

1. Introduction

Since November 1982, when the SEC introduced regulation clarifying its rules surrounding the previously nebulous process of share repurchasing operations, the practice has grown enormously among American companies (Chan et.al, 2003). This growth has been especially explosive throughout the last decade, such that American firms now spend, in aggregate amounts, approximately as much on share repurchases as they do on dividends (Skinner, 2008). Such large increases in shares repurchased has not gone unnoticed, and several newspaper pundits, political activists, and prominent lawmakers have recently addressed this growing trend. Many such commentators have been highly critical of this widespread trend in American finance, having argued that increased share repurchases, by redirecting resources from otherwise more productive areas, have negatively impacted American wages and employment benefits¹.

Nonetheless, though the question has been increasingly studied as of late, the true causes behind the large increase in share buybacks throughout the United States are not yet clear. One hypothesis – often upheld by the critics of share repurchases – maintains that public companies have been increasingly pressured by activist hedge funds to direct their capital towards share buybacks. Activist hedge funds are characterized by their investment strategy of explicitly attempting to influence the management decisions of the companies in which they invest. Thus, those who defend this hypothesis usually maintain that such hedge funds pressure company executives to repurchase shares, in the hopes that doing so will artificially inflate the company's stock price, therefore allowing the investing fund to make a quick, short-term profit². Clearly, is

¹For example, Democratic senators Chuck Schumer and Bernie Sanders have expressed such views, and have called for the introduction of legislation that would seek to curb repurchases (Sanders and Schumer 2019).

² See, for example, Monga, Benoit and Francis (2015) in the WSJ, where the authors defend the position that activist hedge funds have consistently pressured companies to repurchase their shares

important to understand the true nature of the relationship between activist hedge funds and stock repurchases, for it may carry with it important implications concerning the regulation surrounding hedge and share-repurchasing companies.

While one might be able to identify certain high-profile cases of activist investors that have pressured company executives to redirect their capital towards repurchases, the true nature of the relationship between activist hedge fund investment and share repurchases is certainly not obvious, given that there are hundreds of activist hedge funds and thousands of publicly traded companies throughout the United States. Thus, this paper seeks to study whether external pressure from activist hedge funds does increase a company's propensity to repurchase its own stock. It therefore contributes to a growing literature within financial economics on the effects of, and reasons behind, the actions of activist hedge funds. In particular, I propose to examine the following specific research question: does the presence of an activist hedge fund among the shareholders of a public company increase the amount of dollars spent on share repurchases by the company?

In order to address this research question, I collect data on the investments of American activist hedge funds on American public companies, and on the amount of dollars spent on share repurchases by such companies. I then make use of a propensity score matching design, where I pair companies that experienced investment from activist hedge funds to similar ones that did not, so as to compare their relative propensities to invest their capital on share repurchases. That is, I employ a difference-in-difference design to study the difference in dollar amounts spent on share repurchases between those companies which did experience activist hedge fund investment, and those that did not. I use different measures of share repurchases to study this effect. Surprisingly, I find that those companies which experience investment by activist hedge funds tend to spend, on

average, fewer dollars on stock repurchases than their corresponding matched counterparts. I find that these results are robust when considering subsamples of the full data, different time intervals, and different matching designs. These results seem provide evidence against the hypothesis that activist hedge funds drive increases in share repurchases. Thus, this paper ultimately suggests that the nature of the relationship between activist hedge fund investment and shares repurchased is not as straightforward as some might believe, and that the question would benefit greatly from more extensive future study.

2. Literature Review

On the one hand, this research contributes to the large literature surrounding the effects of stock market pressure, and particularly that brought about by activist investors, on the management of public companies. For instance, Brav et. al. (2018) uses a sample of hedge funds from 1991 to 2010 – collected using the SEC’s EDGAR database – to construct a matching difference-in-difference model where firms which have experienced investment by activist hedge-funds are paired to similar ones which have not. This matching procedure is conducted by means of a propensity score, which uses variables identified in the literature as the most effective predictors of company similarity. The authors find that activist investment reduces R&D expenditure, while increasing the quantity and quality of innovation output, as measured both by number of patents filed and the average number of citations per patent. Their results suggest that target firms improve their innovation efficiency, and the authors propose a variety of different mechanism through which this effect could be explained. The matching difference-in-difference design employed in this paper serves the primary inspiration for this research, given its increased ability – relative to a standard OLS regression design – to identify the true effects of activist investment. Moreover, this

paper uses an extended sample of activist hedge fund investment based on this particular paper but including years up to 2014.

In a similar line of research, multiple studies have previously analyzed the impact of activist intervention on the returns of targeted companies. Clifford (2008), for example, uses a sample of hedge funds from 1998 to 2005 (collected using the form 13G/13D filings from the SEC) and stock performance data for this period (collected from the Compustat and CRSP financial databases) to study this effect. Using the Fama-French three factor asset pricing model to measure excess returns, and a logistic regression framework, the author finds that firms targeted by active hedge funds earn increased excess stock returns and experience an improvement of 1.17% in operating performance, as measured by Return on Assets (ROA). This pattern of increased short-term performance of targeted companies in the period following the intervention by an activist investor seems to provide further motivation for the hypothesis that such investors seek to pressure companies into repurchasing their own shares, for doing so should increase share price, all else being equal. Nonetheless, it is conceivable that this pattern might persist without any such increases in the number of shares repurchased by target companies, for it could indeed arise from a variety of other mechanisms. Thus, this paper distinguished itself in that it directly examine the impact of activist hedge-fund investment on the quantity of shares repurchased, regardless of realized stock performance.

Handy (2015) provides perhaps the only other paper which studies this particular consequence of activist investment. His paper constructs its dataset using SEC data on form 13D filings from 1994 to 2011 and share repurchase data collected from 2004 to 2012 using the Standard and Poor's Compustat database. The author first constructs a probit model for his analysis and finds that targeted companies are correlated with a 3.6% increase in repurchase propensity.

The author also uses an OLS regression framework whose results suggest that activist investment is correlated with an increase of 4.51 million shares repurchased per quarter. My paper looks to expand upon these strictly correlational results. Thus, it distinguishes itself in its use of a matching difference-in-difference framework (with added time and company fixed effects), which should allow for a better understanding of the true effect of activist hedge-fund investment on the quantity of shares repurchased by target companies. Furthermore, this paper makes use of a larger dataset of activist investment events and of shares repurchased by public companies.

My paper also relates to the literature surrounding the motivation behind share repurchasing operations, which have become increasingly more common throughout the United States in the last two decades. Various studies have examined different motivating factors behind this increase in share buybacks. For example, Almeida et. al. (2016) uses a regression discontinuing design to study the subset of stock-repurchasing firms which would have just missed earnings-per-share (EPS) forecasts in the absence of share buybacks. The authors find that the probability of an accretive share repurchase increases by 3-5% around the EPS threshold that matches analysts' expectations. They further find that these repurchases come at the of significant decreases in employment, capital expenditures, and R&D spending. These results suggest might suggest that activist investors would in fact pressure companies to raise share buybacks, for they may be focused on the short-term stock performance of the target company, which surely is related to its ability to satisfy market expectations for earnings forecasts. Nonetheless, their paper focuses exclusively on those firms which would have missed EPS forecasts in the absence of stock buybacks. My research, on the other hand, examines a larger subset of firms, namely all of those which have experienced investment by activist investors. It also distinguishes itself in its

methodological approach, in that it employs a matching difference-in-difference design with fixed effects, as opposed to a regression discontinuity framework.

Ultimately, my paper contributes to the existent literature by further developing its understanding of the effects of activist hedge-fund investment, and of the reasons motivating share repurchases, a topic which has increasingly been at the center of public interest. Although several authors have examined different factors which seem to motivate the quantity of shares repurchased, and still others have looked at the effects of activist investment, few have examined the explicit connection between the two, and I am aware of none that employ a matching difference-in-difference approach as first step towards addressing the endogeneity concerns that invariably arise within the subject. In particular, my research seeks to address the lack of research into true effect of activist hedge fund investment on the quantity of shares repurchased their targeted companies.

3. Description of Data and Methodology

It is quite challenging to find data on the activity and holdings of hedge funds, given the inherently secretive and very competitive nature of their industry. One frequently used source on such activity, however, is provided by the SEC. The SEC requires that any investing agent attaining a total ownership of more than 5% of a public company's voting class equity securities file a Schedule 13D "beneficial ownership report" within ten days of making such purchase. Like other more commonly used SEC filings, these forms are made available to the public through the SEC's EDGAR database. The dataset I use in my analysis is an extension of the one found in Brav et al. (2018), who compile all available Schedule 13D filings found in the SEC database through 2012 and going back to 1994. The authors go through the process of removing other types of investment companies from the data – such as mutual funds and pension funds – and manually check it for

any missing events. Therefore, my final sample on activist hedge funds consists exclusively activist hedge funds, along with the dates at which their Schedule 13D filings were submitted to the SEC, and covers the period 1994 – 2014.

Though it may not initially seem so, accurately measuring the amount of money spent by public companies on share repurchases, as well as the price at which they purchase such their shares, is not a trivial matter. Indeed, firms are not often not required to disclose all of the shares which they repurchase, which results in downward biases of publicly available data. When such information is disclosed, however, it is often as an aggregate value and includes such items as fixed-price tender offers, Dutch auction tender offers, privately negotiated purchases, as well as the far more common open market purchases, in which this study is truly interested. This inclusion of additional measures into the measure of shares repurchased, then, biases the publicly available data upwards. Hopefully, one expects two biases somewhat cancel out one each other.

As a result of these inherent challenges in measuring repurchases, different authors have used several distinct measures in their attempt to estimate shares repurchases. In their metanalysis of the different measures used throughout the literature, Banyl et.al (2008) conclude that the so-called Compustat yearly measure is the most accurate among those readily available sources of data. This particular measure is therefore employed throughout my research. It is constructed by subtracting Compustat's measure of "Preferred Stock Redemption Value" from its measure of "Purchase of Common and Preferred Stock", both of which are measured in thousands of dollars. I collect this information, along with several other fundamental variables from the CRSP/Compustat merged yearly database. Therefore, though these self-reported measures of shares repurchased are hardly perfect, they do seem to provide their best available estimation. As my previous dataset, this one also spans the period 1994 – 2015. Having gathered both of these

datasets, I merge them together to create a master dataset, where every firm-year observation is identified as having received investment from some activist hedge fund or not.

My methodology then proceeds in three stages. First, I assign a propensity score to each firm-year observation in my master dataset, which captures the probability that a firm is targeted by an activist hedge fund in a given year. A firm i that was targeted by an activist hedge fund at year t is said to have experienced the investment event. Then, I match every firm that has experienced this investment event to one that has not. The firm matched to firm i is said to have experienced a “pseudo-event” at year t . Finally, I run a difference-in-difference regression to measure the differential effect of having experienced the investment effect versus not having experienced it. Thus, this methodology is similar to what might be used in a experimental study where different observations experience the treatment effect at different times. This approach is not without problems, however. Chiefly, it is harmed by concerns of endogeneity, since target firms are not randomly selected by activist hedge funds.

I begin by first assigning a propensity score to the firm-year observations in my merged dataset. I do so by making use of a logistic regression model on a binary variable $Treatment_{i,t}$ which measures whether a firm-year observation experienced investment by a hedge fund (the investment event). The propensity score approach uses variables identified in the literature as predictors of company similarity, namely Market-to-Book ratio, Return on Assets (ROA), Ln (Assets), and change in ROA between years $t - 3$ and $t - 1$, which helps capture trends in deteriorating firm performance. That is, the propensity score is calculated using the following logit model:

$$Treatment_{i,t} = \beta_1 \ln(at_{i,t}) + \beta_2(mkttobook_{i,t}) + \beta_3(roa_{i,t}) + \beta_4(changeroa_{i,t}) + \epsilon_{i,t}$$

Each firm's propensity score is calculated by inputting their respective values for the independent variables into the equation specified above. Having calculated each observation's propensity score, I then match each firm-year observation where a company experienced hedge fund investment to a similar firm-year observation that did not. That is, for every firm i that experiences the investment event at year t , I match it to a firm in the same industry that did not experience the event, and whose propensity score is closest to that of i (using the traditional Euclidean distance metric). This matched control firm observation is said to have experienced the "pseudo-event" at year t which corresponds to the investment event experienced by the treatment firm at year t . This matching design, then, allows me to make use of a difference-in-difference framework to study the differential effect of hedge fund activism on shares repurchased.

Table 1: Hedge Fund activism and shares repurchased by year. Shares repurchased are estimated by dividing the amount of dollars spent of share repurchases by the stock's average monthly closing price, as is common in the literature.

| Year | # of Events | Dollars Spent on Repurchases (thousands) | | | | | | Shares Repurchased | | | | | |
|------|-------------|--|--------|----------|----------|----------|-----|--------------------|-----------|-----------|-----------|----------|-----|
| | | Sum | Median | Mean | SD | Max | Min | Sum | Median | Mean | SD | Max | Min |
| 1994 | 8 | 191.136 | 9.4235 | 23.892 | 39.92866 | 118.832 | 0 | 11.40596 | 0.6086288 | 1.425745 | 2.360461 | 7.041896 | 0 |
| 1995 | 30 | 127.169 | 0 | 4.238967 | 18.75024 | 103.003 | 0 | 3.212298 | 0 | 0.1070766 | 0.3520994 | 1.898673 | 0 |
| 1996 | 69 | 411.807 | 0 | 5.968217 | 18.44616 | 101.8 | 0 | 19.00245 | 0 | 0.2753978 | 0.7005624 | 3.84151 | 0 |
| 1997 | 132 | 173.286 | 0 | 1.312773 | 4.731423 | 32.755 | 0 | 15.14653 | 0 | 0.1147464 | 0.4329845 | 4.26109 | 0 |
| 1998 | 97 | 790.494 | 0 | 8.149423 | 25.71302 | 205.982 | 0 | 57.64996 | 0 | 0.5943295 | 1.706545 | 12.43665 | 0 |
| 1999 | 69 | 1079.97 | 0.004 | 15.65177 | 51.08003 | 305.666 | 0 | 65.78681 | 0.000252 | 0.9534321 | 3.770346 | 29.46178 | 0 |
| 2000 | 67 | 630.047 | 0 | 9.403687 | 24.65032 | 146.95 | 0 | 45.81837 | 0 | 0.6838563 | 1.517627 | 8.696719 | 0 |
| 2001 | 53 | 906.201 | 0.109 | 17.09813 | 57.95312 | 331.882 | 0 | 48.81875 | 0.0163559 | 0.9211086 | 3.033438 | 20.82725 | 0 |
| 2002 | 80 | 392.001 | 0 | 4.900012 | 14.59914 | 94.47 | 0 | 59.54451 | 0 | 0.7443064 | 2.997506 | 25.192 | 0 |
| 2003 | 87 | 1560.59 | 0 | 17.93777 | 76.36622 | 571.9 | 0 | 79.20305 | 0 | 0.9103798 | 3.480298 | 23.48337 | 0 |
| 2004 | 100 | 671.904 | 0 | 6.71904 | 38.60954 | 370 | 0 | 22.64075 | 0 | 0.2264075 | 1.051177 | 9.133548 | 0 |
| 2005 | 154 | 10619.6 | 0 | 68.95827 | 386.2671 | 4225 | 0 | 274.8498 | 0 | 1.784739 | 6.764508 | 53.15194 | 0 |
| 2006 | 207 | 5893.28 | 0 | 28.46993 | 118.6016 | 1261 | 0 | 245.5222 | 0 | 1.186097 | 4.232915 | 39.94299 | 0 |
| 2007 | 260 | 13422.2 | 0 | 51.62377 | 248.2568 | 2477 | 0 | 679.7755 | 0 | 2.614521 | 11.10661 | 139.604 | 0 |
| 2008 | 219 | 8943.83 | 0.189 | 40.83938 | 214.0569 | 2764 | 0 | 1286.335 | 0.0620579 | 5.873677 | 49.21628 | 723.5602 | 0 |
| 2009 | 105 | 761.421 | 0 | 7.251629 | 27.6391 | 186.485 | 0 | 64.11418 | 0 | 0.6106113 | 2.276691 | 16.18793 | 0 |
| 2010 | 127 | 3007.78 | 0 | 23.68332 | 114.0846 | 1069.9 | 0 | 89.98694 | 0 | 0.7085586 | 2.323592 | 13.76642 | 0 |
| 2011 | 132 | 9849.78 | 0.039 | 74.61951 | 255.4415 | 1663.502 | 0 | 362.1762 | 0.0018404 | 2.743759 | 10.74237 | 103.1309 | 0 |
| 2012 | 143 | 8185.71 | 0.097 | 57.24271 | 204.466 | 1350 | 0 | 268.8069 | 0.0192096 | 1.879768 | 5.751827 | 38.38752 | 0 |

| | | | | | | | | | | | | | |
|-------|------|---------|-------|----------|----------|--------|---|----------|-----------|----------|----------|----------|---|
| 2013 | 147 | 6200.55 | 0 | 42.18059 | 127.0758 | 1000.1 | 0 | 175.5342 | 0 | 1.19411 | 3.28483 | 20.37765 | 0 |
| 2014 | 158 | 8326.81 | 0.284 | 52.70134 | 221.9996 | 2262.5 | 0 | 356.7311 | 0.0230084 | 2.257792 | 9.842178 | 101.3665 | 0 |
| 2015 | 5 | 6305.6 | 49.6 | 1261.12 | 2618.184 | 5939 | 0 | 238.3786 | 4.6293 | 47.67572 | 95.75597 | 218.6672 | 0 |
| Total | 2449 | 88451.1 | 0 | 36.11723 | 217.0696 | 5939 | 0 | 4470.44 | 0 | 1.825415 | 16.48526 | 723.5602 | 0 |

Table 1 reports summary statistics for the amount of dollars spent by target firms on share repurchases, as well as the total number of shares repurchased during investment event years. It is quite clear from the table that in most years the median target firm reports no repurchases of its shares. Moreover, one can observe somewhat upward trend in the mean of both tables, indicating an increase in both total amount of dollars spent on share repurchase, and the average amount of shares repurchased by target firms. This observation is consistent with the literature describing recent trends in public companies, many of which have increasingly preferred to return cash to their shareholders by repurchasing shares, rather than issuing dividends (Skinner 2008). Still, the trend is neither as clean nor as evident as the one observed throughout the entire country, given the small sample of target firms, the non-random nature of their selection, the fact that both measures of share repurchases invariably underestimate their true values.

Table 2 Summary statistics for target firms and non-target firms which were matched using a propensity score approach. Results are measured in the year prior to the investment event (or corresponding “pseudo-event”), in the case of non-target firms. All Dollar amounts are in thousands of dollars.

| | Targets (N = 1371) | | | Non-targets (N = 1304) | | | Difference | |
|------------------------|--------------------|-----------|-----------|------------------------|-----------|-----------|--------------------|-------------|
| | Mean | SD | Median | Mean | SD | Median | Difference in Mean | t-Statistic |
| Ln (1 + Assets) | 5.681011 | 1.79545 | 5.58356 | 5.611554 | 1.83064 | 5.552314 | 0.069457 | 0.41536331 |
| Ln (1 + Market Value) | 5.530467 | 1.767259 | 5.391655 | 5.693085 | 1.878036 | 5.672414 | -0.162618 | -0.9939163 |
| Equity Book Value | 7.715844 | 8.731944 | 5.5955 | 7.182979 | 7.609113 | 5.0767 | 0.532865 | 1.76409347 |
| Return on Assets (ROA) | 0.0639101 | 0.3983974 | 0.0167872 | 0.0389046 | 0.2893102 | 0.0320062 | 0.0250055 | -3.0458063 |
| Return on Equity (ROE) | 0.1097168 | 0.7030505 | 0.0222038 | 0.1316414 | 1.125167 | 0.0291117 | 0.0219246 | 0.70023756 |
| Current Ratio | 2.969957 | 4.71083 | 2.027126 | 3.015295 | 3.91111 | 2.07429 | -0.045338 | -0.3318054 |
| Cash Ratio | 0.9998009 | 3.765862 | 0.4014133 | 1.060835 | 3.181151 | 0.4229558 | 0.0610341 | -0.6601854 |
| Treasury Stock | 95.98075 | 505.0529 | 0 | 95.22712 | 518.928 | 0 | 0.75363 | 0.05154649 |

| | | | | | | | | |
|--------------------|-----------|-----------|---|-----------|-----------|---|-----------|------------|
| Repr | 44.86462 | 259.8095 | 0 | 57.02351 | 303.8908 | 0 | -12.15889 | -1.4289364 |
| Ln (1 + Repr) | 1.122411 | 1.812962 | 0 | 1.181424 | 1.909034 | 0 | -0.059013 | -0.9590873 |
| Shares Repurchased | 1.855151 | 9.614552 | 0 | 1.930379 | 9.919782 | 0 | -0.075228 | -0.2690673 |
| I(Repr) | 0.4499205 | 0.4976836 | 0 | 0.4586597 | 0.498505 | 0 | - | 0.0087392 |
| Repr / Assets | 0.0211641 | 0.0578286 | 0 | 0.023674 | 0.0730736 | 0 | - | 0.0025099 |
| | | | | | | | | -1.1901657 |

Table 2 provides summary statistics for target firms and their matched counterparts, as reported in the year prior to the activist event year (or “pseudo-event” year, in the case of non-target firms). The table allows for a comparison of certain key variables between the sample of target firms and their non-targeted matches. The firms seem to generally agree in terms of important financial and non-financial variables. That is, the two samples display a close proximity in terms of company sizes and financial performance, given that they appear to be comparable in terms of their current assets, market value, and leverage. Their similarity with respect to important financial ratios, many of which were not included when calculating propensity scores, would seem to indicate that the matching algorithm was successful in pairing each target firms with a similar one in its respective industry. Most importantly, the firms report similar results for the different measures of shares repurchased. That is, the two groups generally repurchase a comparable number of shares, and spend a similar amount of dollars on share repurchases, relative to their total assets. None of these measures are included in the calculation of propensity scores and, since these values are collected for the year prior to the investment event (or respective “pseudo-event”, in the non-target case), their similarity would seem to provide support for the “parallel trends” assumption that underpins the difference-in-difference methodology. After completing this matching procedure, I proceed by restricting the sample to those firms which fall within the “treatment” or “control” groups. The sample is then limited to include the time period ranging from $t - 5$ to $t +$

5, where t corresponds to the year of an investment event (or corresponding “pseudo-event”) year.

I then run the following difference-in-difference regression for this specified sample:

$$Y_{i,t} = \alpha_t + \alpha_i + \beta_1(Target_i \times Post_{i,t}) + \beta_2 Post_{i,t} + \gamma Control_{i,t} + \epsilon_{i,t} \quad (1)$$

The subscript in equation (1) indicate the firm i and year t observation. The variable $Target_i$ is an indicator equal to one if firm i is targeted by an activist hedge fund, and zero otherwise. Similarly, $Post_{i,t}$ is an indicator equal to one if the firm-year observation (i, t) falls within the range $t + 1$ to $t + 5$ and zero otherwise. That is, if it follows after an investment event (or “pseudo-event”). The primary independent variable of interest is $Target_i \times Post_{i,t}$. This interaction is a dummy variable equal to one when the firm-year observation (i, t) is both a target of an activist hedge fund and falls within the post activist event timeframe. Hence, this variable captures the differential change in share the repurchase measures in target firms following activist investment, relative to similar changes for matched firms. Moreover, α_t and α_i are year and company fixed effects, respectively, and $Control_{i,t}$ is a vector of control variables. The control variables are: Ln (Assets), Ln (Market Value), Earnings per Share (EPS), Return on Assets (ROA), and Return on Equity (ROE) for firm i in year t . Finally, $Y_{i,t}$ is vector of four different measures of share repurchases for company i during year t . Respectively, these are: the amount of dollars spent on share repurchases, the logarithm of one plus the amount of dollars spent on share repurchases, the amount of dollars spent on share repurchases divided by total assets, and a binary indicator variable which captures whether the company repurchased any shares at all.

4 Empirical Results

Table 3 Results from the difference-in-difference regression measuring the effect of activist hedge fund investment on shares repurchased. Repr is a measure of total dollars spent in year on share repurchases. Dollars spent on repurchases are reported in thousands. The third dependent variable is an indicator variable equal to one if a firm repurchases any amount of stock during a given year, and zero otherwise. The last dependent variable measures dollars spent on repurchases relative to a firm's total assets. Assets are also measured in thousands of dollars.

| | (1) Repr | (2) Ln (1 + Repr) | (3) I(Repr) | (4) Repr / Assets |
|--------------------|---------------------|----------------------|----------------------|----------------------|
| Post x Target | -19.91** (-2.98) | -0.0916* (-2.20) | -0.000545 (-0.04) | -1.921** (-2.91) |
| Post | 19.21** -2.94 | 0.0976* (-2.4) | 0.0109 (-0.84) | 1.899** (-2.95) |
| Ln (Assets) | 0.237 (-0.06) | 0.0953*** (-3.7) | 0.0390*** (-4.7) | -0.203 (-0.50) |
| Ln (Market Value) | 12.63*** (-4.44) | 0.199*** (-11.24) | 0.0146* (-2.57) | 1.463*** (-5.21) |
| Earnings per Share | 0.142 (-1.25) | 0.00114 (-1.62) | 0.000152 (-0.67) | 0.0143 (-1.27) |
| Return on Equity | -0.00555 (-0.15) | -0.000129 (-0.57) | -1.62E-05 (-0.22) | -0.000571 (-0.16) |
| Leverage | 0.000636 (-0.24) | 0.0000149 (-0.91) | 5.31E-06 (-1) | 0.000071 (-0.27) |
| Return on Assets | -1.466 (-0.46) | -0.00125 (-0.06) | 0.00217 (-0.34) | -0.0694 (-0.22) |
| Firm FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| N | 17882 | 17882 | 17882 | 17882 |

Table 3 reports the results of the previously specified difference-in-difference regression for the various measures of share repurchases used as dependent variables. The first independent variable, Repr, measures the total amount of dollars spent by a company during a given year. Surprisingly, the coefficient on the *Target* \times *Post* term is negative for this first regression. That is, target firms spend an average of \$19,910 less on share repurchases following investment by an activist hedge fund, when compared to their matched counterparts which receive no such investment. This result is both economically and statistically significant. The second regression is similar to the third but measures the natural logarithm of one plus the total amount of shares

repurchased by a company in a given year. The negative sign on the coefficient of *Target × Post* is in accordance with the previous observation that target firms repurchase less shares than their matched counterparts. This result is therefore both economically and statistically significant. The third dependent variable of interest, *I(Repr)*, is a dummy variable which measure whether a firm repurchases any amount of stock in a given year. The negative coefficient on *Target × Post* for this independent variable would seem to suggest that that targeted firms are roughly half a percent less likely to repurchase any shares in the years following an activist event than their matched counterparts. Nonetheless, this result is not statistically significant, and not particularly economically decisive. Finally, the further regression measures the total amount of dollars spend on share repurchases relative to a firms' total assets. The negative coefficient the dependent variable of interest again reiterates the notion that targeted firms generally repurchase less of their own stock, relative to their untargeted counterparts, and would seem to indicate that such change is not driven by firm size alone.

It is quite interesting to note that investment by activist hedge funds seems to be negatively related with both parts of the decision process surrounding the repurchase of stock at any given year: namely whether to repurchase any shares and, if so, how much? That is, firms which experience investment by activist hedge funds generally elect to repurchase any amount of stock less frequently than their matched counterparts. Moreover, such firms also spend a smaller amount of dollars in total repurchases. This effect on the second decision seems to be far more salient than that on the first one, which is neither statistically nor economically significant. Such result would seem to indicate that activist hedge funds, if they are in fact involved in the buybacks decision process at all, are far more concerned with the total amount spent on repurchases than whether or not a company repurchases any stock at all in a given year.

As a first robustness check, I exclude the two industries, out of the twelve found in the SIC classification system, which spend the highest and lowest average amount of capital on share repurchases throughout the period, so as to test whether these whether the results are drawn by any outliers. Respectively, I exclude the “Transportation, Communications, Electric, Gas and Sanitary service” and “Agriculture, Forestry and Fishing” industries, and run the same difference-in-difference regression as in equation (1) on this circumscribed sample. I also run these regressions for an extended pre and post investment event (“pseudo-event”) time window of $t - 7$ to $t + 7$, since the initial choice of five years was quite arbitrary. As a second robustness check, I change the matching algorithm and run the regression on equation (1) on both the $t - 5$ to $t + 5$ sample and the expanded $t - 7$ to $t + 7$ sample. In particular I use the following logit model to specify the new propensity score:

$$Treatment_{i,t} = \beta_1 \ln(mkv_{i,t}) + \beta_2(cash_{i,t}) + \beta_3(roe_{i,t}) + \beta_4(roa_{i,t}) + \beta_5(changeroa_{i,t}) + \beta_6(eps_{i,t}) + \epsilon_{i,t}$$

Where the independent variables are, respectively: market value, cash, return on assets, return on equity, change in return on equity, and earnings per share. The results of these two robustness checks are displayed below, where the control terms are the same as the original specification of equation (1), but are omitted for brevity.

Table 4 First Robustness Check. Equation (1) used again in a smaller sample of firms

| First Robustness Check: Restricted Sample of Industries | | | | | | | | |
|---|----------------------|--------------------|--------------------|----------------------|---|---------------------|---------------------|----------------------|
| Restricted Sample, time interval $[t - 5, t + 5]$ | | | | | Restricted Sample, time interval $[t - 7, t + 7]$ | | | |
| | (1) Repr | (2) Ln(1+Repr) | (3) I(Repr) | (4) Repr / Assets | (1) Repr | (2) Ln(1+Repr) | (3) I(Repr) | (4) Repr / Assets |
| Post x Target | -20.78*** (-3.57) | -0.104* (-2.46) | 0.00219 (-0.16) | -1.971*** (-3.34) | -30.73*** (-4.84) | -0.111** (-2.77) | 0.000698 (-0.05) | -2.959*** (-4.71) |

| | | | | | | | | |
|------|---------------------|---------------------|------------------|---------------------|---------------------|---------------------|------------------|---------------------|
| Post | 23.08*** (-4.06) | 0.141*** (-3.39) | 0.0177 (-1.3) | 2.286*** (-3.96) | 27.50*** (-4.36) | 0.174*** (-4.37) | 0.018 (-1.39) | 2.753*** (-4.41) |
| N | 16388 | 16388 | 16388 | 16388 | 19609 | 19609 | 19609 | 19609 |

Table 5 Second Robustness Check. Equation (1) used again but with different propensity scores

| Second Robustness Check: Different Propensity Scores | | | | | | | | |
|--|---|---------------------|---------------------|----------------------|---|----------------------|---------------------|----------------------|
| | Different Propensity Scores, time interval $[t - 5, t + 5]$ | | | | Different Propensity Scores, time interval $[t - 7, t + 7]$ | | | |
| | (1) Repr | (2) Ln(1+Repr) | (3) I(Repr) | (4) Repr / Assets | (1) Repr | (2) Ln(1+Repr) | (3) I(Repr) | (4) Repr / Assets |
| Post x Target | -29.43*** (-4.34) | -0.148** (-3.17) | 0.0192 -1.41 | -3.137*** (-4.43) | -34.54*** (-5.33) | -0.162*** (-3.72) | 0.0183 (-1.44) | -3.689*** (-5.46) |
| Post | 19.90** (-3) | 0.0997* (-2.18) | -0.0297* (-2.24) | 2.165** (-3.12) | 23.51*** (-3.63) | 0.145*** (-3.34) | -0.0266* (-2.10) | 2.609*** (-3.87) |
| N | 17796 | 17796 | 17796 | 17796 | 21207 | 21207 | 21207 | 21207 |

As seen in table 4 and 5, the results of the original difference-in-difference regression in equation (1) are robust under different matching algorithms, different time periods, and restrictions on the size of the original data sample. The signs of the coefficients on the *Target* \times *Post* term remain largely unchanged, except for that in indicator variable I(Repr), which changes direction but continues to be neither economically nor statistically significant. These results suggest that the negative relationship between activist investment and the amount spent on share repurchases is not a statistical fluke, as it remains both economically and statistically significant, to a large degree. Likewise, the result is very similar in magnitude when adjusted for a firm's total assets, or when considering its natural logarithm instead.

Nonetheless, it is challenging to speculate as to the precise mechanism behind these results, for it is not at all clear where the priorities and motivations of activist hedge funds lie. Though

there is a large body of literature investigating the effect of activist hedge fund investment on share performance and excess returns, there are few studies which examine the precise mechanism through which attempt to influence corporate governance and increase profitability. Previous results have suggested, for example that activist hedge funds might be responsible for increases in innovation output, decreased in R&D expenditures (Brav , et al. 2018) and decreases in taxes paid (Cheng, et al. 2012). Perhaps, then, activism hedge funds might generally discourage company management from repurchasing stock, and instead invest company capital into longer-horizon projects. This might be especially true if executive compensation for the company is tied to its stock performance, such that executives might be incentivized to repurchase stock and inflate their company's stock price, as opposed to looking for more "real" ways in which to deploy their capital. Such interpretation might then favor the hypothesis that activist hedge funds behave not as predatory raiders, but as necessarily strict managers, preventing a company's mismanagement.

However, no such conclusions should be drawn from the results of this paper, and these considerations only serve to show that much research is needed into the inner workings of activist hedge funds. Indeed, it is important to emphasize the limitations of the findings of this paper, which largely stem from endogeneity concerns surrounding the firm selection process by activist hedges funds. That is, firms which experience the investment event are not assigned to it randomly. Instead, hedge funds elect to invest in these companies, by studying their observable and unobservable characteristics. Perhaps one of these characteristics is that the companies were already seeking to change their stock repurchasing policies, and hence that is why they were chosen by the activist investor. Moreover, it is entirely possible that most activist investors do not actually influence corporate decision-making very much at all, especially if a company has regulatory protections in place – such as so-called "poison pills" and "golden parachutes" – that deliberately

attempt to curtail the influence of large hostile investors over the company's future. Finally, one might consider the objection that activist hedge funds are not very active at all, and that they only engage in traditional stock-picking, and do not even attempt to directly influence corporate decision-making.

Indeed, in light of these complications, it is impossible to ascertain a causal connection between activist hedge fund investment and share repurchases, even if the previous results seem to indicate a strong negative relationship between the two. Moreover, the findings of this paper do not necessarily disprove the hypothesis that activist hedge fund deliberate pressure companies to repurchase their own. Still, they do seem to indicate that the hypothesis that activist hedge funds pressure companies to repurchase stock is not as obviously true as some might believe, and certainly warrants further empirical investigation

5. Conclusion

This paper began by contemplating the hypothesis – often levied by the critics of the hedge fund industry – that predatory and short-sighted activist hedge-funds pressure companies to invest their excess cash on repurchases of their own stock, in the hopes of artificially inflating their stock price, and at the cost of directing their capital at other more traditional avenues, such as dividend payments or research and development of new products.

Following the matching difference-in-difference design found in Brav et al. (2018), I match every firm that experienced hedge fund investment during the period 1994 to 2014, to one same industry that did not, by making use of a propensity score minimization approach. I then conduct a difference-in-difference regression to compare different measures of shares repurchased during the post activist investment period for target firms and their non-target, matched counterparts.

Contrary to the hypothesis that hedge funds pressure companies to repurchase their own shares, I find that firms in that experience investment from hedge funds generally spend a total amount of dollars on share repurchases that is smaller than their matched counterparts, for the period following this hedge fund investment. These results present strong evidence against this currently quite popular hypothesis. They seem to paint a much stronger picture of the true nature of the relationship between activist hedge funds and buybacks when compared to, for example, the work by Handy, 2015, which uses a simple OLS methodology to find a positive historical correlation – at the national level – between activist hedge fund investment and share repurchases.

Still, these results are limited by the non-random nature of firm assignment, and the fact that it is very hard to measure the true influence of activist hedge funds over company executives and their decisions. Thus, this research cannot entirely disprove the initial hypothesis that it set out to study. Nonetheless, as it pertains to matters of public policy, it evidently seems to be the case that far more research is needed into this particular relationship, and that it would probably be prudent to avoid any sweeping legislation based upon the hypothesis that activist hedge funds cause public companies to repurchase more shares. Indeed, there is clearly much more to be learnt about the inner-workings of activist hedge funds and, in particular, it would be interesting to see further attempts at using different identification strategies to distinguish the true effect of activist hedge funds on share repurchases.

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