The Effects of Hedge Fund Activism

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Abstract

In this paper I explore the relationship between the rise of hedge fund activism and firm outcomes, using a study design that explicitly takes into account how activists pick their targets. Contrary to much prior work, I find no evidence that activism is associated with increased firm operating performance or significant long-term returns once comparing to firms based on their similarity to the targets. However, activism does increase firm payouts to shareholders and decreases investment, consistent with the argument of many critics of activism. I also find that firm-level employment declines significantly following a targeting event, and that the subset of firms that experience an increase in operating performance also engage in higher levels of tax avoidance. The deregulation of proxy access rules, wholesale de-staggering of corporate boards, and the rise in importance of proxy advisory firms who frequently recommend voting for activist proposals have made firms more susceptible to aggressive activism over the past three decades. The results in this paper, coupled with the rhetorical shift in focus from short-term profits to sustainable growth by large institutional investors, suggest a re-framing of the public debate over the benefits of shareholder activism.

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1 Introduction

Activist campaigns conducted by hedge fund institutions have risen in size and frequency over the past three decades. While some argue that activists play a vital supervisory role in the American corporate governance architecture, others view them as new-age corporate raiders, financial "vultures" turning a profit by inducing managers to lay off workers in a hunt for short-term profits at the expense of long-term growth. In the midst of this disagreement, a large literature developed attempting to measure the economic impact of activism on firm outcomes. This article adds a new methodological approach to tease out the effect of activism by explicitly taking into consideration the targeting decision of activists.

A seemingly consistent finding in the activism literature is that there are short-term increases in share price following Schedule 13D filings by hedge fund activists. Section 13(d) of the 1934 Securities Exchange Act mandates that beneficial owners of more than 5% of a publicly traded security, with the intention to influence corporate control, disclose their ownership and intent within ten days of crossing the ownership threshold (Brav, Jiang, and Kim, 2009). Multiple studies explore the short term returns for targeted firms around such disclosures, typically finding price responses in the order of 3-7%. It is unclear, however, whether these short-run price responses are indicative of a change in firm performance, or rather of a myopic response to higher shareholder payouts and takeover likelihood.

As a result, corporate governance research turned to the connection between activism and subsequent changes in operating performance for targeted firms. Here, the literature finds conflicting results, in part driven by different modeling assumptions. In addition, those studies that find increased operating performance following activist events generally report response estimates that display marked differences for the pre-activism trends in outcomes for targeted and control firms. I argue that this result is similar to the labor economics literature on the "Ashenfelter dip", where participants in job training programs have systematically lower labor outcomes before program take-up. I investigate how that literature addresses the selection issue, and the lessons that researchers in corporate governance can draw from the experience.

I apply a modern robust treatment effect model to a large sample of activism events over the period from 1994 to 2016. I rely on industry knowledge for the activist selection decision, as well as research in accounting and finance to generate a large set of potentially confounding variables for both the selection and outcome equations in the analysis. I then apply a data-driven, off-the-shelf machine learning algorithm to generate activism prediction estimates to test the effects of activism. This methodology builds off a burgeoning literature in economics and statistics applying high-dimensional data techniques to causal questions with doubly-robust estimation techniques (Chernozhukov, Chetverikov, Demirer, Duflo, Hansen, Newey, and Robins, 2018; Athey, Imbens, and Wager, 2018; Sant'Anna and Zhao, 2020; Kennedy, Ma, McHugh, and Small, 2017).

The results from this analysis cast doubt on hedge fund activism having any causal effect—either positive or negative—on firm profitability or operating performance in the five year period following an activist event. In addition, once matching to a representative benchmark of comparison firms, there is little evidence for any positive average long-term stock return accruing to activist targets, and activism may in fact actually decrease the five-year return for the typical firm. However, my results are consistent with the claim that hedge fund activism pushes firms into takeovers, while increasing short-term payouts to shareholders and decreasing firm investment. I also provide evidence of significant decreases in employment levels following activist events, even for firms that remain independent, and that the subset of firms that do increase profitability also engage in higher levels of corporate tax avoidance. These results call into question whether the rise in unfettered markets for corporate control has been an desirable trend in corporate governance.

2 Institutional and Legal Setting

The first documented hedge fund was created in 1949 by Alfred Winslow Jones, a sociologist and journalist who established an investment partnership that reduced investment risk by buying and shorting stocks in the same industry (Partnoy, 2015). The hedge fund industry has since grown into a sizeable and influential portion of the investment management community: recent estimates of total managed assets are $2.5-3 trillion with over four thousand unique single-manager hedge funds (Getmansky, Lee, and Lo, 2015). According to Partnoy and Thomas (2007), hedge funds are generally categorized as i) pooled and privately organized investment vehicles, ii) that are administered by professional managers who invest heavily in the fund and have their compensation tied to performance, iii) are not widely available to the investing public, and iv) which operate outside the purview of most securities regulation and registration provisions. While initially formed as traditional long/short equity vehicles, hedge funds eventually moved into more specialized areas of investing, including activism.

Shareholder activism as a practice arose in the mid-1980s, contemporaneous with the increase in institutional asset holding, particularly funds designed to mirror stock index returns. The rise in tracking funds generated a diversification problem, as investment managers who previously would have sold shares in underperforming firms were now incapable of exiting their positions, and were forced to engage in concerted efforts to improve firm performance (Denes, Karpoff, and McWilliams, 2017). However, the early iteration of institutional shareholder activism proved largely ineffective due to regulatory and structural barriers, collective action problems, conflicts of interest from mutual funds who viewed targets as future fund management clients, and legal diversification requirements and insider trading regulations (Black, 1990).

Alongside the growth in index investing, a series of regulatory, legal, and institutional developments decreased the effective cost of launching an activism campaign. Changes in interpretive guidance by the Securities and Exchange Commission and the Department of Labor required large institutional investors to vote the shares of their portfolios companies in situations where they previously might have abstained. Given that many large mutual funds offload the decision on voting matters to proxy advisory services, who have historically tended to support activism, this raised the expected vote share for activists undertaking proxy contests. The spirited effort to remove staggered boards at public corporations also decreased the time it takes to replace a majority of the board of directors, often an explicit aim of activist investors. Finally, the rise of the more effective "wolf pack" campaign, where different activists work in parallel to purchase shares of target firms while remaining below the statutory reporting requirement, was aided by rulings that narrowed when investors could be deemed to have formed a "group" for 13(d) reporting purposes (Coffee and

Palia, 2016).

Hedge fund activists took advantage of these developments to fill the unmet demand for more effective managerial oversight. The unique attributes of hedge funds that make them appealing investment vehicles for high net worth individuals also in theory remove the conflicts of interest that previously inhibited institutional investors from acting as zealous monitors. In particular, hedge fund managers are compensated with performance fees tied directly to the return on their investments, creating a stronger financial incentive to make profits than a mutual or pension fund manager (Brav, Jiang, Partnoy, and Thomas, 2008). In addition, by largely raising money from wealthy individuals and large institutions, hedge funds are not subject to extensive regulation or heightened fiduciary standards (Brav et al., 2009), and their ability to lock up investor capital and aggressively use leverage and options increases their incentive to actively monitor firm management (Clifford, 2008). As a result, some claim that hedge funds occupy an important middle ground between passive investors and the corporate raiders of the 1980s, placing hedge funds "in a potentially unique position to reduce the agency costs associated with the separation of ownership and control" (Brav et al., 2008).

Others argue, however, that hedge fund activism represents a unique and pernicious challenge to our corporate governance infrastructure. The most common critique of the hedge fund industry has been that short holding periods combined with strong management engagement leads to myopic decision-making at the executive level, making them what Kahan and Rock (2007) have termed the "archetypal short-term investor", and that this short-termism "presents the potentially most important, most controversial, most ambiguous, and most complex problem associated with hedge fund activism." Coffee and Palia (2016) share the short-termism concern, pointing out that hedge fund engagements appear to cause substantial reductions in long-term investment for both target and non-target firms. Unlike past activist investors who brought with them subject matter expertise, hedge funds focus their initiatives on financial measures, notably increasing leverage and shareholder payouts while decreasing investments in research and development ("R&D"), rather than improving operational functions.

The activism debate has spread beyond the pages of academic journals, and is now a common topic of discussion for regulators, managers, and journalists. As noted in deHaan, Larcker, and McClure (2019), in 2017 the *The Wall Street Journal* published more than an article a day mentioning activism. A recent feature in the *New Yorker* by Sheelah Kolhatkar chronicled the activist campaign of Paul Singer's Elliott Management Corporation targeting AthenaHealth, which contained colorful allegations of activist pressure techniques, including the use of incriminating social media pictures and unsubstantiated allegations pulled from divorce proceedings (Kolhatkar, 2018). Legislatively, the Senate considered a bipartisan proposal to increase the regulatory requirements of activist hedge funds through higher transparency and disclosure requirements (Whyte, 2017). In the midst of this continued debate, the frequency of hedge fund activism has generally increased, although is lower than its pre-crisis peak, as documented in Figure 1.

Fig. 1. Activism Events Over Time

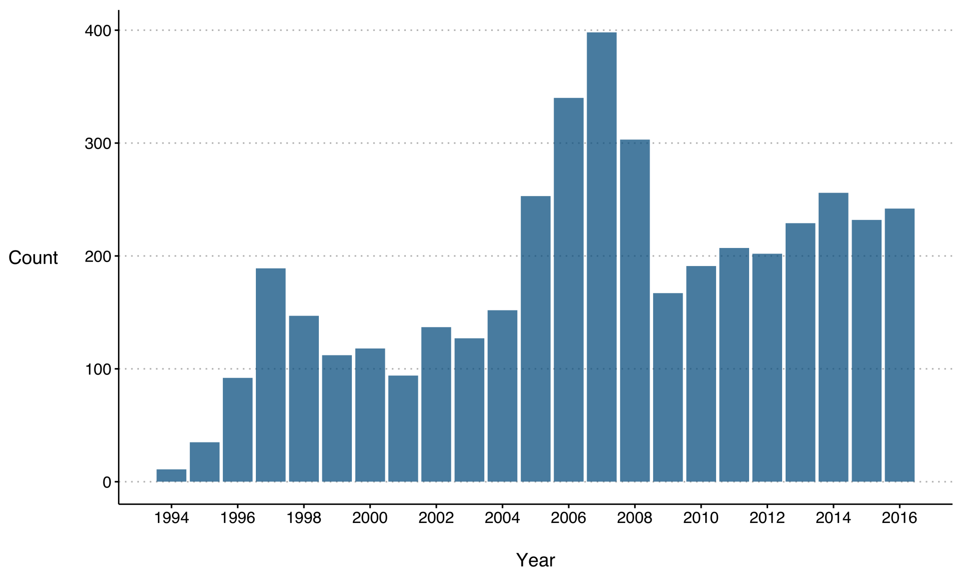


Figure 1 reports the count of unique activism events per year using the activism dataset maintained by Alon Brav and co-authors. Duplicate entries are removed at the firm/filing date level.

3 Literature Review

The causes and consequences of hedge fund activism have been explored in a now-extensive literature, with a particular focus on the link between activism events and shareholder wealth. Clifford (2008) analyzes 13D beneficial ownership filings for 197 unique hedge fund families between 1998 and 2005, finding that activist-targeted firms receive large positive excess returns at the time of purchase when benchmarked against firms with passive blockholdings. Targeted firms were also found to experience positive increases in operating efficiency in the following year, driven by reductions in operating assets rather than an increase in cash flow. These results were interpreted as evidence that activist hedge funds generate positive wealth creation, in contrast with prior work that found inconclusive effects from pension and mutual fund activism.

Brav et al. (2008) use a hand-collected sample of all 13D filings from 2001 to 2006 by activist hedge funds in the United States that proposed strategic, operational, and financial remedies to test whether hedge funds are able to influence corporate boards and management. They find that hedge funds target "value" firms with low market-to-book ratios, but which are profitable, with sound operating cash flows and return on assets. Markets react positively to activism, with abnormal returns averaging 7-8% in the forty days surrounding the filing, and these returns do not reverse over time. Finally, the authors argue that the largest positive excess returns are driven by activism targeting the sale of the company or changes in business strategy, and that activism is associated with improvements in return on assets and operating profit margins.

Two recent survey reviews—Brav et al. (2009) and Denes et al. (2017)—have taken stock of the evidence for hedge fund activism and short- and long-run changes in shareholder wealth and firm performance. According to Brav et al. (2009), the weight of the evidence supports the argument that i) hedge funds are more likely to target firms with sound operating cash flows, as well as low sales growth rates, leverage, and dividend payout rates; ii) the short term pricing effect at announcement is typically in the order of 5-10%, with little evidence of post-event reversion; and iii) there exists heterogeneity in the perceived increase in firm value, with most of the effect concentrated in events targeting firm sale or line-of-business change, and little detectable effect for funds seeking capital structure or corporate-governance related changes.

Denes et al. (2017) synthesize the results of 73 studies examining the consequence of shareholder activism for targeted firms. They find that activism using tactics similar to corporate takeovers is associated with improvements in share values and firm operations, while activism disconnected from the formation of ownership blocks is associated with insignificant or very small changes in target firm value. Moreover, shareholder activism has become more value-increasing over time; there is little detectable effect from activism in the 1980s and 1990s, while activism in recent years is associated with significant positive improvements. The authors believe this evidence supports theoretical findings from Alchian and Demsetz (1972) that "agency problems in modern corporations are controlled in part by a dynamic and sometimes transient coalescence of ownership and share votes to discipline managers and change corporate policy."

Other studies arrive at different conclusions. deHaan et al. (2019) (DLM) demonstrate that the positive impact of activist hedge fund interventions on long-term stock returns is contingent on using equal-weighted rather than value-weighted return portfolios. Given that the largest 20% of public firms comprise 91% of total market value, using equal-weighted returns distorts the true value-effects of activism. DLM find that positive equal-weighted long-term returns are primarily driven by the smallest 20% of targets, and that value-weighted returns are positive in the short-term but insignificantly different from zero within three months of activism. Additionally, tests of post-activism changes in operating performance have typically ignored the "stochastic evolution of accounting metrics," which in turn biases the comparison between target firms and matched control samples. After controlling for the preceding trend in operating performance in the matching process, DLM find an insignificant effect of activism on operating performance.

Cremers, Giambona, Sepe, and Wang (2018) and Cremers, Masconale, and Sepe (2016) also argue that the matching procedures typically used to test the impact of activism on share price and long-term performance suffer from selection effects. Managers of similarly situated non-targeted firms likely take various actions to rectify under-performance, and after matching to comparison firms with similar characteristics (in particular similar valuations in the period prior to activist campaigns) the prior positive findings disappear. Greenwood and Schor (2009) argue that the positive excess returns around hedge fund activism is almost entirely explained by the ability of activists to induce takeovers of target firms, and that announcement and long-term abnormal returns are not statistically significantly different from zero for firms that remain independent.

Other papers explore the ramifications of hedge fund activism above and beyond the direct effect on shareholders and firm performance. Klein and Zur (2011) show that hedge fund activism significantly reduces bondholders' wealth, suggesting that the positive share price response to activism is a result of expropriation of wealth from bondholders to stockholders. Cheng, Huang, Li, and Stanfield (2012) examine the relationship between hedge fund activism and corporate tax avoidance, finding that firms targeted by activists exhibit lower tax avoidance prior to the intervention, but experience significant increases in avoidance in the post-activism period relative to matched control firms. The target firms do not, however, engage in more tax sheltering post-intervention, suggesting an improvement in "target firms' tax efficiencies." Aslan and Kumar (2016) test the product market spillover effects of hedge fund activism on the industry rivals of target firms. They find that activism is associated with negative real stockholder wealth effects on the average rival firm, and that the negative effects on rivals' product market performance are roughly equivalent to the post-activism improvements in target firm productivity, cost and capital allocation efficiency, and product differentiation.

Finally, some recent studies explore the impact of hedge fund activism on different and more granular measures of firm productivity. Brav, Jiang, Ma, and Tian (2018) study the impact of activism on corporate innovation, finding that targeted firms improve their innovation efficiency, as measured by patent counts and citations, over the intervening five-year period despite a tightening in research and development expenditures. Brav, Jiang, and Kim (2015) study the long-term effect of activism on firm productivity using plant-level data from the U.S. Census Bureau. They find that targeted firms improve their production efficiency in the three years post-intervention, with the largest effects present in business strategy-oriented interventions. In addition, while activism appears to increase plant-level allocative efficiency, employees of targeted firms experience stagnation in work hours and wages despite an increase in their labor productivity.

4 Changes in Activism Over Time

4.1 Size and Industry Composition

As evidenced in Figure 1, considerable variation exists in the frequency of activist events over time. While the number of unique events is still below the pre-financial crisis peak, anecdotal and empirical evidence suggests that the impact of activism on corporate America has, if anything, increased with time. Market commentators note that activists now frequently target "large or mega-cap companies", and even firms whose stocks are performing well are no longer immune from an activist campaign (Bryan, 2016). Academic research has also demonstrated that, in a later activist period (2008-2014), the "activism industry indeed has become larger and more dispersed ... with both more participants and more targets", and that the most successful activist investors increasingly target larger firms (Krishnan, Partnoy, and Thomas, 2016). Given these developments in the landscape of activist investing, in this section I explore how the characteristics of targeted firms, and the short-term price response to activism, have changed over our sample period.

Figure 2 presents the total and average size of targeted firms by year, measured by both enterprise value (debt, equity, and cash and short term investments), and market capitalization terms (just equity). Consistent with the anecdotal evidence, there has been a sustained increase in both the aggregate and relative size of firms pursued in activist campaigns. While the simple count of activist events has remained lower than the pre-crisis level, the substantial increase in the average size of targeted firms has caused the dollar value of firm capitalization at risk from an activist campaign to be substantially higher in later years than at any preceding period.

Fig. 2. Change in Activism Target Size Over Time

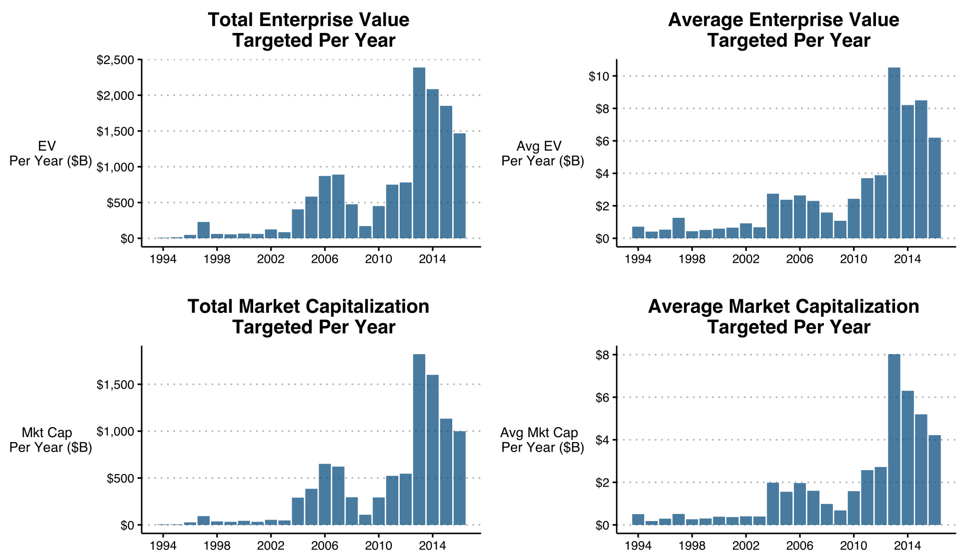


Figure 2 reports the total and average enterprise value and market capitalization of activist targets by calendar year. The market capitalization values come from CRSP, using the value from the last trading date before the announcement of the activism event. If that value is missing the first value of i) the most recent trading date within two months before the activism announcement, or ii) the earliest trading date after the announcement but before the annual filing date if the activism filing event is before the fiscal filing date is used. If the filing is after the last available fiscal filing date, then the most recent trading date before the filing date is used. Finally, if none of these are available the market capitalization value from Compustat (**prcc_f** * **csho**) is used. Enterprise value is defined as the sum of firm market capitalization, long-term (**dltt**) and short-term debt (**dlc**), and cash and short-term investments (**che**). The market capitalization value is used in place of enterprise value for all financial firms (SIC code between 6000 and 6999) because their debt values are not directly comparable.

In addition to trends in campaign counts and firm size, I explore how the industry composition of targets has changed over time. Here I compare the relative distribution of firms within the Fama-French 12 industry categories, splitting the activism sample into four, roughly even periods. The Fama-French 12 industries are defined in Table 1.

Table 1. Fama-French 12 Industry Categorizations

#	Abbrev	Industries
1	NoDur	Consumer Nondurables – Food, Tobacco, Textiles, Apparel, Leather, Toys
2	Durbl	Consumer Durables – Cars, TVs, Furniture, Household Appliances
3	Manuf	Manufacturing – Machinery, Trucks, Planes, Off Furn, Paper, Com Printing
4	Enrgy	Oil, Gas, and Coal Extraction and Products
5	Chems	Chemicals and Allied Products
6	BusEq	Business Equipment – Computers, Software, and Electronic Equipment
7	Telcm	Telephone and Television Transmission
8	Utils	Utilities
9	Shops	Wholesale, Retail, and Some Services (Laundries, Repair Shops)
10	Hlth	Healthcare, Medical Equipment, and Drugs
11	Money	Finance
12	Other	Other – Mines, Constr, BldMt, Trans, Hotels, Bus Serv, Entertainment

Table 1 presents the industry classification for the Fama-French 12 Industries from Ken French's website: https://mba.tuck.dartmouth.edu/pages/faculty/ken.french/Data_Library/det_12_ind_port.html.

In contrast to the pronounced change in the size of firms targeted over time, there is more consistency in the industry composition of targeted firms. However, one observable change is an increased focus on the targeting of firms in the business equipment category, which includes technology firms that produce computers, software, and electronic equipment. In the most recent period (2012-2016), these firms made up a quarter of all activist targets by number. At the same time there has been a small shift away from activism against firms in the nondurable and manufacturing sectors.

Fig. 3. Change in Industry Composition of Targets Over Time

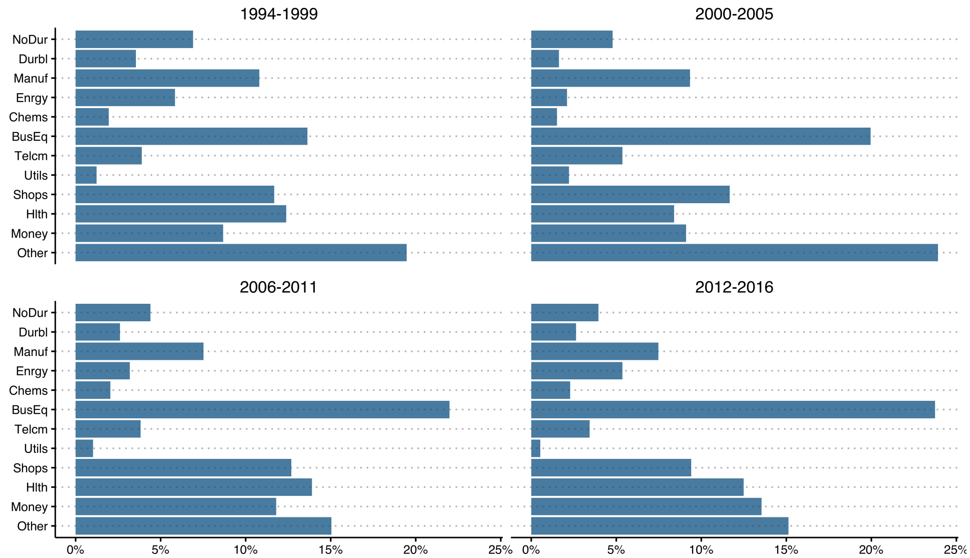


Figure 3 reports the percentage of activist targets in each Fama-French 12 industry classification over four time periods within our activism sample.

4.2 Short-Term Stock Returns

A seemingly consistent result in the academic literature on hedge fund activism is the presence of short-term positive abnormal returns around SEC Form 13D filings by activist hedge funds. As noted by Denes et al. (2017), "the evidence regarding hedge fund activism and contested proposals is consistent and robust across studies", with reported average abnormal returns (the the change in stock price of targeted firms that cannot be explained by the overall changes in the market trends) for U.S. firms averaging 3-7% depending on the sample used. This short-term overperformance is also similar to findings for hedge fund activism in the U.K., Japan, and Germany. However, as shown in deHaan et al. (2019), tests of abnormal returns differ depending on whether the analyst uses an equal-weighted or value-weighted aggregation. In addition, given the changes over time in target size, average abnormal returns could mask substantial differences in the short term abnormal returns.

To test the consistency of average short-term abnormal returns, I calculate the average buy-and-hold abnormal returns (**BHAR**) over the full sample of activism events from 1994 to 2016. Following prior literature, I focus on the BHAR over the 41-day period surrounding the announcement of the activism event through the Form 13D filing (Bebchuk, Brav, and Jiang, 2015). I require that each targeted firm has full trading data for the 41 trading dates in the abnormal return window, and calculate average returns using both an equal-weighted and value-weighted approach. For each security the unadjusted buy-and-hold return for the date $t \in \{-20, 20\}$ relative to the event is calculated as:

$$\text{BHR}_{i,t} = \prod_{t=-20}^{t} (1 + r_{i,t}) - 1 \tag{1}$$

where $r_{i,t}$ is the return for security i on relative date t. The corresponding buy and hold return for the market index over the same time period is calculated as:

$$\text{BHR Index}_{i,t} = \prod_{t=-20}^{t} (1 + I_{i,t}) - 1 \tag{2}$$

where $I_{i,t}$ is the return on the CRSP value-weighted market return index. The equal-weighted BHAR, which can be thought of as the accrued difference in financial return for investors from investing in targeted firms rather than investing in the overall market, is simply the average difference between the two buy-and-hold return series, while the value-weighted BHAR weights the differences by the market capitalization of the firm as of twenty days prior to the Form 13D filing.

Fig. 4. Short-Term Buy and Hold Abnormal Returns

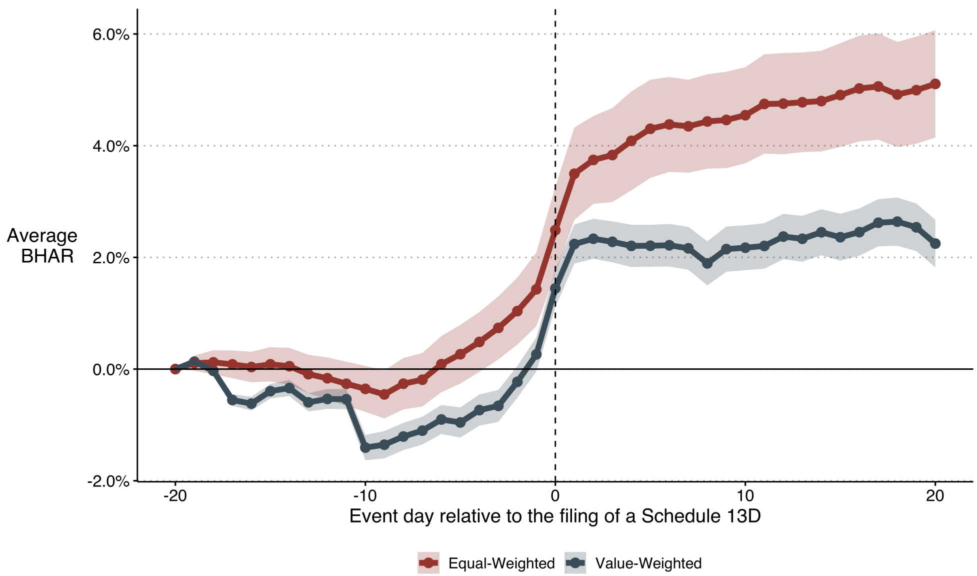


Figure 4 reports the average short-term BHAR across the sample of activist events from 1994 to 2016. The sample for the figure includes each targeted firm's security that has a complete return series over the twenty days prior to and following the trading date closest to the Form 13D filing date. The equal-weighted line tracks the simple average over the return series, while the value-weighted line weights by the market capitalization of each security twenty days prior to the activist event.

The equal-weighted and value-weighted short-term BHAR results for the full sample of activism events that meet the sample restriction are presented in Figure 4. Consistent with prior literature, there are positive abnormal profits to investing in targeted firms around the activist event. However, the buy-and-hold abnormal returns are substantially larger on an equal-weighted basis, given that aggregate returns are driven in large measure by the returns for the smallest targeted firms (deHaan et al., 2019). Over the full sample, the value-weighted short-run BHAR is on the order of two percent.

Figure 5 separates the short-run BHAR estimates into approximately equal samples by time period. Consistent with the time variation in target characteristics, there are substantial differences in the average abnormal returns over time. The positive BHAR over the entire period is driven by the returns in the first half of the sample, from 1994 to 2005. On an equal-weighted basis, there was sizeable short-term abnormal performance for targeted firms in the beginning of the 2000s, while the

BHAR is under five percent in recent years. In terms of market capitalization weighted BHAR, the returns are consistently below the equal-weighted average, with evidence of large positive abnormal returns only in 2000-2005, approximately the same period analyzed in Brav et al. (2008).

Fig. 5. Short-Term Buy and Hold Abnormal Returns By Period

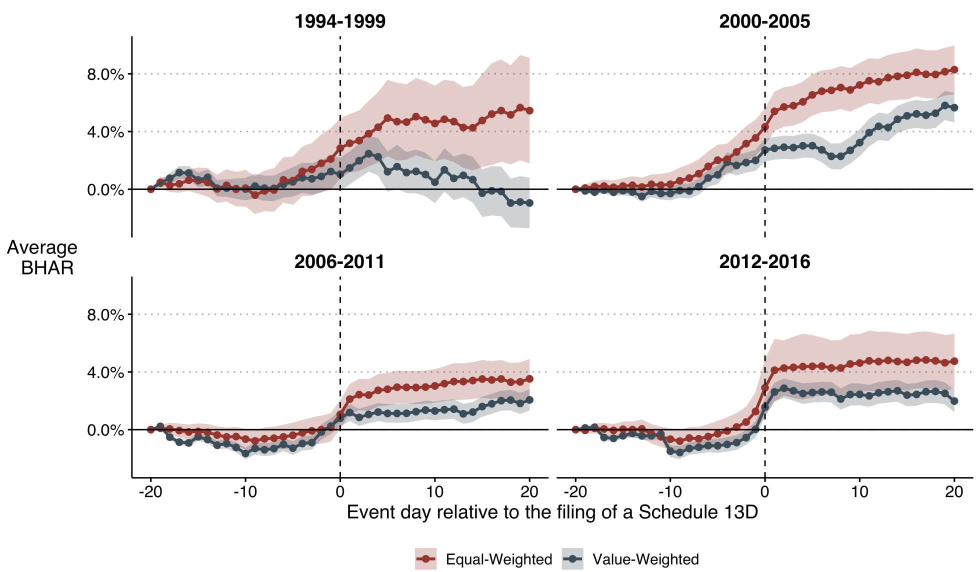


Figure 5 reports the equal- and value-weighted short-term BHAR broken down by year range of the Form 13D filing date.

Finally, I decompose the BHAR estimates into whether the firm is a takeover target. A prominent criticism of hedge fund activism is that it generates value for shareholders only through short-term increases in shareholder disbursements and an increased likelihood of takeover, rather than through sustained increases in firm profitability (Strine Jr., 2017). Greenwood and Schor (2009) document that from 1993 to 2006 the positive abnormal returns to hedge fund activism were largely explained by the ability of activists to force targets into a takeover. Using the larger activism sample, I define takeover targets in a similar manner—whether the target firm was ultimately the subject of a takeover within two years following the Form 13D filing.[1]

[1] Takeovers are identified by whether the associated security had a delisting code in CRSP beginning with either 2 or 3.

Fig. 6. Short-Term Buy and Hold Abnormal Returns
By Take-Over Type

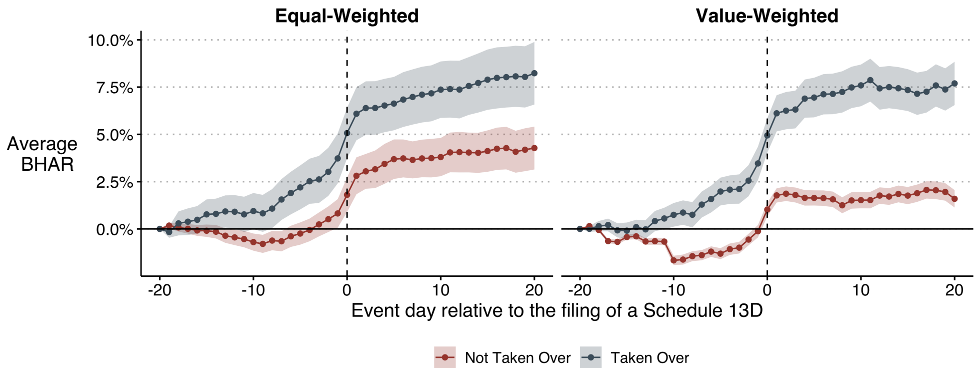


Figure 6 breaks the equal- and value-weighted short term BHAR into separate components for whether the targeted firm is taken over within the next two years.

Figure 6 shows that over the full sample the positive BHAR is in large measure driven by takeover targets, and that on a value-weighted basis there is evidence for short-term positive abnormal returns only in the range of around 2% for firms that remain independent. Figure 7 breaks down the differences between takeover and non-takeover firms by BHAR aggregation type and time period. There is again consistent evidence that, in the latter periods especially, the abnormal returns are driven almost entirely by takeover targets. For the last ten years of our activism sample the value-weighted returns to activism for non-takeover targets are not much larger than zero.

Fig. 7. Short-Term Buy and Hold Abnormal Returns
By Take-Over Type and Time Period

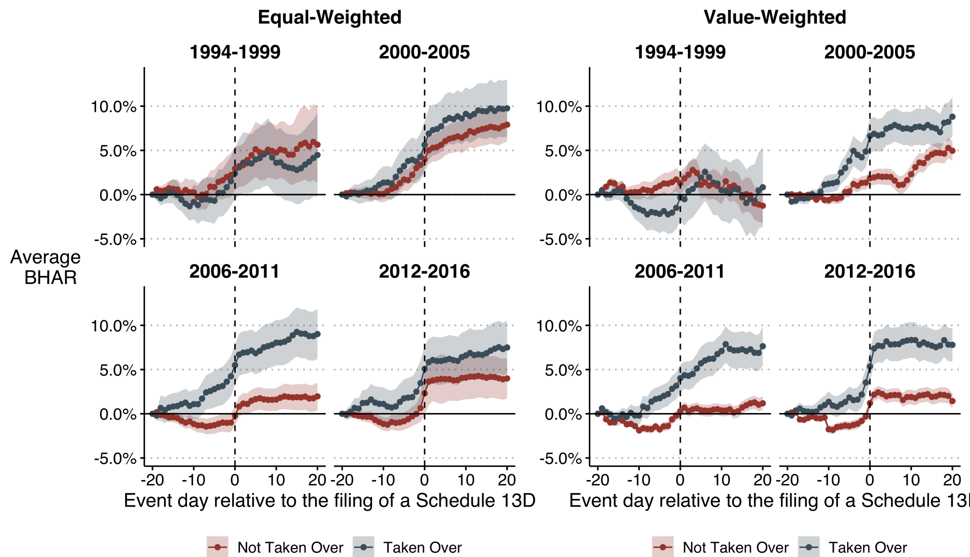


Figure 7 breaks the short-term BHAR into separate components by year range of the Form 13D filing date, whether the firm is a takeover target (defined as being subsequently taken over within the following two years), and whether the returns are calculated on an equal- or value-weighted basis.

5 Replication and Extension of Work on Operating Performance

Policymakers, commentators, and academics disagree about the lessons to draw from the short-term price increases following hedge fund activist events. As demonstrated above, the positive abnormal stock performance is largely driven by firms that are ultimately taken over by a competitor. This raised the ire of many governance professionals, including then-Chief Justice of the Delaware Supreme Court Leo Strine,[2] who argued that activist hedge funds generate returns by forcing target companies to sell themselves off at a purchase premium, inflated through employment reductions and slashed wages, noting that "human investors care not just about whether corporations make money, but also about how" (Strine Jr., 2017).

[2]The Delaware Supreme Court is the court of final review for all questions of corporate law involving Delaware-incorporated firms. As of the most recent data, more than two-thirds of the Fortune 500 are incorporated in Delaware, making the opinions of their justices particularly significant.

Others also allege that activists are "short-term opportunists ... detrimental to long-term value creation," who "push for actions that are profitable in the short term but are detrimental to the long-term interests of companies and their long-term shareholders" (See Bebchuk et al. (2015) for examples of such allegations). The investor-myopia argument culminated in a clarion call from the legendary corporate defense attorney Martin Lipton, who in a widely circulated memorandum challenged supporters of hedge fund activism to demonstrate for targeted firms "the impact on their operational performance and stock-price performance relative to the benchmark, not just in the short period after announcement of the activist interest, but after a 24-month period" (Lipton, 2013).

As a result, researchers have focused their energy on testing the relation between hedge fund activism and short- and medium-term increases in operational performance. There has been substantially less agreement on this issue than in the discussion of short-term returns. In this section I replicate and extend the analysis of four prominent papers that analyze the impact of activism on operating performance to our full sample of activism events—Brav et al. (2008, 2015); Cremers et al. (2018); deHaan et al. (2019). I hew closely to the analysis from the papers, but modify the methods on the margin to make them more directly comparable. The precise details for how each paper implements their method is reported in Appendix A.

I replicate each analysis on the full sample of activism events, with firm profitability, as measured by return on assets, as the dependent variable using the CRSP/Compustat merged dataset from fiscal years 1989 to 2020, and restricting to firms headquartered in the U.S. I remove duplicate firm-year activist filings, and require there to be at least five years between activist events at a given firm, to ensure that we're identifying unbiased relative-time effects. For the regression-based approaches—Brav et al. (2008) (BBJ) and Cremers et al. (2018) (CGSW)—the natural logarithms of market value and firm age are used as controls,[3] and relative-year indicators are included rather than binned time periods.

[3] Cremers et al. (2018) use a broader set of controls in their paper, however they also use the controls as outcome variables in other tests and find significant effects, suggesting that post-treatment bias would be likely if including them as controls.

Fig. 8. Short-Term Buy and Hold Abnormal Returns
By Take-Over Type and Time Period

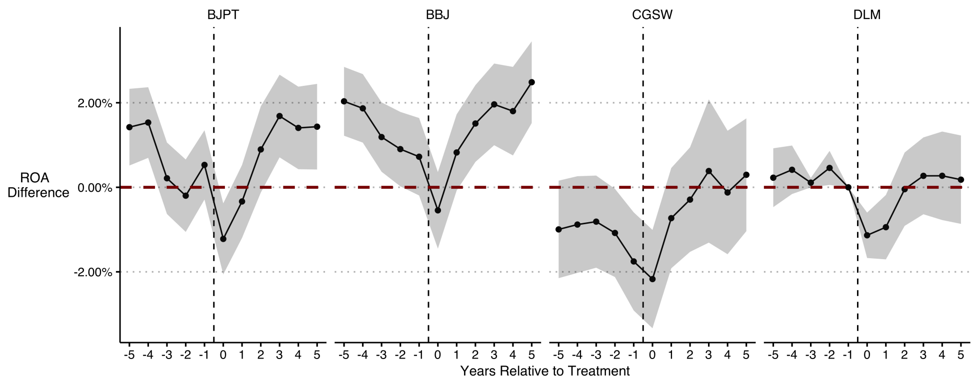


Figure 8 reports the estimated effect of activism on return-on-assets (roa) from five years before to five years after the activism event, using the methods from Brav et al. (2008) (BJPT), Bebchuk et al. (2015) (BBJ), Cremers et al. (2018) (CGSW) and deHaan et al. (2019) (DLM) extended to the full sample of activism events.

The results are presented in Figure 8. Different modeling assumptions lead to large differences in the estimated change in operating performance around activism events. In addition, in most models there is a pronounced V-shaped pattern to the estimated effects around the filing of activism event, indicative of systematic differences between the targeted and comparison units, and the only set of estimates with similar adjusted levels of return on assets between types of firms are those from deHaan et al. (2019). However, because that comparison matches firms directly on the pre-treatment level and trend of the outcome variable, a reasonable pre-treatment fit is ensured.

6 Why to Model Selection into Activism

The V-shaped pattern around Form 13D filings documented across outcomes in the literature on the effects of hedge fund activism[4] casts doubt on the ability of the simple matching and regression-based estimators to produce credible estimates of the impact of hedge fund activism on corporate outcomes. This common pattern in outcome trends is the corporate finance analogue

[4]In addition to the references above, see also Brav et al. (2015) which studies the impact of hedge fund activism on plant-level productivity, and also documents the same striking V-shaped pattern around activism announcement.

to the "Ashenfelter's dip", which is a prevalent feature in the labor economics literature on job training programs. In an influential paper, Ashenfelter (1978) points out a serious limitation in using difference-in-differences type estimators to compare labor market outcomes of participants and non-participants in governmental post-schooling training programs. The Ashenfelter paper noted that program participants consistently exhibit a decline in mean earnings in the period prior to program entry, a finding that has been replicated across numerous studies. Whether the pre-treatment dip in outcomes is permanent or temporary ultimately determines the proper counterfactual comparison to be made (Heckman and Smith, 1999).

There is now a long and robust literature on ways to conduct program evaluation in the presence of pre-treatment changes for treated units. Heckman, Ichimura, and Todd (1997) propose a difference-in-differences estimator of the average treatment effect on the treated (ATT) which is based on conditional identification restrictions. The estimator matches differences in pre-treatment and post-treatment outcomes for the treated to weighted averages of differences in pre-treatment and post-treatment outcomes for the untreated. The differences are matched on the propensity score—or the probability of treatment exposure conditional on covariates—and they non-parametrically determine the weights using local-linear regression. Abadie (2005) proposes a modification to the procedure that uses a simple two-step procedure to adjust for compositional differences between treated and controls that cause non-parallel dynamics in the outcome variable.

Recent work has extended the regression and propensity score weighting approaches of Heckman et al. (1997) and Abadie (2005) to more robustly adjust for differences in observed variables between treated and control units. In practice, researchers often have a substantial number of variables, and may not know the specific functional form for how covariates enter either the propensity score or the outcome model. With the rise of larger and more granular datasets, there has been considerable recent interest in adapting methods from the prior program evaluation literature to higher dimensional settings. Notable work in this area include Chernozhukov et al. (2018); Belloni, Chernozhukov, and Hansen (2013); Belloni, Chernozhukov, Fernandez-Val, and Hansen (2017); Athey et al. (2018). While the methods differ in terms of the estimation strategy and the assumptions for identification, they can all be viewed through the lens of the partially linear regression model of Robinson (1988), which is explained in more detail in Appendix B.

7 Robust Methods for Hedge Fund Activism

Given the observed potential for selection issues driving the rebound in outcome variables following activism events, I use a modern robust treatment effect estimator to jointly estimate the relationships between the activism targeting decision and the underlying firm outcome dynamics. The estimator incorporates a simple machine learning model for the targeting decision that uses penalized regression to predict treatment from a large set of covariates mentioned as drivers of the firm selection process in activism. If correctly specified, the model can remove the residual confounding bias that still remains after the simple matching or regression adjustment done in prior literature.

7.1 Data

I use the universe of firms in the CRSP/Compustat merged file as the basis for the firm data sample, and the activist events maintained by Alon Brav and co-authors, which are extended through calendar year 2016. In order for an activist event to enter the sample, I require that there is a matching Compustat identifier (**gvkey**) that can be linked to the CRSP identifier (**permno**) in the activism dataset. In addition, I winsorize all variables used in the analysis at the 1% level, by year, to mitigate against the influence of outliers common in financial databases. I focus on the time-series dynamics of a set of firm performance variables across the treatment period: measures of operating performance (return on assets, gross margin, and operating margin) as well as other outcomes alleged to be impacted by activism (the payout ratio, firm investment, and firm leverage).

To generate the potential confounding variables for both the treatment and outcome equations, I rely on industry reports and prior academic literature. While past studies of activism have controlled for a small number of researcher-selected variables, there has been little justification for the inclusion or exclusion of relevant controls. In addition, the advantage of modern machine learning techniques is that they allow for a much larger set of potential confounds. The sources for the dependent variables and covariates come from commonly available financial datasets, including CRSP (stock return data), Compustat (firm financial reporting information), MSCI and ISS (governance variables), Execucomp (executive compensation data), I/B/E/S (analyst following information), and Thomson Reuters (Form 13F financial holdings and insider sales data).

For the variables used to predict activism, I rely on a report from the Conference Board's *Director Notes* series, entitled "How Activist Investors Identify Their Targets", written by Damien Park, the managing partner of a consulting and activist investing research firm (Park, 2006). The key firm-level features that are ostensibly associated with the targeting decision include screening variables used to identify undervalued stocks (e.g. total shareholder return, the price-to-book ratio, market capitalization range), measures of the information environment (e.g. analyst following), and corporate structure and governance provisions (e.g. state of incorporation, board size, and characteristics of the shareholder base), among others. For purposes of internal brevity, a list of these control variables and their construction is provided in Appendix C. I rely on prior academic work on the relation between our performance measures and other firm characteristics for the variables that enter into the outcome equations.[5]

7.2 Method

I use a modified version of the doubly robust difference-in-differences estimator from Sant'Anna and Zhao (2020), adapted to the panel nature of the firm-year data using the aggregation framework in Callaway and Sant'Anna (2020). Because I have a much larger set of potential control variables in this analysis than prior literature, I risk having a poorly unrepresentative sample of observations if I impose the standard no-missing-data restriction on the sample. Recent research has shown that causal inference methods are robust to missing data using imputation techniques, in particular when

[5]For return on assets, I use the control variables from Anderson and Reeb (2003), which include the ratio of R&D to sales, long term debt to total assets, return volatility, firm size, and firm age. For gross margin and operating margin, I refer to Lester (2019), which uses firm leverage, size, the book-to-market ratio, and return on assets. The firm leverage model uses the control variables from Bae, Kang, and Wang (2011), which are the percentage of long-term debt to enterprise value and assets, the book-to-market ratio, firm revenue, the ratio of fixed assets to total assets, return on assets, R&D to sales, SG&A to sales, and whether the firm pays dividends. I refer to Bens, Nagar, Skinner, and Wong (2003) for the payout ratio—including firm size, change in firm log sales, the book-to-market ratio, operating cash flows scaled by sales, and current period stock returns. Finally, for firm investment I use the control variables from Balakrishnan, Core, and Verdi (2014), which are cash flow scaled by assets, Tobin's q, firm size, age, and leverage.

the methods used are doubly-robust (Mayer, Sverdrup, Gauss, Moyer, Wager, and Josse, 2020). To avoid unrepresentative samples, I use multiple imputation across the set of control variables in our treatment prediction models, requiring that any potential variable has coverage for at least half of the observations in the treatment-year prediction model.[6] For each activism year in the sample, I create five separate imputed datasets, the resulting estimates from which are ultimately pooled using "Rubin's Rule" (Rubin, 1987).[7]

I proceed with the following steps to calculate average treatment effects on the treated (ATTs) for every activism-year/relative time period combination for each of the outcome variables. First, for every individual treatment year in the data (from 1994 to 2016), I create a sample of the prior five years of data for all treated firms and all potential control firms. I generate these rolling-window samples in order to allow the activism selection model to change over time, consistent with the summary evidence above documenting changes in the landscape for activism targets.[8] Within our overall sample, the percentage of firms being targeted by an activist in any given year is generally very low, on the order of 2-3%. While this type of class imbalance is typical in real-world datasets, it is known to cause serious problems for classification algorithms. As a remedy, I use the Synthetic Minority Over-sampling Technique (SMOTE) from Chawla, Bowyer, Hall, and Kegelmeyer (2002), which generates synthetic observations for the targeted firms using values interpolated among the nearest neighbors based on covariate values.

In Appendix D, I detail the precise mechanics of how the robust estimator is used on this sample to calculate relative-time ATTs for activism across different outcome variables; however, the intuition behind the estimator is straightforward. We want to generate estimates of the impact of activism on the outcome variable for each year with events, which can later be aggregated to a higher level (e.g. the effect in relative or calendar time across events). However, unlike with the standard difference-in-differences approach, the selection into activism makes a comparison of means across all treated and untreated firms inappropriate.

[6]I also require that any firm-year observations has at least 50% coverage for the resulting variables to be included in the model.

[7]In untabulated results I confirm that the treatment effect estimates across the imputed datasets are similar.

[8]Because the activism sample begins in 1996, for the first five years (1996-2000), we estimate the propensity scores in sample rather than with a rolling window.

Here, instead of a simple difference, we weight the comparison firms by their "propensity score", a measure of how similar the characteristics of the firm are to the those of of the targeted firms in a given year. Thus, potential comparison firms that more closely resemble the targeted firms *before* the year in which the activism event occurred are given higher weight in the comparison average. In addition, for each year $t \in \{-4, +5\}$ relative to the activism year, we model the change in the outcome variable from year t to the reference year, using control variables specific to each outcome measure from the reference year. A simple linear model of this relationship on the comparison firm observations are used to predict the values for the targeted firms, in order to remove the changes over time that would have occurred even without activism. Thus, we remove from the treatment effect estimate the portion of the change in the outcome variable driven by other confounding variables, as well as re-weighting the control sample to ensure the comparison of the adjusted differences is made between similarly situated firms.

8 Results

8.1 The Activism Targeting Process

Before reporting treatment effect estimates, I provide insight into which factors appear to empirically drive the activist targeting decision. Given that lasso-based penalized regression is used in the propensity score models,[9] I can determine which variables are most correlated with the decision for activists to target firms in our sample, and how it has evolved over time.[10] To present one measure of variable importance, I calculate the maximum value of λ in the penalization sequence for which the variables are not dropped from the model (Barber and Candés, 2015). λ is the penalty weight assigned by the model to the inclusion of a variable in the objective function, so higher levels of λ will increase the probability that the model assigns a zero coefficient to a variable. As a result, higher maximum levels of λ with non-zero coefficients mean that those

[9] The details for how this is implemented are presented in Appendix D.

[10] Lasso, or the use of the L1 norm, is frequently used for variable selection because it tends to place more mass for estimated coefficients at zero.

variables contribute more to minimizing prediction error in the sample.[11]

I take the average over model-rank by time period, and report the top ten most-important variables from the propensity models in Table 2. In general the combination of variables that seem most capable of predicting activism include measures of the information environment (e.g. the number of analysts following a firm), firm performance (in particular total shareholder return over different time period ranges), payout practices, firm size, return volatility, and measures of business complexity (e.g. the number of reported geographic business units). There is perhaps more stability in these estimates over time than would be expected *a priori*, although there does appear to be a shift in focus to characteristics of the shareholder base and business complexity in later periods.

Table 2. Variable Importance From Propensity Models

Rank	1994-1999	2000-2005	2006-2011	2012-2016
1	Dividend Dummy	# Analysts	Dividend Dummy	Dividend Dummy
2	# Analysts	5 Yr TSR	DE Comp	GEO Sector HHI
3	DE Comp	Return Volatility	# Analysts	3 Yr TSR
4	Any Insider Purchases	Bottom Decile Mkt Cap	1 Yr TSR	Mkt Cap Decile
5	Bottom Decile Mkt Cap	Firm Size	Mkt Cap Decile	Bottom Decile Mkt Cap
6	Sales Growth	3 Yr TSR	Transient SH %	No Analyst Following
7	Payout / Assets	Mkt Cap Decile	Return Volatility	Return Volatility
8	Long Term Debt to Equity	1 Yr TSR	Short Int %	Transient SH %
9	3 Yr TSR	Long Term Debt to Equity	Bottom Decile Mkt Cap	Tobin's Q
10	Intangible Assets Total Assets	Transient SH %	5 Yr TSR	Dedicated SH %

Table 2 presents the top ten variables in feature importance for each period based on the maximum value of λ for which the variable is not given zero weight in the logit-lasso regression. The rank is calculated by first averaging the maximum value of λ across imputed datasets, and then averaging the yearly ranks by variable.

[11] Note that interpreting variable importance measures in classification algorithms should be done with caution. When multiple variables are highly correlated, lasso will tend to drop all but one, as they are redundant to the prediction problem but still get penalized. However, some of those variable could be more correlated with the outcome variable than to some that are kept in the model.

8.2 Long-Run Returns

In addition to the impact of activism on firm operating measures, the targeting decision analysis can aid in generating better estimates for the effect of activism on long-run shareholder returns. In Section 4.2 I provide evidence for the short-run abnormal returns accruing to the targets of hedge fund activism. Consistent with prior research, there are positive 41-day abnormal returns for activist targets on average in our sample of events, but these returns 1) are attenuated when you account for differences in target firm size, 2) have generally decreased over time, and 3) are almost primarily driven by firms which are subsequently taken over. A more contentious and arguably important debate involves whether any such short-run return comes at the expense of long-run growth. According to Martin Lipton, it is more important to know whether companies subject to hedge fund activism outperform the benchmark “not just in the short period after announcement of the activist interest, but after a 24-month period” (Lipton, 2013).

My research design allows me to test the effect of activism on long-run returns in a unique manner. A challenge in these settings is the appropriate benchmark for expected returns, as even small misspecifications present serious problems when aggregating over longer time horizons (Kothari and Warner, 2007). While previous studies typically adjust for the contemporaneous returns on a set of common factors, or firms in the same industry of similar size and relative valuation, I can benchmark the targeted firms to a more design-based sample: the average of comparison firms weighted by how similar they look to the cross-section of targeted firms, as measured in the machine-learning based propensity scores. These scores reflect the probability that a firm would be subject to an activist event given a large set of observable characteristics that would also be expected to correlate with future returns, include past performance, size, sector, and governance characteristics, and provide an intuitive way to benchmark future expected returns.

Fig. 9. Long-Term Buy-and-Hold Abnormal Returns

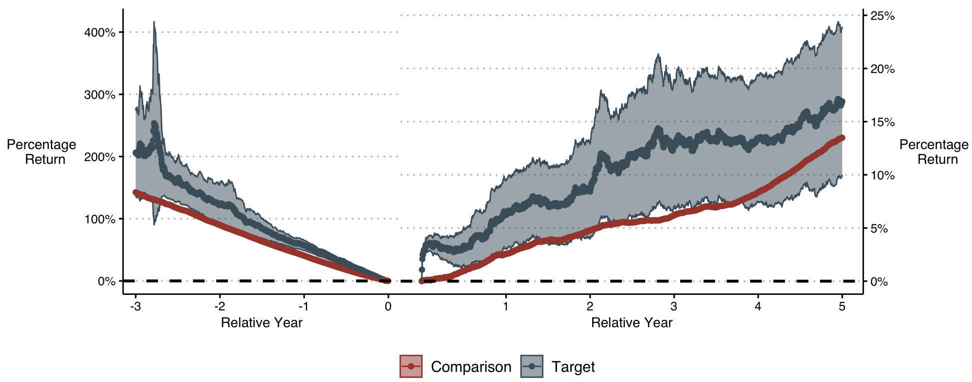


Figure 9 reports the average long-run buy-and-hold abnormal returns for the targeted firms in our sample, along with its 95% confidence interval (in blue). The average of the propensity-score weighted comparison returns for each target is also reported (in red). The returns are presented along separate axes for the pre- and post-activism periods, as the average decline in shareholder returns in the three years prior to the event is much larger than the subsequent increase.

The overall average long-run BHAR is calculated using the method described in Section 4.2, and is reported (along with its 95% confidence interval) in blue in Figure 9. The average for the pre- and post-activism periods are reported on separate axes, as the pre-event negative total shareholder return exceeds the post-activism positive performance by a substantial amount.[12] For each targeted firm I generate a comparison return that is the propensity score weighted average BHAR at each date for the comparison firms.[13] I then average over the event-specific comparison return series, and report the relative-date average in red.

The results in Figure 9 suggest that there are positive long-run returns to activism: the average five-year BHAR is in the order of approximately 17%. Yet, we see a similar, though less pronounced, return pattern when looking at the propensity-score weighted average of non-targeted comparison firms, with prior underperformance and a subsequent five-year BHAR of approximately 14%. These aggregate results, however, mask substantial differences given that many activists target the sale of

[12] To ensure that we don't have large sample composition changes, I follow deHaan et al. (2019) and assume that there are no subsequent abnormal returns following a delisting event. This can be conceptually viewed as investing the value of the shares following delisting in the market portfolio.

[13] These are firms with at least a full return series over the two months prior to and following the event activism date, and which that do not face a targeting event within the next five years.

the company, which is typically associated with large takeover premiums. The debate over long-run value is at its essence about whether activist interventions are associated with stronger or weaker performance for firms that remain independent, and not whether they are successful at convincing firms to sell themselves to the highest bidder.

Figure 10 prior research on activism and takeovers (Boyson, Gantchev, and Shivdasani, 2017) by documenting the increasing relationship in our sample. For each targeted firm, I randomly select a comparison peer within the same Fama-French 12 industry designation, using the propensity scores as sampling weights. I do this for every targeted firm, and calculate the cumulative takeover probability at each date for the following five years for the targets and the randomly selected peers. I repeat this procedure 1,000 times and plot the cumulative probability for the activism firms, as well as the center 95% daily range for the cumulative probability in the bootstrapped distribution.[14] While the comparison firms exhibit a substantial number of takeover in the five year period following the (pseudo-) event, the number of takeovers within the actual activist sample is markedly higher.

[14]The bootstrap is way to estimate the distribution of an estimator or test statistic by resampling your data with replacement. "Under conditions that hold in a wide variety of econometric applications, the bootstrap provides approximations to distributions of statistics, coverage probabilities of confidence intervals, and rejection probabilities of hypothesis tests that are more accurate than the approximations of first-order asymptotic distribution theory" (Horowitz, 2001).

Fig. 10. Cumulative Takeover Probability:
Targets and Propensity Score Weighted Comparisons

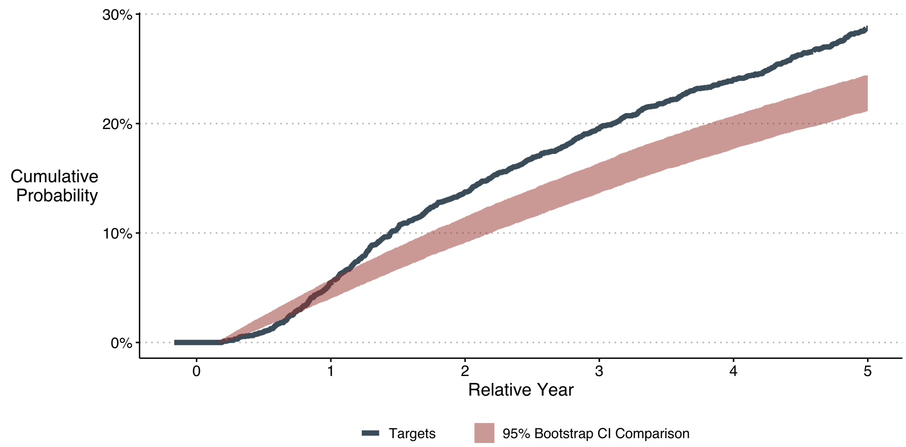


Figure 10 reports the cumulative probability of a takeover event for the activism sample and the bootstrapped distribution of comparison firms. In each bootstrapped sample I randomly select one comparison firm for each targeted event, using the propensity scores as sampling weights. I repeat this process 1,000 times, and report the center 95% of the empirical cumulative probability distributions by date.

Given the increased probability of takeover within our activism sample, it is sensible to separate the long-run returns of takeover and non-takeover targets.[15] In Figure 11, I present a similar plot of the long-run returns to activism, separating the sample among targets that are taken over within the five-year period following the event (Panel (a)) and those that remain independent (Panel (b)). For each targeted firm I identify whether it is (or is not) later the subject of a takeover event, and generate the corresponding comparison return series as the weighted average of the BHARs for the comparison firms that also are (or are not), taken over within the five-year period. These values are then averaged by date relative to the event (targets) or pseudo-event (comparison firms). The results in Figure 11 show that there is little evidence for any substantial average difference between takeover targets and this design-based comparison cohort of firms. Five years following the event the average total return to the comparison firms is nearly identical within the takeover and on-takeover samples.

[15]Note that most prior research on the topic of long-run returns focuses solely on the sample of firms that remain independent. See, e.g., deHaan et al. (2019) and Bebchuk et al. (2015).

Fig. 11. Long-Term Buy and Hold Abnormal Returns
Split By Takeover

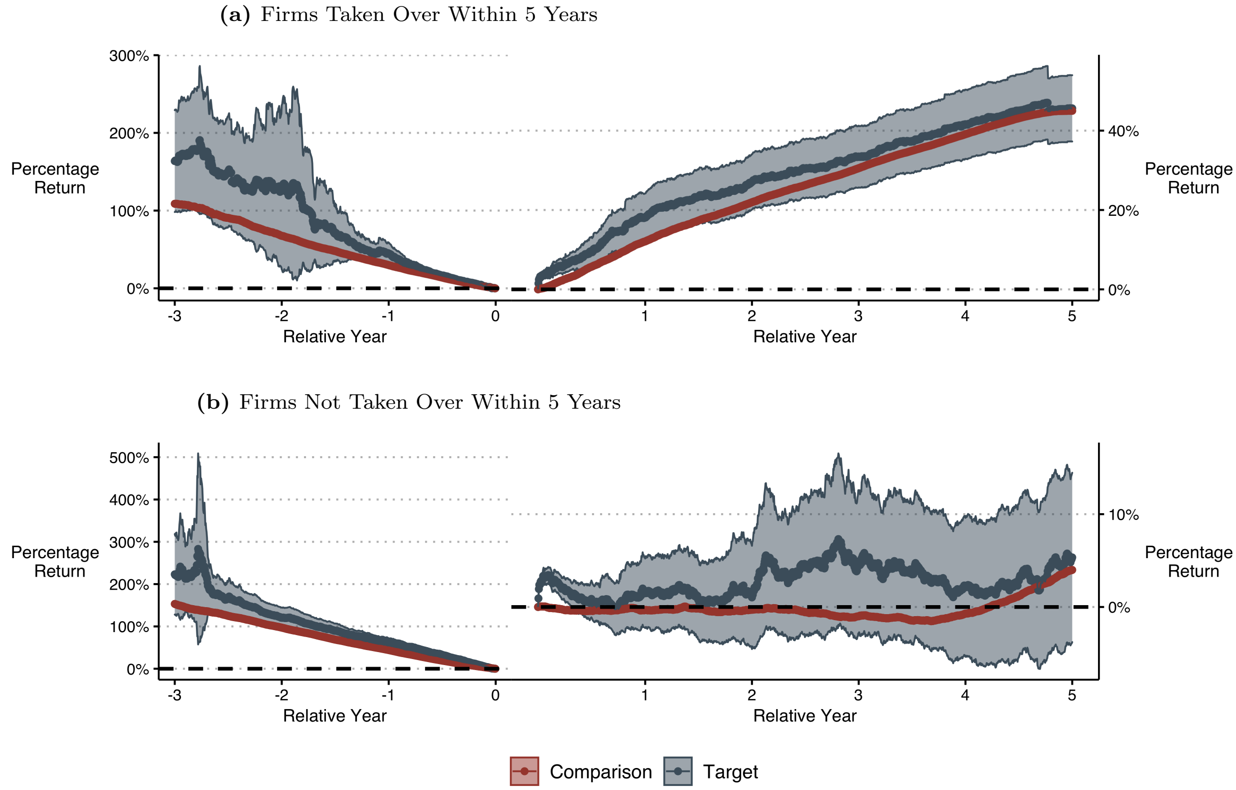


Figure 11 reports the average long-term buy-and-hold abnormal returns for the targeted firms in our sample with its 95% confidence interval (in blue), as well as the average of the propensity-score weighted comparison returns for each target (in red). Panel (a) presents the results for firms that are taken over within the five years following the event or pseudo-event, and Panel (b) reports the results for firms that are not taken over during this period.

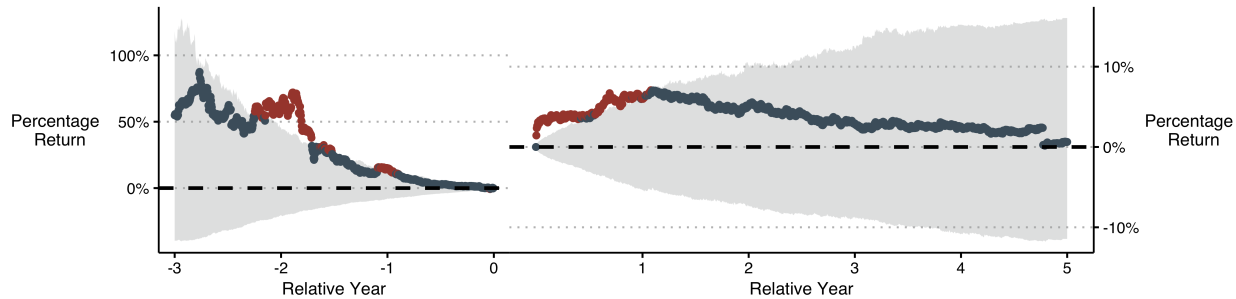
To generate a parsimonious representation of the unexplained portion of the average long-run return that cannot be attributed to the underlying risk profile of the targeted firms, I use a methodology similar deHaan et al. (2019) and Daniel, Grinblatt, Titman, and Wermers (1997). As in Figure 11, I calculate benchmark returns for each targeted firm as the propensity-score weighted average of the BHARs, after splitting on takeover events. The overall unexplained return is the buy-and-hold return of the target, less this matched portfolio return over the holding period. I then use a pseudo-portfolio bootstrapping approach to generate an appropriate test-statistic, following Lyon, Barber, and Tsai (1999). For each target event, I use the comparison firm propensity scores as of the year before the activist event as sampling weights to select at random another firm's returns. I similarly calculate the abnormal buy-and-hold return for this randomly sampled firm relative to the weighted average benchmark.

This process is repeated 1,000 times, after which I compare the targeted firm abnormal returns to the distribution of the pseudo-treated bootstrapped samples. The results are reported in Figure 12, with the relative-time portfolio-adjusted returns represented by colored bullet points, and the center 95% interval of the bootstrapped distribution marked by the grey band. I identify points outside of this interval as being statistically significant, and daily averages within the distribution as being insignificantly different from expectation. The long-run (five year) returns are squarely within the center of the bootstrapped comparison distribution, suggesting that there is no average positive (or negative) return to activism, outside what could be expected given firm fundamentals. While the returns are statistically significant and positive for the first year of the sample for takeover targets, this likely represents the more expedited likelihood of takeover within the sample.[16] In addition, the target firms had higher returns the first three post-event years, but subsequently underperform the benchmark portfolio by roughly 7-8% on average for the final two years.

[16]This can be seen in Figure 10, where the slope of the cumulative probability curve for the takeover targets is much higher in the first year than for the comparison firms.

Fig. 12. Average Long-Term Buy and Hold Abnormal Returns
With Comparison Bootstrapped Confidence Interval

(a) Firms Taken Over Within 5 Years



(b) Firms Not Taken Over Within 5 Years

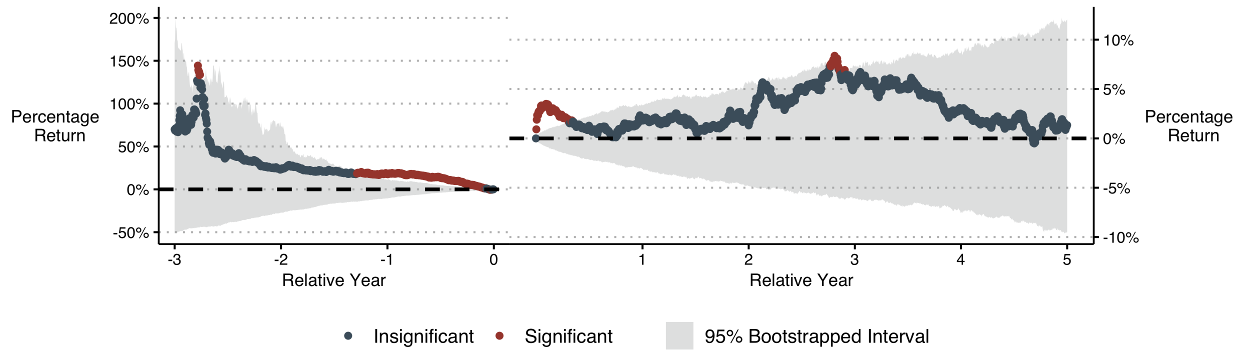


Figure 12 reports the average difference between the buy-and-hold returns for the targeted firms in our sample and the propensity score weighted-average of comparison firms. The grey band represents the center 95% of the bootstrapped distribution of the differences for randomly chosen comparison firms, using the propensity score as the sampling weight. Red points represent daily averages for the targeted firms that are outside of the bootstrapped interval, while blue points represent averages within the interval. Panel (a) presents the results for targeted and comparison firms that are taken over within the five years following the event or pseudo-event, and Panel (b) reports the results for firms that are not taken over during this period.

All of the preceding analyses involve taking the average of buy and hold return series at different points of time within the activism or comparison sample. However, the sample mean is notoriously sensitive to outliers, and financial markets are typified by fat-tailed distribution (Mandelbrot and Hudson, 2004). To the extent that we are interested in what happens to the *typical* firm that faces an activist event, it makes sense to examine other central tendencies that aren't as influenced by the extreme cumulative returns for a small number of firms. In Figure 13, I report the daily median long term buy and hold return, and the center 95% of the bootstrapped distribution for the median of the comparison firms.

These results are calculated identically to the method in Figure 12, except for the use of the median over the mean. Here we see that the story changes, as the typical activist targeted firm does not beat the propensity score weighted-average of comparison firms over the long-run. However, the same is true for the typical comparison firm, which also lags the benchmark. Among activist targets that are subsequently taken over, the median long-run return is generally above the center 95% of the bootstrapped comparison distribution, while for firms that remain independent it is very close to or below the 95% interval. There is thus some evidence that activists may tend to identify more valuable takeover opportunities, while decreasing the long-run performance of the typical targeted firm that remains independent.

Fig. 13. Median Long-Term Buy and Hold Abnormal Returns
With Comparison Bootstrap Confidence Interval

(a) Firms Taken Over Within 5 Years

(b) Firms Not Taken Over Within 5 Years

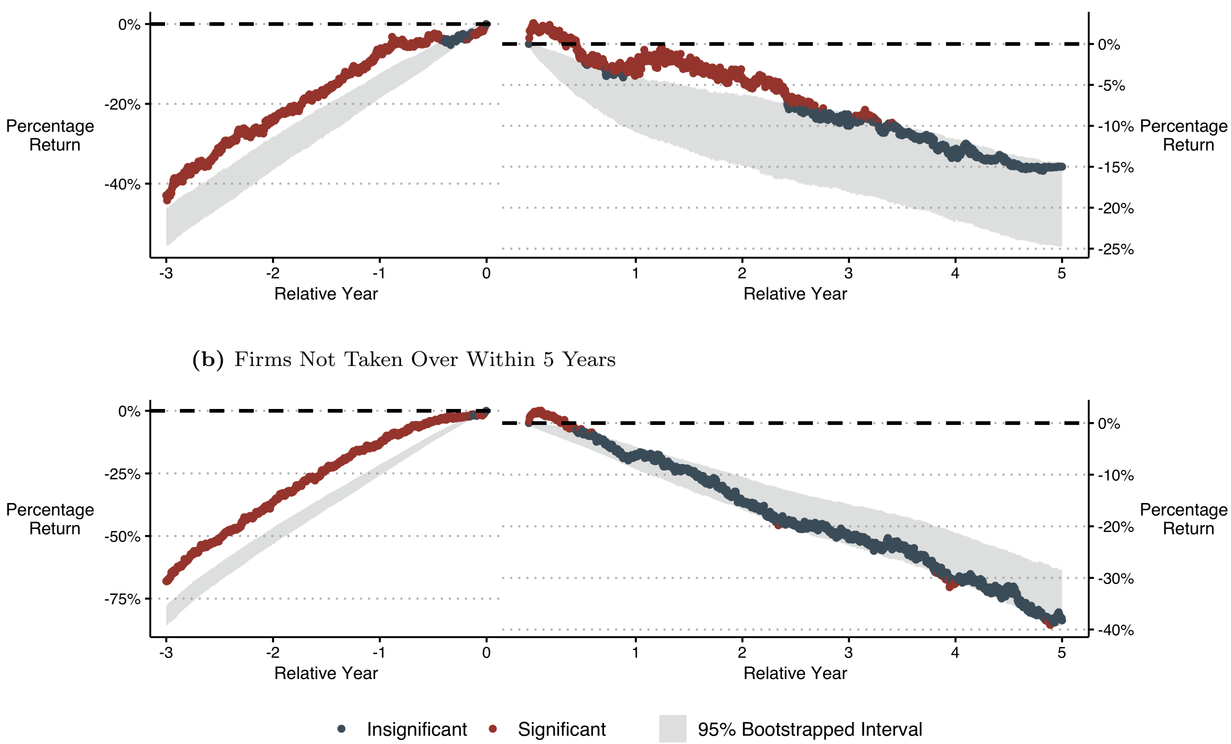


Figure 13 reports the median difference between the buy-and-hold returns for the targeted firms in our sample and the propensity score weighted-average of comparison firms. The grey band represents the center 95% of the bootstrapped distribution of the median differences for randomly chosen comparison firms, using the propensity score as the sampling weight. Red points represent daily averages for the targeted firms that are outside of the bootstrapped interval, while blue points represent averages within the interval. Panel (a) presents the results for targeted and comparison firms that are taken over within the five years following the event or pseudo-event, and Panel (b) reports the results for firms that are not taken over during this period.

8.3 Treatment Effect Estimates for Operating Measures

In this section I use the methodology explained in Section 7.2 to test the effect of activism on long-run measures of operating performance and attributes. Figure 14 provides the relative-time event study estimates for each of three standard measures of firm operating performance: return on assets, gross margin, and operating margin. I initially estimate the full series of activism-year/relative-time treatment effects, which are then aggregated to an overall relative-time level using the number of treated units in each treatment year as the weighting variable, following Callaway and Sant'Anna (2020).[17] The x-axis reports the relative year to the activism event corresponding to each ATT estimate, while the y-axis reports the level of the estimated effect in units for each respective outcome variable. The estimates, and their reported confidence intervals, represent the difference in unexplained trends in the outcome variable between the activism targets and the weighted average of the comparison firms.

The evidence from Figure 14 suggests that the simple penalized regression models used for the treatment and outcome equations for activism are largely effective in removing pre-treatment differences between targeted and untargeted firms for our operating performance measures. Unlike the matching and regression based approaches in the prior literature there is little evidence for consistent differences between targeted firms and the weighted average comparison of the control firms, either in level or trends. In addition, there is no longer a V-shaped treatment-effect pattern around the activism event.

However, unlike the prior results in Brav et al. (2008) and Bebchuk et al. (2015), there is no evidence of any material change in medium-term measures of operating performance for firms subject to an activist campaigns once taking into consideration the activist targeting decision. While there is a one-year dip in return on assets, the difference between treated and comparison firms is effectively zero by the year after the event. There is no discernible trend in either gross margin or operating margin, with all of the relative-time estimates hovering near zero. As a result, the evidence is inconsistent with activism either increasing or decreasing operating performance substantially within the five-year period after the event.

[17] Standard errors are computed using the efficient influence function from each individual treatment effect estimate, and I report the 95% simultaneous sup-t confidence band using a multiplier bootstrap procedure.

Fig. 14. Event Study Estimates of Activism on Operating Performance

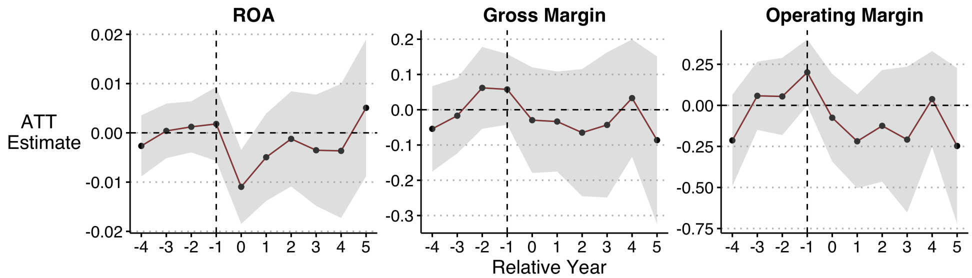


Figure 14 reports the effect of activism on three measures of operating performance - return on assets (roa), the gross margin (the ratio of revenue minus cost of goods sold to revenue), and operating margin (the ratio of earnings before interest to revenue). The estimates are calculated using the doubly robust DiD estimator from Sant'Anna and Zhao (2020) with propensity scores calculated using a logit-lasso model. The 95% simultaneous sup-t confidence band is reported in grey.

In Figure 15, I use the same estimation strategy to test the effect of activism on other measures of firm behavior that are allegedly impacted by activism. In a survey of the topic, Coffee and Palia (2016) express concern that activism is associated with increased leverage, shareholder payouts, and decreases in investment. As they note, leading proponents of hedge activism also believe that such a pattern of behavior may take place, giving it the positive spin of "investment-limiting" interventions (Brav et al., 2015). However, prior evidence on such topics presumably suffers from the same problem of not adequately correcting for the unique characteristics of targeted firms.

Fig. 15. Event Study Estimates of Activism On Operating Performance

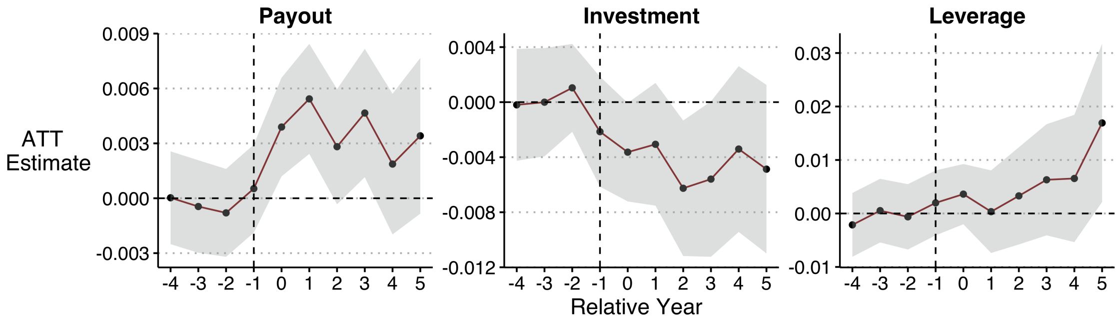


Figure 15 reports the effect of activism on other measures of performance behavior, including the payout ratio (the ratio of dividends and repurchases to assets), firm investment (the ratio of research and development expenses and capital expenditures to asset), and firm leverage (the ratio of long term and short term debt to assets). The estimates are calculated using the doubly robust DiD estimator from Sant'Anna and Zhao (2020) with propensity scores calculated using a logit-lasso model. The 95% simultaneous sup-t confidence band is reported in grey.

In contrast to the weak results in Figure 14, the evidence here is generally consistent with targeted firms lowering investment and increasing payouts, in line with anecdotal comments from lawyers and market commentators. The increase in firm payouts develops immediately, and tapers slightly over time, while firm investment in capital expenditures and research and development appears to decrease following activist events. In addition, there is some minor, suggestive evidence that activism is associated with an increase in firm leverage, although this appears to take longer to develop and is less precisely estimated. While there is a growing belief that the American economy suffers from an environment of "distressingly low business investment" (Gutiérrez and Philippon, 2017), whether these ultimately constitute desirable features in a governance mechanism depends partly on your prior beliefs surrounding firm investment and shareholder disbursement practices.

Table 3 aggregates the post-treatment coefficients (i.e. relative time periods $t = 0$ to $t = +5$) to generate a parsimonious parameter for the total ATT in the post-treatment period for each of our dependent variables. I aggregate the treatment effects over three different relative time ranges—the full post-treatment period ($\widehat{ATT(0-5)}$), relative years $t \in \{2, 5\}$ ($\widehat{ATT(2-5)}$), and the estimate for just the effect in the fifth year post-treatment ($\widehat{ATT(5)}$). While these parameters capture less texture than the event study estimates, increased sample sizes generally lead to more precise estimates.

Table 3 reports the aggregated estimates, standard error, and 95% confidence interval, along with the sample median for treated units in the pre-activism period. Consistent with the event study estimates, the aggregated estimates are close to zero and small relative to the sample median for the effect of activism on operating performance metrics. The ATT for investment and payout is statistically significant (negatively and positively respectively), particularly in the beginning of the post-activism sample. The treatment effect for leverage is only statistically significant at 95% level when looking at the very end of the post-activism window.

Table 3. Aggregated ATT Estimates

Variable	(1) $\widehat{ATT}(0-5)$	(2) $\widehat{ATT}(2-5)$	(3) $\widehat{ATT}(5)$	(4) Median In Sample
Operating Performance				
ROA	-0.003	-0.001	0.005	0.094
	0.005	0.006	0.007	
	[-0.012, 0.006]	[-0.012, 0.010]	[-0.008, 0.018]	
Gross Margin	-0.037	-0.040	-0.086	0.368
	0.068	0.079	0.129	
	[-0.172, 0.097]	[-0.195, 0.114]	[-0.340, 0.167]	
Operating Margin	-0.139	-0.136	-0.247	0.065
	0.126	0.134	0.241	
	[-0.387, 0.108]	[-0.400, 0.129]	[-0.719, 0.225]	
Other Variables				
Payout	0.004	0.003	0.003	0.001
	0.001	0.002	0.002	
	[0.001, 0.006]	[0.000, 0.006]	[-0.001, 0.008]	
Investment	-0.004	-0.005	-0.005	0.058
	0.002	0.003	0.003	
	[-0.009, 0.000]	[-0.010, 0.000]	[-0.011, 0.001]	
Leverage	0.006	0.008	0.017	0.170
	0.004	0.005	0.008	
	[-0.002, 0.015]	[-0.002, 0.019]	[0.001, 0.032]	

Table 3 reports the aggregated DiD estimates for the coefficients on the post-activism event-time indicators. The first column reports the results aggregated over the full post-activism window $t \in \{0, 5\}$, the second column aggregates only $t \in \{2, 5\}$, and the third column reports just the coefficient on the event-time indicator for relative year $t = 5$. The table contains the ATT estimate, standard error, and 95% confidence interval using sup-t critical based t-values, respectively. The fourth column reports the sample median for each variable in the targeted firm sample before the activism event.

Given the time variation in abnormal returns documented earlier, there is reason to believe that such variation might exist in the operational treatment effects. Figure 16 provides the event-time estimates for each variable, broken down by time period range. For clarity, it does not include the uncertainty measures; the estimates and confidence intervals broken down by variable and time period are reported in Appendix E.[18] Although many of the results are similar across cohort, there are clear differences in some of the treatment effect paths. While the treatment effect pattern for investment is consistently negative in the post-2000 period, it was actually positive in the very beginning of the sample. In addition, the positive effect for leverage later in the event window appears to be driven in large measure by the very beginning of the sample. Finally, there is much more pronounced evidence for increases in firm payouts in the latter cohorts, and there is in fact some evidence for short-term decreases in ROA with the most recent activist events in the sample.

Fig. 16. Event Study Estimates of Activism On Operating Performance By Date Range

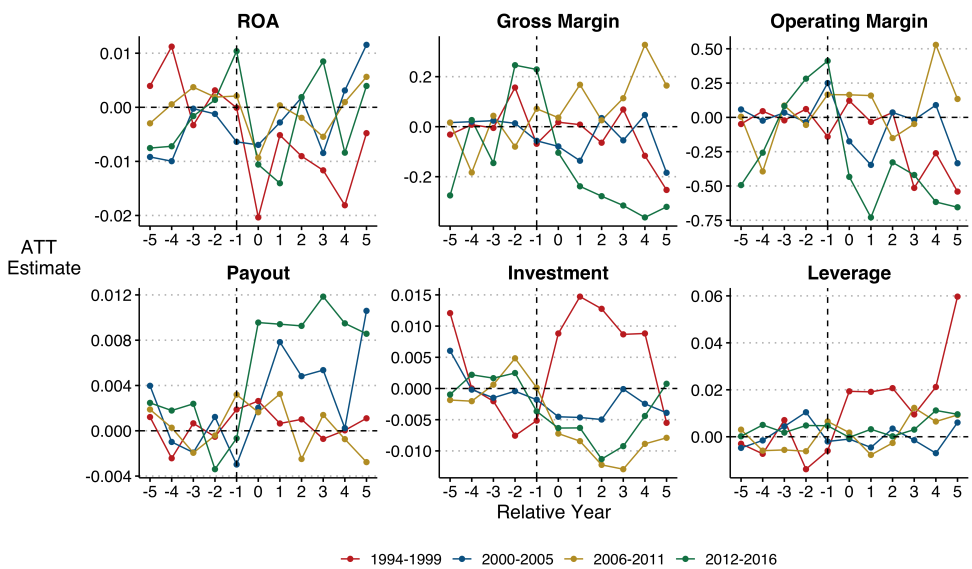


Figure 16 reports the effect of activism on the firm operating variables, broken down by time period. The estimates are calculated using the doubly robust DiD estimator from Sant'Anna and Zhao (2020) with propensity scores calculated using a logit-lasso model. The estimates with confidence intervals are reported in Appendix B.

8.4 Purposeful Activism

In this section I explore whether differences in activism type alter the treatment effect estimates. I test whether there is heterogeneity by activist type, splitting the activism events on the "purposeful

[18] It should be noted that each of the sub-periods have fewer units and thus will naturally have wider confidence intervals

activism" designation from Boyson and Pichler (2019).[19] The authors classify events as purposeful where "hedge funds pursue explicit goals, such as board seats, changes to capital structure, or mergers, and exclude campaigns in which the activist's only stated purpose is a belief that the target is undervalued." They ultimately identify 821 such campaigns with identifying information over the period from 2001 to 2012. I merge this list of purposeful events to the larger sample of events over the period, and separate the two event types.[20]

With the two distinct sets of activist events over the sample period—purposeful and not purposeful—I re-estimate the rolling propensity scores to allow the activism selection models to vary across activism type. I then calculate the treatment effects for each group separately using the robust estimator from Section 7.2. The results, presented in Figure 17, suggest that there is little discernible difference in the estimates by activism type.[21] As a result, there is little evidence that the effects (or lack thereof) identified in the full sample is a result of mixing activism events of different intensity.

[19] I thank Nicole Boyson for generously sharing this sample with me.

[20] There are some events in the Boyson sample that are not included in the 1994-2016 sample maintained by Alon Brav and co-authors. This is because the underlying data source for Boyson and Pichler (2019) is Shark Repellent. I add the non-overlapping events to our sample, dropping any duplicates at the firm-year level to control for arbitrary time differences between sources.

[21] Again, the confidence interval for purposeful activism is expected to be larger than not purposeful activism given the smaller number in the sample.

Fig. 17. Event Study Estimates of Activism On Operating Performance Broken Down By Purpose

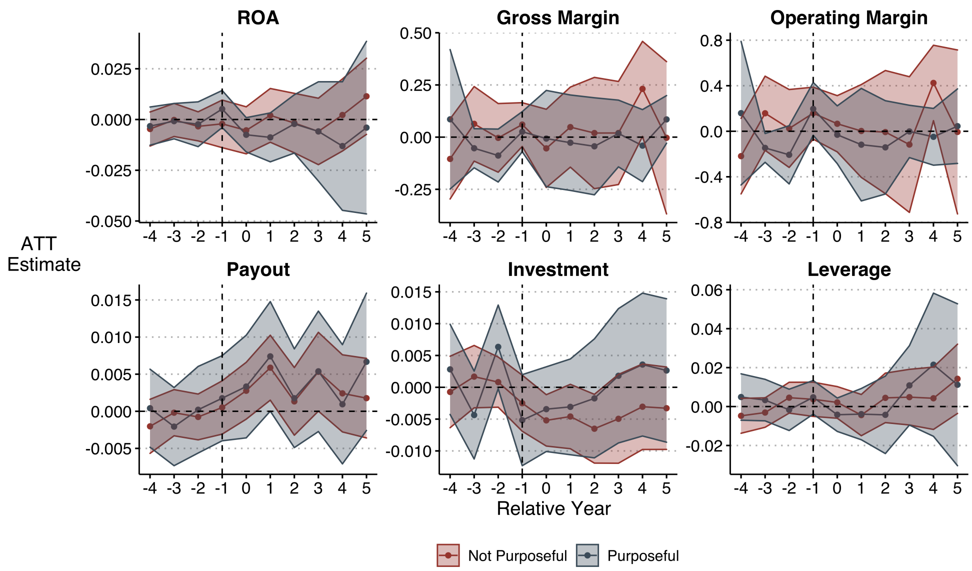


Figure 17 reports the effect of activism on the firm operating variables, split by the purposeful activism distinction, as defined and categorized in Boyson and Pichler (2019). The estimates are calculated using the doubly robust DiD estimator from Sant'Anna and Zhao (2020) with propensity scores calculated using a logit-lasso model separately for each activism type. The 95% simultaneous sup-t confidence band are reported in the shaded bands.

8.5 Stakeholder Outcomes

As noted by Tirole (2001), "the standard definition of corporate governance among economists and legal scholars refers to the defense of shareholders' interests." Building upon the seminal study of Berle and Means (1933), most corporate governance research centers around the agency problems associated with the separation of ownership and control. There now exists widespread acceptance that misaligned incentives may result in managers taking actions against the interests of shareholders, including engaging in "empire building", exerting inadequate effort to the corporation when overcommiting themselves to personal activities, and overlooking internal controls.

Indeed, most of the seminal theories in corporate finance are properly viewed as defining securities or corporate structures to limit agency costs within the corporation (Jensen and Meckling, 1976; Myers and Majluf, 1984; Zwiebel, 1996). However, the actions of managers impact more than just investors; they also “exert externalities on a number of ‘natural stakeholders’ who have an innate relationship with the firm: employees, customers, suppliers, communities where the firm’s plants are located, potential pollutees, and so forth” (Tirole, 2001). While the legal and regulatory structures in the United States have been guided by this focus on the implicit agency relationship, in Germany, Japan, France and other countries, corporations are expected to promote growth, secure employment, and the environment, with “profitability being more an instrument than the ultimate goal” (Tirole, 2001).

Although the United States and other countries within the Anglo-American legal tradition generally support the shareholder value conception of corporate governance, American policymakers have recently signaled receptiveness to a more expansive definition of corporate governance. In August 2018, Elizabeth Warren, the senior senator from Massachusetts, introduced the the Accountable Capitalism Act, which would require any corporation with annual revenue over $1 billion to obtain a federal charter of corporate citizenship instructing directors to consider the interests of all stakeholders (Yglesias, 2018). The bill, were it enacted, would also curb corporate political activities and require that 40 percent of the membership of the board of directors of federally chartered corporations be elected by employees. More topically, former Chief Justice of the Delaware Supreme Court Leo Strine expressed his concern that activist hedge funds generate returns by forcing target companies to sell themselves off at a purchase premium inflated through employment reductions and slashed wages, noting that “human investors care not just about whether corporations make money, but also about how” (Strine Jr., 2017)

The previous results in this paper build upon the prior corporate governance literature in testing whether hedge fund activism benefits shareholders—either directly, through the total return to shareholders, or implicitly through the effect of activism on firm operating performance or investment practices.[22] However, consistent with the rising interest in corporate social responsibility, a natural extension to the standard approach of weighing the costs and benefits of activism is an exploration of the extent to which activism may impose externality costs on other counterparties

[22] There are almost certainly spillover effects from research and development and other firm-level investments, though these impacts can still be considered through the standard principal-agent framework central to corporate governance research.

to the corporation. A connection between activism and negative externalities may not be *ex ante* surprising; to the extent that "good governance" aligns the interests of managers and shareholders, one might expect such a result if the interests of shareholders and other counterparties are not aligned. In fact, the rise of modern governance mechanisms—from stock-based executive compensation to a renewed market for corporate control through leveraged buyouts and hedge fund activism—coincide almost perfectly with a massive "reallocation of rewards to shareholders in a decelerating economy, primarily at the expense of labor compensation" (Greenwald, Lettau, and Ludvigson, 2021).

A practical difficulty with measuring the non-shareholder effects of activism is data availability. Most corporate governance studies use commonly available financial databases on audited accounting statements, or stock and bond returns, to measure the effect of policy changes on outcomes. However, this data is largely mandated through the disclosure provisions of our securities laws to benefit shareholders, and most of the natural data sources for non-shareholder interests are either confidential (e.g. Census data on employee wage histories and unemployment benefits, or OSHA surveys on workplace injuries) or have imperfect and inconsistent mapping to firm ownership (e.g. EPA's public inspection records and self-reported effluent emissions). While future projects will work with measures created from confidential administrative data, there are two readily available public data sources that can be used to get partly at some of these questions: the number of employees retained by the firm, and estimates of corporate tax avoidance.

Figure 18 reports the estimates for the effect of hedge fund activism on the total reported number of the employees at the firm from the annual financial statement.[23] Using my preferred treatment effect estimator and the propensity scores from the machine learning model, we see that there is an immediate decrease in the number of employees, suggesting that firms engage in significant lay-offs following an activist event. Note that this is almost certainly an under-count of the true effect of activism on employment levels, as layoffs following activist-induced takeovers would not be included in the estimates. These results are consistent with prior academic and policy

[23] I follow Pinnuck and Lillis (2007) and include sales growth, firm profit, the change in firm profit, the quick ratio, the change in the quick ratio, the lag of the change in the quick ratio, one year total shareholder return, firm size, and firm leverage as covariates in the outcome regression in the model.

critiques suggesting that potential shareholder gains from activism come at the expense of labor interests by cutting jobs (Strine Jr., 2017).

Fig. 18. Employee Count

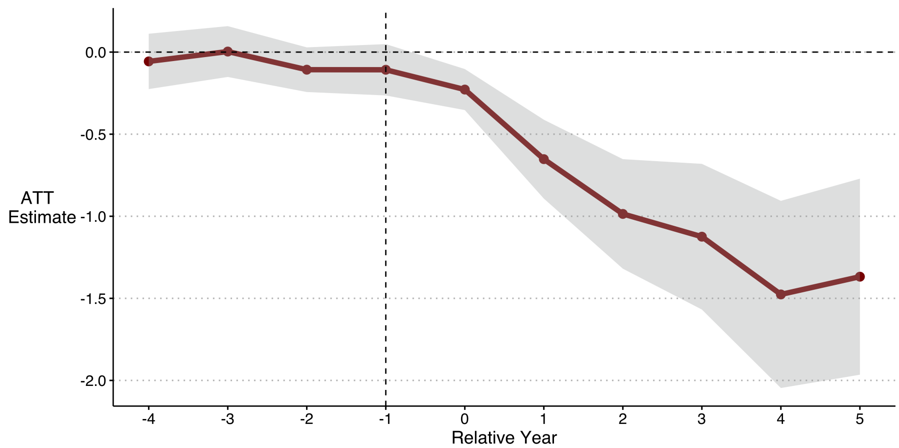


Figure 18 reports the effect of activism on the total reported level of firm employment. The estimates are calculated using the doubly robust DiD estimator from Sant'Anna and Zhao (2020) with propensity scores calculated using a logit-lasso model. The 95% simultaneous sup-t confidence band is reported in grey.

In addition, we can also test the relationship between activism and corporate tax avoidance using standard financial databases. This form of tax avoidance is a growing global problem, and collectively costs governments somewhere between \$500 billion and \$600 billion a year in lost tax revenue (Shaxson, 2019). A prior 2012 study, using a smaller sample of activism events, found that firms targeted by activists experience increases in tax avoidance (Cheng et al., 2012). However, the authors also find that firms targeted by hedge fund activists exhibited lower levels of tax avoidance prior to the intervention in comparison to their matched control sample. This suggests that, as with the studies on operating performance referenced above, the matching procedure may not adequately address the endogeneous relationship between the targeting decision and outcomes.

In Figure 19, I produce the event study results for the dynamics of the book-tax difference (BTD) measure for activist targets in comparison to the propensity-score weighted comparison average around the event. I focus here on BTD—the difference between a firm's reported income and taxable income—rather than a measure of the effective tax rate (ETRs), given that ETRs require positive income and many firms in the sample suffer from financial distress.[24] In contrast to Cheng et al. (2012) I do not find a relationship on average between activism and book-tax difference in our sample once adjusting for the similarity of comparison firms to the cross-section of the targeted sample.

Fig. 19. Book-Tax Difference

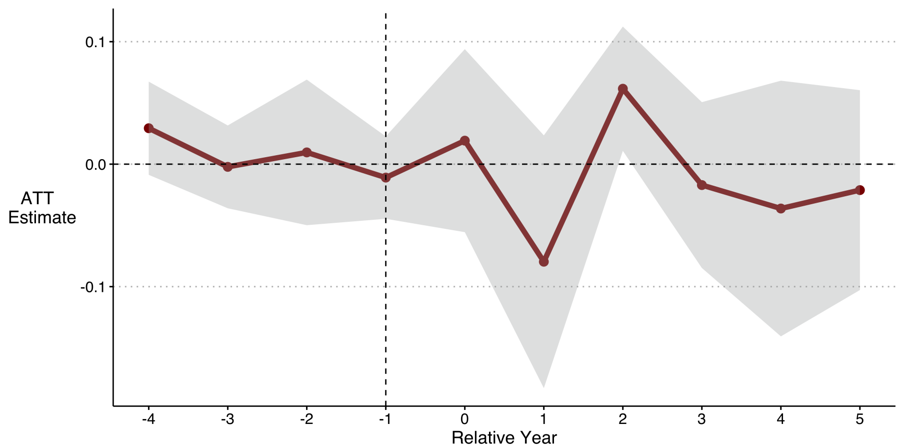


Figure 19 reports the effect of activism on the annual book-tax difference. The estimates are calculated using the doubly robust DiD estimator from Sant'Anna and Zhao (2020) with propensity scores calculated using a logit-lasso model. The 95% simultaneous sup-t confidence band is reported in grey.

While I do not find an average relationship between activism and tax avoidance, it is possible that the aggregate results mask systematic underlying differences effects. Given that there are repeated measures for each firm across the different outcome measures, it is possible to test whether there are correlations across variables. In other words, it could be the case that the firms that have higher levels of unexplained increases return on assets are the same firms that experience increases in the book-tax difference. Here I use the influence functions for the post-activism observations, as detailed in Sant'Anna and Zhao (2020), which can be thought of as standardized measures of the unexplained changed in outcome variables from the underlying model. To compare the relation of changes across outcome measures, I do pairwise binscatters, a simple non-parametric method for visualizing two-way comparisons, of the influence functions. To construct a binscatter, you first

[24]In untabulated results I confirm that the results are similar when using effective tax measures as the outcome variable.

divide the support of the x variable into a number of bins, and then take the average of the y variable within each bin. I use the method from Cattaneo, Crump, Farrell, and Feng (2019), which derives the optimal number of bins and confidence intervals in a data driven manner.

Figure 20 presents the results comparing the relationship between the unexplained changes in the book-tax difference and return on assets, controlling for treated year/relative-year effects. We see that there is a robust positive relationship in the data, suggesting that the subset of firms that most exceed the model's prediction for return on assets are the same ones that most increase their book-tax difference. Thus, even though there is no discernible average effect between activism and either firm operating performance or tax avoidance, there appears to be a clear link between the two outcomes when taking account of heterogeneous impacts.

Fig. 20. Book-Tax Difference

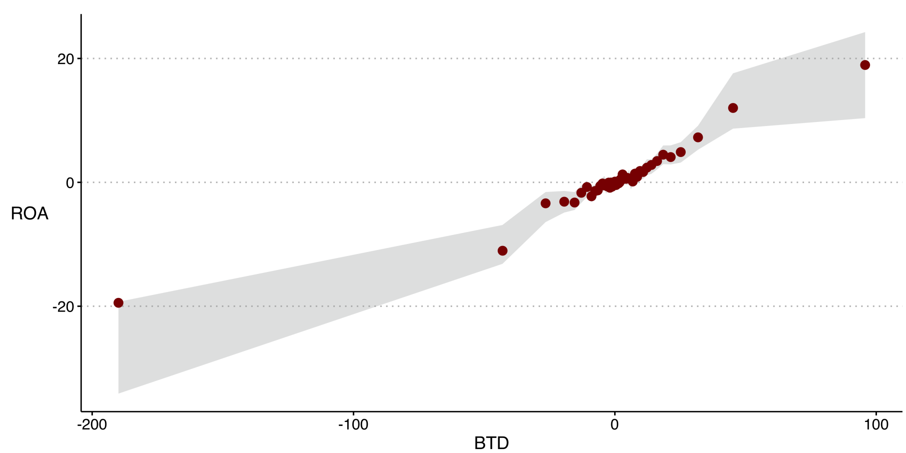


Figure 20 reports the binscatter estimates of the post-treatment influence function values across event study models for the book-tax difference (BTD) and return on assets (ROA), using the data-driven methodology from Cattaneo et al. (2019).

9 Legal and Policy Implications

These findings carry implications for current legal and policy debates, especially in light of growing public interest in corporate purpose and the decline in business investment. The question of "for whom is the corporation managed" (Rock, 2021) is a long one in corporate law, that has only recently escaped the confines of the ivory tower to claim a prominent space in policy discussions. Politicians on both sides of the aisle have spoken on the issue, claiming to reject strong versions of shareholder primacy that are insufficient to provide an adequate "level of business investment in fixed assets" or "steady and constant workplaces for the American people" (Rubio, 2019). Senators Elizabeth Warren and Bernie Sanders have both issued policy proposals designed to shift the legal requirements for firm management, including the creation of federal corporate charters that explicitly stipulate how boards must consider the interests of stakeholders, requiring employee representation on the board of directors, or transferring a portion of the stock to an employee ownership fund (Sanders, 2021). Putting aside whether such policies represent ideal alternatives to the status quo, outside of perhaps allocating voting rights to shareholders, most of the proposals would count for very little if activists can simply change the board of directors.

A number of other pending or potential prospective policies could be used to forestall the potentially harmful effects of unconstrained activism documented in this article. For example, the Securities and Exchange Commission (SEC) is currently holding an open comment period on its proposal for a mandatory universal proxy to be used for contested director elections (Securities and Exchange Commission, 2021). In contested elections, incumbent managers and dissident shareholders both solicit votes for their slate of nominees to the board, and under the current proxy regulations parties are not allowed to solicit proxies for a nominee without the nominee's consent. In effect, the regulation forbids split ticket proxy voting, so that shareholders who do not physically attend the shareholder meeting are required to either vote exclusively with management or the dissident. The pending SEC rule would require firms to issue "universal proxies" that list the nominees from each side and allow shareholders to "mix and match" their preferred directors. Opposition to universal proxy access, led in part by the Chamber of Commerce, is driven by the belief that it will increase the ease of proxy fights by hedge fund activists.[25] If the SEC wanted to stem the rise of hostile activism, they could either rescind the proposed rule, or tailor it in a manner such that it would not benefit transient hedge fund activists.

In addition, the SEC is considering shortening the Form 13D filing period (Ackerman, 2021).

[25] However, Scott Hirst makes a compelling argument that this is unlikely to be true, at least as a partial equilibrium response, given observable characteristics in proxy voting (Hirst, 2018).

Currently, an activist who crosses the 5% ownership threshold has ten days to report their holdings and investment purpose with the Commission, in contrast to a two-business day standard in most other countries. This delay in disclosure arguably allows the activist and coordinating parties to exploit their information advantage at the expense of selling shareholders (Coffee and Palia, 2016). The Dodd-Frank Wall Street Reform and Consumer Protection Act authorizes the SEC to shorten the filing window, and in spite of spirited defenses from the academic and investment communities, SEC Chairman Gary Gensler appears convinced of the need for a change, questioning whether existing rules in the area "continue to make sense" (Gensler, 2021).

Congress could also legislate away the benefits that have accrued to activists through their use of the "wolf pack" strategy, which has arguably been the largest driver of the rise in activism events. This tactic involves the lead activist working in relative concert with a group of other investors, while avoiding the formation of a "group" for legal purposes under Section 13(d)(3) of the Securities Exchange Act. In this manner, the activists and their conspirators cannot be sued for a disclosure violation while accruing shares, as long as no individual investor crosses the 5% threshold. It also allows for a longer period of time during which shares can be purchased by the group before the announcement of the filing event, allowing the funds to recover more of the short-term activist-induced price increase and to evade the potential use of a poison pill to prevent further share purchases (Coffee and Palia, 2016). Under the current "conservataive" understanding of the legal standard, hedge funds acting in such a manner are not a group for reporting purposes, even when they openly discuss strategies among themselves (*see meVC Draper Fisher Juvetson Fund I, Inc. v. Millennium Partner L.P.*, 260 F. Supp. 2d 616 (S.D.N.Y. 2003)). Legislative clarification or extension of the legal standard for group formation would push forward the Form 13D filing, thereby reducing insider group profits.

These issues have coalesced in a pending dispute in the Delaware courts over the legality of "anti-activist" poison pills. At the onset of the recent pandemic, the American energy firm The Williams Company adopted a shareholder rights plan designed explicitly to prevent an activist event given the market uncertainty. The pill at issue included a low five-percent ownership threshold and an expansive definition of "acting in concert" for purposes of beneficial ownership, and would have effectively removed the risk of activist-initiated shareholder pressure on management and the board. A Chancery Court opinion by Chancellor McCormick enjoined the pill, finding that those provisions exceeded the scope of protection afforded under the *Unocal/Unitrin* line of cases. The case has been appealed to the Delaware Supreme Court, and as noted by Professor Jeffrey Gordon, given Delaware's history of doctrinal reversals in the face of public pressure, it is certainly possible that the Court could overturn the decision and affirm anti-activist pills (Gordon, 2021). Such a decision would largely end hedge fund activist campaigns in the most important state for corporate law purposes.

However, these changes in policy or doctrine carry clear risks of unintended consequences with uncertain benefits. Split ticket voting has near universal appeal as a tool of democratic accountability within the corporation, and maintaining the archaic distinction between in-person and proxy voting simply to stifle activist pressure would be an inefficient remedy. While there are strong arguments for both reducing the 13D reporting period and cracking down on coordinated conduct among activists, such changes would at best reduce the overall level of activism, and at worst reduce the gains to the hedge funds while keeping the activism level constant; neither would target the specific harmful effects of the activism events I identify in my empirical tests. Finally, as noted by Professor Gordon, judicial acceptance of the activist poison pill would remove one of the more promising recent strategies for pushing firms to consider the consequences of their actions for stakeholders—ESG activism (Gordon, 2021). In a recent proxy contest, the ESG activist Engine No. 1 was able to successfully place three dissident directors on the board of Exxon Mobil with the aim of pushing the company to reduce its carbon output (Sommer, 2021). An expansive judicial ruling that grants managers and boards the ability to prevent almost all effective coordinated strategic action among shareholders risks reducing the accountability of public firms to society.

A more straightforward and potentially effective remedy would involve pressuring large institutional investors play a more active role in defining the amount and scope of activism that they will support going forward. Hedge funds almost always require the backing of other investors to succeed in an activist campaign, given that they are generally capable of acquiring only a small percentage of a firm's outstanding voting shares. In his widely-shared annual letters to CEOs, Larry Fink, the head of the largest money manager in the world, has taken to highlighting the issues that he believes are "pivotal to creating durable value", including a focus on long-term investment rather than short-term payouts, and a corporate purpose that prioritizes all stakeholders (Fink, 2015, 2021). Were the largest institutional investors to collectively refuse to support activists whose actions cause firms to sacrifice investment for payouts, or engage in firm-wide layoffs, the practice would likely disappear.

10 Conclusion

The rise of hedge fund activism has undoubtedly been one of the more disruptive changes to the corporate governance environment of recent decades. Depending on the source, activism either represents a necessary corrective to the classic principal-agent problem endemic to the corporate form, or a driver of managerial myopia, causing firms to make long-term value destructive decisions to placate ephemeral shareholders. Amidst this debate researchers have attempted to tease out the medium-term impact of activism on corporate performance, using a variety of methods resulting in divergent conclusions.

However, given the clearly nonrandom nature of the activism targeting decision, proper inference requires a method that controls for both the characteristics that lead to activism events, and the natural dynamics of the outcome being modeled. In this paper I adopt an estimation technique that adjusts for both of these processes, incorporating a data-driven approach for modeling the activism decision. This form of estimation has become more common in other areas of economic research, but to the best of my knowledge has not been used in law or finance. Such methods hold promise for other strands of governance research, given the presence of similarly endogenous treatment assignment processes—from private equity buyouts to the decision to stagger or destagger the corporate board.

When applied to the activism question, the model leads to different conclusions from at least a portion of the prior published research. First, unlike recent work by Bebchuk et al. (2015), modeling the activism assignment decision removes the pronounced V-shaped pattern of conditional differences in the outcome variable around the activism event. The resulting treatment effect paths suggest an increase in firm payouts and a decrease in investment following activism events, with additional weaker evidence for a increase in leverage. However, there is little to no evidence consistent with any change in firm operating performance or average long-run returns. In subsequent tests I show that the results for firm payouts appear to be increasing over time, will little cause to believe that the results differ based on the intent and purpose of the activism event (at least as defined in Boyson and Pichler (2019)). Finally, activism appears to lead to significant reductions in the labor force at impacted businesses, and corporate tax avoidance increases within the subset of firms that do become more profitable following an event. I argue that, given their publicly stated policy positions, these empirical results could be used to push institutional investors to reconsider their position on hedge fund activism.

Appendix A Methods From Replicated Papers

In this appendix I explain how each paper replicated in Section 5 tests the effect of activism on firm performance measures. I replicate and extend the estimates from these papers to the full activism sample, from 1994 to 2016, using return on assets as the outcome variable.

A.1 Brav, Jiang, Partnoy, and Thomas (2008)

Brav et al. (2008) is an influential study that developed the method used for identifying activist events in future papers. While the bulk of the paper is devoted to studying the impact of activism on the stock returns of targeted firms, Table VII tracks the change in target firm performance for two years prior to and following activist events. The authors' methodology:

- Matches each targeted firm to a set of comparable firms in the same year, same three-digit SIC industry, in the same Fama-French 10×10 size/book-to-market portfolio.
- If there are no available matches they use two-digit SIC codes, and match to 5×5 portfolio splits.
- The difference between each targeted firm's operating performance is compared to the average of the comparable firms for each relative time period.

A.2 Bebchuk, Brav, and Jiang (2015)

This paper was written in response to the Lipton memo, and examines a longer five-year window around activist events. Rather than using a matching framework, the authors do regression adjustment and model operating performance (here return on assets, or **roa**) as:

$$roa_{it} = \alpha_j + \lambda_t + \sum_{k=-3}^{5} \gamma_k D_{it} + \beta X_{it} + \epsilon_{it}$$

where α_j and λ_t are industry[26] and year fixed effects, D_{it} are a series of dummy variables for the relative time periods from three years before to five years after an activist event, and X_{it} is

[26] The authors also report results with firm rather than industry fixed effects, although the estimated effects are less clear in that specification.

a matrix of time-varying control variables, which includes the natural logarithms of firm age and market capitalization.

A.3 Cremers, Giambona, Sepe, and Wang (2018)

Cremers et al. (2018) argue that the regression framework in Bebchuk et al. (2015) does not adequately address the selection issues inherent to hedge fund activism. The authors instead use a matching framework to deal with the activist targeting decision, matching directly on the variables they find to be correlated with the propensity to be targeted using a logit model (Tobin's q - one and five-year lags, the log of market value - one year lag, leverage - one year lag, and roa - one year lag). Within the matched sample, the authors then run the following regression difference-in-difference estimator:[27]

$$roa_{it} = \alpha_j + \lambda_t + \sum_{k=X_{Pre}}^{X_{Post}} \gamma_k D_{it} + \sum_{k=X_{Pre}}^{X_{Post}} \delta_k D_{it} \times Target_i + X_{it} + \epsilon_i$$

where all variables are defined similarly, except now there are lead/lag dummies for the relative year to treatment (or pseudo-treatment) for both treated and control firms, and a separate set of indicators for the actually-treated firms. Instead of year-indicators, the paper uses binned-date ranges X_{Pre} and X_{Post} to capture time periods before and after treatment.

A.4 deHaan, Larcker, and McClure (2019)

Finally, deHaan et al. (2019) also use a matching framework to compare targeted firms to a set of comparable non-targeted firms. However, the authors note that existing studies do not account for the stochastic evolution of accounting metrics, which is evidenced by pre-activism differences in the estimated trends. For each firm/year they create a score metric:

$$score_{i,t-1} = \frac{at_{i,t-1}}{\sigma^{at}_{j,t-1}} + \frac{roa_{i,t-1}}{\sigma^{roa}_{j,t-1}} + \frac{\Delta roa_{i,t-1}}{\sigma^{\Delta roa}_{j,t-1}}$$

[27]Note that the primary analyses in the paper uses Tobin's q as the outcome variable. Given the documented issues with using this variable in empirical analyses, I avoid its usage besides as a predictor (Bartlett and Partnoy, 2020).

where at is firm assets, roa is the return on assets, Δroa is the change in roa between years $t-3$ and $t-1$, and the σ terms are the industry-year standard deviations of these measures. They match targeted firms to a control firm on the score metric, and calculate the difference in differences between targeted and control firms for future years less the value for each in the year before treatment.

Appendix B The Partially Linear Model

The partially linear model from Robinson (1988) assumes that the data-generating process follows:

$$Y = D\theta_0 + g_0(X) + U, \qquad E[U|X, D] = 0 \tag{1}$$

$$D = m_0(X) + V, \qquad E[V|X] = 0 \tag{2}$$

where Y is the outcome variable you want to explain, D is the policy or treatment variable of interest, and X consists of a vector of control variables. The first equation is the main outcome equation, and θ_0 is the regression parameter on the policy variable that we would like to determine (e.g. in our setting it would be the impact of hedge fund activism on firm performance). The second equation keeps track of confounding for the treatment variable, where the variables in X determine the policy variable D through the function $m_0(X)$ and potentially the outcome variable through the function $g_0(X)$. The key result from Chernozhukov et al. (2018) is that you can recover an unbiased estimate for θ_0 using modern machine learning techniques if you employ an orthogonalized formulation of the treatment equations, where you directly partial out the effect of X on D. By orthogonalizing the treatment variable with respect to the covariate vector, and approximately removing the direct effect of confounding by subtracting an estimate of g_0, the predicted treatment effect removes the regularization bias that impacts naive machine learning estimators. The resulting estimator has "doubly robust" properties, where the resulting estimand identifies the ATT even if either the propensity score model or the outcome regression models are misspecified.

Appendix C Data Appendix

Variable Name	Abbrevation	Description
Amihud Illiquidity Measure	amihud	The Amihud measure of illiquidity, defined as 1000 times the square root of the absolute return, over the absolute value of traded shares (prc*vol). Zero volume observations are dropped and we take the average over the fiscal year, requiring at least 50 observations.
Analyst Following	analyst	Analyst following from IBES. We use only one year ahead eps forecasts (fpi = 1) for annual reports (fiscalp = "ANN"), and take the average number of estimates (numest) for a given firm/reporting date (cusip/fpedats) combination. We use both the US and international files (ibes.statsum_epsus and ibes.statsum_epsint).
Any Insider Purchases	any_ins_buy	An indicator for whether there were any purchases by corporate insiders during the fiscal year.
Any Insider Sales	any_ins_sell	An indicator for whether there were any sales by corporate insiders during the fiscal year.
Average Age of Board of Directors	avg_age	The average age of the board of directors for the most recent meetingdate within the past two fiscal years, from the ISS directors database.
Average Assets	avg_assets	The average of assets and lag assets. If lag assets are missing it is just present year assets (at).
Average Tenure of Board of Directors	avg_tenure	The average tenure of the board of directors for the most recent meetingdate within the past two fiscal years, from the ISS directors database.
Average Volume	avg_volume	The average of the volume of traded shares over the fiscal year, scaled by shares outstanding. We require at least 50 non-missing observations.
Majority Outside Board	bdoutsidemaj	Whether the majority of the board is constituted of outside directors. This comes from MSCI GMI rankings, and is available for 2001 - 2018. Because they have many missing cusips, we used the first hit on year, cusip/cik (a preference for cusip merge).
Board Size	board_size	The Count of directors for a given cusip/meetingdate combination from ISS.
Book-Tax Difference	btd	The difference between a firm's reported income and taxable income, measured as (pi - (txfed + txfo)/.35 - (coalesce(tlcf, 0) - coalesce(lag(tlcf, 1), 0)))/at.

(continued)

Variable Name	Abbrevation	Description
Book to Market	btm	The book-to-market ratio, where book value of equity is defined as the sum of the 1) first non missing value of seq, ceq + pstk (set to 0 if missing), or at - lt, and 2) txditc (set to zero if missing), less the first non-missing value of pstkrv, pstkl, or pstk (if none are non-missing then set to 0).
Cash to Asset Ratio	cash_assets	Cash to asset ratio defined as che/avg_assets.
Cash to Sale Ratio	cash_sale	Cash to revenue, defined as che/avg_sale, where the average sale ratio is defined the same as above.
Classified Board	cboard	Whether a firm has a classified board, as measured in the ISS riskmetrics governance database.
CEO Chairman	ceoischairman	Whether there is duality between CEO and chairman. This comes from MSCI GMI rankings, and is available for 2001 - 2018. Because they have many missing cusips, we used the first hit on year, cusip/cik (a preference for cusip merge). For this variable, it does not start until 2002, and observations are inexplicably missing from 2004 to 2007.
Operating Cash Flow to Assets	cf_assets	Cash flow over assets, defined as oancf / avg_assets.
Current Ratio	current	The Current Ratio, defined as act/lct
Incorporated in Delaware	de_incorp	A dummy variable for whether a firm is incorporated in Delaware.
Dedicated Investor Percentage	ded_perc	The percentage of shares held by dedicated investors, as defined in Bushee (1998, 2001).
Delta	delta	The calculated incentive delta for the CEO (and if the CEO is missing in the Execucomp dataset, then the highest paid employee). Note, these calculations come from John Kepler, and are at the executive level, and the data starts in 1992.
Change in Net Operating Losses	delta_nol	The change in net operating losses (tlcf) divided by average assets (avg_assets)
Change in Profit	delta_profit	The YOY change in profit (ni)
Change in Quick Ratio	delta_quick	The YOY percentage change in the quick ratio (quick_ratio).

(continued)

Variable Name	Abbrevation	Description
Board Size	directorstotal	Board size, as defined from MSCI GMI rankings and is available for 2001 - 2018. Because they have a lot of missing cusips, we used the first hit on year, cusip/cik (a preference for cusip merge). It is a duplicate of board_size but MSCI has larger coverage in later years.
Indicator for Any Dividends	div_dummy	A dummy variable for whether there are any dividends issued, defined as non-missing and positive values of dvc.
Dual Class Shares	dualclass	Whether there are dual class shares, as measured in the ISS riskmetrics governance database.
Operating Margin	ebit_margin	This is the operating, or ebit, margin, defined as earnings before interest and taxes (ebit) scaled by revenue (sale)
EBITDA to Enterprise Value	ebitda_ev	Ebitda to enterprise value ratio, defined as oibdp / ev, where ev is enterprise value, defined as dlc + dltt + mv + pstkrv - che, where missing values of dltt, dlc, pstkrv and che are set to 0.
Equity Income	equity_income	Equity income (esub), scaled by average assets (avg_assets)
Earnings to Market Ratio	etm	The earnings to price ratio, defined as ib / mv
Executive Compensation	exec_pay	The sum of the total pay for the top five employees divided by market value. Data comes from Execucomp.
Free Cash Flow to Market Ratio	fcf_m	Free cash flow to market value ratio, defined as (oancf - capx)/mv, where missing values of capx are set to 0.
Firm Age	firm_age	The difference between the fiscal year for a firm and the first year that firm is available in the Compustat database.
Ratio of Fixed to Total Assets	fixedat_at	Fixed asset to total assets ratio, defined as ppent / avg_assets.
Foreign Income	foreign_income	The amount of foreign income (pifo) over average assets, set to 0 if NA
HHI Concentration for Geographic Units	geo_hhi	The HHI index for the geographic segments (stype = GEOSEG).
Number of Geographic Units	geo_num	The number of geographic reporting units for a firm/year combination.
Golden Parachute	goldenparachute	Whether there is a severance agreement/contract between a company and an executive contingent on a change in corporate control, as measured in the ISS riskmetrics governance database.

(continued)

Variable Name	Abbrevation	Description
Gross Margin	gross_margin	The gross margin, defined as revenue (sale) less cost of goods sold (cogs), scaled by revenue
Historical State of Incorporation	hist_incorp	Historical state of incorporation, from Holger Spamann and Colby Wilkinson.
Insider Buy-Sell Imbalance	insider_bsi	The Insider Buy-Sell Imbalance, measured as the difference in shares bought minus shares sold, divided by the sum of shares bought and shares sold.
Insider Percentage	insiderspctg	The percentage of shares outstanding held by senior executives or the board of directors, as measured in their proxy statements. This comes from MSCI GMI rankings, and is available for 2001 - 2018. Because they have a lot of missing cusips, we used the first hit on year, cusip/cik (a preference for cusip merge).
Institutional Holdings	inst_perc	The percentage of shares outstanding held by institutional investors. The shares held by institutions comes from the Thomson Reuters 13F database (tfn.s34). We keep the first filing date by rdate/mgrno/cusip combination, and then take the sum of the shares variable at the rdate/cusip level.
Intangible Assets	intangible_assets	Intangible assets (itan) scaled by average assets (avg_assets)
Investment	investment	Firm investment, defined as (xrd + capx)/avg_assets, where missing values of xrd and capx are set to 0.
Leverage to EBITDA	lev_ebitda	Firm leverage as a percentage of EBITDA, defined as (dltt + dlc)/oibdp, where dltt and dlc are set to 0 if missing.
Leverage	leverage_at	Leverage as a percentage of assets, defined as (dltt + dlc) / avg_assets, where missing values of dltt and dlc are set to 0.
Long Term Investor Percentage	lio_perc	The long term investor percentage as calculated by Yan and Zhang (2009 RFS).
Log of Firm Age	log_age	The natural logarithm of firm_age (+1).
Firm Size (assets)	log_at	Defined as the log of assets (at).
Log Market Value	log_mv	Log of market value, defined as csho*prcc_f.
Log of Revenues	log_sale	Log revenues, defined as log(sale).
Change in Log Sales	log_sale_delta	Change in log revenues, defined as log(sale) - lag(log(sale)).

(continued)

Variable Name	Abbrevation	Description
Long Term Debt to Assets	lt_at	Defined as dltt / avg_assets, where missing values of dltt are set to 0.
Long Term Debt to Assets	lt_debt_at	Long term debt to asset ratio, defined as dltt / avg_assets, where missing values of dltt are set to 0.
Long Term Debt to Equity Ratio	lt_debt_e	Long term debt to debt and equity ratio, defined as dltt/avg_debt_equity, where missing values of dltt are set to 0, and avg_debt_equity is the average value of debt_equity as described above with avg_assets.
Majority Vote	maj_vote	Whether a firm has has a majority vote requirement for directors elections, as measured in the ISS riskmetrics governance database.
Market Cap Bottom Quintile	mkt_bottom	An indicator variable for whether a firm is in the bottom quintile of market cap by year.
Market Cap Decile	mkt_cap_decile	The yearly decile ranking by firm of the market cap variable mv.
Net Insider Market Value	net_insiders_mktval	The net insider market value of trades, defined as the difference in market value of shares purchased and sold during the fiscal year scaled by the market value of the firm.
Net Insiders Trades	net_insiders_trade	The net insiders trades, defined as the difference between shares bought and sold, scaled by cash shares outstanding.
Indicator for no analysts	no_analysts	A dummy variable for whether there are no analysts following the stock.
NOL to Asset Ratio	nol_assets	Net operating loss carryforwards to assets, defined as tlcf / avg_assets.
NOL Indicator Variable	nol_dummy	A dummy variable for whether a firm has a NOL carryforward, set to 1 when tlcf is not missing and greater than 0, otherwise set to 0.
Incorporated in NY	ny_incorp	A dummy variable for whether a firm is incorporated in New York.
Operating Cash Flow to Revenue	opcash	Operating cash flows deflated by revenue, defined as oancf / avg_sale.
Pay Slice	pay_slice	The ratio of the pay for CEO (or the highest paid employee if CEO is missing) to the sum of totalpay for the five top highest paid employees. Data comes from Execucomp.

(continued)

Variable Name	Abbrevation	Description
Payout Ratio	payout	The payout ratio, defined as (dvc + repurchase)/avg_assets. Missing values of dvc are set to 0. Repurchase is defined as prstkc - change_pref, where missing values on both are set to 0. If this value is negative it is also set to 0. Finally, change_pref is defined as abs(pstkrv - lag(pstkrv)) if pstkrv - lag(pstkrv) is less than 0, otherwise set to 0.
Poison Pill	ppill	Whether a firm has a poison pill in place, as measured in the ISS riskmetrics governance database.
Profit	profit	Profit (ni) scaled by assets (at)
Quasi-Index Investor Percentage	qix_perc	The percentage of shares held by quasi-indexers, as defined in Bushee (1998, 2001). The designations are downloaded from Professor Bushee's website, and merged into the holding data from Thomson Reuters.
Quick Ratio	quick_ratio	Cash and short term investments (che) plus recievables (rect) plust total short term investments (ivst) (set to zero if missing), scaled by current liabilities (lct).
Return Volatility	return_vol	The standard deviation of returns over the fiscal year. Need at least 50 non-missing observations.
R&D to Revenue	rnd_rev	R&D to revenue ratio, defined as xrd / sale, where missing values of xrd are set to 0.
Return on Assets	roa	Defined as operating income to average assets - oidbp/avg_assets.
Return on Equity	roe	Return on equity, defined as oibdp / avg_book, where the book is book value of equity and the average is as defined above for avg_assets.
Return on Invested Capital	roic	Return on invested capital, defined as oiadp / avg_icapt, where the average of invested capital icapt is defined the same as above.
Revenue to Assets	sale_asset	The ratio of revenues to assets, defined as sale/avg_assets.
Sales Growth	sales_growth	The YOY change in sales (sale)
HHI Concentraion for Segments	seg_hhi	The HHI index by firm/year for the revenues from business segments (stype = BUSSEG) within a firm. We download the full segments data from Compustat compseg.wrds_segmerged. We keep the closest srcdate to the datadate and only observations where there are positive sales. The HHI is calculated as sum((sales)/sum(sales))ˆ2.

(continued)

Variable Name	Abbrevation	Description
Number of Segments	seg_num	The number of reporting business segments for a firm/year combination.
SG&A Expense	sga	SG&A expense, defiend as xsga/ avg_sale, where missing values of xsga are set to 0.
Short Interest Percentage	shint_perc	Short interest as a percentage of shares outstanding. We get the short interest data from the Compustat supplemental short interest file comp.sec_shortint.
Short Term Investor Percentage	sio_perc	The short term investor percentage as calculated by Yan and Zhang (2009 RFS).
Supermajority Provision	supermajor	Whether there is a supermajority provision for takeovers, as measured in the ISS riskmetrics governance database.
Tobin's Q	tobin_q	Tobin's Q, defined as (at + mv - (ceq + txdb)))/at, where missing values of txdb are set to 0.
Transient Investor Percentage	tra_perc	The percentage of shares held by transient investors, as defined in Bushee (1998, 2001).
1/3/5 Year Total Shareholder Return	tsr_1/3/5	The 1/3/5 year cumulated total shareholder return over the fiscal year period ending on datadate. We require at least 50 observations for 1-year TSR, 100 extra for 3 year, and 100 more for 5 year.
Vega	vega	The calculated incentive vega for the CEO or highest paid executive (and if the CEO is missing in the Execucomp dataset, then the highest paid employee). Note, these calculations come from John Kepler, and are at the executive level, and the data starts in 1992.

Appendix D Description of Doubly Robust Estimator

I model one-year out activism events over the rolling-window SMOTE-samples using logit-lasso, which predicts treatment using the following penalized cross-validated model:

$$\hat{\beta} = \text{argmin} \left\{ - \left[\frac{1}{N} \sum_{i=1}^{N} y_{i,t} \cdot \left(X_{i,t}^{T} \beta \right) - \log \left(1 + e^{X_{i,t}^{T} \beta)} \right) \right] + \lambda ||\beta||_1 \right\} \tag{3}$$

Here the outcome variable y_{it} is an indicator variable for whether the firm is subject to an activism event and λ is the penalty weight applied to the coefficient values for each standardized variable in the covariate matrix X_{it} alleged to drive the activism selection process, as well as indicators for the Fama-French 48 industry classification. An optimal value of λ (λ^*) is identified to minimize the out-of-sample mean squared prediction error using 10-fold cross-validation.[28] I calculate the propensity score for each firm i in the underlying sample as the predicted value from the fitted coefficients $\hat{\beta}$ for the year prior to the treatment year at λ^*.

The doubly-robust difference-in-differences estimates are computed for the relative years from $t = -4$ to $t = +5$ around each treatment year using the fitted propensity scores. The sample for the final estimate includes all firms treated in that year, as well as all firms that have not faced an activist event within the sample period.[29] I additionally require that any included firm in the analysis has observations for the three-year period surrounding the treatment year. For each relative year $t \in \{-4, 5\}$ the doubly-robust treatment effect estimate is equivalent to:

$$\tau_t^{dr} = \mathbb{E} \left[\left(w_1^p(D) - w_0^p(D, X; \pi) \right) \left(\Delta Y - \mu_{0,\Delta}^p(X) \right) \right] \tag{4}$$

where $w_1^p(D) = \frac{D}{\mathbb{E}[D]}$ is the treatment indicator scaled by its expected value, $w_0^p(D, X; g) = \frac{g(X)(1-D)}{1-g(X)} / \mathbb{E} \left[\frac{g(X)(1-D)}{1-g(X)} \right]$ are inverse probability weights applied to the control units, using our estimate of the propensity score $g(X)$, ΔY is the difference between the outcome variable in year t and the value of the outcome variable in the reference period,[30] and $\mu_{0,\Delta}^p(X)$ is the prediction for the

[28]The cross-validation procedure is repeated multiple times, averaging the out-of-sample prediction error for each value of λ, to ensure that the stochasticity in the splitting procedure does not drive the results.

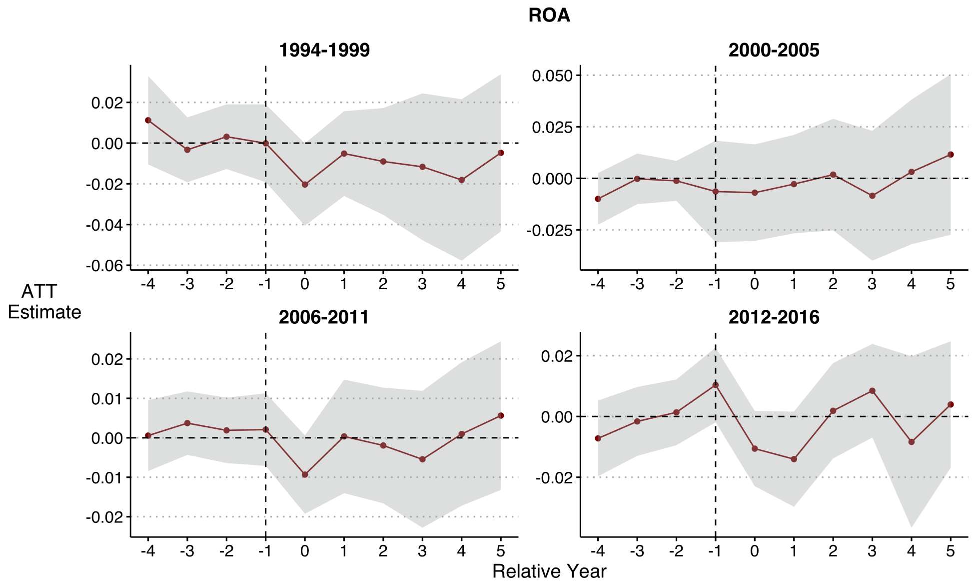
[29]I require that potential control firms for the outcome model have not been subject to an activist event for the preceding five years, or for the subsequent five year period.

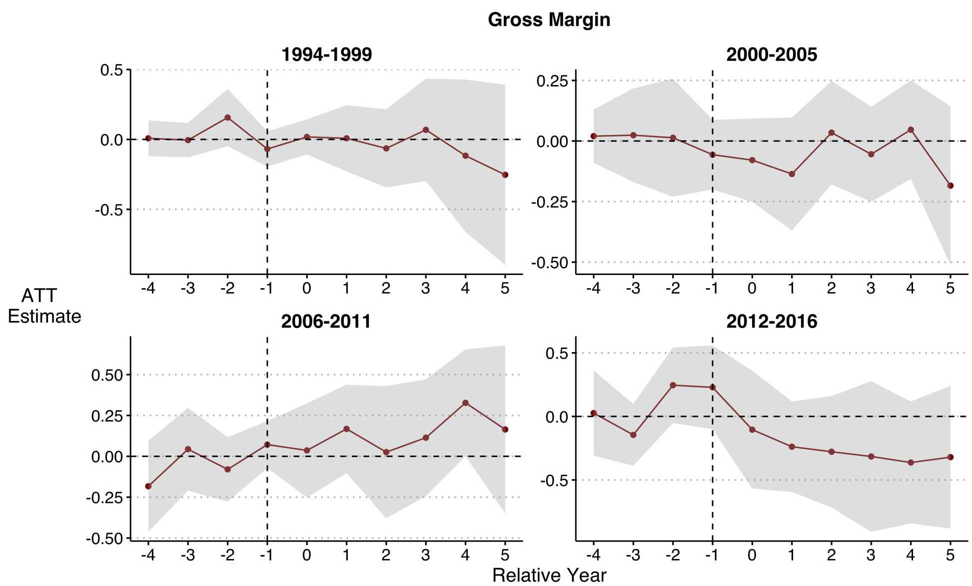
[30]Following Callaway and Sant'Anna (2020), we set the reference period to the year before treatment ($t = -1$) for

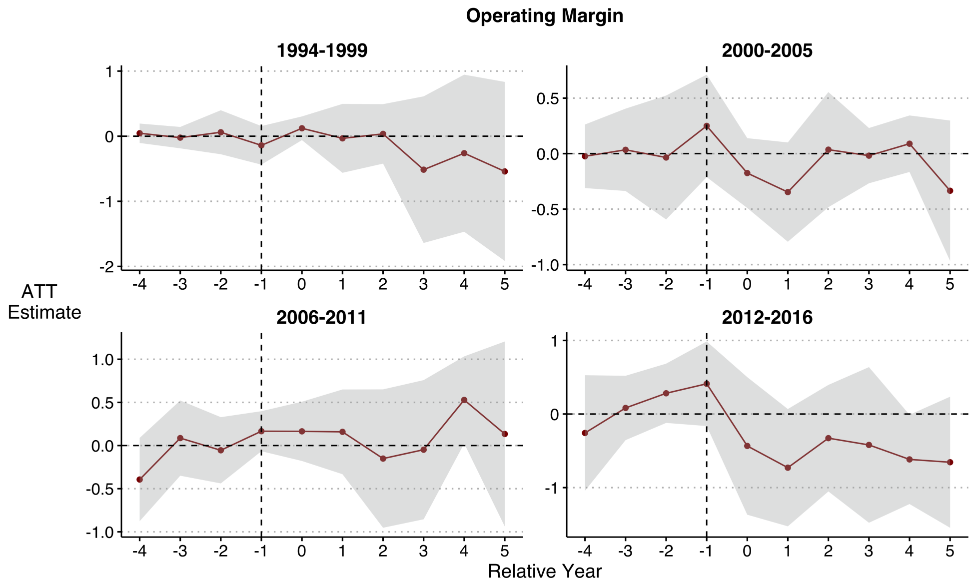
value of ΔY derived from an outcome regression model fit over the control units only. Given that we have only a handful of covariates for the outcome regression models, I follow Sant'Anna and Zhao (2020) and use a simple linear OLS model for each outcome with the dependent variable-specific covariates identified in the literature.

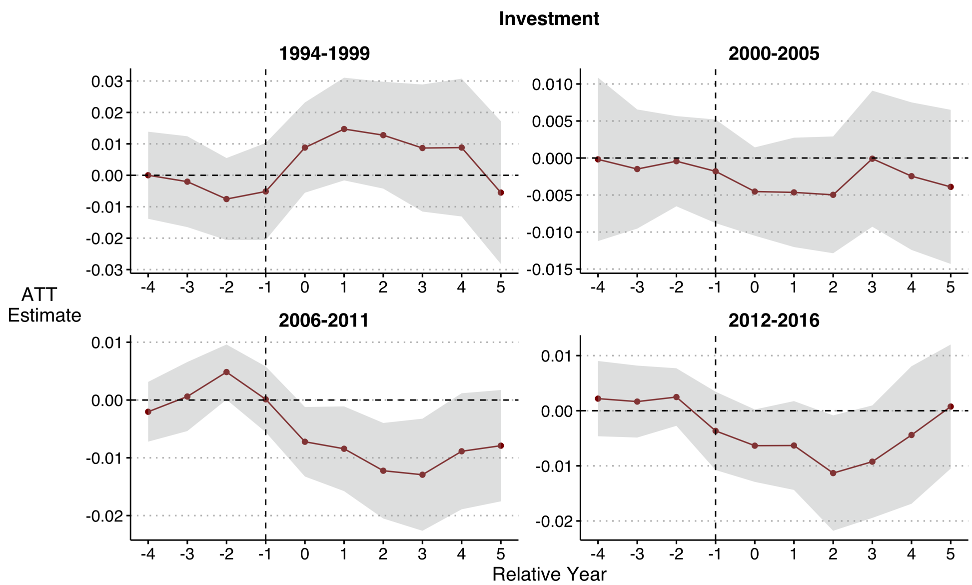
all post-treatment years, and the lagged year for pre-treatment observations.

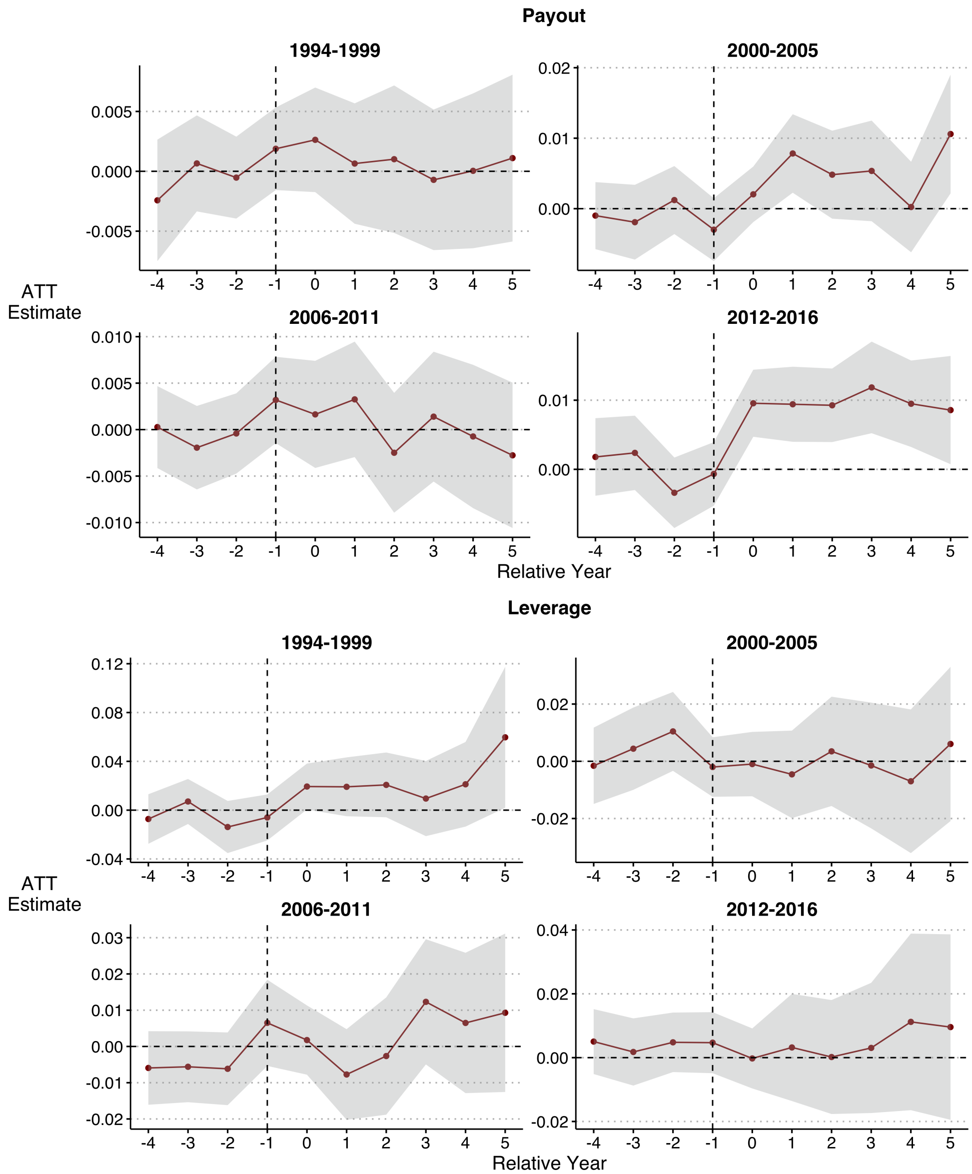
Appendix E Event Study Plots By Date Range and Variable






Operating Margin
1994-1999
2000-2005
2006-2011
2012-2016
ATT
Estimate
Relative Year


Investment
1994-1999
2000-2005
2006-2011
2012-2016
ATT
Estimate
Relative Year



Payout
1994-1999
2000-2005
2006-2011
2012-2016
ATT Estimate
Relative Year
Leverage
1994-1999
2000-2005
2006-2011
2012-2016
ATT Estimate
Relative Year

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