' ability to gain stakes in target firms, and U.S. House and Senate members have proposed changing the tax code to discourage "cut-and-run activists" (Sorkin 2015; Orol 2017). SEC Commissioners have likewise

Electronic copy available at: https://ssrn.com/abstract=3260095

raised concerns about hedge fund activism (Gallagher 2015) while also expressing reservations about curbing the practice (Gandel 2017). In response to rising criticism, in 2016, a coalition of hedge funds created a lobbying group to promote the benefits of activism (Reuters 2016). In recent years, the firms targeted by hedge fund activists' interventions have increased in both number (Black 2017) and size (Moyer 2017), and likely many more firms have been subject to activism threats. Therefore, understanding the long-term economic consequencs of activism and providing evidence to help inform the debates in academic and professional literatures is important.

Our study contributes to these debates by identifying and investigating two shortcomings in the academic literature. First, researchers have gauged long-term effects based on *equal-weighted* mean abnormal stock returns. Specifically, they have found equal-weighted returns ranging from 3.4% to 7% in the days around the 13-D filing, reaching up to 11% over one or two years (Denes et al. 2016). These results support the inference that activist interventions improve long-term value for the average firm. However, these results do not necessarily indicate activist interventions enhance the wealth of the average *investor*. Because the largest 20% of U.S. public firms comprise 91% of the total market value, an activist intervention in a large firm likely has a far bigger impact on investors than an intervention in a small one.[1] Thus, for regulators evaluating the impact of activist interventions on shareholder wealth and the market at large, meaningful analysis should examine the distribution of returns across firms and, in particular, *value-weighted* average long-term stock returns (Fama 1998; Brav and Gompers 1997; Brav et al. 2000; Mitchell and Stafford 2000).[2] A specific concern is that significantly positive equal-

[1] Market value data are calculated annually for the CRSP universe, averaged over our sample period.
[2] We do not assert that equal-weighted returns are irrelevant but rather that the choice between equal- and value-weighted returns depends on the researcher's objective. For example, in a study of returns to equity issuances, Brav et al. (2000) note: "If we are interested in the managerial implications [of an event], equal weighting returns might

Electronic copy available at: https://ssrn.com/abstract=3260095

weighted returns could be driven by small firms and obscure negative or insignificant returns for larger firms, the latter of which "more accurately capture the total wealth effects experienced by investors" (Fama 1998).[3]

A second shortcoming of the activism literature is that tests of post-activism changes in operating performance typically do not adequately control for the stochastic evolution of accounting metrics (e.g., Penman 1991), which can create differences between the target firms and matched control samples. Specifically, papers that find post-activism improvements in accounting-based operating performance either do not use a benchmark control group or identify a control group without accounting for pre-activism performance trends. Failing to match on pre-activism performance trends is problematic because many targets experience atypical performance patterns prior to activist interventions, which raises concerns about the inferences in pre- and post-activism tests. Correctly understanding post-activism changes in operating performance is essential in the debate over how hedge fund activists impact the economy.

The purpose of this paper is to extend the literature on the long-term effects of hedge fund activism on firm value and operating performance while also considering the aforementioned shortcomings. We implement our tests using a sample of 1,964 activist interventions from 1994 through 2011.

Our first analyses examine stock returns. We measure short-term abnormal returns in the 21-day window surrounding the activist intervention. We measure the long-term impact of

be more appropriate. If the researcher's goal, however, is to quantify investors' average wealth change subsequent to an event, then it follows that value weighting is the correct method" (p. 212).

[3] A similar sentiment is expressed by Delaware Supreme Court Chief Justice Leo Strine in a monograph on the pros and cons of hedge fund activism: "Unless we consider the economic realities of ordinary human investors … we are not focused on what is most important in assesing the public policies shaping our corporate governance system" (Strine 2017, p. 1871). Strine (2017) also notes that activism also has a significant impact on the employees of target firms. The fact that larger firms employ the vast majority of workers is another reason for focusing on value-weighted long-term consequences.

Electronic copy available at: https://ssrn.com/abstract=3260095

interventions based on cumulative pre- to post-activism returns from one month before the intervention through the one and two years afterward. Abnormal returns are based on a matched-portfolio approach developed by Daniel et al. (1997) and Chan et al. (2009).

Similar to prior research, we find that short-term equal-weighted mean returns are significantly positive at 5.4%, and the cumulative pre- to post-activism equal-weighted mean one-year and two-year returns are significantly positive at 6.8% and 5.9%. However, examining returns by size decile shows that the positive equal-weighted long-term returns are primarily driven by the smallest 20% of targets, with an average market value of just $22 million (see Figure 1). Equal-weighted average returns for the larger 80% of targets are initially positive but become insignificant within three months of activism and become an insignificantly negative -1.6% at the end of two years (see Figure 2). On a value-weighted basis, short-term returns for the pooled sample are significantly positive but less than half the size of the equal-weighted returns, whereas the cumulative pre- to post-activism long-term returns differ insignificantly from zero. Fewer than half of all activist targets experience positive long-term returns, and the mean net impact of activism in terms of shareholder dollars (i.e., total change in shareholder wealth) is insignificant.

Our equal-weighted returns tests clearly indicate a minority of small firms drives the significantly positive equal-weighted mean long-term returns found in prior papers. Interpreting the implications of our value-weighted returns tests for shareholder wealth depends somewhat on how much weight one places on the short- versus long-term tests. At best, the short-term value-weighted returns tests indicate activist interventions have a positive but far smaller impact on the typical shareholder than indicated by the equal-weighted returns in the literature. A less favorable interpretation is that the long-term value-weighted returns are inconsistent with

Electronic copy available at: https://ssrn.com/abstract=3260095

interventions benefiting shareholder wealth over a longer horizon, consistent with critics' concerns about activists causing temporary price increases.[4] Altogether, we interpret our returns tests as providing minimal support for the hypothesis that activist interventions drive long-term increases in wealth for the typical shareholder.

Our second set of analyses focuses on long-term operating performance for the 1,455 targets that survive as public companies for at least two years following activism. (The remaining 26% of sample firms delist and are discussed below.) We match control firms not only on size, industry, and level of return on assets (ROA), but also on the recent *trend* in ROA over the years leading up to the activist date.[5]

Using a difference-in-differences approach, we first confirm prior findings that the operating performance of target firms appears to improve when compared with control firms that are matched on the level but not the trend in pre-activism ROA. However, the matched firms are dissimilar from target firms along many dimensions, including the pre-activism trend in ROA. Matching on both the level and trend in ROA produces more similar matches and finds no evidence of post-activism changes in ROA for target firms, regardless of whether we examine the equal-weighted mean, the value-weighted mean, the median, or the aggregate dollar effects. We further extend the literature by examining a more comprehensive set of accounting-performance measures, including return on equity, return on net operating assets, profit margin, asset turnover, and spread over borrowing costs, but again fail to find consistent evidence of

[4] Long-term returns are difficult to precisely estimate and test. Despite these difficulties, we do identify significant long-term returns on an equal-weighted basis in the pooled sample, and tests partitioning on market value find statistically significant long-term returns in the smallest 20% of firms. Further, the larger 80% of firms experience a negative equal-weighted average return, which is inconsistent with value creation.

[5] Barber and Lyon (1996) and Holthausen and Larcker (1996) illustrate the difficulty in developing valid benchmarks for assessing changes in operating performance, especially for settings where large changes occur in operating performance prior to some event. Additional analyses discussed in section 4.2 expand our analyses to control for differences in other covariates, including market value, book-to-market, leverage, cash holdings, payout, analyst following, sales growth, and firm complexity.

Electronic copy available at: https://ssrn.com/abstract=3260095

improvements following activist interventions. We also examine post-activism investments in R&D, advertising, and equipment, and find little evidence of consistent increases or decreases. Nor do we find consistent evidence of improvements in operating performance among subsamples of firms formed based on ex-post outcomes. We also find no evidence of *expected* changes in operating performance based on post-activism changes in analyst EPS forecasts. In sum, across a large battery of appropriately matched tests, we fail to find consistent evidence that activists drive changes in accounting-based operating performance.

Given that we find no evidence of improved operating performance, a final set of descriptive analyses investigate which—if any—of the traditional explanations for activist interventions *do* produce long-term positive stock returns.[6] The 1,455 firms included in our operating-performance tests experience insignificant value-weighted mean two-year returns of -2.3%. Descriptive evidence based on ex-post outcomes finds that firms with asset sales, a CEO change, or board turnover tend to have neutral to negative abnormal long-term returns, whereas those with high future payouts tend to experience neutral to positive changes in shareholder value. Overall, we find little evidence that commonly discussed strategy and governance motivations for activist interventions have consistent associations with improvements in shareholder wealth.

Turning to the 26% of our sample that delist and are not included in our operating-performance tests, 19% are acquired by another firm and experience significantly positive long-term returns. Specifically, the value-weighted mean two-year return for acquired targets is 26.4%. The remaining 7% of firms delist for other reasons and experience significantly negative

[6] Studies find mixed evidence on whether hedge fund activists successfully prompt governance or operational changes, and whether post-intervention changes are linked to long-term value creation or destruction is similarly unclear (Denes et al. 2016). For example, see the mixed results of Brav et al. (2008), Brav et al. (2010), Boyson and Mooradian (2011), and Klein and Zur (2009).

Electronic copy available at: https://ssrn.com/abstract=3260095

returns. Consistent with the findings of Greenwood and Schor (2009), these results indicate nearly all the positive long-term returns to activist interventions are concentrated among firms that are subsequently acquired.

In sum, our study provides two new insights. First, we confirm prior findings of significantly positive equal-weighted mean short- and long-term returns to activist interventions but find these positive returns are primarily driven by the smallest 20% of targets. Value-weighted short-term returns are less than half the size of the equal-weighted returns, and cumulative pre- to post-activism long-term returns are insignificantly different from zero. Consistent with the findings of Greenwood and Schor (2009), nearly all of the positive long-term returns to activist interventions are concentrated among firms that are acquired. Second, using an appropriately matched sample, we find no evidence that activist interventions induce long-term improvements in a broad set of accounting-performance variables.

Our findings also provide important inputs into the debate regarding the costs and benefits of hedge fund activism. Public discourse frequently cites academic findings that activist interventions improve long-term value and operating performance, and these findings have likely influenced investors and regulators. Our findings do not strongly support arguments that activist interventions drive long-term wealth for the average investor. At the same time, we find no evidence that activist interventions destroy value, so our findings also fail to support critics' proposals to restrict activism. However, like most studies of hedge fund activism, our results speak solely to the first-order effects of activist interventions on the shareholders of target firms. Broad policy analyses should also consider a comprehensive set of costs and benefits, including the externalities of activist interventions for peer firms and the effects of activism threats (Aslan and Kumar 2016; Gantchev et al. 2016; Bourveau and Schoenfeld 2017).

Electronic copy available at: https://ssrn.com/abstract=3260095

2. Review of Prior Literature

Denes et al. (2016) comprehensively review the literature on hedge fund activism. Discussion in this section primarily focuses on studies of firm value and operating performance, which serve as the motivation for our empirical tests. Also, our discussion primarily focuses on published and forthcoming studies. Although other working papers also examine the long-term consequences of hedge fund activist interventions, those papers generally find positive effects that are consistent with the published papers discussed below.

2.1. Hedge fund activism and firm value

Research has consistently documented positive equal-weighted mean returns in the short window around activist interventions (e.g., Brav et al. 2008; Klein and Zur 2009; Becht et al. 2017; Bebchuk et al. 2015). These results are generally interpreted as evidence that activism is accretive to target firms' shareholders. However, a frequent criticism of hedge fund activists is that they induce temporary increases in share price to extract wealth from long-term shareholders (Denning 2015). Thus, the more important assessment is how target shareholders fare over a longer horizon.

Prior examinations of long-term returns use one of two methods. The first is to measure long-term returns, starting in the month after the activist intervention (e.g., months [+1, +T]). Studies interpret a lack of significantly negative results over [+1, +T] as indicating the initial positive short-term returns do not reverse and therefore activist interventions are overall value-enhancing (e.g., Brav et al. 2008; Bebchuk et al. 2015). However, the cumulative pre- to post-activism return over months [-1, +T] might also be insignificant, which would not support the

Electronic copy available at: https://ssrn.com/abstract=3260095

notion that activism enhances long-term shareholder value.[7] Examining cumulative pre- to post-long-term returns is especially important for activist interventions given critics' concerns about activists profiting from temporary price increases. Accordingly, the second method for evaluating long-term effects is to measure cumulative long-term returns including the activist intervention (e.g., months [-1, +T]). Studies following this method tend to find significantly positive returns in pooled samples, again indicating activism is value-enhancing (e.g., Greenwood and Schor 2009; Swanson and Young 2016).

Regardless of the long-term-returns measurement window, a critical observation is that prior studies focus almost exclusively on equal-weighted mean stock returns among target firms, without considering the distribution of returns across target firms. In a study of long-term returns to equity issuances, Brav et al. (2000) note that examining equal-weighted returns is useful if the prediction is that small stocks are more mispriced than large stocks or if one is interested in the actions of a typical manager, but that value-weighting is the correct method to gauge investor wealth effects. A similar idea is expressed by Brav and Gompers (1997), Fama (1998), Mitchell and Stafford (2000), and numerous other studies.

Our review of published and forthcoming studies identifies four that provide some evidence on value-weighted returns, but these results appear in late tables with little, if any, interpretation. Brav et al. (2008, Table 6, Panel C) tabulate insignificant value-weighted mean calendar portfolio returns during and after activist interventions and briefly suggest larger firms receive less favorable responses. Brav et al. (2010, Table 6, Panel B) tabulate results similar to those of Brav et al. (2008) but do not mention value-weighted returns. Bebchuk et al. (2015,

[7] This interpretation resembles that of Loughran and Vijh (1997), which notes that studies of aggregate long-term wealth gains to shareholders, following acquisitions, should examine returns accumulated over the combined event and post-event period.

Electronic copy available at: https://ssrn.com/abstract=3260095

Table 9) show insignificant value-weighted mean returns over months [+1, +36] and [+1, +60], and the paper interprets these returns as evidence that initial short-term returns do not reverse. However, Bebchuk et al. (2015) do not show whether short-term value-weighted returns are positive or evaluate the net long-term return from before to after the activist intervention. In Becht et al. (2017), the final row of Table 8, Panel A, shows that the value-weighted mean long-term return in North America is insignificant. However, the authors make little mention of this result.

From a policy perspective, the distribution of long-term returns across firms should be a primary research focus. The importance and implications of long-term value-weighted returns should be discussed to aid academics and regulators in debating the costs and benefits of activism for the economy.

2.2. Hedge fund activism and long-term operating performance

A review by Denes et al. (2016) finds that eight of 11 studies on hedge fund activism conclude that earnings-based measures of operating performance improve after activist interventions, whereas the remaining three find no change. Most studies use ROA as the dependent variable, but the methodological approach varies.

Greenwood and Schor (2009) examine the average within-firm change in ROA pre and post activism, whereas Clifford (2008) examines changes in within-firm industry-adjusted ROA. Brav et al. (2008), Boyson and Mooradian (2011), and Klein and Zur (2009) are more cognizant that changes in ROA could be driven by firm characteristics that correlate with activist interventions and therefore investigate post-activism performance, relative to firms matched on industry, size, and book-to-market (BTM). However, due to well-documented stochastic trends in accounting measures, Barber and Lyon (1996) and Holthausen and Larcker (1996) show that

Electronic copy available at: https://ssrn.com/abstract=3260095

tests of changes in operating performance are misspecified when control firms are not matched on pre-event performance. Furthermore, the summary statistics of Brav et al. (2008) show that matching on industry, size, and BTM identifies a control sample that is highly dissimilar from target firms on many dimensions, including pre-activism ROA. Thus, the operating-performance tests in the aforementioned studies should be interpreted with caution.

Bebchuk et al. (2015, Table 6) and Brav et al. (2008, Panel B of Table 7) perform a few tests matching on the pre-activism level of ROA. However, these tests are not the primary analyses in either paper, and little information is provided about covariate balance between target and control firms. Further, whereas Brav et al. (2008) and Bebchuk et al. (2015) match based on industry and the *level* of ROA, Brav et al. (2015, Figure 1) show that targets have highly abnormal *trends* in ROA prior to the activist interventions. We further examine the pre-activism trends in ROA in our Figure 3, both for target firms and firms matched on industry and level of ROA, similar to Brav et al. (2008). The solid line in Figure 3 shows that target firms experience an overall decline in ROA in the three years prior to the activist intervention, whereas the dashed line shows that matched control firms experience an increase in ROA. These data raise serious concerns about the parallel-trends assumption in differences-in-differences tests of operating performance. Therefore, considering the pre-activism level and *trend* in ROA is important to eliminate normal post-activism trends. Ensuring that matching procedures produce covariate balance between the treatment and control firms is also essential, as is evaluating both mean and median effects (Barber and Lyon 1996).

Finally, prior research has typically focused on ROA as a measure of operating performance, but this metric provides only a partial view of a firm's operating efficiency. For example, a target's ROA may be inflated because the denominator has shrunk, due to cash

Electronic copy available at: https://ssrn.com/abstract=3260095

payouts, even though its use of operating assets has not changed. Similarly, other aspects of firms' operations, such as profit margin or asset turnover, might improve, even though summary measures, such as ROA, remain unchanged. Investigating a broader set of accounting measures that can tease apart changes in the income statement versus balance sheet, as well as changes in investment behaviors, is therefore important.

2.3. Hedge fund activism and other measures of long-term operating performance

Other research has also used several operating-performance metrics other than ROA and its subcomponents. For example, Bebchuk et al. (2015) and Cremers et al. (2015) use Tobin's Q as a measure of operating performance. We do not examine Tobin's Q due to theoretical and practical concerns about using Q as a measure of operating performance (Dybvig and Warachka 2015). Most importantly, because market value is a primary input to Q, it likely captures the effects of acquisition probability or other factors that have little to do with operating outcomes.

Swanson and Young (2016) use the Piotroski (2000) FSCORE as a measure of operating performance. Although FSCORE includes some elements of financial performance, it also includes liquidity and capital-structure metrics. Thus, we do not believe the FSCORE is an appropriate measure of long-term operating performance.

Finally, Brav et al. (2015) use plant-level data from manufacturing firms to assess the operational effects of hedge fund activism. They find that factories that are activism targets experience abnormal declines in productivity in the years preceding the activist intervention, followed by productivity increases. The biggest improvements in productivity are concentrated among plants that were sold after the activist intervention. Although this analysis is useful and interesting, it has two drawbacks. First, the sample size of Brav et al. (2015) is modest, and results for manufacturing plants may not generalize to other types of firms. Second, most of the

Electronic copy available at: https://ssrn.com/abstract=3260095

tests of Brav et al. (2015) compare activism targets with non-target firms with dissimilar pre-activism performance trends, so are subject to our same concerns about matching.[8]

3. Sample selection and summary statistics

Data on hedge fund activism were kindly provided by Alon Brav and cover all hedge funds that filed a Schedule 13D with the SEC from 1994 to 2011. We obtain data from Compustat, CRSP, IBES, ExecuComp, and Equilar.[9] Our sample selection is outlined in Panel A of Table 1. We eliminate duplicate observations and keep only the first instance of activism per fiscal year. We also require that the target firm have necessary data to calculate abnormal stock returns based on portfolio assignments using firm size, BTM, and momentum as of the month prior to the activist date. Calculating firm size requires CRSP price and shares data. Assigning a firm to a BTM portfolio requires Compustat data on the firm's most recent publicly available book value and a valid SIC code in order to de-mean market-to-book as in Daniel et al. (1997). Finally, calculating momentum requires CRSP monthly returns data for at least six months over the one-year period prior to activism. These restrictions eliminate 720 observations, for a final sample of 1,964 activist interventions.

Table 2 reports descriptive statistics for variables defined in the table header. The first three rows report summary statistics for the variables used in the returns-portfolio assignments, so they are available for all firms. The bottom rows report descriptive statistics for additional

[8] Brav et al. (2015) do a robustness test matching on the pre-activism trend in performance, but whether the paper's other analyses would survive matching on the pre-activism trend in performance is unclear. Further, the matched-analyses robustness test pools both surviving and acquired firms, so whether operating improvements exist for surviving firms alone is unclear.

[9] We rely on the Equilar data when possible, because Equilar provides broader coverage than ExecuComp. We do not use ExecuComp for director information, because its director coverage only begins in 2006. Because Equilar data are only available starting in 2001, we use ExecuComp as the source of CEO data prior to 2001 and Equilar after 2001.

Electronic copy available at: https://ssrn.com/abstract=3260095

measures used in subsequent tests. Columns (5) and (6) report that target firms are smaller than the typical CRSP/Compustat firm, with an average market value of $791 million and total assets of $1,436 million. Targets tend to have below-average ROA and ΔROA from *t-3* to *t-1*, relative to the CRSP/Compustat universe, negative average sales growth, and above-average BTM, indicating activists tend to target underperforming firms. Targets' shareholder payouts are lower, and targets maintain higher cash balances than average, consistent with these firms tending to hoard cash (Klein and Zur 2009; Gantchev et al. 2016).

Panel B of Table 1 details the disposition of target firms as of 24 months following the activist intervention. The sample includes 1,455 "surviving" targets that remain as publicly traded companies for at least 24 months and 380 "acquired" firms that delist from CRSP, due to merger or exchange. The remaining 129 "delist" firms delist for other reasons. For the 1,455 surviving firms, we further categorize the sample into the four non-exclusive outcomes, detailed in Panel C. The first category, *Asset Sales*, consists of targets in the highest tercile of percentage decrease in total assets from *t-1* to year *t+2*, where *t* is the year of activism. The second category, *New CEO*, includes 453 targets that replace the CEO within two fiscal years following the activism date. *Board Turnover* includes the 449 firms with above-median board turnover.[10] *High Payout* includes targets in the highest tercile of change in shareholder payouts. Finally, we also define firms not in any of the four categories above as those with "No Change."

4. Analysis and results

4.1. Market-value tests

In designing our returns tests, careful consideration must be given to the appropriate (i)

[10] Because some of the targets are not covered in either the ExecuComp or Equilar databases, the sample size to calculate the median board turnover percentage is less than the maximum sample size of 1,964.

Electronic copy available at: https://ssrn.com/abstract=3260095

holding period, (ii) benchmark for calculating target firms' abnormal returns, and (iii) test statistics. We use a holding period of days [-10, +10] for tests of short-term returns immediately around the activist intervention. The start date is selected to capture return movements in advance of 13D filings (Brav et al. 2008; Bebchuk et al. 2015). As discussed in section 2.1 and recommended by Loughran and Vijh (1997), we measure long-term returns over one- and two-year periods, starting in the month prior to the activist intervention (i.e., months [-1, +12] and [-1, +24]). We begin in month -1 to capture return run-up in advance of the activist intervention.

Similar to Daniel et al. (1997), we compute benchmark returns using a matched 5x5x5 portfolio of firms based on size, BTM, and momentum. Because many of the targets experience significant changes in market value leading up to activist intervention, we create matched portfolios using public data as of the start of month -1.[11] We measure abnormal returns as the buy-and-hold return of the target firm over the holding period, less the matched portfolio's return. If a target delists, we include the delisting return and assume no subsequent abnormal returns exist. We report results using non-rebalanced portfolios that better reflect the typical investor's experience (Loughran and Vijh 1997), but results are similar if we assume monthly rebalancing. For value-weighted returns, we weight each target by its fraction of the total NYSE/NASDAQ/AMEX market in the month the reference portfolio is formed. Abnormal changes in market value are calculated as the abnormal return multiplied by the firm's market value from just prior to the returns window.

We examine portfolio-adjusted buy-and-hold abnormal returns in lieu of calendar-time portfolios, because prior literature has found that calendar-time portfolios can be biased toward zero by ignoring the possibility of market timing (e.g., Loughran and Ritter 2000). Many targets

[11] Because many targets are very small firms, we make one addititional adjustment from Daniel et al. (1997). Specifically, we do not require the portfolio firms to have two years of Compustat data prior to portoflio formation.

Electronic copy available at: https://ssrn.com/abstract=3260095

experience substantial negative returns prior to activism, suggesting market timing is a selection factor. Regardless, untabulated calendar-time portfolio tests have similar results: significantly positive equal-weighted long-term returns and insignificant value-weighted long-term returns.[12] We use a matched-portfolio approach, instead of a factor model, because firms' risk profiles likely change during hedge fund interventions.

With regards to the appropriate test statistic, we evaluate significance using a pseudo-portfolio bootstrap approach, similar to that discussed by Lyon et al. (1999) and Kothari and Warner (2007, 1997). For each target, we draw, with replacement, another firm in the same 5x5x5 portfolio and compute its buy-and-hold abnormal return, relative to its portfolio over the specified period. We repeat this process 1,000 times and compare the actual target returns with the distribution of the bootstrapped sample. We assess significance by examining whether the actual target returns are within the extreme 10%, 5%, or 1% of the bootstrapped distribution.

Table 3, Panel A, reports equal-weighted abnormal returns. Similar to prior studies, the equal-weighted short-term return is 5.4% and significant at 1%. The cumulative pre- to post-activism one- and two-year returns are 6.8% and 5.9%, and both are significant at 1%. However, less than half of targets experience positive long-term returns.

Figure 1 extends prior research by examining the distribution of equal-weighted returns across targets, and indicates large positive abnormal returns are concentrated among the smallest 20% of targets. Data in Table 3, Panel B, show that these targets are economically small, with an average market capitalization of just $22 million. The larger eight deciles of targets experience more modest or even negative average long-term returns. The only significant two-year returns in deciles 3 through 10 have inconsistent signs, being positive in decile 7 and negative in decile

[12] Throughout this paper, "similar" results mean significant test coefficients remain significant at 10% and insignificant test coefficients remain insignificant.

Electronic copy available at: https://ssrn.com/abstract=3260095

8. On a pooled basis, the larger 80% of firms have a significantly positive short-term return of 4.4% but an insignificantly negative two-year return of -1.6% (rightmost column of Panel B), suggesting the initially positive returns are temporary.

Panels A and B of Figure 2 further examine the trends in long-term equal-weighted returns separately for the smallest 20% of firms and largest 80% of firms. Panel A of Figure 2 shows the smallest 20% of firms experience consistently positive long-term equal-weighted returns, reaching 36% at the end of two years. Panel B of Figure 2 shows that the larger 80% of targets experience initially positive equal-weighted returns followed by an apparent reversal. Tests in Panel C of Table 3 find the equal-weighted long-term returns for the smallest 20% of firms are significantly positive each month, whereas for the largest 80% of targets, they are no longer significantly positive within just three months of the activist intervention. Although untabulated tests fail to find the post-activism reversal is statistically significant, the combination of results provide minimal support for the hypothesis that activist interventions drive long-term value enhancements for anything but the smallest 20% of firms.

Panel D of Table 3 examines value-weighted returns and abnormal changes in nominal market value. Column (1) shows value-weighted short-term returns are significantly positive but are just 2.4%, as opposed to the 5.4% equal-weighted return in Panel A. Value-weighted long-term returns are insignificantly different from zero, which is again consistent with the significant equal-weighted long-term returns in Panel A being driven by small firms. Column (2) reports that target firms' mean abnormal change in dollar market value is a statistically significant $18.8 million in the short window surrounding the activist intervention. However, the long-term changes in market value in the middle and lower rows of column (2) are insignificantly different from zero.

Electronic copy available at: https://ssrn.com/abstract=3260095

Finally, Panel E of Table 3 examines returns after dividing the sample into three sub-periods of approximately equal lengths. Two trends are apparent. First, activist targets were smaller in the earliest period (1993–1999) than the latter two periods (2000–2006, 2007–2011), increasing in MVE from $333 million to over $900 million. Second, the equal-weighted mean long-term returns decline over the three sub-periods. Although conjecture, these data could be consistent with a decline in the supply of the most desirable targets, an increase in competition among hedge funds, or both driving down the profitability of activism.

4.2. Operating performance

We next investigate long-term changes in operating performance. Section 4.2.1 investigates ROA, using a matching technique from prior papers that does not consider pre-activism performance trends. Section 4.2.2 examines ROA using improved matching criteria. Section 4.2.3 extends the literature by examining measures of accounting performance other than ROA. Section 4.2.4 examines changes in analyst EPS forecasts as a forward-looking assessment of changes in performance.

4.2.1. Operating performance – examining ROA without matching on pre-event trend in ROA

Table 4 presents analyses matching on industry, year, and pre-event level of ROA, similar to the methods used by Brav et al. (2008) and Bebchuk et al. (2015).[13] We define ROA as operating income before depreciation and amortization, scaled by total assets. Matched firms must be in the same two-digit SIC industry (expanded to one-digit if no match is available) and have ROA between 90% and 110% of the target in the year prior to the activist intervention. Panel A of Table 4 shows that, of the 1,455 firms available for our performance tests, six do not

[13] As discussed in section 2.2, matching procedures that do not include any measure of pre-event performance are misspecified. Thus, for brevity, we do not investigate results using matching procedures from past papers that do not include any measure of performance, such as those matching on size and BTM.

Electronic copy available at: https://ssrn.com/abstract=3260095

have sufficient data to calculate ROA. We lose another 23 firms without any adequate matching firm. Panel B of Table 4 reports covariate balance for our matching variable ROA as well as other variables previously found to be associated with activism (Brav et al. 2008; Clifford 2008; Boyson and Mooradian 2011). ROA is similar between the target and control firms, but significant differences exist for other variables.[14] The absence of covariate balance is problematic because whether the observed differences in post-activism operating performance between target pairs and their matched control are due to activism or covariate differences is unclear.

Brav et al. (2008) find that the activists' holding periods range from the 25th to 75th percentile of appoximately six months and two years, respectively. Still, for completeness, we examine changes in operating performance over each of the five years following activist interventions. The sample size decreases over time, due to delistings and missing data.

The upper rows of Panel C tabulate within-firm pre- to post-activism changes in ROA for target firms for years *t+1* through *t+5*, all relative to year *t-1* (denoted as ΔROA_{t+i}). We report the equal-weighted mean (column (1)); the value-weighted mean, which is scaled by assets (column (2)); and the median (column (3)). Like Greenwood and Schor (2009), we find little evidence that within-firm operating performance changes with activism.[15] The lower rows of Panel C tabulate differences in ΔROA_{t+i} between the target and control firms. All differences in means and medians are significantly positive, which is consistent with prior inferences that

[14] The mean ROA for target firms in Table 4 is 0.045 versus 0.024 in Table 2. This difference is primarily due to requiring that firms survive for 24 months to be included in our long-run performance tests. Firms not satisfying this requirement have an average ROA of -0.034.

[15] Observing no improvement in within-firm ΔROA for the activist targets reduces concerns that activist interventions improve ROA for both target and control firms (e.g., due to spillover effects), in which case, comparing ΔROA for target firms to ΔROA for control firms mitigates the effects we are investigating.

Electronic copy available at: https://ssrn.com/abstract=3260095

activist interventions boost operating performance.[16]

4.2.2. Operating Performance – examining ROA with matching on pre-event trend in ROA

We next expand the matching procedure to include industry, year, size, the level of ROA, and the pre-activism trend in ROA. Within each two-digit SIC industry-year, we match simultaneously on these variables using the following metric:

$$Score_{i,t-1} = \frac{AT_{i,t-1}}{\sigma_{j,t-1}^{AT}} + \frac{ROA_{i,t-1}}{\sigma_{j,t-1}^{ROA}} + \frac{\Delta ROA_{i,t-1}}{\sigma_{j,t-1}^{\Delta ROA}}. \quad (1)$$

$AT_{i,t-1}$ is the total assets for firm i in year $t-1$; $ROA_{i,t-1}$ is the ROA level; $\Delta ROA_{i,t-1}$ is the firm's change in ROA over years $t-3$ to $t-1$; and σ_{jt} is the standard deviation of AT, ROA, or ΔROA in the firm's industry j for year $t-1$. We scale the components of *Score* by the standard deviation to prevent the variable with the largest variance from having an outsized impact on *Score.* We also require the matched firm to be within [20%, 500%] of assets and ± 0.05 for ROA and ΔROA. We impose these calipers to prevent instances of targets being matched to firms with similar values of *Score* but significant differences along two dimensions that offset each other (Angrist and Pishke 2008). Thus, our matched firm is selected as a firm in the same industry and year with the closest absolute difference in *Score*, subject to the caliper restriction.

Panel A of Table 5 shows that 41 firms do not have sufficient data to calculate pre-activism ROA or ΔROA. We lose another 288 firms without any adequate match.[17] Panel B of Table 5 reports that the matching variables are similar between our target and control firms.

[16] Some tests of Brav et al. (2008) and Bebchuk et al. (2015) find a positive but insignificant change in operating performance in the first year or two after the activist intervention, whereas we find a positive and significant change in all years. This difference may arise from differences in sample size, because the aforementioned papers have sample periods ending in 2007.

[17] The fact that we cannot find adequate matches for 288 target firms highlights the unusual nature of firms that are subject to activist interventions, and indicates the matched samples used in prior studies are potentially unlikely to have covariate balance. In untabulated analysis, the 288 firms that could not be matched tended to be smaller, with mean assets of $489 million, and more extreme values of *ROA*, with an average value of -0.052. Dropping these firms explains why the target firms' mean ROA increases from 0.045 in Table 4 to 0.071 in Table 5.

Electronic copy available at: https://ssrn.com/abstract=3260095

Significant differences do exist for other characteristics of target and control firms. However, incorporating these characteristics into our matching equation or into a traditional propensity-score model produces matches that do not achieve covariate balance for the level and change in ROA (untabulated). Therefore, we use *SCORE* to match firms, because it better achieves covariate balance in our main variables of interest, and we use alternate procedures below to adjust for other covariates.

The upper rows of Panel C of Table 5 find the post-activism within-firm equal weighted mean changes in ROA are generally negative, but results are more mixed for value-weighted means and medians. The lower rows of Panel C tabulate differences in ΔROA_{t+i} between the target and control firms. All differences in means and medians are statistically insignificant. These results are in stark constrast to the significantly positive differences in Table 4 that match excluding trend in ROA. Instead, Panel C of Table 5 indicates the post-activism operational performance of target firms is generally no different from that of comparable control firms.

Similar to our analysis of both abnormal returns and changes in aggregate market value, Panel D of Table 5 examines post-activism changes in operating performance based on dollars of income. We calculate dollar-income effects by multiplying each target firm's abnormal ΔROA by its total assets from year t-1. The mean and median abnormal changes in income are all insignificantly different from zero. Like the ROA tests, these tests provide no indication that activist interventions affect operating performance.

We also adjust for pre-treatment differences between treatment- and control-firm variables for the covariates in listed in Panel B. Our tests are based on the intercept (α) from the following regression. Differences in means (medians) are based on an OLS (median) regression:

Electronic copy available at: https://ssrn.com/abstract=3260095

$$\Delta ROA^{Diff}_{(t+i)-(t-1)} = \alpha + \sum_{k=0}^{K} VAR^{Diff}_{t-1} + \varepsilon. \quad (2)$$

$\Delta ROA^{Diff}_{(t+i)-(t-1)}$ is the difference-in-differences between ΔROA for the target firm minus the ΔROA for its individually matched control firm. Similarly, VAR^{Diff} is the difference between each covariate reported in the middle rows of Panel B, all measured prior to the activism date. The α coefficient is our variable of interest and is the estimated average value of the dependent variable conditional on zero pre-activism differences in the included covariates (Stuart 2010). As presented in Panel E, the results controlling for differences in covariates resemble those in Panel C, except that the value-weighted mean change is positive in years *t+4* and *t+5*. However, given that tests of operating-performance medians are known to be better specified than tests of means (Barber and Lyon 1996) and given that all other results in Table 5 are insignificant, we draw little inference from the two significant test statistics.

Another concern with the analyses in Panel C is that our sample-selection procedure requires that the target firms not be delisted or acquired within 24 months, whereas we did not impose a similar requirement on the matched control firms. Panel F tabulates results repeating Panel C but after requiring that the control firm has available Compustat data through year *t+2*. All results remain insignificant.

Panel G tabulates changes in ROA for subsamples of surviving firms based on their realized outcomes. Categorizing firms based on realized outcomes raises selection concerns, but we present these results for descriptive purposes. For brevity, we tabulate only equal-weighted mean changes in ROA, although value-weighted mean and median changes produce largely similar results. In general, we do not find consistent evidence of significant changes in ROA among any of the groups of firms based on realized outcomes. Overall, the analyses in Table 5

Electronic copy available at: https://ssrn.com/abstract=3260095

provide little evidence consistent with activism affecting the operating performance of target firms.

4.2.3. Operating performance – measures other than ROA

Most papers on activism focus on ROA as a measure of accounting performance. However, the accounting literature typically studies a variety of metrics to provide a more complete understanding of firms' operating outcomes. Our analyses in this section are based on the framework and variables developed by Nissim and Penman (2001). These tests maintain the matches based on *Score* from equation (2) for comparability purposes, as well as because our existing matches are largely balanced (see Table 5, Panel B).

Activist interventions might induce changes in non-operating assets or operating liabilities, either of which could confound using total assets as a scalar in measuring operating performance. Panels A and B of Table 6 reports the difference-in-differences for return on net operating assets (RNOA) and return on common equity (ROE), respectively. For RNOA, just one of 15 tests finds a significant improvement in RNOA relative to the matched firms. For ROE, three tests find significantly negative changes, and two find significantly positive changes. These findings are generally similar in untabulated tests requiring matches to survive until $t+2$, as well as when we control for pre-treatment differences using equation (2). In sum, we interpret the analyses of RNOA and ROE as failing to find consistent evidence of either increases or decreases in performance.

Panels C and D of Table 6 further decompose RNOA into profit margin (PM) and asset turnover (ATO). An advantage of examining profit margin is that it avoids using assets altogether. Examining the revenue-based measure asset turnover is especially important because activism may induce investments in long-term projects that are immediately expensed and

Electronic copy available at: https://ssrn.com/abstract=3260095

decrease accounting earnings but may not indicate worse long-run performance (e.g., R&D or brand-buiding). Of the 15 tests of PM in Panel C, two are significantly positive and one is significantly negative. Of the 15 tests of asset turnover in Panel D, three are significantly positive and the rest are insignificant. The majority of evidence supports neither an increase nor decrease in operating performance.

For completeness, in untabulated tests, we examine the remaining two components of the Nissim and Penman (2001) decomposition of ROE: financial leverage (FLEV) and the spread of RNOA in excess of net borrowing cost (SPREAD). The results for financial leverage are mixed, with generally negative changes in terms of medians, positive changes for value-weighted means, and insignificant changes for equal-weighted means. Thus, the data do not provide a clear indication of either increases or decreases in financial leverage. Results for SPREAD fail to find any significant increases or decreases.

Finally, in untabulated tests, we further examine whether the target companies alter their investments after activist interventions. We examine two measures of investments in intangible assets, R&D intensity (RND) and advertising intensity (ADV). Our measure of investment in tangible assets is firms' capital expenditures (CAPEX). One of 15 tests finds a decline in R&D intensity, whereas the results for advertising intensity find generally positive changes in equal-weighted means, negative changes in value-weighted means, and insignificant changes in medians. For CAPEX, we find consistently negative changes in value-weighted means and insignificant results for equal-weighted means and medians.

In sum, although we observe occasionally significant positive and negative coefficients across our battery of tests, the results in this section fail to find evidence of consistent trends in performance, leverage, or investments in the years following hedge fund activist interventions.

Electronic copy available at: https://ssrn.com/abstract=3260095

4.2.4. Operating performance – measured by analyst forecasts surrounding 13D filing

An alternative approach for assessing changes in operating performance is to examine the EPS forecasts of sell-side equity analysts. This analysis has three advantages over the analysis based on ROA realizations. First, the ex-post analysis of operating performance in the previous section can only include firms that remain public to report earnings after the intervention, which may introduce sample-selection biases. If the firm has analyst coverage, measuring ex-ante expected changes in performance based on equity analyst forecasts is possible even for firms that delist after the activist intervention. Second, the positive short-term market reaction to activist interventions might be caused by the belief that operations will improve, even if these improvements do not actually occur. In this case, analyst EPS forecasts could reflect such beliefs. Finally, equity analysts are sophisticated market participants, and studying their response to the 13D announcement can provide insight into how sophisticated market participants likely view the impact of activism on future performance.

Our tests of analyst EPS recommendations are distinct from tests of analysts' buy, hold, and sell recommendations of Brav et al. (2008) and Swanson and Young (2016), who find that analysts issue more favorable recommendations after activist interventions. Examining these recommendations is not necessarily informative about expectations of future *operating performance*, but rather reflects analysts' opinions about the value of a firm's stock. Observing fewer downgrades or more upgrades after activist interventions could indicate analysts expect firm value to rise for reasons unrelated to operating performance (e.g., due to an acquisition). Also, the analyst-based tests of Brav et al. (2008) do not match or adjust to control for mean reversion in analysts' outlooks following periods of declines.

We use the IBES detail file to construct consensus forecasts for years t, t+1, and t+2, both

Electronic copy available at: https://ssrn.com/abstract=3260095

before and after the activism date. Our pre-activism mean and median consensus are based on the most recent forecast for each analyst issued or reconfirmed during days [-180, -10], relative to the 13D filing. Our post-activism mean and median consensus are based on the first forecast issued or reconfirmed by each analyst within days [0, +30], relative to the 13D filing. We use a 30-day post-activism window to increase the likelihood that the analysts' forecast revisions are responding to the activist intervention. In instances when an analyst does not issue or explicitly reconfirm a forecast within the 30-day window, we use the analyst's most recent forecast issued prior to the 13D filing. Using the last available forecast assumes the analyst's failure to revise the forecast is an implicit reaffirmation of the existing forecast. We further require at least two analyst forecasts within these windows to calculate the analysts' consensus. We scale earnings forecasts by price to reduce concerns about scale effects. These data requirements reduce our analysis to 1,082 activist events.

Columns (1)–(3) of Table 7 report the equal-weighted mean and median consensus forecasts pre- and post-activism, although untabulated results of the value-weighted mean are similar. Panel A (B) reports consensus pre- (post-) activism, whereas Panel C describes the paired difference. Although the results in Panel C indicate the mean analyst forecast for years t and $t+1$ decreases after the activist intervention, these results do not control for the well-documented downward drift as a given fiscal period-end approaches (Richardson et al. 2004). To address this concern, we use a within-firm analysis and compare the pre- and post-activism changes in consensus with the equivalent change over a pseudo-event date, which we define as one-year prior actual activism. Columns (4)–(6) in Panel C show that consensus forecasts also significantly decline around the pseudo-event dates. Columns (7)–(9) in Panel C report the difference-in-differences between the forecast changes following the two dates. The median t

Electronic copy available at: https://ssrn.com/abstract=3260095

forecast is significantly negative, and the mean *t+1* forecast is significantly positive, but all other tests are insignificant. These results are inconsistent with analysts changing their forecasts, due to activism, and do not support the hypothesis that the market expects significant changes in operating performance. Thus, like the ex-post ROA results in the prior section, we do not find consistent ex-ante evidence that hedge fund activism improves the operating performance of target firms.

4.3. Further analysis of firm value based on ex-post outcomes

The analyses in the preceding two sections raise a logical question: Given the absence of evidence of improvements in operating performance, following activist interventions, what drives the significantly positive long-term stock returns observed for at least some firms? In this section, we examine cross-sectional variation in the market-value tests based on realized outcomes. We focus our analysis on realized outcomes, rather than stated objectives, because Brav et al. (2015) find that 61% of hedge funds do not state their specific objectives. Again, categorizing firms into groups based on outcomes raises selection and survivorship concerns, but we present these results for descriptive purposes.

Given that Greenwood and Schor (2009) find long-term positive stock returns to activist interventions only among firms that are subsequently acquired, Panel A of Table 8 first investigates equal- and value-weighted mean returns for firms that are acquired (column (1)) versus all other firms (column (2)). The upper rows of column (3) show a significant difference in short-term returns between acquired and nonacquired firms, which could indicate shareholders predict which firms are more likely to be acquired, or that some acquisition negotiations are potentially revealed within the [-10,+10] window. Column (1) of the lower rows finds the equal- and value-weighted long-term returns for acquired firms are all significantly positive and

Electronic copy available at: https://ssrn.com/abstract=3260095

economically large. The equal-and value-weighted long-run returns for nonacquired firms in column (2) are all insignificant.

Columns (4) through (6) disaggregate nonacquired firms into those that do and do not survive as public companies for at least two years. Unsurprisingly, column (4) finds that firms that delist for nonacquisition reasons experience negative long-term returns. Column (5) finds that surviving firms experience positive equal-weighted returns of 7.2% and 7.5% over the one- and two-year holding period, respectively. However, the value-weighted returns for surviving firms are negative and insignificant. Untabulated tests examining changes in dollar market value, instead of returns, produce inferences similar to those of the value-weighted returns tests. In sum, the results in Table 8 indicate positive long-term average effects of activism occur primarily for acquired targets. Within surviving but nonacquired firms, small surviving firms experience positive returns, whereas larger firms end up neutral or worse off.

Table 9 provides further detail on the short- and long-term returns for surviving firms based on our five ex-post outcome categories. Short-term equal-weighted mean returns are positive, regardless of the firms' outcomes. However, we find insignificant value-weighted mean returns for firms with high asset sales or a new CEO. Column (1) shows that firms in the highest tercile of asset sales have negative long-run returns (both equal-weighted and value-weighted). Column (2) shows that firms with CEO turnover have negative value-weighted long-term returns and insignificant equal-weighted returns, which is consistent with Keusch's (2016) finding that poorly performing CEOs of target companies are more likely to be replaced. Meanwhile, firms with significant board turnover have mostly neutral returns. Column (4) shows that firms with high future payout tend to experience positive returns. Finally, column (5) shows that firms not falling into any of the aforementioned categories tend to have positive returns. Untabulated tests

Electronic copy available at: https://ssrn.com/abstract=3260095

of changes in market values are again largely consistent with the value-weighted returns tests.

5. Concluding Remarks

This paper examines the long-term economic consequences associated with hedge fund activist interventions. Most research concludes targets experience increases in shareholder value and operating performance after an activism event. We challenge two aspects of this research.

First, when research examines long-term returns, it mostly focuses on equally weighted returns earned by a portfolio of activist targets. However, for policy-oriented research evaluating the impact of hedge fund activism on shareholder wealth and the economy, a focus on *value-weighted* pre-to-post activism long-term stock returns is worthwhile. We find that the positive equal-weighted long-term returns to activist interventions are driven by firms with market values of less than $40 million, and we fail to find that larger targets experience consistent increases in shareholder value. On a value-weighted basis, long-term returns are insignificantly different from zero.

Second, when examining operating performance, prior research does not adequately control for known stochastic behavior of accounting metrics, such as ROA, or address covariate imbalance between matched target and non-target firms. Prior work also provides little analysis of other accounting-based measures of operating performance. With an appropriately matched sample, we find no evidence of improvements in multiple measures of operating efficiency, following activist interventions, nor do we find that analysts expect earnings improvements.

In contrast to some studies, our results do not strongly support the hypothesis that hedge fund interventions drive long-term improvements in shareholder wealth or firms' operating performance. We also fail to find that activist interventions harm shareholders. Our findings

Electronic copy available at: https://ssrn.com/abstract=3260095

provide new evidence to inform the debate among academics, regulators, and managers about the costs and benefits of hedge fund activism for shareholders and the economy.

Electronic copy available at: https://ssrn.com/abstract=3260095

References

Angrist, J., & Pischke, J. (2008). *Mostly Harmless Econometrics: An Empiricists Companion*. Pricenton, NJ: Princeton University Press.

Aslan, H., & Kumar, P. (2016). The product market effects of hedge fund activism. *Journal of Financial Economics,* 119(1), 226-248.

Barber, B. M., & Lyon, J. D. (1996). Detecting abnormal operating performance: The empirical power and specification of test statistics. *Journal of Financial Economics,* 41(3), 359-399.

Bebchuk, L. (2013). The Myth of Hedge Funds as 'Myopic Activists'. *The Wall Street Journal*. https://www.wsj.com/articles/SB10001424127887323309404578614004210782388. Accessed 8 Aug 2016.

Bebchuk, L. A., Brav, A., & Jiang, W. (2015). The Long-Term Effects of Hedge Fund Activism. *Columbia Law Review,* 115(5), 1085-1155.

Becht, M., Franks, J. R., Grant, J., & Wagner, H. F. (2017). The returns to hedge fund activism: An international study. *Review of Financial Studies,* 30(0), 2933-2971.

Black, J. (2017). The Activist Investing Annual Review 2017. *Harvard Law School Forum on Corporate Governance and Financial Regulation*. https://corpgov.law.harvard.edu/2017/02/21/the-activist-investing-annual-review-2017. Accessed 22 Apr 2017.

Bourveau, T., & Shoenfeld, J. (2017). Shareholder activism and voluntary disclosure. *Review of Accounting Studies*, 22 (3), 1307-1339.

Boyson, N. M., Mooradian, & R. M. (2011). Corporate governance and hedge fund activism. *Review of Derivatives Research,* 14 (2), 169-204.

Brav, A., Geczy, C., Gompers, & P. A. (2000). Is the abnormal return following equity issuances anomalous? *Journal of Financial Economics,* 56(2), 209-249.

Brav, A., Gompers, P. A. (1997). Myth or reality? The long-run underperformance of initial public offerings: Evidence from venture and nonventure capital-backed companies. *Journal of Finance,* 52(5), 1791-1821.

Brav, A., Jiang, W., & Kim, H. (2015). The Real Effects of Hedge Fund Activism: Productivity, Asset Allocation, and Labor Outcomes. *Review of Financial Studies*, 28(10), 2723-2769.

Brav, A., Jiang, W., Partnoy, F., & Thomas, R. (2008). Hedge fund activism, corporate governance, and firm performance. *Journal of Finance*, 63(4), 1729-1775.

Chan, L. K., Dimmock, S. G., & Lakonishok, J. (2009). Benchmarking money manager performance: Issues and evidence. *Review of Financial Studies*, 22(11), 4553-4599.

Clifford, C. P. (2008). Value creation or destruction? Hedge funds as shareholder activists. *Journal of Corporate Finance*, 14(4), 323-336.

Cremers, M., Giambona, E., Sepe, S. M., & Wang, Y. (2015). Hedge fund activism and long-term firm value. Unpublished manuscript.

Electronic copy available at: https://ssrn.com/abstract=3260095

Daniel, K., Grinblatt, M., Titman, S., & Wermers, R. (1997). Measuring mutual fund performance with characteristic-based benchmarks. *Journal of Finance*, 52(3), 1035-1058.

Denes, M. R., Karpoff, J. M., & McWilliams, V. B. (2016). Thirty years of shareholder activism: A survey of empirical research. *Journal of Corporate Finance*, 44, 405-424.

Denning, S. 2015. (2015). The Seven Deadly Sins of Activist Hedge Funds. *Forbes.com*. https://www.forbes.com/sites/stevedenning/2015/02/15/the-seven-deadly-sins-of-activist-hedge-funds/#4e3571e244d0. Accessed 25 Apr 2018.

Dybvig, P. H., & Warachka, M. (2015). Tobin's q does not measure firm performance: Theory, empirics, and alternatives. Unpublished manuscript.

Fama, E. F. (1998). Market Efficiency, long-term returns, and behavioral finance. *Journal of Financial Economics*, 49(3), 283-306.

Gallagher, D. (2015). Activism, Short-Termism, and the SEC. In *21st Annual Stanford Directors' College*. https://www.sec.gov/news/speech/activism-short-termism-and-the-sec.html . Accessed 22 Apr 2017.

Gandel, S. (March 19 2015). SEC's Mary Jo White criticizes shareholder activism and Bill Ackman deal. *Fortune.* http://fortune.com/2015/03/19/mary-jo-white-activist-investors-ackman. Accessed 22 Apr 2017.

Gantchev, N., Gredil, O., & Jotikasthira, C. (2016). Governance under the gun: Spillover effects of hedge fund activism. Unpublished manuscript.

Greenwood, R., & Schor, M. (2009). Investor activism and takeovers. *Journal of Financial Economics*, 92(3), 362-375.

Holthausen, R. W., & Larcker, D. F. (1996). The financial performance of reverse leveraged buyouts. *Journal of Financial Economics*, 42(3), 293-332.

Keusch, T. (2016). Shareholder Power and Managerial Incentives. Unpublished manuscript.

Klein, A., & Zur, E. (2009). Entrepreneurial Shareholder Activism: Hedge Funds and Other Private Investors. *Journal of Finance*, 64(1), 187-229.

Kothari, S., & Warner, J. (1997). Measuring long-horizon security price performance. *Journal of Financial Economics,* 43(3), 301-339.

———. 2007. Econometrics of Event Studies. In E. Eckbo (Ed.), *Handbook of Corporate Finance: Empirical Corporate Finance*. Oxford, UK: Elsevier, 3-36.

Lipton, M. (2013). Empiricism and Experience; Activism and Short-Termism; the Real World of Business. *Havard Law School Forum on Corporate Governance and Financial Regulation*. https://corpgov.law.harvard.edu/2013/10/28/empiricism-and-experience-activism-and-short-termism-the-real-world-of-business/. Accessed 8 Aug 2016.

Loughran, T., & Ritter, J. R. (2000). Uniformly least powerful tests of market efficiency. *Journal of Financial Economics*, 55(3), 361-389.

Loughran, T., & Vijh, A. M. (1997). Do long-term shareholders benefit from corporate acquisitions? *Journal of Finance*, 52(5), 1765-1790.

Electronic copy available at: https://ssrn.com/abstract=3260095

Lyon, J. D., Barber, B. M., & Tsai, C. L. (1999). Improved methods for tests of long-run abnormal stock returns. *Journal of Finance*, 54(1), 165-201.

Mitchell, M., & Stafford, E. (2000). Managerial decisions and long-term stock price performance. *Journal of Business*, 73(3), 287-329.

Moyer, L. (2017). Activist hedge funds target bigger and bigger US companies in year of the 'super campaign'. *CNBC*. https://www.cnbc.com/2017/08/09/activist-hedge-funds-target-big-companies-in-year-of-super-campaign.html. Accessed Apr 25 2018.

Nissim, D., & Penman, S. H. (2001). Ratio analysis and equity valuation: From research to practice. *Review of Accounting Studies*, 6(1), 109-154.

Orol, R. (2017). Senate to Debate Tax Reform Plan that Includes Hit for Activist Fund Managers. *The Street.com.* https://www.thestreet.com/story/14395963/1/latest-tax-plan-would-hit-activist-funds-hardest.html. Apr 25 2018.

Penman, S. H. (1991). An evaluation of accounting rate-of-return. *Journal of Accounting, Auditing & Finance*, 6(2), 233-255.

Piotroski, J. D. (2000). Value investing: The use of historical financial statement information to separate winners from losers. *Journal of Accounting Research*, 38, 1-41.

Reuters. (2016). Major Activist hedge funds just launched a D.C. lobby goup. *Fortune*. http://fortune.com/2016/05/19/activist-lobby-group. Accessed 25 Apr 2018.

Richardson, S., Teoh, S. H., & Wysocki, P. D. (2004). The walk-down to beatable analyst forecasts: The role of equity issuance and insider trading incentives. *Contemporary Accounting Research*, 21(4), 885-924.

Sorkin, A. R. (2015). Hillary Clinton Aim is to Thwart Quick Buck on Wall Street. *The New York Times*. https://www.nytimes.com/2015/07/28/business/dealbook/clinton-aim-is-to-thwart-quick-buck-on-wall-st.html. Accessed 25 Apr 2018.

Strine, L. E. (2017). Who bleeds when the wolves bite: A flesh-and-blood perspective on hedge fund activism and our strange corporate governance system. *Yale Law Journal*, 126, 1870-1970.

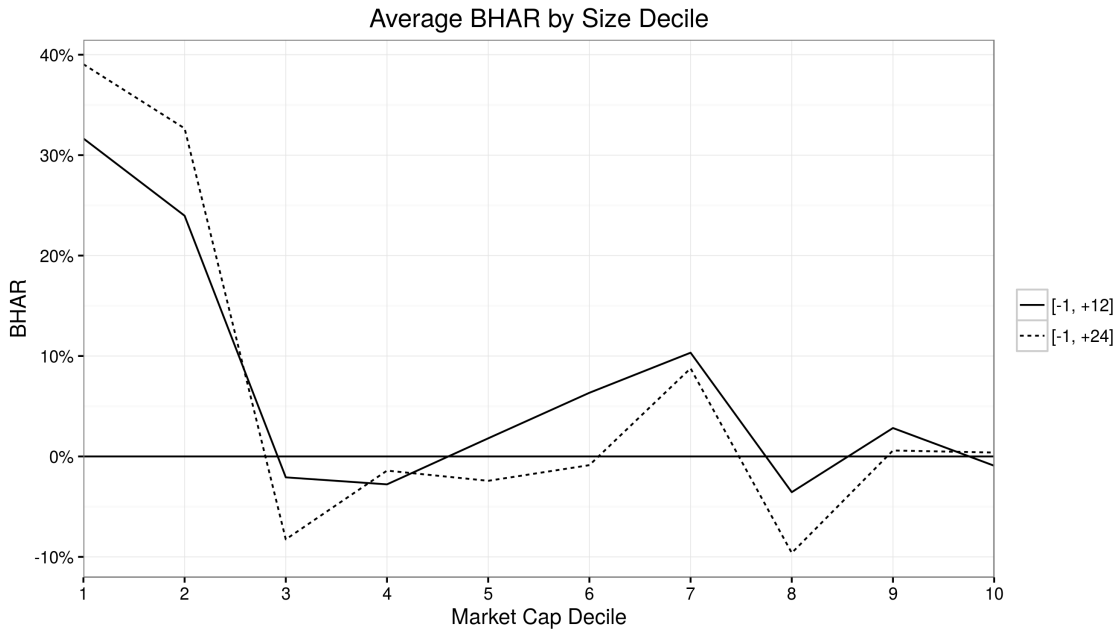
Stuart, E.A., (2010). Matching methods for causal inference: A review and a look forward. *Statistical Science: A Review Journal of the Institute of Mathematical Statistics*, 25(1), 1-21.

Swanson, E. P., & Young, G. (2016). Are Activist Investors Good or Bad for Business? Evidence from Capital Market Prices, Informed Traders, and Firm Fundamentals. Unpublished manuscript.

Electronic copy available at: https://ssrn.com/abstract=3260095

Figure 1 – Long-Run Returns to Activist Interventions, by Size Decile

This figure plots equal-weighted [-1, 12] and [-1, 24] monthly buy-and-hold abnormal returns for different size deciles, where deciles are calculated within the sample of target firms. The underlying data and significance tests are provided in Panels B and C of Table 3.

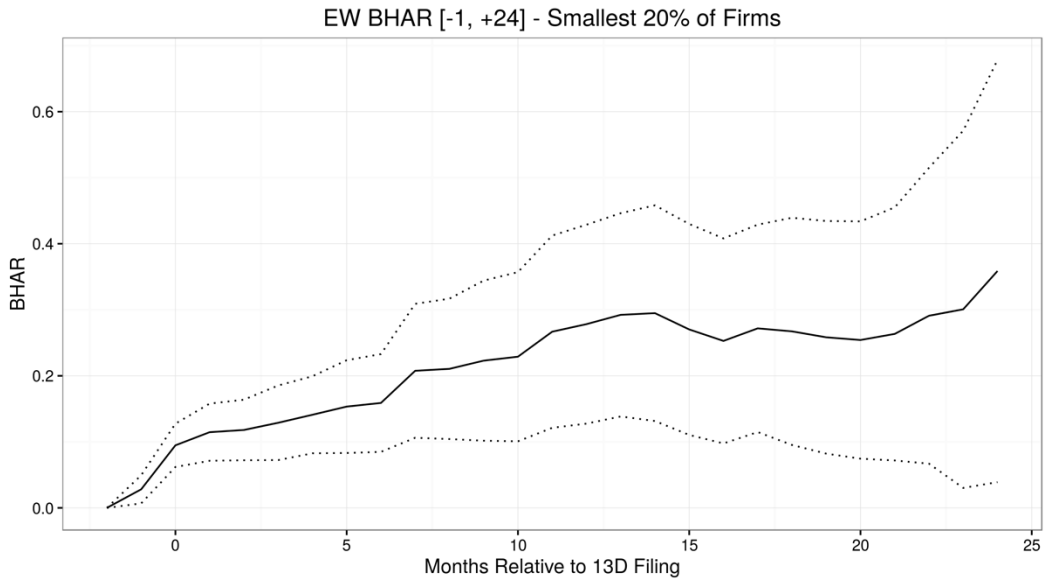


Electronic copy available at: https://ssrn.com/abstract=3260095

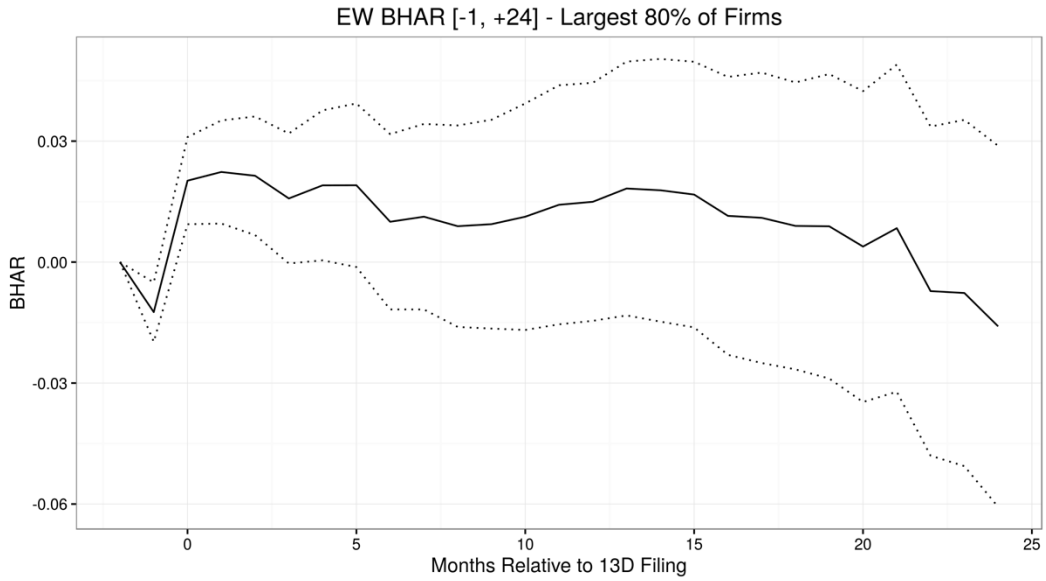
Figure 2 – Long-Run Returns to Activist Interventions for Small and Large Targets

This figure plots the evolution of equal-weighted buy-and-hold abnormal returns for small and large targets. Panel A (B) reports the monthly buy-and-hold returns for the smallest 20% (largest 80%) of targets. The dashed lines in these graphs are the 95% confidence intervals using a standard test statistic.

Panel A: Abnormal Returns for Smallest 20% of Targets



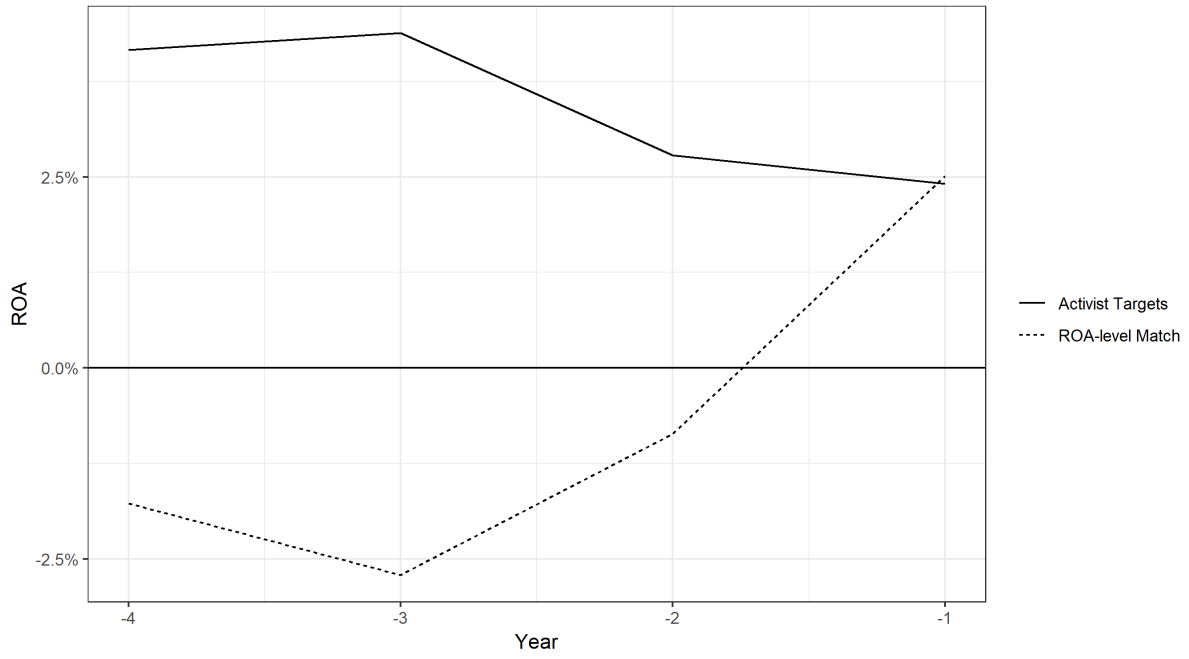
Panel B: Abnormal Returns for Largest 80% of Targets



Electronic copy available at: https://ssrn.com/abstract=3260095

Figure 3 – Pre-Activism Trends in ROA

The solid line plots the trend in ROA for activism target firms. The dotted line plots the trend in ROA for control firms matched on industry, year, and the level of ROA as of the year prior to the activist intervention. See section 4.2.1 and Table 4 for further details on matching. The vertical axis plots percentage ROA.



Electronic copy available at: https://ssrn.com/abstract=3260095

Table 1 - Sample of Activist Hedge Fund Targets

This table presents the sample selection of hedge fund targets from 1994 to 2011. Panel A presents the filters we use to create the final sample. Calculating portfolio-adjusted returns requires the firm to have valid return data, industry classification, and shares-outstanding data during the month of activism as well as at least six months of return data in the preceding 12 months. Panel B reports delisted and surviving firms. Surviving firms are those that remain in Compustat/CRSP for at least 24 months after the 13D filing. Delisted firms are split into two categories. Acquired firms are those with a CRSP delisting code of 2 (merger) or 3 (exchange). All other delisted firms are categorized as nonacquired delisted firms. Panel C details the non-exclusive subsamples for the surviving firms. *Assets Sales* (*High Payout*) are those observations in the tercile of firms with the largest percentage decrease (increase in levels) in total assets (payout, defined in Table 2) from *t-1* to year *t+2*, where *t* is the year of activism. *New CEO* is the set of firms that replace the CEO within two years of activism. *Board Turnover* are those firms that had an above-median percentage of turnover in the board in the two fiscal years following activism. *No Change* are those firms that are not in any of the other categories.

Panel A: Sample of Activist Targets

	Obs.
13D filings: 1994–2011	2,684
Remove duplicates and instances when multiple 13Ds filed on the same day	(27)
Observations without both a valid PERMNO and GVKEY	(329)
Observations without necessary data to calculate portfolio-adjusted returns	(250)
Keep first instance of activism in a fiscal year	(114)
Final sample of activist targets	**1,964**

Panel B: Surviving, Acquired, and Delisting Firms

	Obs.
Surviving in CRSP/Compustat at least 24 months post-activism ("surviving firms")	1,455
Delisted from CRSP due to merger or exchange within 24 months ("acquired firms")	380
Delisted from CRSP due to other reasons within 24 months ("delist firms")	129
Total activist targets	**1,964**

Panel C: Detail on Surviving Target Firm Outcomes (not mutually exclusive)

	Obs.
Survive: Asset Sales	440
Survive: New CEO	453
Survive: Board Turnover	449
Survive: High Payout	457
Survive: No Change	150

Electronic copy available at: https://ssrn.com/abstract=3260095

Table 2 – Summary Statistics

The upper rows in the table below report summary statistics for the variables used in calculating each firm's abnormal returns in relation to a portfolio of firms matched on market value, book-to-market, and momentum, all measured as of the month-end prior to the activist date. The lower rows report summary statistics for other variables, all measured as of the most recent year-end prior to the activist date. Columns (2) through (4) report summary statistics in levels, and columns (5) through (7) report summary statistics based on CRSP/Compustat percentiles. For the return summary statistics, these percentiles are calculated for a given month, whereas the remaining variables have percentiles calculated for a given fiscal year. *MV* is market value (in millions) calculated using CRSP data. *BTM* is the book value of common equity from the most recent filing that is publicly available, divided by market value. *Momentum* is the monthly momentum in the 12 months prior to targeting with a one-month reversal gap. *Assets* is the total assets. *ROA* is defined as operating income before depreciation scaled by current total assets. *ΔROA* is the change in *ROA* from *t-3* to *t-1*. *RNOA* is the return on net operating assets, which is defined as operating income before depreciation divided by the sum of net financial assets, as defined by Nissim and Penman (2001), and common equity, which is defined as book value of common equity plus treasury stock less preferred dividends in arrears. *ROE* is the return on equity and is defined as operating income before depreciation scaled by common equity. *PM* is profit margin, defined as operating income before depreciation scaled by revenue. *ATO* is asset turnover, defined as revenue scaled by net operating assets *Leverage* is defined as (book value of debt) / (book value of debt + book value of common equity + book value of preferred). *Cash* is cash and short-term investments scaled by total assets. *Payout* is (common dividends + purchase of common and preferred stock – net change in preferred stock outstanding) / total assets. *Sales Growth* is year-over-year sales growth scaled by assets. *HHI* is the within-firm Herfindahl-Hirschman index based on two-digit SIC codes for reported segments. In cases when no segments are reported, *HHI* is set to 1. *Number of Analysts* are the number of analysts that follow the firm and is set to 0 if missing. Except returns data, unbounded variables are winsorized at the 1% and 99% levels for each fiscal year.

		Levels			CRSP/Compustat Percentile		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	N	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.
Returns portfolio matching variables							
MV	1,964	791	136	3,159	0.41	0.37	0.25
BTM	1,964	0.80	0.66	1.53	0.55	0.60	0.29
Momentum	1,964	0.04	-0.08	1.11	0.41	0.34	0.28
Other variables							
Assets	1,963	1,436	269	6,657	0.43	0.41	0.25
ROA	1,958	0.02	0.08	0.36	0.47	0.46	0.28
ΔROA	1,912	-0.01	-0.01	0.23	0.47	0.44	0.30
RNOA	1,958	0.13	0.15	3.07	0.48	0.46	0.28
ROE	1,931	-0.38	0.09	3.85	0.43	0.40	0.27
PM	1,963	2.01	1.62	6.30	0.53	0.55	0.28
ATO	1,958	0.12	0.17	0.93	0.45	0.41	0.29
Leverage	1,963	0.33	0.27	0.69	0.47	0.46	0.30
Cash	1,963	0.21	0.11	0.24	0.52	0.54	0.29
Payout	1,963	0.02	0.00	0.08	0.46	0.42	0.28
Sales Growth	1,950	-0.01	0.03	1.38	0.47	0.48	0.29
HHI	1,964	0.69	1.00	0.40	0.49	0.61	0.25
Number of Analysts	1,964	4.10	2.00	5.16	0.45	0.44	0.26

Electronic copy available at: https://ssrn.com/abstract=3260095

Table 3: Returns Tests

This table details the short- and long-term market impacts of 1,964 activist interventions. The prefix "EW" ("VW") indicates equal-weighted (value-weighted) returns. Panel A reports EW buy-and-hold abnormal returns, calculated as the firm's return less the return of a matched portfolio of firms. The matched portfolio is based on 5 × 5 × 5 sorts on size, book-to-market, and momentum. Portfolios are formed using publicly available data in the month prior to activism. The first (second) row reports short-term abnormal returns (fraction of positive abnormal returns) calculated over days [-10, +10] around the activist announcement. The bottom four rows perform a similar analysis measured over months [-1, +12] (rows 3 and 4) and [-1, +24] (rows 5 and 6). Panel B reports the average market value and equal-weighted buy-and-hold returns for targets split into deciles by market value. Panel C reports the abnormal buy-and-hold returns for various holding periods following activism. Panel D, column (1), reports the short- and long-run VW returns. Column (2) performs similar analyses based on nominal changes in firms' market values, calculated as the buy-and-hold abnormal return multiplied by the firm's market value of equity calculated at day -11 (month -2) for short-term (long-term) returns. Panel E reports the average market value, EW, and VW buy-and-hold abnormal returns for different vintages of activism. Return significance is determined using the empirically derived bootstrap distribution with 1,000 pseudo-portfolios. *** indicates statistical significance at 1%, ** at 5%, and * at 10% (two-tail).

Panel A: Equal-Weighted (EW) Mean Abnormal Returns

		EW Mean Abnormal Return
Days [-10,+10]	EW Mean	0.054***
	% > 0	62%
Months [-1,+12]	EW Mean	0.068***
	% > 0	47%
Months [-1,+24]	EW Mean	0.059***
	% > 0	43%

Panel B: EW Abnormal Returns by Within-Sample Deciles of Market Value

	Within-Sample Decile of Market Capitalization										
Decile	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**Combo: 3 – 10**
Mean MVE	13.5	31.0	50.3	75.4	113.1	170.6	275.7	494.7	1036.0	5638.3	
EW Return											
Daily [-10, +10]	0.118***	0.068***	0.051***	0.037***	0.046***	0.065***	0.064***	0.029***	0.035***	0.027***	0.044***
Months [-1,+12]	0.317***	0.24***	-0.021	-0.028	0.018	0.063***	0.103***	-0.036*	0.028	-0.009	0.015
Months [-1,+24]	0.391***	0.327***	-0.083**	-0.014	-0.024	-0.009	0.088***	-0.096**	0.006	0.004	-0.016

Panel C: EW Buy-and-Hold Abnormal Returns by Month – Smallest 20% and Largest 80% of Targets

Month	**0**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8…**	**12**
EW Return										
Small 20% of Targest	0.095***	0.115***	0.118***	0.129***	0.141***	0.153***	0.159***	0.208***	0.211***	0.278***
Large 80% of Targets	0.020***	0.022***	0.021**	0.016	0.019	0.019	0.010	0.011	0.009	0.015

Electronic copy available at: https://ssrn.com/abstract=3260095

Panel D: Short-Term and Long-Term Value-Weighted (VW) Mean Abnormal Returns & Change in Market Value

		(1) VW Mean Abnormal Returns		(2) Abnormal Change in Market Value (in Millions)
Days [-10,+10]	VW Mean	0.024***	Mean	18.8***
Months [-1,+12]	VW Mean	0.018	Mean	3.4
Months [-1,+24]	VW Mean	0.019	Mean	13.2

Panel E: Buy-and-Hold Abnormal Returns by Vintage

Period	**1993–1999**	**2000–2006**	**2007–2011**
Mean MVE	332.8	935.4	921.2
Mean Return			
EW Daily [-10, +10]	0.046***	0.054***	0.032***
VW Daily [-10, +10]	0.020***	0.020***	0.024***
EW Months [-1,+12]	0.109***	0.072***	0.052**
EW Months [-1,+24]	0.169***	0.060**	0.056**
VW Months [-1, +12]	-0.044	0.018	0.028
VW Months [-1, +24]	-0.088*	0.049	0.014

Electronic copy available at: https://ssrn.com/abstract=3260095

Table 4 – Operating-Performance Tests – Matching Without Pre-Activism Trend in ROA

This table presents the difference in ROA between target and control firms. The target firms in this table are those that remain publicly traded for at least 24 months following activism and are matched to a set of control firms based on year, industry, and pre-activism level of ROA, as described in section 4.2.1. Panel A details the sample size. Panel B tabulates differences in covariates between our target and control firms. All covariates are defined in Table 2. Panel C tabulates future changes in ROA for the target firms as well as differences in changes, relative to the matched control firms. In Panel C, column 1 (2) reports the equal-weighted (value-weighted by assets in *t-1*) average, and column 3 reports the median. Significance for the difference in medians is based on Mood's median test, and all other tests are based on t-tests. *** indicates statistical significance at 1%, ** at 5%, and * at 10%.

Panel A: Target firm sample

Surviving in CRSP/Compustat at least 24 months post-activism ("surviving firms")	1,455
Less: Firms without data to calculate matching variables	(6)
Less: Firms without an adequate match	(23)
Surviving firms available for operating-performance tests	**1,426**

Panel B: Matched-firm summary statistics

	Mean			Median		
	Target Firm	**Matched Controls**	**Paired Difference**	**Target Firm**	**Matched Controls**	**Paired Difference**
Matching variables						
ROA	0.045	0.045	0.000	0.082	0.082	0.000
Other variables						
Assets	1,536	3744	-2,208***	288	1,201	-535***
ΔROA	-0.010	0.044	-0.055***	-0.006	0.005	-0.010***
MV	913	2272	-1,338***	161	755	-345***
BTM	0.761	1.719	-0.962***	0.608	0.777	-0.176***
Leverage	0.350	0.409	-0.059*	0.287	0.349	-0.030***
Cash	0.198	0.188	0.011**	0.095	0.140	-0.020***
Payout	0.025	0.030	-0.006**	0.001	0.014	-0.004***
Analysts	4.194	3.871	0.354***	2.000	3.500	-0.296***
Sales Growth	-0.014	0.055	-0.070*	0.033	0.066	-0.010***
HHI	0.666	0.717	-0.051***	1.000	0.712	0.000***

Electronic copy available at: https://ssrn.com/abstract=3260095

Panel C: Analysis of ROA

	N	EW Mean	VW Mean	Median
Pre-to-post change in ROA for target firms, relative to t-1				
$(\Delta ROA_{t+1})_{target}$	1,416	-0.011	0.000	-0.001
		(-1.37)	(0.15)	(-0.89)
$(\Delta ROA_{t+2})_{target}$	1,327	-0.014**	-0.001	-0.002
		(-2.09)	(-0.19)	(-1.22)
$(\Delta ROA_{t+3})_{target}$	1,217	-0.016	0.004*	-0.002
		(-1.58)	(1.76)	(-0.86)
$(\Delta ROA_{t+4})_{target}$	1,113	0.000	0.012***	0.000
		(-0.06)	(4.47)	(0.16)
$(\Delta ROA_{t+5})_{target}$	1,005	-0.020	-0.006	0.002
		(-1.23)	(-1.27)	(0.71)
Differences from matched control firm				
$(\Delta ROA_{t+1})_{target} - (\Delta ROA_{t+1})_{control}$	1,388	0.033***	0.016***	0.005***
		(3.16)	(4.75)	(3.14)
$(\Delta ROA_{t+2})_{target} - (\Delta ROA_{t+2})_{control}$	1,289	0.051***	0.019***	0.005***
		(3.92)	(4.15)	(3.10)
$(\Delta ROA_{t+3})_{target} - (\Delta ROA_{t+3})_{control}$	1,171	0.065***	0.022***	0.007***
		(3.81)	(4.37)	(2.82)
$(\Delta ROA_{t+4})_{target} - (\Delta ROA_{t+4})_{control}$	1,063	0.072***	0.029***	0.007***
		(4.53)	(4.56)	(2.81)
$(\Delta ROA_{t+5})_{target} - (\Delta ROA_{t+5})_{control}$	951	0.057***	0.036***	0.011***
		(2.66)	(6.73)	(3.86)

Electronic copy available at: https://ssrn.com/abstract=3260095

Table 5 – Operating-Performance Tests – Matching Using Pre-activism Trend in ROA

This table presents the difference in ROA between the target firms and control firms. The target firms in this table are those that remain publicly traded for at least 24 months following activism and are matched to a set of control firms matched on year, industry, size, and both the pre-activism level and trend in ROA, as described in section 4.2.2. Panel A details the sample size. Panel B tabulates differences in matching variables (upper rows), covariates between our target and control firms (middle rows), and other outcome variables (lower rows), all measured at fiscal year-end in the year prior to activism. All covariates are defined in Table 2. Panel C tabulates future changes in ROA for the target firms as well as differences in changes, relative to the matched control firms. Panel D reports the abnormal change in ROA multiplied by assets from *t-1*. Panel E reports the intercept from equation (2). Panel F performs a similar analysis as Panel C but requires control firms to remain publicly traded for two fiscal years. Panel G tabulates similar analyses based on subsamples of surviving firms based on ex-post outcomes. In Panels C, E, and F, column 1 (2) reports the equal-weighted (value-weighted, by assets in *t-1*) average, and column 3 reports the median. Significance for the difference in medians is based on Mood's median test, and all other tests are based on t-tests. *** indicates statistical significance at 1%, ** at 5%, and * at 10%.

Panel A: Target-firm sample

Surviving in CRSP/Compustat at least 24 months post-activism ("surviving firms")	1,455
Less: Firms without data to calculate matching variables	(41)
Less: Firms without an adequate match	(288)
Surviving firms available for operating performance tests	**1,126**

Panel B: Matched-firm summary statistics

	Mean			Median		
	Target Firm	**Matched Firm**	**Paired Difference**	**Target Firm**	**Matched Firm**	**Paired Difference**
Matching variables						
ΔROA	-0.009	-0.009	0.000	-0.006	-0.005	0.001
ROA	0.071	0.072	-0.001	0.086	0.089	0.000
Assets	1,815.8	1,798.6	17.2	364.1	352.9	3.8
Other variables						
MV	1047.3	1,216.8	-170.0	189.3	228.4	-3.6*
BTM	0.804	1.361	-0.558***	0.655	0.584	0.038***
Leverage	0.342	0.345	-0.003	0.311	0.285	0.001
Cash	0.171	0.176	-0.005	0.081	0.082	-0.001
Payout	0.024	0.021	0.002	0.002	0.002	0.000
Analysts	4.502	4.874	-0.372***	3.000	3.000	0.000
Sales Growth	0.027	0.052	-0.026*	0.033	0.045	-0.007*
HHI	0.634	0.667	-0.032**	1.000	1.000	0.000
Other outcome variables						
RNOA	0.208	0.140	0.068	0.159	0.162	-0.001
ROE	0.202	0.182	0.020	0.199	0.198	0.005
PM	-0.079	-0.174	0.100	0.106	0.118	-0.006*
ATO	0.948	0.919	0.030	0.842	0.794	0.006

Electronic copy available at: https://ssrn.com/abstract=3260095

Panel C: Analysis of ROA

	N	EW Mean	VW Mean	Median
Pre- to post-change in ROA for target firms, relative to t-1				
$(\Delta ROA_{t+1})_{target}$	1,119	-0.018**	0.002	0.000
		(-2.16)	(0.86)	(-0.32)
$(\Delta ROA_{t+2})_{target}$	1,052	-0.012***	0.001	-0.002
		(-2.72)	(0.24)	(-1.21)
$(\Delta ROA_{t+3})_{target}$	962	-0.011**	0.006**	-0.001
		(-2.16)	(2.26)	(-0.69)
$(\Delta ROA_{t+4})_{target}$	877	-0.004	0.012***	0.000
		(-0.96)	(4.45)	(0.09)
$(\Delta ROA_{t+5})_{target}$	794	-0.013*	-0.006	0.001
		(-1.65)	(-1.64)	(0.63)
Differences from matched control firm				
$(\Delta ROA_{t+1})_{target} - (\Delta ROA_{t+1})_{control}$	925	0.000	0.001	-0.001
		(-0.02)	(0.45)	(-0.52)
$(\Delta ROA_{t+2})_{target} - (\Delta ROA_{t+2})_{control}$	813	0.010	0.002	-0.003
		(1.04)	(0.65)	(-1.26)
$(\Delta ROA_{t+3})_{target} - (\Delta ROA_{t+3})_{control}$	702	0.004	0.003	-0.002
		(0.47)	(1.10)	(-0.99)
$(\Delta ROA_{t+4})_{target} - (\Delta ROA_{t+4})_{control}$	601	0.002	0.004	0.002
		(0.32)	(1.49)	(0.39)
$(\Delta ROA_{t+5})_{target} - (\Delta ROA_{t+5})_{control}$	514	0.005	0.004	0.002
		(0.43)	(1.38)	(0.59)

Panel D: Analysis of income effect: Abnormal ΔROA x Assets$_t$

	N	Mean	Median
Differences from matched control firm			
$Assets_t$ x $[(\Delta ROA_{t+1})_{target} - (\Delta ROA_{t+1})_{control}]$	925	2.8	-0.41
		(0.29)	(-0.88)
$Assets_t$ x $[(\Delta ROA_{t+2})_{target} - (\Delta ROA_{t+2})_{control}]$	813	4.2	-0.8
		(0.46)	(-1.44)
$Assets_t$ x $[(\Delta ROA_{t+3})_{target} - (\Delta ROA_{t+3})_{control}]$	702	7.5	-0.8
		(0.80)	(-0.98)
$Assets_t$ x $[(\Delta ROA_{t+4})_{target} - (\Delta ROA_{t+4})_{control}]$	601	9.8	0.3
		(0.93)	(0.30)
$Assets_t$ x $[(\Delta ROA_{t+5})_{target} - (\Delta ROA_{t+5})_{control}]$	514	11.2	0.5
		(0.85)	(0.55)

Electronic copy available at: https://ssrn.com/abstract=3260095

Panel E: Robustness test: ΔROA differences from matched control firm, with covariates

	N	EW Mean	VW Mean	Median
$(\Delta ROA_{t+1})_{target} - (\Delta ROA_{t+1})_{control}$	924	-0.001	0.002	-0.001
		(-0.08)	(0.58)	(-0.58)
$(\Delta ROA_{t+2})_{target} - (\Delta ROA_{t+2})_{control}$	812	0.010	0.002	-0.002
		(1.00)	(0.77)	(-0.73)
$(\Delta ROA_{t+3})_{target} - (\Delta ROA_{t+3})_{control}$	701	0.003	0.004	-0.002
		(0.32)	(1.39)	(-0.84)
$(\Delta ROA_{t+4})_{target} - (\Delta ROA_{t+4})_{control}$	600	0.002	0.006**	0.003
		(0.22)	(2.10)	(0.93)
$(\Delta ROA_{t+5})_{target} - (\Delta ROA_{t+5})_{control}$	513	0.002	0.006*	0.004
		(0.18)	(1.94)	(1.02)

Panel F: Robustness test: Analysis of ROA using a balanced set of surviving control firms

	N	EW Mean	VW Mean	Median
Differences from matched control firm				
$(\Delta ROA_{t+1})_{target} - (\Delta ROA_{t+1})_{control}$	1072	0.000	0.002	-0.001
		(-0.01)	(0.58)	(-0.42)
$(\Delta ROA_{t+2})_{target} - (\Delta ROA_{t+2})_{control}$	1004	0.007	0.000	-0.003
		(0.83)	(-0.14)	(-1.28)
$(\Delta ROA_{t+3})_{target} - (\Delta ROA_{t+3})_{control}$	853	0.005	0.003	-0.002
		(0.57)	(1.05)	(-1.02)
$(\Delta ROA_{t+4})_{target} - (\Delta ROA_{t+4})_{control}$	733	0.005	0.004	0.002
		(0.71)	(1.64)	(0.61)
$(\Delta ROA_{t+5})_{target} - (\Delta ROA_{t+5})_{control}$	625	0.005	0.004	0.000
		(0.49)	(1.52)	(0.00)

Panel G: Analysis of ROA: Subsamples of surviving firms

	EW Mean Changes				
Survive subsample	**Asset Sales**	**CEO Turnover**	**Board Turnover**	**High Payout**	**No Change**
N	267	304	303	296	101
$(\Delta ROA_{t+1})_{target} - (\Delta ROA_{t+1})_{control}$	-0.025	-0.007	-0.007	0.008	0.004
	(-0.62)	(0.77)	(-0.70)	(1.02)	(0.58)
$(\Delta ROA_{t+2})_{target} - (\Delta ROA_{t+2})_{control}$	0.004	-0.006	-0.003	0.008	-0.007
	(0.19)	(0.13)	(-0.30)	(0.88)	(-0.69)
$(\Delta ROA_{t+3})_{target} - (\Delta ROA_{t+3})_{control}$	0.006	-0.006	0.003	0.013	-0.005
	(0.22)	(-0.50)	(0.14)	(1.45)	(-0.31)
$(\Delta ROA_{t+4})_{target} - (\Delta ROA_{t+4})_{control}$	-0.002	0.002	-0.003	0.007	0.008
	(-0.12)	(-0.38)	(-0.17)	(0.58)	(0.38)
$(\Delta ROA_{t+5})_{target} - (\Delta ROA_{t+5})_{control}$	-0.003	0.000	0.020	0.017*	0.003
	(-0.08)	(-0.27)	(1.32)	(1.74)	(0.15)

Electronic copy available at: https://ssrn.com/abstract=3260095

Table 6 – Alternative Operating-Performance Tests

This table presents the difference in alternative operating-performance measures between the target firms that survive 24 months after targeting and control firms. Control firms are the same as in Table 5. Panel A reports the difference-in-difference in return in net operating assets (RNOA), as defined by Nissim and Penman (2001). Panel B reports the return to common equity (ROE), as defined by Nissim and Penman (2001). Panels C and D report profit margin and asset turnover, respectively. In these panels, column 1 (2) reports the equal-weighted (value-weighted) average, and column 3 reports the median. Operating performance is value-weighted by total assets from *t-1*. Significance for the difference in medians is based on Mood's median test, and all other tests are based on t-tests. *** indicates statistical significance at 1%, ** at 5%, and * at 10%.

Panel A: ΔRNOA differences from matched control firm

	N	EW Mean	VW Mean	Median
$(\Delta RNOA_{t+1})_{target} - (\Delta RNOA_{t+1})_{control}$	925	-0.005	0.014	0.000
		(-0.04)	(0.20)	(-0.05)
$(\Delta RNOA_{t+2})_{target} - (\Delta RNOA_{t+2})_{control}$	813	-0.057	-0.001	0.005
		(-0.46)	(-0.01)	(0.29)
$(\Delta RNOA_{t+3})_{target} - (\Delta RNOA_{t+3})_{control}$	702	-0.096	-0.102	-0.005
		(-0.69)	(-1.15)	(-0.54)
$(\Delta RNOA_{t+4})_{target} - (\Delta RNOA_{t+4})_{control}$	601	-0.089	-0.031	0.020**
		(-0.58)	(-0.43)	(2.17)
$(\Delta RNOA_{t+5})_{target} - (\Delta RNOA_{t+5})_{control}$	514	0.049	-0.010	0.006
		(0.31)	(-0.14)	(0.39)

Panel B: ΔROE differences from matched control firm

	N	EW Mean	VW Mean	Median
$(\Delta ROE_{t+1})_{target} - (\Delta ROE_{t+1})_{control}$	925	-0.058	-0.088*	-0.012*
		(-0.94)	(-1.86)	(-1.77)
$(\Delta ROE_{t+2})_{target} - (\Delta ROE_{t+2})_{control}$	813	-0.043	0.012	-0.011
		(-0.71)	(0.27)	(-1.06)
$(\Delta ROE_{t+3})_{target} - (\Delta ROE_{t+3})_{control}$	702	-0.026	0.050	-0.017**
		(-0.39)	(0.95)	(-2.04)
$(\Delta ROE_{t+4})_{target} - (\Delta ROE_{t+4})_{control}$	601	0.049	0.242***	-0.011
		(0.64)	(3.43)	(-0.96)
$(\Delta ROE_{t+5})_{target} - (\Delta ROE_{t+5})_{control}$	514	0.113	0.152***	0.005
		(1.40)	(2.92)	(0.41)

Panel C: ΔPM differences from matched control firm

	N	EW Mean	VW Mean	Median
$(\Delta PM_{t+1})_{target} - (\Delta PM_{t+1})_{control}$	913	0.003	0.038	-0.005*
		(0.02)	(0.33)	(-1.66)
$(\Delta PM_{t+2})_{target} - (\Delta PM_{t+2})_{control}$	800	0.213	0.034	-0.007
		(0.78)	(0.25)	(-1.60)
$(\Delta PM_{t+3})_{target} - (\Delta PM_{t+3})_{control}$	688	0.316**	0.092	-0.004
		(2.00)	(0.83)	(-0.91)
$(\Delta PM_{t+4})_{target} - (\Delta PM_{t+4})_{control}$	589	0.593**	0.144	0.000
		(2.00)	(1.00)	(0.05)
$(\Delta PM_{t+5})_{target} - (\Delta PM_{t+5})_{control}$	503	0.265	0.140	-0.003
		(0.99)	(0.98)	(-0.80)

Electronic copy available at: https://ssrn.com/abstract=3260095

Panel D: ΔATO differences from matched control firm

	N	EW Mean	VW Mean	Median
$(\Delta ATO_{t+1})_{target} - (\Delta ATO_{t+1})_{control}$	928	-0.156	0.037	0.040
		(-0.47)	(0.21)	(1.36)
$(\Delta ATO_{t+2})_{target} - (\Delta ATO_{t+2})_{control}$	814	0.336	0.073	0.058*
		(1.00)	(0.37)	(1.75)
$(\Delta ATO_{t+3})_{target} - (\Delta ATO_{t+3})_{control}$	707	-0.531	-0.354	0.109**
		(-1.53)	(-1.62)	(2.31)
$(\Delta ATO_{t+4})_{target} - (\Delta ATO_{t+4})_{control}$	604	-0.026	-0.128	0.099*
		(-0.08)	(-0.60)	(1.73)
$(\Delta ATO_{t+5})_{target} - (\Delta ATO_{t+5})_{control}$	518	-0.350	-0.023	0.007
		(-0.89)	(-0.09)	(0.08)

Electronic copy available at: https://ssrn.com/abstract=3260095

Table 7 – Changes in Analyst Forecasts around Activism

Panel A (Panel B) presents the pre-activism (post-activism) consensus EPS forecasts around 13D filing, and Panel C reports the difference. To construct the pre-activism forecast sample, we use the most recent forecast for each analyst but require the forecast to be issued or reconfirmed within 180 days prior to the 13D filing. Post-activism consensus is based on the first forecast for each analyst issued within 30 days following the 13D filing or the last forecast prior to the 13D filing if no updates are issued. Columns (1)–(3) report statistics for forecasts around the 13D filing. Columns (4)–(6) report forecasts around pseudo-activism dates that are defined as the date one year prior to activism. Columns (7)–(9) report the paired difference between actual and pseudo activism. All data are scaled by the prior fiscal year-end stock price. Test statistics are in parentheses. Paired median tests are based on Mood's median test, and all other tests are based on t-tests. *** indicates statistical significance at 1%, ** at 5%, and * at 10%.

	Activism Dates			**Control Dates**			**Difference-in-Differences**		
	(1) N	(2) EW Mean	(3) Median	(4) N	(5) EW Mean	(6) Median	(7) N	(8) EW Mean	(9) Median
Panel A: Pre-activism									
t	1,082	0.010*	0.044	1,035	0.014**	0.052			
t+1	871	0.035	0.057	824	0.044	0.064			
t+2	301	0.045	0.067	276	0.057	0.069			
Panel B: Post-activism									
t	1,068	0.008	0.042	1,023	0.013**	0.050			
t+1	919	0.034	0.056	876	0.043	0.063			
t+2	330	0.049	0.065	304	0.060	0.072			
Panel C: Paired difference									
t	1,040	-0.005***	0.000	994	-0.006***	0.000	994	0.000	-0.001**
		(-6.61)	(1.36)		(-4.55)	(0.78)		(-0.13)	(5.30)
t+1	858	-0.002***	0.000	811	-0.004***	0.000	811	0.005*	0.001
		(-2.68)	(0.32)		(-3.53)	(0.12)		(1.81)	(1.22)
t+2	277	-0.003	0.000	254	-0.006**	0.000	254	0.007	0.002
		(-1.35)	(0.04)		(-2.27)	(0.45)		(1.46)	(2.50)

Electronic copy available at: https://ssrn.com/abstract=3260095

Table 8: Returns Tests – By Acquired, Delist, and Survive

This table details the returns to activist interventions based on ex post categorizations of firm outcomes. Buy-and-hold abnormal returns are calculated as the firm's return less the return of a matched portfolio of firms. Columns (1)–(3) present differences in returns between firms that are acquired and not acquired. Columns (4)–(6) evaluate difference in returns between nonacquired firms that delist versus those that survive. The upper rows report short-term returns calculated over days [-10, +10] around the activist announcement. The bottom rows report long-term returns starting before the intervention, over months [-1 +12] and [-1, +24]. Return significance is determined using the empirically derived bootstrap distribution with 1,000 pseudo-portfolios. *** indicates statistical significance at 1%, ** at 5%, and * at 10% (two-tail).

		Acquired versus Nonacquired			Nonacquired: Delist versus Survive		
		(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
		Acquired	Nonacquired	Diff. (1) – (2)	Nonacquired: Delist	Nonacquired: Survive	Diff. (4) – (5)
	N	380	1,584		129	1,455	
Short-term							
[-10,+10]	EW Mean	0.076***	0.049***	0.027**	0.073***	0.047***	0.026
	VW Mean	0.052***	0.018**	0.034**	-0.021***	0.018***	-0.040
	% > 0	69%	61%		55%	61%	
Long-term							
[-1,+12]	EW Mean	0.245***	0.025	0.220***	-0.506***	0.072***	-0.578***
	VW Mean	0.182***	-0.016	0.198***	-0.596***	-0.009	-0.587***
	% > 0	66%	42%		15%	45%	
[-1,+24]	EW Mean	0.254***	0.012	0.241***	-0.830***	0.087***	-0.917***
	VW Mean	0.264**	-0.032	0.295***	-0.797***	-0.023	-0.774***
	% > 0	65%	37%		11%	40%	

Electronic copy available at: https://ssrn.com/abstract=3260095

Table 9: Returns Tests – By Surviving-Firm Outcomes

This table details the short- and long-term returns to activist interventions based on ex-post categorizations of firm outcomes for those that are not acquired and do not delist. *Asset Sales* in column (1) are targets with the highest decrease in assets for all targeted firms between *t-1* to *t+2*. *New CEO* in column (2) are instances when a new CEO was appointed within two fiscal years of activism, according to Equilar and ExecuComp. *Board Turnover* (*High Payout*) in column (3) ((4)) are those firms in the highest tercile in percentage change in board seats (change in dividends and share buybacks) during the two fiscal years after the 13D filing. Column (5) contains firms that do not fall into any of the aforementioned categories. The upper rows report short-term returns calculated over days [-10, +10] to the activist announcement. The bottom rows report long-term returns starting before the intervention, over months [-1, +12] and [-1, +24]. Return significance is determined using the empirically derived bootstrap distribution with 1,000 pseudo-portfolios. *** indicates statistical significance at 1%, ** at 5%, and * at 10% (two-tail).

		(1) Survive: Asset Sales	**(2)** Survive: New CEO	**(3)** Survive: Board Turnover	**(4)** Survive: High Payout	**(5)** Survive: No Change
	N	440	453	449	457	150
Short-term						
[-10,+10]	EW Mean	0.049***	0.045***	0.037***	0.048***	0.037***
	VW Mean	0.012	0.009	0.013*	0.022***	0.058***
	% > 0	61%	59%	59%	58%	61%
Long-term						
[-1,+12]	EW Mean	-0.143***	-0.020	0.015	0.122***	0.144***
	VW Mean	-0.157***	-0.051*	0.019	0.025	0.222***
	% > 0	45%	32%	42%	44%	53%
[-1,+24]	EW Mean	-0.261***	-0.015	0.006	0.160***	0.201***
	VW Mean	-0.18***	-0.038	0.06	0.104*	0.036
	% > 0	40%	24%	37%	40%	48%

Electronic copy available at: https://ssrn.com/abstract=3260095

european corporate governance institute

about ECGI

The European Corporate Governance Institute has been established to improve *corporate governance through fostering independent scientific research and related activities.*

The ECGI will produce and disseminate high quality research while remaining close to the concerns and interests of corporate, financial and public policy makers. It will draw on the expertise of scholars from numerous countries and bring together a critical mass of expertise and interest to bear on this important subject.

The views expressed in this working paper are those of the authors, not those of the ECGI or its members.

www.ecgi.org

Electronic copy available at: https://ssrn.com/abstract=3260095

european corporate governance institute

About ECGI

ECGI is an international non-profit membership organisation that provides a platform for debate and dialogue among academics, policymakers, and business leaders, with a focus on major corporate governance, ESG, and stewardship issues.

The views expressed in this working paper are those of the authors, not those of the ECGI or its members.

www.ecgi.global

european corporate governance institute

ECGI Working Paper Series in Finance

Editorial Board

Editor	**Nadya Malenko,** Professor of Finance, Boston College
Consulting Editors	**Renée Adams,** Professor of Finance, University of Oxford
	Franklin Allen, Professor of Finance and Economics, Imperial London
	Julian Franks, Professor of Finance, London Business School
	Mireia Giné, Professor of Finance, IESE Business School
	Marco Pagano, Professor of Economics, Facoltà di Economia Università di Napoli Federico II
Series Manager	**Asif Malik**, ECGI Working Paper Series Manager

https://www.ecgi.global/publications/working-papers