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Hedge Fund Activism in the United Kingdom

An Empirical Analysis

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Abstract

In this study, we investigate if hedge fund activists in the United Kingdom improve the operating performance or alter the capital structure of target firms. We do this by examining operating performance and capital structure of activist targets relative to non-target matched peers pre and post activist intervention. Our data sample consists of 62 activist campaigns, which occurred in the period 2010-2015. In order to strengthen our research we use two different methods of matching targets and peers that isolate the effect of activist intervention in two different ways. Our findings show that operational performance improves in target firms relative to non-target peers during the first two years after the activist intervention. These findings are consistent with previous research.

Furthermore, we find leverage increases in target firms relative to non-target peers during the first two years after the activist intervention. Moreover, we find that target firms increase investments relative to non-target peers, however we find no evidence suggesting any increase in payout ratio.

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Tutor*: Mariya Ivanova*

Keywords*: Hedge fund activism, Shareholder activism, Agency Theory, Operational Performance, Capital Structure*

Table of Contents

1. Introduction 3
1.1 Background *3*
1.2 Purpose *4*
1.3 Contribution *5*
1.4 Limitation *6*
2. Literature Review 6
2.1 Previous Research *6*
3. Framework for Activism and Hypotheses Development 8
3.1 Hedge funds as activists *8*
3.2 Demands by activist hedge funds *8*
3.2.1 Home Depot and Relational Investors 9
3.3 Theoretical Framework *9*
3.3.1 Agency Theory 9
3.3 Hypotheses Development *11*
4. Method 12
4.1 Accounting Measures *12*
4.2 Research and Statistical Methods *13*
4.3 Methods of Matching *14*
4.3.1 Propensity Score Matching 14
4.3.2 Performance Matching 14
4.4 Statistical method *15*
4.5 Presentation and Expectation Results *16*
5. Data 17
5.1 Sample selection *17*
5.2 Data Collection *18*
5.3 Industry classification *18*
5.4 Descriptive Statistics *19*
6. Results 20
6.1 Effect of hedge fund activism on Operational Performance *20*
6.2 Effect of hedge fund activism on Leverage, Investment Level and Payout Ratio *25*
7. Analysis & Discussion 28
7.1 Evaluation Results *28*
7.1.1 Effects of hedge fund activism on operational performance 28
7.1.2 Effects of hedge fund activism on Leverage, Investment Level and Payout 30
7.2 Robustness check and sensitivity analysis *32*
7.2.1 Using two methods of matching 32
7.2.2 Removed outliers 32
7.3 Research method discussion *33*
8. Conclusion and Future Research 34
8.1 Conclusion *34*
8.2 Future Research *35*
References *36*
Appendix 39

1. Introduction

1.1 Background

Shareholder activism, in all forms, is a mechanism of governance, where one or several shareholders seek to influence decisions of corporate boardrooms and management to create shareholder value. The phenomenon of shareholder activism gained popularity in literature during the 1980s and has from the mid-1990s to present day become widespread (Denes et al, 2017). However, not all activism comes equal. Much of the shareholder activism depicted in previous research consists of interventions made by pension and mutual funds, who are subject to much regulation and sometimes conflict of interest (Brav et al, 2008). These forms of shareholder activism have not proven to be especially efficient and have not lead to significant improvements in performance of target firms (Brav et al, 2008; Clifford, 2008).

In later years, increased focus has been put on hedge fund activism (Brav et al, 2008; Clifford, 2008; Bebchuk et al, 2015; Denes et al, 2017). There is no exact definition of a hedge fund, as it is not a legal entity in itself. However, the definition proposed by Brav et al. (2008) is:
"*(1) They are pooled, privately organized investment vehicles; (2) they are administered by professional investment managers with performance-based compensation and significant investments in the fund; (3) they are not widely available to the public; and (4) they operate outside of securities regulation and registration requirements."*

Activist hedge funds have been proven to be far more effective than other forms of shareholder activists (Brav et al, 2008; Clifford, 2008; Bebchuk et al, 2015; Denes et al, 2016), with previous research finding activist hedge funds are able to produce both abnormal portfolio returns for their investors, as well as improved operational performance for target firms.
However, although the effects of hedge fund activism are becoming well-documented, the presence of these actors is subject to much controversy (George and Lorsch, 2014; Coffee and Palia, 2016). Simply put, many view activists as propaganda-breeding, short-term oriented and overconfident investors, with too much influence. It is argued that these interventions are value-destroying, because the activist hedge funds seek short-term gains at expense of long-term value creation (Financial Times, 2016). A most recent example of this is when the chairman of AB Volvo, Carl-Henric Svanberg, commented on the Swedish activist hedge fund Cevian: "*What we know is that a short-sighted activist without significant competence have sold their shares and have been replaced by an industrial owner […] This must fundamentally be good (for Volvo)*" (Dagens Industri, 2018).

There is, however, no theoretical explanation for these claims (Bebchuk et. al 2015). The claims assume markets are inefficient, as otherwise increased share prices resulting from the activist campaign would only reflect the short-term improvement and not the long-term value destruction. The claims are thus based exclusively on empirics (Bebchuk et al, 2015). Bebchuk et al. (2015) exemplify the case of activist investor David Einhorn and Apple, where Einhorn initiated an activist campaign to pressure Apple into paying out some of its excess cash to shareholders. Opponents of hedge fund activism thus claimed this was an example of short-term gains at expense of long-term value creation.

In short, to answer whether hedge fund activism increases efficiency in markets and promotes shareholder value (Brav et al, 2008; Bebchuk et al, 2015) or if it is a trend sacrificing long-term value creation for short-term gains (George and Lorsch 2014), more research is needed on the subject, as we aim to supply with this study.

1.2 Purpose

This study aims to find out how campaigns by activist hedge funds affect target firms the years following the activist intervention. The research is based exclusively on listed, or previously listed, firms on the London Stock Exchange (LSX). However, the activist hedge funds themselves may or may not be located in the United Kingdom. The approach for our analysis is similar to that of Brav et al. (2008), who trace the operational performance and capital structure decisions of target firms the years preceding, as well as proceeding the activist intervention. The development of performance is traced through comparing our sample of target firms to peers of the same size, industry and book-to-market, as well as to peers of similar performance before the intervention occurs. By examining this development, focusing on operational performance and capital structure, we are able to test the prediction that activist hedge funds, on average, generate improved operational performance through their campaigns. Our research question which we aim to answer is, thus:

"Do activist hedge funds improve the operational performance of the firms they target?"

In addition, we will examine the idea that activist hedge funds affect capital structure decisions in target firms. Thus, additionally, we aim to answer:

"*Do activist hedge funds affect the capital structure in target firms?"*

1.3 Contribution

The impact of hedge fund activism has become of increased importance since the recent financial crisis. More investors have chosen to since invest with funds specializing in such strategies and between 2009 and 2014 the estimated Assets under Management (AUM) for activist hedge funds increased from $36.2bn to $112.1bn (JP Morgan, 2015). This represents a percentage increase of roughly 210% in just five years. As described in the background section, it is also a very controversial topic.

Much research has previously been done on the topic of shareholder activism as a general concept (Vogel 1983; Pound, 1989; Smith, 1996; Black, 1998) and in the past decade, studies have begun specializing on hedge fund activism in particular (Brav et al, 2008; Clifford, 2008; Bebchuk et al, 2015). All these studies have primarily, or even exclusively, focused on the US markets and few have looked beyond to other parts of the world. In Europe, the UK has by far been the most heavily targeted county for hedge fund activism, and is the fastest growing market for such investments outside of the US (Citibank, 2013).

Because of this, we intend to focus our analysis on the UK market. Thus, our study contributes, by being one of the first to exclusively focus on this geographical setting. By doing so, we may be able to see whether there are different outcomes on the effect of hedge fund activism, depending on which country the activism occurs in. Furthermore, the study also contributes by further developing the research on hedge fund activism. This is highly relevant, as hedge fund activism in particular has been shown to have a positive effect on target firms (Brav et al, 2008; Clifford 2008; Bebchuk et al, 2015). Finally, we only study activist campaigns that have occurred after the recent financial crisis. Thus, we are able to provide more recent evidence, which may indicate whether increased regulation and scrutiny have had an impact on the effectiveness of hedge fund activism.

1.4 Limitation

We limit our study to companies listed on the London Stock exchange (LSX). We have collected data from Thomson Reuters EIKON Datastream for the period 2008-2017, but focus exclusively on activist campaigns that have occurred 2010-2015. The reason for the extended years in the data collection is to enable analysis of years preceding, and in the later years, proceeding the activist hedge fund's intervention.

We only study hedge funds and exclude other actors such as mutual and pension funds, as well as individual investors, management buyouts, etc. In addition, we do not look at what kind of activism the activist hedge funds apply, whether it be CEO turnover, board representation, force sales, assets divestures, etc. Rather, we view the entire spectrum, to achieve a relatively general approximation of the effects activist campaigns appear to have on target firms. The primary reason for this limitation is insufficiency of data. Activist hedge funds in the UK do not need to publicly disclose their intentions (Becht et al, 2008) and, thus, to perform a more in-depth analysis, private fund information would be required.

We have also made limitations on which industries to include in our sample and control group. First off, the TRBC (Thomson Reuters Business Classification) was chosen as standard for industry classification ahead of a more conventional industry classification, such as GICS (Global Industry Classification Standard). This was done for the simple reason that the latter lacked data for several of our sample firms. Furthermore, certain adjustments have been made to our total sample for practical purposes. First, all companies falling under the two-digit TRBC classification code 55 (financials) have been excluded from our study. These companies have been excluded due to the difficulty in differentiating between operating and financial metrics for such firms. Moreover, all companies lacking financial data (oil exploration, etc.) have also been excluded. This adjustment removed all firms in the two-digit TRBC classification code 59 (Utilities) and thus our final sample includes no companies within this industry either.

2. Literature Review

2.1 Previous Research

Previous research on shareholder activism can be divided into two separate time periods. The first period stretches from the late 1980's, throughout the 1990's until the first half of the 2000's. This research, with some exceptions, looks at shareholder activists as a homogenous

group (including pension funds, mutual funds and other institutions). The findings from this period find, to a large extent, that shareholder activism has no or little material impact on target firms (Vogel 1983; Pound, 1989; Smith, 1996; Black, 1997). Karpoff (2001), in a summarizing literature review, concludes that "*[…] most of those emphasizing changes in share values, earnings, or operations, in contrast, characterize shareholder activism as having negligible effects on target companies.*" The root cause of why shareholder activists are unable to create value is, according to him, because they target change in policies and operations that have little or no potential effect on firm value. Furthermore, he explains that shareholder activists may fail because they do not put sufficient effort into their endeavors. Lipton and Rosenblum (1991) present another explanation for the inability of shareholder activists to impact firm value. They argue that shareholder activists lack adequate competence to improve the decisions of managers and, thus, may harm the long-term financial health of target firms. Wohlstetter (1993) agrees with this argument and argues that activists tend to focus on short-term gains and do so at expense of long-term value creation.

The second time period of research stretches between the second half of the 2000's to present day. This period differs from the previous one with regards to two key aspects: Scope and findings. The research during this period has more distinctly divided shareholder activists into subgroups, and have found results suggesting shareholder activism can be effective (Brav et al, 2008; Clifford, 2008; Bebchuk et al, 2015; Denes et al, 2017). From the research, the most effective form of shareholder activism appears to be hedge fund activism (Brav et al, 2008; Clifford, 2008; Bebchuk et al, 2015). Brav et al. (2008) find that firms targeted by activist hedge funds experience increased operational performance two years post the activist intervention. Similar results are found by Bebchuk et al. (2015), who find that increased operational performance lasts as long as five years post the activist intervention. Furthermore, they find no evidence that this increased performance comes through the kind of short-term behavior described in previous research on shareholder activism (Lipton and Rosenblum, 1991; Wohlstetter, 1996).

In more recent research, new topics within hedge fund activism have been examined. Brav et al. (2016) focus on corporate innovation and find that innovation output increases during the five-year period following the activist intervention. Bebchuk et al. (2017) examine boardroom settlements and find evidence that when activists have credible threats, they are more likely to win board seats in proxy fights. Coffee and Palia (2016), on the other hand, find severe cutbacks in target firms' long-term investments post the activist intervention.

Although much research has been done on hedge fund activism the past decade, the views on the phenomenon remain divided (George and Lorsch, 2014). More research is needed to develop the claims from both proponents and opponents, as well as to make the subject as a whole more nuanced.

3. Framework for Activism and Hypotheses Development

In the following section, we define the qualities of hedge funds as activists, the changes they demand and the theoretical framework surrounding activism. From this we develop our hypotheses.

3.1 Hedge funds as activists

First off, hedge funds are subject to far less regulatory requirements than both pension and mutual funds. Hedge funds have long-term and highly capable investors, which allows them to take highly concentrated positions. Moreover, they can use derivatives and leverage as tools and can hold positions for long periods without withdrawals. All of these are traits which are typically not prevalent in pension or mutual fund (Brav et al, 2008). Furthermore, hedge fund managers are highly incentivized to produce significant returns, as most of their income tends to be derived from performance fees (Goetzmann et al, 2003). Finally, hedge funds (unlike pension funds) are typically not subject to political pressures or other conflicts that may restrict their ability to promote activist campaigns in target firms (Romano, 1993).

3.2 Demands by activist hedge funds

There are various demands an activist hedge fund may make (Brav et al, 2008). Demands may be related to capital structure, where activist hedge funds pressure target firms to alter payout policy, leverage or investment levels. Furthermore, campaigns may be constructed to affect governance in target firms. This includes de-staggering boards, removing poison pills, firing CEOs, changing compensation for management, etc. Another demand may be capital re-allocation, which includes pressuring firms to sell particular assets or a business division. These demands may be made publicly, may include hostile takeovers or proxy contests, or may simply be the result of active communication with corporate management (Brav et al, 2008).

Brav et al. (2008) describe the latter as the most common form of hedge fund activism. This includes the activist hedge fund believing the target firm is fundamentally undervalued and that there are improvements to be made in order to maximize value for shareholders. To illustrate

such a situation, the case of Home Depot and Relational Investors provides a framework (George and Lorsch, 2014).

3.2.1 Home Depot and Relational Investors

George and Lorsch (2014) describe how the activist hedge fund Relational Investors were able to provide new information to the management and board of Home Depot to influence change. The process began when Relational Investors found an accounting error in Home Depot's accounts and subsequently wrote a letter to the board asking for an opportunity to meet.

Home Depot met with Relational Investors, and once they understood the accounting errors, they decided to follow Relational Investors' plan for improvement. This would come to include the departure of CEO Robert Nardelli, appointment of a new member of the board representing Relational Investors and embracing several changes proposed by the activist investor. The changes would come to significantly increase Home Depot's performance, and thus, Relational Investors were able to create substantial shareholder value.

Whether or not this case is an anomaly, it provides a framework for how activist investors may provide value in target firms (George and Lorsch, 2014).

3.3 Theoretical Framework

The theoretical theme underlying hedge fund activism begins with the consequences resulting from the separation of ownership and control in public companies. This idea was originally developed by Berle and Means (1932) in their work "*The Modern Corporation and Private Property"*, where the two authors explain how high dispersion in ownership leads to loss of control for shareholders. One of the main theories developed from this argument is *agency theory*, which provides a framework to describe why activist hedge funds may be able to increase shareholder value.

3.3.1 Agency Theory

Agency theory originates from the idea that there exist varying goals and agendas in a contractual relationship between two different parties: a principal and an agent. This relationship occurs when the principal and agent agree on an arrangement, where the principal delegates work to the agent and the agent is responsible for acting in the principal's interest. When the agent acts in its' own interest and at expense of the principle, a troublesome situation occurs. To remedy or control the situation, the principal may need to monitor the agent, which

of course comes at a cost – an agency cost. This contractual relationships can exist in various contexts, but for our study the shareholder represents the principal, whilst the corporate manager represents the agent.

It is of high interest to discuss the implications from agency theory when researching hedge fund activism, as this provides a situation where one or several shareholders in some manner act as monitors. We will go on to explain a few distortions in managerial incentives that may sub-optimize value creation for shareholders.

Although theory of financing predicts that, in efficient markets, firms will invest in all NPV positive projects it can finance and avoid negative NPV projects (Modigliani and Miller 1958), managers may be investing in projects that promote their own self-interest at expense of shareholders (Jensen, 1986; Stulz 1990). Jensen (1986) describes several reasons why management may be biased toward doing precisely so. One reason, he explains, is that managers' power grow with increased resources under control. This resembles the idea of empire building, where managers grow firms beyond their optimal sizes to create larger organizations for their personal benefit, but by doing so decrease operating performance and reduce firm value (Hope and Thomas 2008). Furthermore, managers may be compensated for increased growth in sales and may, thus, be incentivized to increase investments, regardless if optimal for shareholders (Kevin Murphy 1985; Jensen 1986).

The issue of inefficient capital allocation does not only concern managers investing in excess of what is profitable, but also concerns the reluctance to divest underperforming assets (Denis et al, 1997; Bertrand and Mullainathan 2003; Owen et al, 2010). Managers have a natural reluctance to divest, as they risk losing benefits associated with managing large firms (Owen et al, 2010). Furthermore, it appears managers do not voluntarily reduce levels of diversification in firms they control, but rather do so in light of external corporate control threats (Denis et al, 1997). Thus, it seems, refocusing is a consequence of external monitoring.

Another reason managers may be reluctant to divest assets is to reduce the need of making difficult decisions (Bertrand and Mullainathan, 2003). This is referred to as "the quiet life", where managers prefer maintaining firm size rather than having to make decisions on whether to expand or divest.

Because managers prefer to re-invest free cash flows, other activities such as dividend payouts are sacrificed at the expense of shareholders (Jensen 1986; Stulz 1990). Jensen (1986) describes

that firms with excess cash are especially prone to overinvestments by managers. To reduce this agency cost, debt can be issued to tie future free cash flows to the repayment of debt, which unlike dividend payouts cannot be delayed (Jensen, 1986). There may, however, be increased costs to issuing debt as well, as firms may be left financially constrained to invest in profitable projects once they present themselves (Jensen, 1986; Stulz 1990).

By summarizing the different scenarios in which agency costs may occur, there appears to be theoretical support for the need of monitors to reduce the agency costs that occur from the separation of control between managers and owners (Denis et al, 1997). Brav et al. (2008) propose that activist hedge may be especially well-suited to perform the monitoring as they are more independent, flexible and incentivized than other kinds of investors.

3.3 Hypotheses Development

The theoretical framework presents an idea of why activist hedge funds may be able to reduce agency costs in target firms. We conclude that activist hedge funds should be able to act as monitors, thereby incentivizing managers to act in accordance with shareholder interests'. We therefore predict firms targeted by activist hedge funds should experience improved operational performance. To test this hypothesis we define two different periods: A pre-event and post-event period, where event describes the year the activist intervention occurs. The pre-event period is defined as one year prior to the activist intervention and the post-event period as the two years post the activist intervention. Thus, our first hypothesis is formulated as follows:

H1: Relative to non-target peers, firms targeted by activist hedge funds will improve their operational performance between the pre- and post-event period.

Furthermore, activist hedge funds may also be able to reduce agency costs in target firms in other ways. We predict activist hedge funds are able to increase shareholder value by altering capital structure decision in target firms and these alterations are predicted to follow the theoretical framework. Moreover, we define the same pre- and post-event periods as for our first hypothesis. For our second hypothesis, we propose the following:

H2: Relative to non-target peers, firms targeted by activist hedge funds increase leverage, reduce investments and increase payouts between the pre-event and post event period.

4. Method

4.1 Accounting Measures

Table 1 displays the accounting measures used to test our hypotheses. The first four are defined as measures of operational performance. The use of operating income is preferred to earnings for measures of performance for two reasons (Barber and Lyon, 1996). First off, special items that may obscure operating performance are more prevalent within earnings measures than within measures of operating income. Second, earnings are affected by the level of leverage, as increased leverage generally leads to higher financial costs. Operating income, on the other hand, is unaffected by leverage (Barber and Lyon, 1996).

Return on Assets and *EBITDA Margin* are two of the primary measures used by Brav et al. (2008) and are often used for examining operational profitability in companies (Barber and Lyon, 1996; Klein and Zur 2006; Clifford 2008). Return on Assets has been defined as EBITDA divided by Lagged Assets and EBITDA margin as EBITDA divided by Sales, following Brav et al. (2008). In addition to Brav et al. (2008), we have chosen to include two additional measures for operational performance. The first, *Net Working Capital to Sales*, defined as Non-cash current assets less short term payables divided by Sales, has been added to our analysis as a measure for operational efficiency. The second, *Sales Growth*, defined as the percentage increase in sales between each year, has been added as a measure for operational growth.

In addition to the analysis of operational performance, we examine three additional measures to analyze the effect on capital structure decisions.

First, *Leverage* is defined as Long-term Debt divided by Total Capital (where Total Capital is defined as the sum of book value of equity, interest-bearing debt and minority interests).

Capex-to-Sales is defined as Capital Expenditure divided by Sales. This measure has been added as a proxy for investment level.

Finally, *Payout Ratio* is measured as total dividend payout divided by lagged market value of equity. This measures provides a perspective on the level of shareholder distribution.

Table 1. *Accounting Measures*

Measure	Type	Definition
Return on Assets	Operational Profitability	$ROA = \frac{EBITDA_{t+1}}{Assets_t}$
EBITDA Margin	Operational Profitability	$EBITDA\ Margin = \frac{EBITDA_t}{Sales_t}$
Net Working Capital-to-Sales	Operational Efficiency	$NWC\ \%\ of\ Sales = \frac{NWC_t}{Sales_t}$
Sales Growth	Operational Growth	$Sales\ Growth = \frac{Sales_{t+1}}{Sales_t} - 1$
Leverage	Leverage Ratio	$Leverage = \frac{LongTerm\ Debt}{Total\ Capital}$
Capex-to-Sales	Investment Level	$Capex\ \%\ of\ Sales = \frac{Capex_t}{Sales_t}$
Payout Ratio	Payout	$Payout = \frac{Div.Payout_{t+1}}{MV\ Equity_t}$

4.2 Research and Statistical Methods

To analyze whether firms targeted by activist hedge funds improve operational performance, we will apply the approach used by Brav et al. (2008). In this approach, firms targeted by activist interventions are matched with a control sample of non-targets. To do this, we apply two different methods of matching: A Year-by-Year size, industry and book-to-market match, proposed by Brav et al. (2008) and a pre-event Performance match, proposed by Barber and Lyon (1996).

Once matched, we investigate the difference in performance of our accounting measures to gain insight on the development of target firms compared to non-targets. To do this, we use panel data, where the difference in performance has been measured from one year prior to the hedge fund activist's intervention up until two years post intervention. There are some issues we face by examining the development over several years. First, it is not uncommon that activist hedge funds push for the divestiture of a firm to a potential buyer. Thus, for some of the firms included in our sample, data is limited to certain years. To display comparable measures for all years, the difference between each year post event and the difference pre-event will be examined for

all firms with data available both respective years. More specifically, if a post event year has fewer observations than the pre-event year, only the matched pairs available in both periods will be tested against each other.

4.3 Methods of Matching

As mentioned above, we use two different matching methods to find appropriate peers to compare with our target firms. Whilst many various methods exist for matching, we have chosen to focus on (1) Propensity Score Matching and (2) Performance matching. The match point is set as the year prior to the event year, where the event year is defined as the year of the activist intervention. This year is recommended as starting point by Barber and Lyon (1996), however unlike Brav et al. (2008), who choose two years prior. In the cases where no matches are found, we exclude them from the sample. Another solution would of course be to ease the matching criteria. However, as our sample only includes 62 firms, deviations in matching criteria for only a few number of firms could greatly alter the results, hence, we exclude them.

4.3.1 Propensity Score Matching

Propensity Score Matching (PSM) is a statistical method common for matching a treatment group with a control group (Caliendo and Kopeinig, 2008). PSM finds the most relevant match for each treatment individual based on a set of characteristics. In our study, we use this method to identify the best matched pairs on size, industry and book-to-market, to conduct a year-by-year comparison measure. Size is defined as the logarithm of market value of equity and book-to-market is defined as the book value of equity divided by the market value of equity. The match between targets and their respective peers are made on, with regard to the conditions presented above, the pre-event year. The development is then examined from this point up until two years post event.

4.3.2 Performance Matching

In the Performance match, peers are chosen based on similar performance in regards to the variable being tested. Similar performance is, in this context, defined as lying between 90% and 110% of the variable value, as proposed by Barber and Lyon (1996). If possible, each peer is contained within the same two-digit TRBC code to adjust for differences in economic nature between industries, which may otherwise skew results. If two or more peers have been found under the former conditions, the most appropriate peer will be selected based on the size dimension, defined as the logarithm of market value of equity.

Through this matching method, we are able to examine the firm and its' matched peer from an almost identical starting point and follow the development up until two years post event.
As an exception, the Performance match for Sales Growth is performed in a different manner than previous measures. Instead of the matching firms on sales growth, the match has been done on Return on Assets. This aberration is motivated by sales growth being too volatile to accurately find matched pairs within the specified limitations.

4.4 Statistical method

To perform our analysis, we use a paired samples t-test. This test is commonly used when seeking to evaluate potential effects of a certain treatment (Newbold et al, 2013). The t-test analyses the average difference in the mentioned accounting measures between target firms and non-target peers. It then evaluates whether this results is significant. The idea with using a paired samples t-test is to isolate the treatment effect by controlling for certain characteristics deemed to affect the outcome (Newbold et al, 2013). The test statistic of the paired samples is as follows below:

$$t = \frac{\overline{D} - \mu_0}{S_D/\sqrt{n}} \sim t_{n-1}$$

Where,

$$D = Accounting\ measure_{target\ firm_t} - Accounting\ measure_{control\ firm_t}$$

.The paired samples t-test requires the following criteria to be met (Newbold et al, 2013):

1. The variables must be measured on a continuous scale.
2. The sample of matched pairs must be independently selected from each other.
3. The differences between the matched pairs must be normally distributed.

The first assumption is met as the accounting measures used in this thesis are per se measured on a continuous scale, as they can assume any value within a specified or unspecified interval. It is also fair to assume that the second assumption is met as the sample selection, described under the Data section, is random. Furthermore, all target firms are removed from the control group, to avoid that target firms match against other targets. However, we cannot completely rule out that an activist intervention has occurred in one of our control firms, as our sample may not contain the entire population of target firms.

Regarding the third assumption, the distributions of our samples are unknown, but as our sample exceeds 30 observations we make use of the Central Limit Theorem (CLT). The CLT tells us that: If the sample and control group are independently and randomly drawn, and follow the same distributions, the distribution of matched pairs tends to approach the normal distribution as the number of observations becomes sufficiently large. The test-statistic is as follows below:

$$Z = \frac{\overline{D} - \mu_0}{S_D/\sqrt{n}} \sim N(0,1)$$

As using the CLT requires that the sample of matched pairs follow the same distributions, it is important to be cautious of outliers. This is because, if outliers are present, it could be questioned whether the sample of matched pairs follow the same distributions. In the analysis section we therefore perform a robustness check, where we evaluate whether there are potential outliers in the samples. We also analyze their potential impact by comparing our main results with the outlier-adjusted results.

4.5 Predicted Signs

In Table 2, we provide guidance on how we expect different measures to develop between the pre- and post-event period.

For our first hypothesis, to test whether operational performance is improved, we expect the difference between target and non-target firms to be positive in Return on Assets, EBITDA Margin and Sales Growth between the pre and post event periods. On the other hand, for NWC-to-sales we expect the difference to be negative, as lower levels of NWC-to-sales indicate higher operating efficiency.

For our second hypothesis, to test whether various capital structure measures are affected, we expect the difference between target and non-target firms to be positive for Leverage and Payout Ratio. However, because we want to test whether Investment Levels decrease, we expect the difference between target and non-target firms to be negative for capex-to-sales.

Table 2. *Predicted Signs*

Accounting Measure	Predicted Signs
Return on Assets	+
EBITDA Margin	+
Net Working Capital % of Sales	–
Sales Growth	+
Leverage	+
Capex-to-Sales	–
Payout Ratio	+

5. Data

5.1 Sample selection

The initial sample selection process included finding what UK-based firms have been targeted by activist hedge funds during the period 2010-2015. As there is no UK counterpart to the US 13D[1] and therefore no public database in the UK containing information on past activist campaigns, we deem media coverage to be the best available source of information and proxy for determination of activist intent. To find sample data, we therefore used the Factiva search engine with the key words "activist investor" and "activist hedge fund", filtering articles based on region (United Kingdom) and three main subject sections (commodity/financial market news, corporate/industrial news and economic news). This resulted in roughly 5000 news articles and from these, 82 activist campaigns were extracted.

Through the Thompson Reuters EIKON database, an additional 19 campaigns were found, leading the total sample to include 101 campaigns. These campaigns were chosen by downloading the available data of the "Activist Campaign" section in EIKON for the period 2010 – 2015 in the UK and only selecting the campaigns that we had not found from our Factiva searches. However, not all companies were included: Financial companies, companies without public financial information and companies with negative revenues (exclusively utility companies) were excluded. In addition to this, a few target firms in the sample involved campaigns led by funds/investors with higher regulatory requirements than hedge funds such as mutual funds and pension funds. These campaigns were also excluded, as we have limited our study to only examine activist campaigns led by hedge funds. Our final sample selection is presented in Table 3, together with the amount of firms removed due to each limitation. Furthermore, although the total sample size equals 62 firms, the number of firms for each year

[1] A 13D filing is an SEC filing that US investors acquiring more than five percent of a publicly traded company must file within 10 days of passing the threshold. The acquirer must disclose not only passing the threshold as in the UK, but also their intent with the acquisition. (E.g. conducting different forms shareholder activism)

we measure may differ, as some firms are delisted, demerged or acquired during the measurement period. To determine whether the activist was a hedge fund, we searched for self-classification on the activists' webpage. If no webpage or classification was found, we used the news article in the Factiva database together with the EIKON database to determine whether the shareholder activist was a hedge fund or not.

Table 3. *Sample Selection*

Criteria	Adjustments	Number of firms
Factiva	+82	82
EIKON Database	+19	101
Non-comparable industry	–9	92
Data unavailable/inadmissible	–12	80
Mutual/Pension fund campaigns	–18	62
Total		**62**

5.2 Data Collection

Once the sample of companies was finalized, relevant data was acquired for the period 2008-2017 through Thomson Reuters Datastream. Part of our sample included firms that have divested part of their business, have been delisted or have been acquired as a result of the hedge fund activist's intervention. We decided to keep the data for all firms that, even though being subject to any of the above, were either considered active by the Thomson Reuters database or had sufficient financial data for some of the years included in our analysis.

After the input data for the target sample was collected, we began collecting data for our control group. First off, ISIN-codes for all publicly traded firms on the London Stock Exchange (LSX) were downloaded. These codes were then used as inputs to receive data on the same accounting measures as for our sample. The control firms were then cleared to only include the same industries as the sample, thus excluding two-digit TRBC codes 55 and 59.

Due to the timing of this study, some of the smaller firms in the sample were yet to release their annual report for 2017. This, of course, leads to some firms missing data for this year. However, as no campaign measured has occurred after 2015, we are still in these cases able to record at least one year of post-activism performance, making these observations highly relevant nonetheless.

5.3 Industry classification

There are several different classification standards that could be used for a research paper such as ours. Our initial intention was to use the Global Industry Classification Standard (GICS)

system, as has been recommended for capital market research by previous research (Bhojraj et al, 2003). However, due to the effect hedge fund activism may have on target companies (delisting, divesture, etc.), sufficient amount of data could not be collected on our sample with this standard. Instead, the only standard that had available data for every single firm in our sample was the Thomson Reuters Business Classification (TRBC). We used the two-digit, economic sector code to classify each firm by industry.

Table 4. *Firms observed and industry*

Sector Code	Industry	Number of firms
51	*Basic Materials*	1
52	*Industrials*	21
53	*Cyclical Consumer Goods & Services*	18
54	*Non-Cyclical Consumer Goods & Services*	9
56	*Healthcare*	3
57	*Technology*	9
58	*Telecommunication Services*	1
Total		**62**

5.4 Descriptive Statistics

In Table 5, Descriptive statistics for the sample of target firms are presented and in Table 6, Descriptive statistics for the control group are presented.

We present both tables to give an overlook of the entire universe of firms. For the first three measures: Return on Assets, EBITDA Margin and NWC-to-sales, we see that the target firms in our sample appear to have higher levels of operational performance than the control group (a negative NWC-to-sales value indicates higher performance for target firms relative to non-targets). However, for sales growth, it seems that the median rate is higher among the control group than the target sample. Capex-to-sales appears to be lower for the sample than the control group. Both leverage and payout ratio appear to be on similar levels in both groups. Finally, the logarithmic market value of equity is somewhat higher for the target sample and book-to-market appears slightly higher for the control group.

Table 5. *Descriptive statistics - Sample*

	Mean	St. Dev.	Median	Quartile 1	Quartile 3
ROA	0.1379	0.2332	0.1142	0.0808	0.1713
EBITDA Margin	0.1416	0.1377	0.1265	0.0764	0.1899
NWC % of Sales	– 0.0308	0.1967	– 0.0308	– 0.1453	0.0748
Sales Growth	0.0397	0.1917	0.0297	– 0.0273	0.0987
Leverage	0.3117	0.2416	0.3071	0.0827	0.4622
Capex-to-Sales	0.0515	0.0620	0.0321	0.0174	0.0588
Yield	0.0322	0.0308	0.0293	0.0143	0.0425
LN MV of Equity	19.8724	2.4334	20.0459	17.9773	21.9006
Book-to-Market	0.7571	0.8348	0.5639	0.3295	0.9317

Table 6. *Descriptive statistics - Control group*

	Mean	St. Dev.	Median	Quartile 1	Quartile 3
ROA	0.0934	0.1914	0.1106	0.0549	0.1773
EBITDA Margin	0.1091	0.1929	0.1185	0.0506	0.2012
NWC % of Sales	– 0.0056	0.2335	- 0.0080	– 0.1163	0.1048
Sales Growth	0.1034	0.3079	0.0658	– 0.0214	0.1905
Leverage	0.3142	0.2294	0.2928	0.1245	0.4475
Capex-to-Sales	0.0702	0.1075	0.0365	0.0146	0.0770
Yield	0.0306	0.0384	0.0248	0.0079	0.0401
LN MV of Equity	18.4328	2.7119	18.1083	16.4467	20.2120
Book-to-Market	0.7928	0.9799	0.5082	0.2482	0.9475

6. Results

In the following two subsections, we present the results from testing our two hypotheses. We begin by following the development of each measure for each year of the measurement period. Thereafter, we compare each year post event, individually, to the pre-event year. To follow the count of matched pairs for each year and each measure, Appendix A and B provide guidance.

6.1 Effects on Operational Performance

The results for our measures on operational profitability, Return on Assets and EBITDA Margin, are presented in Table 7.

Return on Assets. This measure yields no significant results through the Year-by-Year match for any of the years measured. However, non-significant results show that the difference on the pre-event year is negative, meaning target firms, on average, have lower performance than non-target peers prior to the activist intervention. Following the development, the difference remains

stable on event year. One and two years post event the difference shrinks, indicating a relative improvement in performance for target firms. When comparing the first and second year post event to the pre-event year ((t+1)–(t–1) and ((t+2)–(t–1)), we observe non-significant positive differences, indicating that a relative improvement has occurred in target firms compared to non-target peers.

The Performance match for *Return on Assets* indicates significantly better performance by target firms relative to non-targets on event year. The difference experience a dip one year post event, but increases to a new high two years post event. The same holds true when comparing the second year post event to the pre-event year ((t+2)–(t–1)), where a significant improvement in performance for target firms relative to non-targets is observed. Comparing the first year post event to the pre-event year, we find a small, non-significant, improvement in performance.

EBITDA Margin. This measure displays no significant results through the Year-by-Year match for any of the years measured. Non-significant results show a small, positive, difference on the pre-event year, indicating a somewhat higher performance for target firms than non-targets. On event year, the difference experiences a dip. One year post event the difference has recovered and is miniscule, and two years post event there is no difference in performance between target and non-targets. A comparison of the first and second years post event to the pre-event year ((t+1)–(t–1) and (t+2)–(t–1)) yields no significant results.

The Performance match for *EBITDA Margin* indicates significantly higher performance for targets than non-targets on event year. The relative performance declines one and two years post event, however the difference remains positive, which indicates target firms have higher performance than non-targets for both years post event. Furthermore, the Performance match yields significantly positive differences when comparing the first and second years post event to the pre-event year ((t+1)–(t–1) and (t+2)–(t–1)).

The measure for operational efficiency, Net Working Capital-to-sales, and the measure for operational growth, Sales Growth, are presented in Table 8.

NWC-to-Sales. Through the Year-by-Year match, non-significant results display a positive difference on the pre-event year, which for this particular measure indicates target firms have *lower* performance than non-target peers. On event year, a substantial decline occurs and the difference turns negative, which thus indicates target firms have *higher* performance than non-target peers. The difference turns positive again one year post event, indicating target firms,

once again, have lower performance than non-targets. Two years post event the difference declines somewhat, however it remains positive, indicating performance of target firms remains lower than non-target peers. Comparing the first and second years post event to the pre-event year ((t+1)–(t–1) and (t+2)–(t–1)), non-significant results indicate target firms have improved their performance relative to non-target peers.

In the Performance match for *NWC-to-Sales*, non-significant results show the difference is negative on event year. The difference remains negative one year post event and two years post event the difference is significantly negative, indicating that target firms, on average, have higher performance than non-target peers.

When comparing the second year post event to the pre-event year ((t+2)–(t–1)), significant results display that the difference is negative, indicating target firms improve performance relative to non-target peers. Moreover, when comparing the first year post even to the pre-event year ((t+1)–(t–1)), non-significant results indicate a small relative improvement for target firms.

Sales Growth. Through the Year-by-Year match, results indicate target firms, on average, have significantly lower sales growth than non-targets on the pre-event year. The difference declines on event year, indicating further underperformance for targets relative to non-targets. One year post event the difference shrinks and continues to further shrink two years post event. However, both one and two years post event, the difference remains negative, indicating lower performance for targets than non-targets. When comparing the first and second year post event to the pre-event year ((t+1)–(t–1) and (t+2)–(t–1)), small positive differences are observed, however non are significant.

The Performance match for *Sales Growth* indicates target firms, on average, have significantly lower growth than non-target peers on the pre-event year. Non-significant results show target firms recover relative to non-targets on event year and one year post event, however two years post event, the relative performance declines. When comparing the first year post event to the pre-event year ((t+1)–(t–1)), significant improvements in sales growth are observed for target firms relative to non-target peers. Moreover, when comparing the second year post event and the pre-event year ((t+2)–(t–1)), non-significant relative improvement in performance is observed.

Table 7. Operational Profitability

	Operational Profitability							
	EBITDA/Assets				EBITDA/Sales			
	Year-by-Year PSM Match		(t–1) Performance Match		Year-by-Year PSM Match		(t–1) Performance Match	
	Diff w/ Match	*t*-statistic	Diff w/ Match	*t*-statistic	Diff w/ Match	*t*-statistic	Diff w/ Match	*t*-statistic
(t–1)	–3.06%	–1.057	–0.04%	–0.771	0.58%	0.174	0.01%	0.445
Event	–3.03%	–1.398	1.98%*	1.711	–0.70%	–0.282	3.79%**	2.124
(t+1)	–2.32%	–0.947	0.55%	0.521	–0.046%	–0.178	2.33%**	2.074
(t+2)	–0.44%	–0.172	2.69%**	1.958	0.00%	0.000	1.37%*	1.442
(t+1)–(t–1)	0.11%	0.059	0.42%	0.389	–0.43%	–0.255	2.29%**	2.028
(t+2)–(t–1)	1.77%	0.802	2.51%**	1.861	0.09%	0.054	1.33%*	1.380

Table 7 provides statistics of target firms *Operational Profitability* in excess of matched peers for one year before and two years after the activist intervention (event year). The two measures of operational profitability are defined as EBITDA/Assets and EBITDA/Sales. In the columns marked "Year-by-Year PSM Match" each target firm is matched year-by-year to a peer on size, industry and book-to-market characteristics. In the columns marked "(t–1) Performance Match", each target firm has been matched to a peer with similar performance in the measure tested. The column marked "Diff w/ Match" displays the average difference between all matched pairs for each year. The t-statistics for each test is also presented alongside the differences.

Table 8. Operational Efficiency and Growth

	Operational Efficiency and Operational Growth							
	NWC-to-Sales				Sales Growth			
	Year-by-Year PSM Match		(t–1) Performance Match		Year-by-Year PSM Match		(t–1) Performance Match	
	Diff w/ Match	*z*-statistic	Diff w/ Match	*z*-statistic	Diff w/ Match	*z*-statistic	Diff w/ Match	*z*-statistic
(t–1)	9.35%	1.540	–0.06%	–0.962	–19.58%*	–1.781	–10.82%***	–3.077
Event	–3.75%	–0.548	–1.36%	–1.121	–31.55%*	–1.951	–2.88%	–0.770
(t+1)	12.57%*	1.630	–0.45%	–0.182	–14.52%**	–1.840	–1.67%	–0.455
(t+2)	7.44%*	1.342	–3.03%*	–1.563	–11.52%**	–1.778	–5.73%*	–1.377
(t+1)–(t–1)	3.61%	1.079	–0.41%	–0.167	0.29%	0.043	9.02%**	1.677
(t+2)–(t–1)	–3.60%	–0.920	–3.01%*	–1.554	0.68%	0.091	3.27%	0.600

Table 8 provides statistics of target firms *Operational Efficiency and Operational Growth* in excess of matched peers for one year before and two years after the activist intervention (event year). The two measures of *Operational Efficiency and Operational Growth* are defined as NWC-to-sales and Sales Growth. In the columns marked "Year-by-Year PSM Match" each target firm is matched year-by-year to a peer on size, industry and book-to-market characteristics. In the columns marked "(t–1) Performance Match", each target firm has been matched to a peer with similar performance in the measure tested. The column marked "Diff w/ Match" displays the average difference between all matched pairs for each year. The t-statistics for each test is also presented alongside the differences

6.2 Effects on Capital Structure decisions

The results for our measures of Leverage, Capex-to-sales and Payout ratio are presented in Table 9 and 10.

Leverage. In the Year-by-Year match, target firms, on average, have a higher leverage ratio than non-target peers on the pre-event year. On event year, non-significant results indicate a sharp decrease in leverage for target firms relative to non-targets. However, one and two years post event, the difference increases significantly. These increased differences indicate that target firms, on average, have higher leverage ratios than non-target peers one and two years post event. When comparing the first and second year post event to the pre-event year ((t+1)–(t–1) and (t+2)–(t–1)), the difference is significantly positive, indicating leverage ratio increases for target firms relative to non-targets between the pre- and post-event period.

The Performance match for *Leverage* displays non-significant, positive, difference on event year. The difference increases one year post event, and two years post event, the difference is significantly higher, indicating target firms have a higher leverage ratio than non-target peers. When comparing the second year post event to the pre-event year ((t+2)–(t–1)), we observe that the leverage ratio has significantly increased for target firms relative to non-target peers.

Capex-to-sales. In the Year-by-Year match, target firms have lower investment levels than non-target peers on the pre-event year. On event year and one year post event, non-significant results display that the difference is still negative. However, two years post event the difference is positive, indicating target firms now have higher investment levels than non-target peers. When comparing the first and second years post event to the pre-event year ((t+1)–(t–1) and (t+2)–(t–1)), we find significant results indicating increased investment levels for target firms relative to non-targets.

The Performance match for *Capex-to-Sales* yields no significant results for any years measured. The non-significant results show positive difference on event year, which declines one and two years post event. Comparing each year post event with the pre-event year ((t+1)–(t–1) and (t+2)–(t–1)), no significant results are found. However, non-significant results indicate decreased investment levels for target firms relative to non-target peers

Table 9. Leverage Ratio and Investment Level

	Leverage Ratio and Investment Level							
	LT Debt/Total Capital				CAPEX/Sales			
	Year-by-Year PSM Match		(t–1) Performance Match		Year-by-Year PSM Match		(t–1) Performance Match	
	Diff w/ Match	*z*-statistic	Diff w/ Match	*z*-statistic	Diff w/ Match	*z*-statistic	Diff w/ Match	*z*-statistic
(t–1)	21.28%*	1.800	0.13%	0.877	–4.19%*	–1.710	0.04%	1.622
Event	11.60%	1.489	1.70%	0.634	–2.31%	–1.285	0.31%	0.579
(t+1)	27.46%**	1.930	3.45%	0.960	–0.85%	–0.588	–0.49%	–0.516
(t+2)	27.27%**	1.883	5.72%*	1.352	1.83%	1.204	–0.83%	–0.853
(t+1)–(t–1)	1.47%	0.356	3.31%	0.923	2.98%*	1.767	–0.53%	–0.570
(t+2)–(t–1)	6.86%	1.104	5.62%*	1.334	2.82%*	1.781	–0.89%	–0.912

Table 9 provides statistics of target firms *Leverage Ratio and Investment Level* in excess of matched peers for one year before and two years after the activist intervention (event year). The two measures of Capital Structure and Capital Expenditure are defined as LT Debt/Total Capital and CAPEX/Sales, respectively. In the columns marked "Year-by-Year PSM Match" each target firm is matched year-by-year to a peer on size, industry and book-to-market characteristics. In the columns marked "(t–1) Performance Match", each target firm has been matched to a peer with similar performance in the measure tested. The column marked "Diff w/ Match" displays the average difference between all matched pairs for each year. The t-statistics for each test is also presented alongside the differences

Table 10. Payout Ratio

	Payout Ratio			
	Dividend Payout/Lagged MV of Equity			
	Year-by-Year PSM Match		(t–1) Performance Match	
	Diff w/ Match	*z*-statistic	Diff w/ Match	*z*-statistic
(t–1)	0.01%	0.012	–0.00%	–0.229
Event	0.81%	1.179	0.81%	1.221
(t+1)	0.97%	1.032	–0.36%	–0.899
(t+2)	0.60%	1.283	0.02%	0.040
(t+1)–(t–1)	0.64%	0.685	–0.35%	–0.888
(t+2)–(t–1)	0.55%	0.872	0.03%	0.047

Table 10 provides statistics of target firms *Payout Ratio* in excess of matched peers for one year before and two years after the activist intervention (event year). The measure of Payout Ratio is defined as Dividend Payout/Lagged MV of Equity. In the columns marked "Year-by-Year PSM Match" each target firm is matched year-by-year to a peer on size, industry and book-to-market characteristics. In the columns marked "(t–1) Performance Match", each target firm has been matched to a peer with similar performance in the measure tested. The column marked "Diff w/ Match" displays the average difference between all matched pairs for each year. The t-statistics for each test is also presented alongside the differences

Payout Ratio. In the Year-by-Year match no significant results for any years are observed. The non-significant results show no difference between the payout ratio for targets and non-targets on the pre-event year. On event year and one year post event, the difference increases and is positive, indicating target firms have higher payout ratios than non-target peers. Two years post event, the difference decreases somewhat, however remains positive. Comparing each year post event with the pre-event year ((t+1)–(t–1) and (t+2)–(t–1)), non-significant results show increased payout ratios for target firms relative to non-target peers.

The Performance match for *Payout Ratio* shows no significant results in any of the years measured. The difference increases and is positive on event year, however declines and is negative one year post event. Two years post event the difference increases again, however the value is close to 0. Comparing the second year post event to the pre-event year ((t+2)–(t–1)), non-significant results indicate that the payout ratio neither increases nor decreases for target firms relative to non-target peers. Moreover, when comparing the first year post event to the pre-event year ((t+1)–(t–1)), non-significant results indicate a small decrease in payout ratio for target firms relative to non-target peers.

7. Analysis & Discussion

7.1 Evaluation Results

In this section we evaluate our results to determine the outcome for our first and second hypothesis.

7.1.1 Effects of hedge fund activism on operational performance

From our results we find evidence suggesting firms targeted by activist hedge funds tend to improve their operational performance relative to non-target peers post event. This supports our first hypothesis and appears to be consistent over all four measures, at varying levels of significance. In the following section, we will discuss our results. Moreover, we explain the economic effects of our results, which we define as how large the increase or decrease of an accounting measure is relative to the standard of that same measure.

Our first accounting measure tested, *Return on Assets*, was used to test for increased operational profitability in target firms relative to non-target peers. The results from both of our matching methods displayed improved performance for target firms relative to non-target peers when comparing the periods before and after the activist intervention. However, only the Performance

match yields statistically significant results, and does so only when comparing the second year post event to the pre-event year, at a confidence level of 0.05. When putting the improved performance, yielded by the Performance match, in relation to the Descriptive Statistics (Table 5 and 6), we find that the magnitude of the relative increase in ROA of 2.51% represents not only a statistical significance, but also deems the increase economically significant.

The improvements observed support our first hypothesis and may be explained by the theoretical framework, as reallocation of capital to more efficient uses could be a sign of decreased agency costs (Jensen, 1986; Stulz, 1990). The activist hedge funds would in such a case act as the catalyst that initiates the divestment and through doing reduces inefficiency. The results are also in line with previous research (Brav et al, 2008; Clifford, 2008; Bebchuk et al, 2015), where the improved performance is explained by divestments of inefficient assets, whilst maintaining operating margins (Brav et al, 2008; Clifford, 2008).

Secondly, we tested our second accounting measure, *EBITDA Margin*, to further test for increased operational profitability in target firms relative to non-target peers. For this measure, however, our two matching methods displayed different results. Whilst the Year-by-Year match showed no sign of improved EBITDA Margin between the pre- and post-event periods, the Performance match displays significant improvement when comparing both the first and second year post event to the pre-event year at confidence levels of 0.05 and 0.10, respectively. Putting the results yielded by the Performance match in relation to the Descriptive Statistics (Table 5 and 6), we find that the magnitude of the relative increases of 2.29% and 1.33% are not only statistically significant, but are also economically significant.

The results for *EBITDA Margin* support our first hypothesis and are in line with Brav et al. (2008), but contradictory to Clifford (2008). Clifford (2008) suggests that most of the gains in operational profitability are due to reduction in firm's assets and not from improvement in operating income. Brav et al. (2008), on the other hand, find improved EBITDA Margin in combination with efficient capital reallocation.

Net Working Capital-to-Sales. Our third accounting measure, NWC-to-Sales, was used to test for increased operational profitability in target firms relative to non-target peers. The results from both of our matching methods displayed improved operational efficiency for target firms relative to non-target peers when comparing the second year post event to the pre-event year. However, only the Performance match yielded significant results, and did so at a confidence level of 0.10. When comparing the relative *decrease* in NWC-to-sales to the Descriptive

Statistics, we find that the magnitude of the result is not only statistically significant, but also economically significant

Our results *NWC-to-sales* supports our first hypothesis, as it seems target firms are able to increase their operational efficiency relative to non-target peers post the activist intervention.

Sales Growth. The fourth and final accounting measure on operational performance was used to test for increased operational growth for target firms relative to non-target peers. For this measure, however, our two matching methods display different results. Whilst the Year-by-Year match shows signs of miniscule improvement between the pre- and post-event periods, the Performance match displays significant improvement when comparing the first year post event to the pre-event year at a confidence level of 0.05. Putting the results yielded by the Performance match in relation to the Descriptive Statistics (Table 5 and 6), we find that the magnitude of the relative increase of 9.02% is not only statistically significant, but also economically significant.

Through our results for *Sales Growth* we find support for our first hypothesis, as it seems target firms are able to recover underperformance in sales growth relative to non-target peers post the activist intervention.

7.1.2 Effects of hedge fund activism on Leverage, Investment Level and Payout

Leverage. Our first accounting measure connected to capital structure was used to test for increased leverage for target firms relative to non-targets. The results from both of our matching methods display that leverage increases for target firms relative to non-target peers, when comparing the periods before and after the activist intervention. However, only the Performance match yields statistically significant results, and does so only when comparing the second year post event to the pre-event year, at a confidence level of 0.10. When viewing the relative increase in leverage, yielded by the Performance match, in relation to the Descriptive Statistics (Table 5 and 6), we find that the magnitude of the relative increase of 5.62% represents not only a statistical significance, but that the result is economically significant.

These results support our hypothesis, as we predicted a relative increase in leverage between target and non-target firms. Theoretically, increased leverage could be a sign of decreased agency costs (Jensen 1986; Stulz 1990), and may be an explanation for why we observe these results. Previous research also finds firms targeted by activist hedge funds experience increased

leverage (Brav et al, 2008; Clifford 2008), however the increase is much smaller than what we find.

Capex-to-sales. This measure was used to determine whether firms targeted by activist hedge funds experience decreased investment levels post activist intervention relative to non-target peers. When comparing our two matching methods we find contradictory results. Whilst the Year-by-Year match indicates increased Capex-to-sales when comparing both the first and second year post event to the pre-event year, the Performance match find the opposite to be true. However, only the results from the Year-by-Year match are statistically significant and are so at a confidence level of 0.05 for both years.

The results for *Capex-to-sales* from the Year-by-Year match strongly reject our hypothesis for this particular measure and thus, it provides no indication that activist hedge funds are able to decrease any potential overinvestments in target firms. Bebchuk et al. (2015) also find no sign of any relative decrease in investment levels post the activist intervention, however, unlike us they do not find any increase either. A possible explanation for why investment levels increase is that capital received from divesting underperforming assets frees up cash flow to be re-invested in new assets.

Payout Ratio. This measure has been used to test for increased payout for target firms relative to non-target peers. Our methods of matching present contradictory results in regards to this measure. Whilst the Year-by-Year match displays increased payout ratio for target firms relative to non-targets between the pre- and post-event periods, the Performance match, show no sign of any increase. None of the methods provide any significant results and thus we cannot conclude that the hypothesis for this measure is in any way supported.

As our results do not provide any significant results, little can be said on whether activist intervention have any effect on the payout ratio of target firms. Brav et al. (2008) find that firms targeted by activist hedge funds experience a relative increase in total payout ratio (dividend and share repurchases). It could thus be argued that a better measure for our study would have been to include both forms of payout in our ratio. However, due to data limitations this was not possible and thus we settled for a ratio based solely on dividends.

7.2 Robustness check and sensitivity analysis

7.2.1 Using two methods of matching

The method of using two different matching methods in itself serves as a robustness check. This is because the two methods aim to isolate the effect of the activist hedge funds in different ways by adjusting for different factors. The validity of any potential results are therefore strengthened if the results of the two methods point in the same direction.

In our results, most measures appear to develop along similar patterns for both matching methods. However, in many cases only one method of matching presents significant result. Thus, it is difficult to say how large effect the non-significant measures should have on the interpretation of our results. We conclude that, for each measure, the matching method with the most significant results have the strongest bearing for our interpretation.

7.2.2 Removed outliers

One of the assumptions when using the paired samples t-test is, as mentioned in the method section, that the distribution of differences between the sample and control group must follow a normal distribution. To examine whether this assumption holds, we must test whether our results contain outlier, as outliers may presently be skewing results. Therefore, following a qualitative evaluation of potential outliers, we eliminate observations deemed to be extreme values and then run the tests again.[2]

The results from the tests are presented in Appendix C, D and E. We find that the values for the majority of our accounting measures are unharmed or experience only small alterations. The only measure with a significant change was EBITDA Margin in the Year-by-Year match, where the results change quite dramatically when controlling for outliers. This measure showed no

[2] When looking at outliers we take into consideration what Barber and Lyon (1996) discuss, that accounting data is by nature often skewed. Given that our sample is relatively small, we deem a more qualitative approach, even though to a certain extent subjective, to be more appropriate. The removal of outliers is consequently done in two steps, and even though the approach is more subjective we deem it to be the most appropriate approach. First we identify all outliers using boxplots. We then make a qualitative evaluation of each observation marked as an outlier where we look at the difference between the target firm and the matched non-target for an individual as well as how the difference changes over time. The observation is removed if it is both marked as an outlier by the boxplots and is inconsistent with the overall pattern of differences between target and matched non-targets the previous and following years. An example of this would be if the difference is initially small, then for a single year is very big and then the following years goes reverts to a small difference. A case where the difference changes and then remains stable on the new level, or changes year by year but shows a consistent pattern would be kept. In most of the cases between zero and two observations per year and metric is removed. It is important to remember that removing outliers is controversial as the outliers may contain important information (Newbold et al, 2013). It is therefore the results from our non-outlier adjusted dataset that shall be seen as our main results.

significance in the main analysis, but after controlling for outliers, a significant, positive, difference is observed when comparing the second year post event to pre-event year **(**(t+2)–(t–1)**)**. We therefore conclude that it is probable that the paired samples follow the same distribution, in line with the assumption presented in the Method section.

7.3 Research method discussion

In order to provide the most accurate research possible, we have attempted to make decisions regarding our study to provide the least amount of biases, flaws and other irregularities.

To begin, we have limited ourselves to the UK market, to ensure that all firms being targeted are present within the same legal system. To enlarge our sample, we have, however, included hedge funds located in other parts of the world than the UK. Although differences in regulation surrounding hedge funds exist between different countries, the shareholder rights remain the same for all hedge fund activism in the UK.

Furthermore, we have sought to follow each target firm for the entire measurement period. However, because successful hedge fund activism often leads to capital reallocation, divestments and M&A-activity (Brav et al. 2008), some firms are removed from the sample during the measurement period. This inherently leads to our sample to be biased, where the "surviving" target firms (i.e. firms not delisted, divested or demerged) are the only being measured for both years post event. Brav et al. (2008) describe this as one of the main challenges when performing post event analysis.

Moreover, we have defined our accounting measures to be highly unaffected by one-time events and other anomalies, as well as being consistent in selecting measures from the same source (Thomson Reuters EIKON) to ensure comparability.

We have also applied a couple of methods for measuring sensitivity and robustness to further strengthen our results. We find, in most cases, that our results are consistent over two separate approaches to matching and the same holds true even when extreme observations (outliers) are removed.

Furthermore, we believe our results are replicable. This is partly due to our highly qualified sources of data, primarily Factiva, London Stock Exchange (LSX) and Thomson Reuters EIKON, as well as the fact that we examine the majority of all hedge fund activist campaigns that took place during the measurement period.

A final important discussion is our choice of statistical method. First, even though assuming normality, the paired samples t-test is itself relatively robust against outliers (Newbold et al, 2013). This speaks in favor of using the method. As a precautionary measure, to increase the

validity of our results, we decide to use the central limit theorem. It is both common and often appropriate to do so when the sample distribution is unknown and the sample size is sufficiently large. Another possible course of action would have been to use, for example, a non-parametric test such as the Wilcoxon signed rank test (hereafter, WSRT) for paired or matched samples (Newbold et al, 2013). Barber and Lyon (1996) argue that it is more appropriate to use the WSRT for accounting data, even though the statistical significance of this test is lower than that of the paired samples t-test. The reason is that accounting data tends to be skewed and the WRST is less sensitive to extreme observations. In the end, we decided to use the paired samples t-test in combination with the central limit theorem, because previous research that apply a similar research method to ours use the paired samples t-test. (Klein and Zur, 2006; Brav et al, 2008; Clifford, 2008; Bebchuk et al, 2015).

8. Conclusion and Future Research

8.1 Conclusion

In this study, we have attempted to increase the knowledge surrounding hedge fund activism.

Using two different methods of matching, we have examined the development of several accounting measures between firms targeted by activist interventions against non-targeted firms. Through doing so, we find evidence supporting the claim that activist hedge funds improve operational performance in firms targeted. Furthermore, we also test for changes in capital structure, where the evidence is not as supportive. We find increased leverage in target firms, increased capital expenditures, in relations to sales, and no significant difference in dividend payouts.

Previous studies have mainly conducted research based on U.S. data, whereas we have attempted to differ by examining a relatively new geographical setting, the United Kingdom, as well as by exclusively looking at data from years post the recent financial crisis. Even though the geographical setting differs, our results are similar to previous studies on the matter, indicating that the effects of hedge fund activism observed in the US appear to be prevalent in the UK as well. Moreover, it seems that the abilities of hedge fund activists have no changed significantly since the recent financial crisis. Thus, our results indicate that any post-crisis regulation has not adversely affected the effectiveness of hedge fund activism.

In conclusion, we believe our study sheds new light on a topic which has received much attention in recent years. However, we recognize that we have made several restrictions on our

study, which could be further developed to increase the explanatory power of our results. In the following section we will present a few ideas and suggestions for future research.

8.2 Future Research

Through our study, we have attempted to further develop the academic research on hedge fund activism. However, because certain limitations have been met, there are several areas left for future researchers to study.

First, we believe there is much research to be done on new geographical settings. Several markets remain relatively unobserved, where it could be examined whether hedge fund activism has proven effective. Furthermore, more research focusing on the time period post the recent financial crisis gives further indication to whether new regulation and increased scrutiny has affected the effectiveness of hedge fund activism. Here, it would be interesting to examine countries were the largest changes have been made to such rules and regulation.

Moreover, there are many limitations we faced, where opportunities for new research have been observed. First, if possible to differentiate, it could be examined how well various forms of hedge fund activism perform. One could, for example, examine how well changes to managerial compensation fare in relation to CEO turnover. Furthermore, other differentiations could be made, such as examining the effect of hedge fund activism in different industries.

In our research, we also have a selection bias. If an activist hedge fund runs a campaign where assets and divisions are pushed to be sold by target firms, it remains to be answered how well these fare post being sold. Future research could perform tests to measure the firm performance of the acquirer of those assets or division. If not only the firm targeted by activism is improved from divestment of assets, but also the acquirer of those same assets, it would seem hedge fund activism is beneficial for wealth creation for all parties.

Finally, in spite of recent research observing significant value creation by activist hedge funds (Brav et al, 2008; Clifford, 2008; Bebchuk et al, 2015; Denes et al, 2017), the opinion surrounding the effectiveness of activists remains divided (George and Lorsch, 2014). Bebchuk et al. (2015) explain that opponents of hedge fund activism see research as being too theoretical, and lacks the “depth of real-world experience”. We find this to be an interesting suggestion and believe future research would benefit from taking *qualitative* approaches as well. In-depth case studies uncovering the interaction between activist hedge funds and target firms could increase the understanding surrounding the subject of hedge fund activism.

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Appendix

Appendix A. *Performance Match: Count*

Count	ROA	EBITDA Margin	NWC-to-Sales	Sales-Growth	Leverage	Capex-to-Sales	Payout
(t-1)	57	57	57	58	57	57	62
Event	55	57	57	57	57	57	56
(t+1)	52	53	55	53	53	55	55
(t+2)	46	49	49	49	49	49	51
(t+1)- (t-1)	52	53	55	53	53	55	55
(t+2)- (t-1)	46	49	49	49	49	49	51

Appendix B. *Year-by-Year Match: Count*

Count	ROA	EBITDA Margin	NWC-to-Sales	Sales-Growth	Leverage	Capex-to-Sales	Payout
(t-1)	53	52	54	54	52	54	47
Event	53	52	54	53	52	53	47
(t+1)	50	49	51	49	50	52	46
(t+2)	43	43	45	43	45	44	45
(t+1)- (t-1)	50	49	51	49	50	52	46
(t+2)- (t-1)	50	49	51	49	50	52	46

Appendix C. *Operational Profitability: Outliers Removed*

	Operational Profitability							
	EBITDA/Assets				EBITDA/Sales			
	Year-by-Year PSM Match		(t-1) Performance Match		Year-by-Year PSM Match		(t-1) Performance Match	
	Diff w/ Match	*z*-statistic	Diff w/ Match	*z*-statistic	Diff w/ Match	*z*-statistic	Diff w/ Match	*z*-statistic
(t–1)	–3.47%	–1.515	–0.03%	– 0.671	–5.38%	–1.495	0.01%	0.445
Event	–3.88%*	–1.904	3.88%*	1.750	–3.31%	–1.179	2.30%**	2.294
(t+1)	–3.05%	–1.278	0.55%	0.639	–0.90%	–0.351	1.77%**	1.782
(t+2)	–0.90%	–0.347	2.69%**	1.958	0.00%	0.000	1.03%	1.139
(t+1)–(t–1)	–0.08%	–0.067	0.46%	0.530	1.56%	1.112	1.73%**	1.731
(t+2)–(t–1)	1.94%*	1.562	4.54%**	1.752	2.35%*	1.585	0.99%	1.075

Appendix D. *Operational Efficiency and Growth: Outliers Removed*

	Operational Efficiency and Growth							
	Net Working Capital				Sales Growth			
	Year-by-Year PSM Match		(t-1) Performance Match		Year-by-Year PSM Match		(t-1) Performance Match	
	Diff w/ Match	*z*-statistic	Diff w/ Match	*z*-statistic	Diff w/ Match	*z*-statistic	Diff w/ Match	*z*-statistic
(t–1)	9.35%	1.540	–0.06%	–1.009	–13.85%*	–1.783	–10.82%***	–3.077
Event	6.03%	1.120	–1.64%	–1.375	–22.28%*	–1.953	–4.76%	–1.444
(t+1)	12.57%*	1.630	–2.20%**	–1.790	–14.53%**	–1.840	–3.13%	–0.914
(t+2)	7.44%*	1.342	–3.82%***	–2.962	–8.51%*	–1.678	–3.81%	–1.010
(t+1)–(t–1)	3.61%	1.079	–2.16%**	–1.765	–4.11%	–0.834	6.85%*	1.364
(t+2)–(t–1)	–3.60%	–0.920	–3.80%***	–2.936	0.40%	0.074	6.31%*	1.361

Appendix E. *Capital Structure and Expenditure: Outliers Removed*

	Capital Structure and Expenditure							
	LT Debt/Total Capital				CAPEX/Sales			
	Year-by-Year PSM Match		(t-1) Performance Match		Year-by-Year PSM Match		(t-1) Performance Match	
	Diff w/ Match	*z*-statistic	Diff w/ Match	*z*-statistic	Diff w/ Match	*z*-statistic	Diff w/ Match	*z*-statistic
(t–1)	16.67%*	1.725	0.13%	0.877	–1.37%	–1.263	0.04%	1.622
Event	11.60%	1.489	1.70%	0.634	0.69%	0.766	0.31%	0.579
(t+1)	27.46%**	1.930	3.45%	0.960	–1.04%	–0.911	–0.18%	0.269
(t+2)	22.08%**	1.847	2.76%	0.894	0.16%	–0.154	–0.13%	-0.192
(t+1)–(t–1)	3.93%	0.892	3.31%	0.923	1.41%	1.290	0.13%	0.189
(t+2)–(t–1)	5.51%	1.052	2.69%	0.870	1.11%	1.140	–0.19%	–0.273

Appendix F. *Payout Ratio: Outliers Removed*

	Payout Ratio			
	Dividend Payout/Lagged MV of Equity			
	Year-by-Year PSM Match		(t-1) Performance Match	
	Diff w/ Match	*z*-statistic	Diff w/ Match	*z*-statistic
(t–1)	0.01%	0.012	-0.00%	–0.229
Event	0.81%	1.179	0.02%	0.052
(t+1)	0.29%	0.440	–0.14%	–0.424
(t+2)	0.60%	1.283	–0.39%	–0.902
(t+1)–(t–1)	–0.06%	–0.104	–0.14%	–0.406
(t+2)–(t–1)	0.55%	0.872	–0.39%	–0.894