Targeted by an Activist Hedge Fund, Do the Lenders Care?*

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Abstract

Do banks worry about expropriation when an activist hedge fund targets their borrowers or are they reassured that their borrowers will perform better after such targeting? We study 1,435 events during the 1996-2013 period in which an activist targeted a US corporation, to examine what happens to loan contract terms post-targeting. We present two new results. First, we show that when a firm is targeted by an activist hedge fund, the lenders of that firm charge a significantly higher rate on future loans and demand collateral more frequently than the loans made to risk- and industry-matched non-targeted firms. Second, we find that this increase in loan rate and the likelihood of collateral demand is limited only to those targets that experience a large positive announcement return when the news of an activist's involvement is first announced. We argue that higher interest rates and greater collateral requirements reflect the increased credit risk for these borrowers due, in part, to the possibility of wealth expropriation by the shareholders. Thus, we provide empirical evidence that an increase in equity value due to an activist's targeting may partially be due to wealth expropriation from creditors.

Keywords: Hedge fund activism, Corporate governance, Bank lending

JEL classifications: G21, G23, G32, G34

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> ConAgra Foods' smorgasbord is ready to carve. The conglomerate's $6.8 billion acquisition of Ralcorp in 2013 left a bad taste with shareholders. The activist investor Jana Partners is ready to shake up the board. ConAgra's myriad brands probably fit better elsewhere. The question is how to slice it. The $17 billion packaged-food group's shares jumped after Barry Rosenstein's hedge fund on Thursday disclosed a 7.2 percent stake and announced that it was prepared to nominate three new directors.
>
> *The New York Times, June 19, 2015*

> ConAgra Foods Inc, under pressure from an activist investor, said it plans to jettison its struggling private-label business less than three years after spending $5 billion to buy it.
>
> *The Wall Street Journal, June 30, 2015*

1. Introduction

Recent studies show that the announcement of an activist hedge fund investment in a firm's equity generates a significant increase in the stock price of that firm.[1] However, scholars provide conflicting explanations for this increase in shareholder wealth. Brav et al. (2008) and Brav et al. (2015) argue that the operational improvements imposed by the activists make their targets more valuable. Klein and Zur (2011), on the other hand, offer a less benign explanation. They show that the announcement of an activist investment is associated with a significant *decrease* in the value of targets' bonds. Based on this finding, they argue that shareholder gains are a form of wealth expropriation from the target's debtors. These interpretations yield opposite predictions about how the lenders to a target firm should adjust the terms of their loans when their borrower is targeted by an activist hedge fund. In this paper, we provide additional evidence on this debate. We show that, on average, loan interest rates go up significantly for targeted firms. However, this increase is limited exclusively to the subsample of targets that experience the highest stock price reaction to the news of hedge fund activism. Our evidence suggests that large increases in equity value may partly be due to wealth transfer from the creditors.

[1] A number of studies using different event windows have reported average excess returns ranging from 5% to 12% for targets of activist funds. Examples include Brav et al. (2008); Klein and Zur (2009); and Krishnan et al. (2016).

Although Klein and Zur (2011) provide evidence that bondholders react negatively to the news of an activist intervention, their sample is limited to only those targets that have a bond outstanding when an activist announces its investment.[2] Thus, the small and exclusive sample of bond issuing firms in their study makes it difficult to generalize Klein and Zur (2011)'s findings for *all* forms of debt. What distinguishes our study from Klein and Zur (2011) is our emphasis on bank loans rather than bonds. We find that lenders on average charge a higher interest rate on loans made in the post-activism period. Thus, our study broadens the bond event study results of Klein and Zur (2011) to a larger set of firms. We first confirm and extend the earlier findings of Sunder et al. (2014) who show that banks increase the rate on their loans in the post-activism period. Our first contribution is to provide a more robust test of this finding. While Sunder et al. (2014) rely only on the *pre-* and *post-activism* loans of the target firms to identify changes, we use a matching procedure to create a control sample of loans made to non-targeted firms. Thus, for every target loan that was outstanding at the time of activism, we identify a "matching" loan made to a non-targeted firm in the same industry, issued within 180 days of the target firm loan. In addition, the matching loan is similar to the loan of the targeted firm with respect to the loan spread, type, and maturity. This allows us to conduct a difference-in-differences (DiD) test. Arguably our approach yields stronger causal inferences and provides a more robust estimation of the impact that activists have on bank loans. We find that the effect on loan rates is almost three times as large when we employ the matched sample methodology compared to the approach employed by Sunder et al. (2014). We also examine how targeting by an activist affects three additional loan term features: collateral requirement, maturity, and relative size of the loan.

Our second contribution is to explore an alternative mechanism that can help explain the cross-sectional heterogeneity in the evolution of loan terms in the post-activist period. We use the response of shareholders to the activist announcement as our unbiased predictor of value gain for the shareholders. Existing studies report significant positive cumulative abnormal return (CAR) around the date of activist involvement disclosure. However, there is considerable variation in the announcement CARs. For example, Brav et al. (2008) report a median 41-day CAR of 4.6% but the 25^{th} and the 75^{th} percentile values are -5.3% and 17.3% respectively. Though the average CAR

[2]Faulkender and Petersen (2006) report that bond issuance is fairly uncommon with only 19% of the public listed firms having a bond rating in any given year.

is 7.2%, more than 30% of target firms experience a negative CAR. We argue that the stock market reaction provides an unbiased forecast of total value that is likely to accrue to the shareholders from the future actions of the activist. The assumption of using event-study based market reaction to gauge the potential value creation is well established in finance research (see MacKinlay (1997) for a comprehensive survey). This assumption yields a clean empirical prediction about the response of lenders. If the large increase in equity value reflects the activist's focus on operational improvements both the lender as well as the shareholders should benefit. This explanation implies that loans made after the activist intervention will carry more favorable terms; including a lower interest rate. Alternatively, if the announcement CAR is largely determined by wealth expropriation of debtholders by shareholders, subsequent loans will reflect this higher credit risk. We find that the equity CAR is a significant predictor of interest rates charged on subsequent loans. We find that target firms whose shareholders' CAR is over 10% pay an additional 47 basis points on their loans in the post-activist period compared to the loan in the pre-activist period. Since the sample average of loan spreads is 240 bps, this translates into an economically significant increase of almost 20% in the cost of new loans. For targets that did not experience large announcement CARs, there is no significant change in spreads of loans contracted in the pre- and post-activist periods. Similarly, about two-thirds of all loans made in the pre-targeting period were secured by collateral. However, in the post-targeting period, 75% of the loans made to firms with high CAR are secured by collateral but there is no increase in collateral demand for the low CAR borrowers. This provides strong evidence that activist intervention announcements in which equity holders gain significantly, the source is likely to be wealth transfer from the creditors.

The paper proceeds as follows. In the next section, we provide a brief literature review of studies that have examined hedge fund activism. Section 3 describes our data sets and provides variable definitions. Section 4 presents our findings. Section 5 concludes.

2. Related Literature

Empirical evidence shows that activist hedge funds have a significant impact on the corporate policies of their targets (Brav et al., 2008; Klein and Zur, 2009; Brav et al., 2015). Brav et al. (2008) find that target firms increase dividend payouts and leverage, and improve operating performance following activism. They also report that, on average, target firms' abnormal stock return around the activist event is significantly positive (7%), with no reversal during the subsequent year. Klein

and Zur (2009) confirm increases in dividend payouts and leverage at target firms but are unable to find support for improvements in operating performance. In fact, they document a deterioration in the target firms' operating performance and average cash balances following activism. In contrast, Brav et al. (2015) employ plant-level data from the U.S. Census Bureau and report a significant *increase* in total factor productivity (TFP) in the post-activism period. They report that these gains are largest for targets in which the activist had a stated goal of changing the strategy or forcing the sale of the target. Thus, there is some debate about the extent of operational improvement achieved by targeted firms. While the impact of activism on target performance is an important issue it is not the focus of our study. Rather than explain the evolution of firm-level operating measures we simply employ them as control variables in our tests of how hedge fund activism impacts loan contract terms.

A key challenge for any study focusing on hedge fund activism is identifying which activists are indeed hedge funds. There is no legal definition of a hedge fund. For example, in his 2003 testimony to the Senate Committee on Banking, Housing and Urban Affairs, William Donaldson, the then Chairman of U.S. Securities and Exchange Commission (SEC) states "The term *hedge fund* is undefined, including in the federal securities laws. Indeed, there is no commonly accepted universal meaning. As hedge funds have gained stature and prominence, though, *hedge fund* has developed into a catch-all classification for many unregistered privately managed pools of capital." (SEC, 2003). Furthermore, the SEC in its publication for investors states that "Hedge fund is a general, non-legal term used to describe private, unregistered investment pools that traditionally have been limited to sophisticated, wealthy investors. Hedge funds are not mutual funds and, as such, are not subject to the numerous regulations that apply to mutual funds for the protection of investors — including regulations requiring that mutual fund shares be redeemable at any time, regulations protecting against conflicts of interest, regulations to assure fairness in the pricing of fund shares, disclosure regulations, regulations limiting the use of leverage, and more."[3] We use the broad characteristics outlined in these SEC documents to construct the methodology for identifying specific investors as hedge funds. Specifically, we define hedge funds as having the following four features: (i) pooled investment vehicles that are privately organized, (ii) administered by highly incentivized professional money managers, (iii) available only to sophisticated investors, and (iv)

[3]Available at https://www.sec.gov/reportspubs/investor-publications/investorpubsinwsmfhtm.html

not registered with the Securities and Exchange Commission. The last characteristic underscores a key difference between hedge funds and mutual funds as the former are subject to fewer regulatory controls and oversights.

Furthermore, hedge funds require large minimum investment thresholds and are only accessible to qualified investors and institutions. They are not subject to the Investment Company Act of 1940, which limits performance fees.[4] Partnoy and Thomas (2007) argue that hedge fund activists have more radical objectives than traditional activists. For instance, hedge funds call for board changes at the target firm (e.g. Harbinger Capital Partners at The New York Times Company in 2008), seek to engage in merger and acquisition activities (e.g. Carl Icahn at Blockbuster Inc. in 2004), or sometimes even put forward an outright sale of the firm (e.g. Breeden Capital Management at Applebee's International Inc. in 2006).

Hedge fund activism literature has focused extensively on the impact of targeting on shareholder wealth.[5] However, formal evidence related to the impact of hedge fund activism on other stakeholders is limited. These studies mainly focus on the impact of activism on firm executives and debtholders. Typically, the target firm's CEO compensation level is lower and the CEO compensation is more performance-sensitive following activism (Brav et al., 2008). There is also a significant increase in the CEO turnover rate following a hedge fund activism event (Brav et al., 2012). Furthermore, hedge fund activism has been found to exacerbate shareholder expropriation of bondholder wealth through increased credit risk (Klein and Zur, 2011). This suggests that lenders are concerned about activist initiatives that exacerbate the shareholder-debtholder conflict (Chava et al., 2009). In an unpublished paper, Xu and Li (2010) investigate the expropriation effect relating to lenders and find that hedge fund activism significantly impacts bank loan contracting. In a similar vein, Sunder et al. (2014) also find that loan spreads tend to increase when activism is related to financial restructuring, but decrease when activists aim to address managerial entrenchment.

In this paper, we seek to combine the two strands of existing literature on the impact of hedge fund activism by focusing on both equity holders as well as on debt providers. Our paper is closest

[4]While many private equity and venture capital funds also share these characteristics, these funds can be distinguished from hedge funds due to their focus on private capital markets. In addition, private equity investors tend to acquire a larger percentage ownership stakes than hedge fund activists. Venture capital firms invest at much earlier stages than both private equity and activist hedge funds.

[5]Clifford (2008) shows that firms targeted by hedge funds with an activist agenda exhibit larger abnormal short term stock returns than a control group of firms that are targeted by the same hedge funds without such an agenda. Becht et al. (2009) and Klein and Zur (2009) provide similar results.

in spirit to Sunder et al. (2014) who also focus on the effect of hedge fund activism on bank loan characteristics. We extend their work along two dimensions. First, we employ a matching approach to create a control sample of loans that are similar to the sample of loans made to targeted firms. We follow how the loan terms change for each pair of treated (targeted) firm and the control (non-targeted) firm. Our results show that the economic significance of the results reported by Sunder et al. (2014) may be understated. In our tests, we obtain a marginal effect of hedge fund activism on loan interest rates that is almost three times larger when we use the matched-sample approach. We obtain a similar increase in the effect of activism on demand for collateral when using the matched sample. Our second contribution is to exploit the large variation in cumulative abnormal returns (CARs) around the activist involvement announcement. We argue that the potential for wealth transfer is highest for target firms for which the increase in equity value is the highest. We find strong evidence in support of this conjecture that is robust across multiple tests. For example, both the interest rates as well as collateral requirement increases only for the firms that exhibit large announcement CARs.

3. Data

3.1. The Hedge Fund Sample

There is no standard database of hedge fund activism events. Researchers have used regulatory filings to construct their samples of activist investments. We follow the approach outlined by Brav et al. (2008) and use a manually-collected data set based on Schedule 13D filings, which are mandatory federal securities law filings under section 13(d) of the 1934 Exchange Act. Under section 13(d), investors must file a Schedule 13D with the Securities and Exchange Commission (SEC) within 10 days of acquiring more than five percent of any class of equity securities of a publicly traded company if they have an interest in influencing the management of the company. Section 13(d) is aimed at informing the market that an investor might seek to force changes or seek control at target companies.[6] In fact, Item 4 of Schedule 13D requires the filer to disclose

[6]Investors that acquire more than 5% but less than 10% of the company's stock and do not have an interest in influencing the management of the target company, but are merely investing passively, file a Schedule 13G within 45 days of the end of the calendar year in which they cross this ownership threshold. Passive institutional investors that acquire more than 10% of the company's stock must file within 10 days after the end of the first month in which they exceed 10%. The filing of a Schedule 13G does not suggest an activist event. If an investor changes its purpose from passive to active it is required to file a Schedule 13D to announce its change of status from passive investor to one seeking active involvement.

the purpose of the transaction, which can range from changes in dividend policies and/or board of directors to mergers and acquisition activities. To illustrate a typical case, we reproduce the item 4 of the schedule 13D filing by the activist campaign of Jana Partners at ConAgra Foods in Appendix B.

To identify whether a firm is targeted for shareholder activism by a hedge fund, we employ the following procedure. First, we compile a comprehensive list of hedge fund names. This list is our primary source to identify if the activist investor is a hedge fund. This list is comprised of the names of hedge funds used in Brav et al. (2008), and Brav et al. (2012).[7] We supplement this initial list with the hedge fund names listed by Kühne (2011), and the National Investor Relations Institute's list of the 'Top 200 Activist Hedge Funds.' We conduct extensive news searches in Factiva to confirm if the investors listed are indeed hedge funds, and not pension funds or mutual funds. We conduct two additional data sorting procedures on this refined list. First, following Brav et al. (2008), we exclude from this list any investor who submitted only one Schedule 13D filing over the 1996 to 2013 sample period. Second, we gather all Schedule 13D filings made by these hedge funds on the SEC's EDGAR filing system. We exclude all filings where no explicit purpose was disclosed under Item 4. At this stage, we are left with a list of investors that we have confirmed as being hedge funds who have conducted multiple activist campaigns and have stated the purpose of their activist campaign. From the Schedule 13D filings, we extract information about the filing date, the date on which the five percent threshold was reached, and the identity of the target firm.

Our initial sample consists of 3,210 activist events filed by 506 unique hedge fund activists over the 18-year period spanning 1996 to 2013. To identify post-activism changes in loan terms (and to control for changes in operating performance) for a firm that was targeted in year t, we follow the target for the next three years (i.e., t+3). Thus, while our sample of activist events ends in 2013, we require data until 2016 to capture the impact of activism on loans contracted after the involvement of the activist. Since a number of firms are targeted more than once in our sample period the 3,210 activist campaigns involved 2,418 unique target firms. Our primary focus in this paper is to examine the role hedge fund activism plays in the evolution of loan contract terms. This requires us to gather data on loan terms for the targeted firms. Furthermore, we also need to collect data on a number of borrower characteristics such as size and profitability, since they

[7] We thank Alon Brav for sharing the list of hedge fund activists used in Brav et al. (2008) and Brav et al. (2012).

play an important role in how lenders design various contract terms. We first check and match the targeted firms to the firms in Compustat based on the firm name. We are able to identify the Global Company Key (GVKEY) for 2,330 unique target companies involving 3,118 activist events. To ensure that repeated targeting of the same firm does not affect our findings, we exclude any activist event if the firm has been targeted by a hedge fund in the previous five years. We do allow a targeted firm to re-enter the sample if it has not been targeted by a hedge fund for at least five years. Next, we obtain accounting data for the period immediately before the activist event. A number of targeted firms do not report accounting data for the year in which the activist event took place. We focus on a key accounting measure of total assets (Compustat item *at*) and find that 1,904 targeted firms report this measure for the year of activism.[8] We exclude 316 target firms in the finance, insurance, and real estate industry (SIC codes 6000 - 6999). Finally, we exclude small firms which we define as those with a total book value of assets (Compustat item *at*), sales (Compustat item *sale*), or market value below $1 million. We also exclude firms for which the sum of the book value of debt and the book value of equity is less than $1 million. Panel A of Table 1 describes the sample selection process.

The sample includes 1,363 unique target firms and 1,435 activist interventions. The second column of Panel B of Table 1 lists the calendar time evolution of hedge fund activism events in our sample. The four-year period spanning 2005 to 2008 shows an unusually high level of hedge fund activism with over 100 events in each of these four years. In 2009, there is sharp drop-off which may be partly due to the 2008 financial crisis. As we are primarily interested in how activism shapes lending terms, the last two columns of Panel B report how many of the target firms have issued bank loans around the date of activism. Of the 1,435 activist events, we are able to identify 814 events where the target firm issued at least one loan in the six-year [-3,+3] window around the date of activism. We refer to this sample as the "Loan Sample". Note that as long as there was even a single loan obtained by a target in the six-year window around the activist intervention, that event is included in the loan sample. We also create a more restrictive sample, which we denote as the "Balanced Loan Sample." To construct this sample, we only retain an event if the target obtains at least one loan within three years before the activist targeting and at least one loan within three

[8] We examine the sample of firms that could not be matched to Compustat to understand why they do not report accounting information. We find that most of these firms are delisted in the year of activism, and thus are not required to publish financial accounts.

years after the activist involvement. We report the calendar distribution of this subsample in the last column. This subsample should allow for a sharper identification as the same firm is providing the treated as well as the control observation. Effectively, this sample is similar to using firm fixed effects and allows us to control for time-invariant firm characteristics which may determine loan contract terms.

3.2. The Bank Loan Sample

We obtain data on individual loan facilities from the Loan Pricing Corporation's (LPC) Dealscan database. LPC collects information on loans to companies through attachments on SEC filings, self-reporting by lenders, and the financial press. Dealscan has become the standard source of data for bank loans.[9] Our starting universe of primary loan sample consists of 91,497 loan facilities made to 22,398 US firms during the 1993 to 2016 period. The start and cutoff point for the loans sample period are dictated by the fact that our activist event sample starts in 1996 and ends in 2013. The 1993 start of the loan sample ensures that we capture all loans made in the three-year window before the earliest target event in our sample. Similarly, by including all loans made until 2016 we ensure that any loan made within three years of an activist event are captured in our loan universe. A large majority ($\sim$82%) consists of loans made by a syndicate of two or more banks. Dealscan contains comprehensive information on the loan terms (e.g. maturity, collateral, and interest rate), as well as the identity and role(s) of the lending bank(s), and the identity of the borrowing firm. However, the data set lacks detailed accounting information about the borrowing firm. We therefore merge LPC with Compustat using the merger file compiled by Michael Roberts.[10]

When merging the Dealscan database with Compustat we ensure that we only use accounting information that is publicly available at the time the loan is granted. We follow the procedure outlined in Bharath et al. (2011). For those loans made in the calendar year *t*, if the loan activation date is six months or later than the fiscal year ending month in the calendar year *t*, we use the data of that fiscal year. If the loan activation date is less than six months after the fiscal year ending month, we use the data from the fiscal year ending in the calendar year *t-1*. All lenders (banks) are aggregated to their parent company. We control for bank mergers and acquisitions to trace lending

[9] Some of the studies using the Dealscan data as the primary data source include Sufi (2007), Chava and Roberts (2008) and Bharath et al. (2011).

[10] See Chava and Roberts (2008) for further details.

relationships through time. The acquiring bank inherits previous borrowing firm relationships of the acquired bank. From this data set, we include only loan facilities for which the deal status as reported by LPC is either "Completed" or "Closed." The bank loan sample consists of 83,713 loan facilities to 11,636 companies. We construct relationship lending variables as in Bharath et al. (2011). Finally, we merge the target firm sample to our loan data set.

3.3. Firm Characteristics

Table 2 reports descriptive statistics for the Compustat universe of firms and target firms. Panel A describes firm characteristics for the Compustat universe, excluding all target firm observations. Panel B reports the descriptive statistics for the sample of target firms in the year prior to activism. A casual comparison of Panel A and B suggests that firms targeted by a hedge fund are not a random subsample of the wider Compustat universe. The targeted firms differ in remarkable ways along almost every dimension including size and profitability. We perform a formal t-test for differences in means for the Compustat universe of non-targeted firms and our sample of targeted firms. The results reported in Panel C provide strong statistical evidence that target firms are not representative of the broad Compustat universe. Hedge fund activists tend to target firms that are significantly smaller than the average Compustat firm. Total assets of target firms amount to \$1.7 billion, relative to \$5.0 billion across the sample of non-targeted firms. This difference is statistically significant at the one percent level. Target firms tend to grow at a slower pace than firms in the Compustat universe of firms, suggesting that hedge funds tend to target smaller firms with low growth. On average, target firms are less profitable both in terms of *Return on Assets* and *EBITDA/Sales*. Surprisingly, the targeted firms, on average, have a higher dividend yield (*Total Payout Yield*) and hold higher cash balances prior to activism compared to the non-targeted firms. In contrast, target firms maintain book debt ratios (*Leverage*) similar to the remainder of the Compustat universe. Overall, the results reported in Panel C suggest that hedge fund target selection is driven by firm size, growth, and profitability. Figure 1 illustrates the evolution of return on assets (ROA) for target firms three years before and three years after hedge fund targeting. We find a clear "V" shaped pattern similar to the one reported by Brav et al. (2015), who argue that hedge fund arrival is typically the low point of operating performance with a significant uptick in performance in the post-activism period.

While the results reported in Table 2 describe our entire universe of targeted firms, the main

focus of our paper is to examine how the arrival of a hedge fund activist affects the loan characteristics. Thus, for the remainder of the paper, we focus only on those activism events for which we are able to obtain data on loans contracted by the targeted firms. We focus on the six-year period around the year of the 13D filing announcement (i.e. activist event). We are able to identify at least one loan origination in this period for 814 activist events out of our sample of 1,435 events. We denote this sample of target events as the "Loan Sample". All of our analyses are based on this sample. Since a large fraction (∼43%) of the original sample of hedge fund activism fails to meet our requirement of obtaining a loan, we conduct a few tests to examine if and how the two subsamples differ. The results are reported in Table 3. Panel A summarizes the characteristics of hedge fund targeted firms for which we were able to locate the origination of a new loan facility (i.e. Loan Sample). In Panel B we describe the same information for targeted firms for which there was no origination of new loan facilities in the six-year window around activism. Panel C reports the results for a t-test for differences across these two groups. Our results show that the targeted firms with loans differ significantly from the targets that did not obtain a loan. The loan sample of targeted firms are substantially larger than target firms without loans in terms of both *Assets* and *Market Value* (differences are significant at the one percent level). Specifically, target firms with loans are around six times larger in terms of *Assets* than target firms without loans. In fact, target firms with loans differ significantly across almost all dimensions when compared to targets with no loans. For example, target firms without loans are less profitable (*Return on Assets* and *EBITDA/Sales*) than target firms with loans. Interestingly, target firms with loans hold significantly less cash than target firms without loans. One explanation for this observation is that firms with access to bank financing can more easily rely on revolving facilities in the event of liquidity needs, effectively allowing them to hold lower cash reserves.

The focus on firms with bank loans limits our analysis to larger and more profitable firms within the sample of all firms that were targeted by hedge fund activists. Thus, the results we present below do not necessarily extend to all targeted firms. However, our loan sample makes up over 55% of the total sample and the analyses we present provide meaningful tests of how creditors view the activist hedge funds' involvement in the governance of their borrowers. As an additional test described below, we check whether ex-ante differences in loan and non-loan sample firms outlined above translate into significant differences in post-activist performance and find no evidence that this is the case.

Previous studies have investigated the performance of target firms *after* activism (e.g. Brav et al. (2008). If the ex-post performance of target firms with loans is different from target firms without loans, then our results for the former subsample may capture a combination of different effects. For example, lenders will adjust loan terms based on the level of improvement on firm performance. To assess if there is a significant difference in the ex-post performance between target firms with and without loans, we perform a difference-in-differences test by first estimating the change in performance for each firm from one year before the activism event to one year after (first difference). We compute the average change in performance for loan sample firms and for non-loan sample firms separately. Next, we compare the difference between these two average values for loan and non-loan samples (difference-in-differences). We report these results in Table 4. The first column presents the average change in firm characteristics from year *t-1* to year *t+1* for target firms with loans. Column 2 shows the change for target firms without loans. Finally, columns 3 and 4 show the difference of the differences in columns 1 and 2, and t-statistics for the difference-in-differences, respectively. We find that there is no significant difference in the *change* in main firm characteristics. Thus, our focus on the subsample of firms with loans does not introduce any performance-related bias since the targets with or without loans appear to experience a similar change in performance in the year following the activist event.

All subsequent analyses in the paper are restricted to the sample of targets that issued a new loan facility in the six-year window around hedge fund activism (the loan sample). Thus, the unit of analysis is an individual loan facility. We first provide descriptive statistics on the loan sample and focus on the association between hedge fund activism and bank loan terms using univariate tests. Next, we estimate multivariate regressions on the target firm loan sample and construct a matched sample to perform a difference-in-differences test. Finally, we exploit the heterogeneity in stock price reaction to hedge fund activism to explore the wealth expropriation hypothesis. Even though the average equity cumulative abnormal return (CAR) around activism is positive, there is a large cross-sectional variation across our sample. We provide evidence that this variation in stock price reaction is a significant predictor of a key loan term; the interest rate charged. We show that in the post-activism period, firms experiencing a large positive CAR pay a significantly higher interest rate on their loans.

4. Empirical Results

We argue that banks will respond to the targeting of their borrower by activist investors either if such activism leads to a better–managed firm or if the activists follow a wealth expropriation strategy. The stated goals of many activists include enhancing shareholder value by mitigating managerial entrenchment as well as making it easier for the target to be acquired by another firm. In fact, forcing the target to sell itself is a common goal of many activists. While these end goals are unambiguously good for the equity holders, their impact on debtholders is ambiguous. Lower managerial entrenchment can help debtholders if it increases the overall value of the firm. This implies a reduction in loan rates after the arrival of a hedge fund activist. However, as argued by Chava et al. (2009), entrenched managers are less likely to engage in risk-shifting and to expropriate wealth from lenders. Lenders will provide less favorable loan terms if they conclude that activism will deteriorate their borrower's creditworthiness. Thus, examining how lenders adjust the terms of new loans after the hedge fund targeting should help shed light on this issue. To examine the impact of activism on loan terms we restrict our sample to those target firms that obtain at least one loan in the six-year window around the year of activist involvement. We structure our data set to the individual loan level.

4.1. Summary Statistics and Univariate Analysis

The loan sample analyzed in this paper is an unbalanced panel as not all firms obtain loans every year. Table 5 presents the descriptive statistics of the loan sample used in this study, which consists of all loans made to targeted firms in the three years prior and three years after the year of activist intervention. Our sample consists of 3,050 loan facilities, which is about 10% larger than the sample size reported by Sunder et al. (2014). We report the mean, median, standard deviation, and quartile values for selected loan characteristics. The average loan is $325 million with the median loan being $140 million. Other studies employing a larger set of Dealscan loans report lower loan size. For example, Bharath et al. (2011) report an average loan size of $190 million (median $50 million) for a sample of over 31,000 loan facilities. Thus, the universe of borrowers in our sample skews towards those with larger loans. The loan spread is defined as the all-in-spread drawn (AISD), which equals the coupon spread over LIBOR on the drawn amount plus the annual fee in basis points. The average interest spread is 240 basis points above LIBOR, with a standard deviation of 150 basis points, which is also somewhat higher compared to the sample mean of 217 basis points reported by

Bharath et al. (2011). The median syndicate size comprises five lenders. Further, 66% of loans in our sample are secured by collateral, and borrowers obtain on average around 47% of loan volume from their relationship lender.[11]

We also report the covenant intensity of loans in our sample. Following the methodology adopted by Sunder et al. (2014), we consider the number of financial and general covenants, as well as the total number of covenants for each loan in our sample. Financial covenants are based on accounting ratios, such as current ratio or interest coverage, and represent limits placed on the level that must be maintained while the debt is outstanding. The loan can be renegotiated or the lender can ask for immediate repayment if these limits are violated. We construct the variable *Financial Covenants* by adding up the number of financial covenants included in the loan. General covenants can be classified in two broad categories. The first consists of "sweep covenants" (Asset, Debt, Equity and Insurance). These covenants require early repayment of the loan when additional funds are raised through sale of assets, debt, equity or settlement of insurance claims. In Dealscan, sweeps are indicated as percentages, which correspond to the part of the loan that requires early repayment in the event of violation.[12] From the reported variable in Dealscan we construct a binary variable indicating the presence of the sweep. The second type of general covenant places restrictions on the borrower's ability to declare dividends. Dividend restriction covenants reduce the ability of the firm to pay dividends to shareholders if certain conditions are not met, and are indicated with a binary variable in Dealscan. We construct the variable *General Covenants* as the sum total number of debt, equity, asset, and insurance sweeps, and dividend restriction covenants included in the loan. The variable *Total Covenants* is the sum of *Financial Covenants* and *General Covenants*. We find that loans in our sample include on average 4.49 covenants. We also examine the descriptive statistics for each of the two underlying financial and general covenants. The average number of financial covenants is 1.96 in our sample, and the average number of general covenants is 2.40.[13]

We begin our analysis by showing that certain key loan terms differ markedly depending on if

[11] It will be interesting to explore if an activist investor's borrowing relationship with a lender who also happens to be the lender to the activist's target firm plays a role in the evolution of loan terms post activism. However, data on such relationships between hedge fund and their banks is extremely sparse. We were able to locate only 34 activist hedge funds as borrowers in the Dealscan database. Thus, we are unable to explore this issue.

[12] For example, a loan containing a 40% equity sweep may specify that if the firm sells more than a certain fraction of its equity, it must repay 40% of the principal.

[13] Our sample means are comparable to Sunder et al. (2014), who report average total, financial and general covenant intensity of 4.78, 2.26 and 2.52, respectively.

the loan was obtained before or after an activist got involved with the borrower. For our sample of 3,050 loans, around half (1,546) are obtained *before* the activist announcement and the remaining 1,504 loans are obtained *after* the arrival of the activist. The average interest rate spread paid on the loan before activism is 229 basis points and on loans obtained after activism, it is 251 basis points. As reported in Table 6 this 22 basis point difference is significant at the one percent level. Sunder et al. (2014) document a similar pattern and report an average increase of 29.5 basis points in loan spread following an activist intervention. It is natural for lenders to adjust both priced and non-priced loan terms if they expect a material change in the creditworthiness of borrowers. We find that lenders are significantly more likely to demand collateral for loans once the borrower has been targeted by an activist hedge fund. While 64% of the pre-activist loans are secured this increases to 68% for the post-activist loans. Again this change is significant at the one percent level. Average syndicate size is 7.5 for pre-activism loans but declines significantly to 7.0 lenders for post-activism loans. Sufi (2007) shows that borrowers that require more intensive monitoring tend to have more concentrated syndicates. The decrease in syndicate size can be interpreted as a greater need for monitoring. We also explore the impact of activism on covenant intensity. On average, the total number of covenants for pre-activism loans is 4.54 and for post-activism loans it is 4.43. The difference is not statistically significant. When we examine the two types of covenants separately, we find that the number of financial covenants decreases significantly (from 2.03 for pre-activism loans to 1.90 for post-activism loans). The change in number of general covenants (from 2.36 to 2.45) is statistically insignificant. Thus, the total number of covenants appear to be unaffected while there is a decrease in financial covenants.

Taken together, the increase in loan interest rate, higher demand for collateral and smaller syndicate size following the involvement of the activist suggests that lenders are reacting to lowering of the creditworthiness of their borrower. While these results are consistent with the wealth expropriation arguments proposed by Klein and Zur (2011), these univariate tests do not control for loan and borrower-specific characteristics. In the next section, we describe results of various multivariate tests which also suggest a heightened level of concern for the lenders of firms targeted by activists.

4.2. Multivariate analysis: Effect of Hedge Fund Activism on Loan Terms

While the univariate results described in Table 6 suggest that the arrival of activist investors is followed by a significant loan interest rate increase as well as higher collateral requirements, these

results do not take into account various loan and borrower specific characteristics that are also key determinants of loan contract features.[14] A higher interest rate on new loans is driven by changes in these underlying characteristics. For example, if activists are more likely to target firms experiencing a deterioration in their performance, the higher loan interest rate and collateral requirements may simply reflect the continued performance decline of the target firm. To better isolate the impact of involvement of an activist hedge fund on loan terms, we estimate the following basic regression:

$$(Loan\ Term)_{i,j,t} = \alpha + \beta(Post\ Activism)_{i,j,t} + \gamma(BC)_{j,t} + \delta(LC)_{i,t} + \epsilon_{i,j,t} \quad (1)$$

where i indexes loan facility, j indexes the targeted borrower and t indexes time, The dependent variable $(Loan\ Term)_{i,j,t}$ is one of the four loan contract features of interest that are described below. $(BC)_{j,t}$ and $(LC)_{i,t}$ are borrower-specific and loan-specific controls respectively. Our key variable of interest is $(Post\ Activism)_{i,j,t}$, a dummy variable equal to one if loan i to borrower j was issued after the announcement date of an activist hedge fund's schedule 13D filing. Finally $\epsilon_{i,j,t}$ is an error term. The coefficient β captures our estimate of the effect of hedge fund activism on loan contract terms.

We focus on four key features of loan contracts. First is the interest rate charged on the loan. As done in previous work (Chava et al., 2009; Sunder et al., 2014) we define the interest rate as the natural logarithm of the all-in-spread drawn (AISD) and denote it as *Log(Loan Spread)*. The all-in-spread drawn is the coupon spread over LIBOR on the drawn amount plus the annual fee in basis points. The second key loan term we examine is *Collateral* which is a dummy variable equal to one if the loan is secured by collateral, and zero otherwise. The third term is *Log(Maturity)* which is the natural logarithm of the loan maturity in months. Our final loan term is *Loan Size/Assets* and it captures the size of the loan relative to the size of the borrower. This is the ratio of the loan amount (in dollars) scaled by the borrower's book value of assets.

We include a number of borrower-specific variables to control for their impact on these loan

[14] See for example, Ivashina (2009), Bharath et al. (2011) and Prilmeier (2017).

terms. These include *Size*, *Leverage*, *Market-to-Book*, *Coverage*, and *Profitability*. *Size* is the natural logarithm of the book value of the borrower's assets in year 2000 dollars. *Leverage* is calculated as the book value of debt divided by the book value of equity. *Market-to-Book* is the ratio of market value of assets to the book value of assets. *Coverage* is the natural logarithm of 1 + (EBITDA divided by total interest expense). *Profitability* is the ratio of EBITDA to sales. Loan control variables include *Log(Loan Size)*, *Log(Maturity)*, and *REL(Amount)*. *Log(Loan Size)* is the natural logarithm of the loan value (in million USD). *Log(Maturity)* is the natural logarithm of the loan maturity in months. Repeated borrowing from the same lender has been shown to be associated with significantly lower loan spreads (Bharath et al., 2011). *REL(Amount)* is a continuous variable defined at the loan level as the dollar amount of loans by bank b to borrower i in the last 5 years, divided by the total dollar amount of loans by borrower i in the last 5 years. Thus, in our case, it measures the relationship strength between a borrowing target firm and its bank at the time the loan is issued. Finally, we include calendar year dummies, industry dummies based on the one-digit SIC code of the borrower, loan purpose dummies[15], loan type dummies, and dummies for the long-term credit rating (S&P). We provide a detailed description of all the variables in Appendix A.

We present the results of estimating the regression model in equation 1 in Table 7. Consistent with the prior work of Sunder et al. (2014), we find that holding all else constant, a loan obtained after the involvement of an activist hedge fund has a significantly higher loan spread. Columns 1 and 2 illustrate the incremental impact of activist engagement on loan spread. The coefficient for *Post Activism* is 0.043 (significant at the five percent level). This translates into a significant economic magnitude of over 10 basis points increase interest spreads that is attributable to an activist intervention.[16] Arguably, if lenders are worried about wealth expropriation by the activist, we expect them to charge a higher interest rate for the heightened risk. Thus, our results corroborate the evidence for bond yields documented by Klein and Zur (2011) who attribute their results to wealth expropriation from bondholders to shareholders.

Table 7 also presents how an activist's involvement affects a number of other loan terms. Since,

[15]Following Carey et al. (1998), we group the different stated loan purposes in Dealscan into one of four categories: "General purposes", "Recapitalization", "Acquisition", and "Miscellaneous".

[16]The percentage change in loan spread is $100\times((exp(\beta)-1))$ as *Post Activism* is a dummy variable. A coefficient of 0.043 translates into a 4.39% increase in spread. Since the loan sample has an average spread of 240 bps, 4.30% implies an increase of 10.54 bps.

on average, lenders charge a higher spread after an activist involvement it is reasonable to argue that lenders may also mitigate the credit risk by asking for collateral. In columns 3 and 4 we estimate a probit regression in which the dependent variable is *Collateral*, a dummy variable equal to one if the loan was secured and zero otherwise. We also include two additional control variables that have been used in prior studies focusing on the role of collateral (e.g. Berger and Udell (1990)). First, *Tangibility* is defined as the ratio of Net Plant, Property and Equipment (NPPE) to total book value of assets. Firms with a larger proportion of fixed assets are in a better position to offer collateral. The second additional control variable we employ is $Loan\ Concentration = \frac{Loan\ Amount}{Existing\ Debt + Loan\ Amount}$. Berger and Udell (1990) show that the higher is the loan concentration, the greater is the likelihood that a lender will demand collateral. The coefficient for *Post Activism* is positive and significant. Thus, there appears to be a significant increase in collateral requirement for loans after the activists' involvement. Holding all other variables at the sample mean, the coefficient of 0.140 on Post Activism implies that the probability of a lender requiring collateral on loans issued after activism increases by 4.77% compared to loans issued before activism.

In columns 5 and 6 we examine the impact of hedge fund activism on loan maturity. Again, we employ additional control variables that have traditionally been shown to be related to loan maturity. Hart and Moore (1994) develop a theoretical model which predicts that assets with a long (short) economic life are likely to be financed by longer (shorter) maturity debt. Following Barclay et al. (2003) we estimate *Asset Maturity* as the weighted average of current assets and NPPE.[17] Following Barclay and Smith (1995) we also include a dummy variable that equals one if the borrower is from a regulated industry (i.e. SIC code of 4900 to 4999). While the coefficient on *Post Activism* is negative, it is not significant. Similarly, the involvement of a hedge fund does not appear to make a significant difference in the size of the loan (columns 7 and 8). Importantly, all the control variables have coefficients with sign and significance predicted by prior studies. For example, *Leverage* has a significant and positive coefficient for both the loan spread as well as collateral regressions. Similarly, *Asset Maturity* has a significantly positive coefficient for loan maturity. Overall, the results of Table 7 show that while the arrival of an activist hedge fund is

[17] We estimate $Asset\ Maturity = \frac{CA}{(CA+NPPE)} \times \frac{CA}{COGS} + \frac{NPPE}{(CA+NPPE)} \times \frac{NPPE}{Depreciation}$

associated with significant increase in loan spreads and the probability of pledging collateral, the other loan terms (maturity and relative loan size) are not impacted in a significant manner.

4.2.1. Potential Channels for Wealth Expropriation

We next explore a number of potential channels via which the activists expropriate wealth from lenders. We are especially interested in examining if activism leads to increases in credit risk of the target. Table 8 reports the results of our analysis. To investigate changes in the target firm's credit risk, we group all loans into two categories: loans made before the date of targeting (pre-activism) and loans made after the date of targeting (post-activism). For each group we retain those loans for which there is a S&P credit rating (Compustat item *splticrm*) for the borrower for fiscal year ending immediately before the year in which the loan was contracted. We transform the letter based rating into a numerical credit score assigning the highest value to the highest credit rating (i.e., AAA = 22, AA+ = 21, etc.). Thus, a one unit decrease in credit score reflects a downgrade by a single notch (e.g., AA+ to AA). Next, we calculate the average credit rating scores for the two groups. The first row of Table 8, Panel A shows that the average credit rating of loans made in the pre-activism period is 11.20 (this corresponds roughly to a BB rating) compared to the average rating score of 10.94 for loans made in the post-activism period. While the difference is statistically significant at the ten percent level, in terms of economic significance it translates into a difference of a quarter of a credit notch. Note that for this test we focus on *all loans* made in the pre- and post-activism period for which a borrower rating is available. Thus, it includes borrowers that may have borrowed only in the pre- or only in the post-activism period. In contrast to this "unbalanced" sample, we construct a sample including only those firms that borrowed at least once before and once after the date of activist targeting. We denote this sample as the "balanced" sample and repeat the same analysis focusing on loans obtained by borrowers where the S&P rating is available. The second row of Panel A reports the results from this test. The results are similar to those reported for the unbalanced sample, the average rating score drops from 11.29 in the pre-activism period to 10.96 in the post-activism period. The change is significant at the five percent level, and implies a downgrade of almost one third of credit rating notch. Thus, it appears that an increase in credit risk is one possible channel through which hedge fund activism impacts bank loan terms.

Lenders may also tighten target firm loan terms as a result of increased firm information asymmetry following activism. We examine the changes in information asymmetry for firms following

the involvement of an activist. Panel B of Table 8 reports the results for changes in target firm information asymmetry after compared to before activism. We rely on a number of information asymmetry proxies identified by previous studies: analyst following, dispersion of analyst estimates, firm size, and asset tangibility (Leary and Roberts, 2010). To construct the first two measures, we merge our data with the IBES summary history files. Number of analysts is the natural logarithm of (1 + the number of analysts). The count is based on the number of analysts with a one-year ahead EPS forecast for the first month of each fiscal year. Forecast dispersion is the standard deviation of the one-year ahead EPS forecast for the first month in each fiscal year. Firm size is measured by the book value of assets (Compustat item at), and Tangibility is the ratio of property, plant and equipment to the book value of assets (Compustat items ppent/at). Dahiya et al. (2017) show that the Amihud measure of illiquidity is also a significant proxy for information asymmetry. We follow the methodology of Amihud (2002) to construct this variable.[18] We test how these information asymmetry measures change for loans made before and after hedge fund activism using the same approach as we employed for estimating changes in the credit risk. We conduct this analysis for both the unbalanced as well as the balanced sample of loans. The only variable that shows a significant difference is the asset tangibility ratio, which shows a decline from 0.32 to 0.30 for the unbalanced sample. However, this decline is insignificant for the balanced sample. Overall, we are unable to find strong statistical evidence for changes in firm information asymmetry after the involvement of an activist.

Greenwood and Schor (2009) classify activism into 9 distinct categories based on the stated strategy of the activist. In an additional test we explore if certain hedge fund strategies are more damaging to debtholder's value. Following the methodology of Greenwood and Schor (2009) we collect information on the Schedule 13D filing's Item 4, under which the activist states the purpose of activism. Some of the 13D filings are also accompanied by additional materials as exhibits, which may include a letter from the activist to the target's board of directors. Such letters typically contain accounts of earlier private discussions with management, or arguments on the preferred plan of action as set out by the activist. Taken together, the stated purpose section and the

[18] We obtain daily stock data from CRSP and apply the following sample selection criteria: (i) the stock has return and volume data for more than 200 days in a given year; (ii) the stock price is greater than \$5 at the end of year. Low-price stock returns may result in extreme values and add noise to the variable. Amihud illiquidity is the annual average ratio of the daily absolute return to the (dollar) trading volume on that day.

accompanying material can signal to market participants the activist's intended course of action for the target. We explore whether the activist purpose represents a potential channel of wealth expropriation, i.e., whether lenders incorporate the activist's stated purpose into their loan pricing.

We collect the stated purpose section (Item 4) from the initial Schedule 13D filing for all 814 activism events in our sample. If a letter to the board of directors is attached as an exhibit to the Schedule 13D, we read the letter and note the activist's intention in addition to any information listed directly under Item 4 of the filing. We classify the activism purposes in nine categories as identified by Greenwood and Schor (2009): (1) intention to "engage" with management, (2) changes to the capital structure, (3) changes to corporate governance, (4) changes to the business strategy, (5) seeking strategic alternatives, including a spin-off or business restructuring, (6) seeking a sale of the company or part of its assets, (7) blocking a merger, both as acquirer and target, (8) provide financing during bankruptcy of financial distress, and (9) pursuing a proxy contest. To test if lenders consider the activists' stated purpose in their lending decisions we re-estimate the multivariate specification examining the impact of stated purpose on loan spreads (Column one of Table 7). We estimate this specification for each of the nine activist purposes separately. We find that loan rates increase significantly in the post-activism period for two categories of the stated purpose at the time of announcement: "capital structure" and "corporate governance". The capital structure category includes instances where the activist requests a recapitalization, stock or debt issuance, restructuring of debt, dividends or a stock repurchase. Almost all of these actions divert cash flow from creditors to equity. Thus, it seems reasonable that the lenders will be most concerned when the stated purpose of activist targeting is to demand change in capital structure. The other category where we see significant increase in loan spreads is corporate governance. In this category the activist seeks to declassify the board, remove a poison pill, elect activist-selected directors, or fire a company officer or board member. We find no significant impact of activism on loan rates for the remaining seven categories. Thus, it appears that lenders are especially sensitive to activism events where the activist intends to implement significant changes in capital structure or corporate governance. We report these results in the Internet Appendix.

Finally, we also explore whether a pre-existing close relationship between banks and firms can have an impact on loan contracting in the post-activism period. In Table 7, we include a relationship variable REL(Amount), which has a negative sign for the loan spread and collateral specifications, implying that relationship loans (regardless of the loan being pre- or post-activism) have lower

spreads and collateral requirements. However, it remains unclear how relationship lenders behave *after* their borrower was targeted. To explore this issue in more detail we re-estimate all specifications in Table 7 and include an interaction term Post Activism × REL(Amount). The interaction term isolates the effect of getting relationship versus non-relationship loan in the post-activism period. We find that the coefficient for the interaction term is negative but insignificant for both loan spread as well as collateral regressions. This implies that relationship lending does not have a significant effect on the target's cost of borrowing post-activism. The coefficients for the Post Activism dummy remain positive and significant. Thus, we find that targets, on average, pay a higher spread post-activism even after controlling for past relationships. To conserve space we report these results in the Internet Appendix.

4.3. Matched Sample

One potential drawback of our approach is that the sample of hedge fund targeted firms is not a random subset of the Compustat or Dealscan data set. The same factors that make certain firms appealing to hedge fund activists may also play a role in how lenders reset loan terms. As reported by Brav et al. (2015) and as illustrated in Figure 1, hedge fund activists tend to target firms experiencing a decline in their operating performance. A potential concern about the results reported in Table 7 (and also in previous studies) is that an increase in loan spreads in the post-activism period is simply a response to this anticipated decline in performance and not necessarily related to hedge fund activism. To better isolate the impact of activism on loan contract terms, we create a matched group of control loans. We then re-estimate our difference-in-differences tests to evaluate how the loan terms evolve for the targeted (treatment) borrowers versus the control group of borrowers.

We create a control sample as follows. We first restrict the targeted firms' sample to those firms that obtain at least one loan before and one loan after the activist involvement. For this subset of borrowers, we create a matched pair for each target's pre-activism loans. We start by examining all loans to firms in the same one-digit SIC code as the target (industry-match). From these same industry loans, we only retain loans made within 180 days before or after the loan date of the target firm (time-match). Of these time-matched loans we retain only those loans that have an AISD spread within 90% to 110% of the AISD spread of the target firm's loans (spread-match). Finally, from the universe of industry, time and spread matched loans we only keep the loans that

are the same loan type as the loan obtained by the target firm (loan type match). If at this stage we have multiple loans that match all of the four criteria described above we choose the loan closest to the target loan (in the following order), maturity, AISD, loan size and loan origination date.[19]

As we show earlier (In Panel C of Table 2), targeted firms differ significantly from non-targeted firms. We focus on measures of firm performance (profitability, size, etc.) that lenders pay special attention to in deciding the loan terms. We find that firms targeted by hedge funds differ significantly from the universe of Compustat firms. Our four-step matching process described above is aimed at creating a control set consisting of a twin loan for each loan obtained by a target firm before the activist's involvement. By matching on various loan characteristics, we reduce the heterogeneity in the ex-ante credit risk. Thus, each loan pair (targeted and matched) consists of loans of almost identical risk since both loans are made to firms in the same industry, at about the same time, at a similar rate and of the same loan type. Again, it is worth highlighting that the loans of the targeted firms that are matched were all from the period *before* the activist involvement. This allows us to follow the targeted and matched borrower over time past the date of the activist announcement. Thus, for each loan pair, one firm experiences the activist targeting (treatment) while the other firm does not (control). Each control loan is assigned a pseudo-activism target date, corresponding to the Schedule 13D filing date of its targeted counterpart. We are able to find a comparable loan for 377 out of 411 target firms with both loans before and after activism.

Table 9 shows the diagnostics to assess the validity of our matching procedure. Ideally, the loans to the control firm sample and the loans to the target firm sample should be indistinguishable from each other prior to activism. Panel A of Table 9 describes the accuracy of our matching process. We report average values for key loan characteristics for the control sample in the first column and the targeted sample in the second column. A casual examination shows that the sample averages are almost identical for the two groups. The last column provides a formal t- test for differences in means. Except for the number of lenders, none of the loan characteristics are significantly different across the two groups which provides validation for our matching procedure.

After we match each targeted firm with a control firm, we employ a difference-in-differences approach to assess the impact of hedge fund activism. Since by construction all the targeted firms

[19]There was only one loan to a target firm where after all the screens there were still multiple potential matched loans. For this loan, we chose the control loan randomly.

went on to borrow after being targeted, we have the data on loan terms for these post-activism loans. Since each targeted firm has a matching control firm, we follow the borrowing behavior of the control firm and collect data on loans made to these firms after the activism event date of the matched target firm. Since the control firms are not targeted, comparing loan terms for post-activism period for the targeted firms to the post (pseudo-) activism period for control firms allows us to isolate the impact of hedge fund activism on loan terms. As discussed above, Panel A shows that loan contract terms before the activist event are almost identical for the two groups. Panel B highlights the change in loan terms for these two groups in the post-activism period. Column 1 reports the average loan terms for the control firms after the (pseudo-) activism event. Column 2 describes the same for loans made to the targeted firms after an activist intervention. While the loan spread (AISD) remains essentially unchanged for the control group (200 basis points before activism and 209 basis points after activism), the targeted firms experience an almost 23% increase in the loan spread (going from 200 bps to 245 bps). The last two columns report the difference in specific loan terms for the control and target firms and the statistical significance of those differences. The target firm loan spread is around 36 basis points higher after activism compared to loans to matched, non-targeted firms. This is both statistically significant (at the one percent level) and economically large (representing an almost 18% increase over the pre-targeting loan spread of 200 bps). Lenders also demand collateral more frequently (68%) for targeted firms compared to the control group (58%) - a difference that is significant at the 1% level. The results also suggest that target firms obtain smaller loans post activism compared to non-targeted firms. The size of the loan made to targeted firms is over 40% smaller compared to the control group - a difference that is significant at the five percent level. The other key difference in loan terms is the syndicate size which is significantly larger for the control firms.

The univariate results described in Table 9 suggest that seemingly similar firms that have borrowed on almost identical terms obtain loans with markedly different terms depending on if they were or were not targeted by an activist hedge fund. To explore this more fully, we estimate a multivariate analysis using target firm loans and matched counterparts. The equation takes the following form:

$$
\begin{aligned}
(Loan\ Term)_{i,j,t} &= \alpha + \beta_1(Post\ Activism)_{i,j,t} + \beta_2(Target\ Firm)_{i,j} \\
&+ \beta_3(Post\ Activism)_{i,j,t} \times (Target\ Firm)_{i,j} \\
&+ \gamma(BC)_{j,t} + \delta(LC)_{i,t} + \epsilon_{i,j,t} \qquad (2)
\end{aligned}
$$

where *Loan Term* is one of four loan terms for loan i obtained by firm j at time t. These are *Log(Loan Spread)*, *Collateral*, *Log(Maturity)*, or *Loan Size/Assets*, all defined earlier in section 4.2. Similarly $(Post\ Activism)_{i,j,t}$, a dummy variable equal to one if the loan i to borrower j was issued after the announcement date of an activist hedge fund's Schedule 13D filing. For the control sample, this variable switches from zero to one after the (pseudo) date of activist involvement in the firm to which the control firm was matched. To distinguish control firms from target firms we include a dummy variable *Target Firm* that is equal to one if the loan is issued to a target firm, and zero otherwise. The variable of interest is the interaction term between *Post Activism* and *Target Firm*, which captures the difference-in-differences effect of activism on loan spreads. A positive coefficient for β_3 would suggest that lenders increase loan spreads following activism and that this effect is attributable only when the event is not a pseudo-event. All other control variables and fixed effects are identical to equation (1).

We report our results of the difference-in-differences specification in equation 2 in Table 10. The first two columns highlight the notable impact of activist intervention on loan spreads. The interaction term has a coefficient of 0.123 (significant at the one percent level). This implies an increase of over 12% on loan rates for targeted firms. Thus, lenders charge significantly higher loan spreads to target firms post-activism relative to before activism when compared to matched, non-targeted firms. The effect on the target firm loan spread for our matched sample is around three times larger compared to our test that only focused on loans to target firms (Post Activism has a coefficient of 0.043 as reported in Table 7). Compared to control firms, target firms are also significantly more likely to provide collateral post activism (columns 3 and 4). Here, the economic interpretation is that the probability of target firms to use collateral on post-activism loans is 12.6% higher compared to the probability of matched, non-targeted firms to provide collateral after

pseudo-activism (evaluated at the mean of all other covariates).

In addition, the final column in Table 10 suggests that target firms obtain smaller loans post activism. The significant coefficient of -0.0628 for the interaction term between Post Activism and Target Firm implies that target firms obtain post-activism loans that are 6.28% smaller than loans issued post-pseudo-activism to non-targeted firms. Thus, after matching our target firm loans to similar non-target firm loans we document results that are similar to those reported in Table 7 in terms of significance but are much larger in terms of impact. By using the matched sample, we provide a robust test of the findings by Sunder et al. (2014), that loan spreads tend to increase following activism. In addition, we provide new results that show a greater demand for collateral if the borrower has been targeted by an activist hedge fund. However, the question that still remains is why do lenders to a target firm adjust loan pricing upward (and demand more collateral) when their borrower is targeted. We explore this in the section below.

4.4. Multivariate Analysis: Effect of Equity CAR Heterogeneity on Loan Terms

Previous studies have documented large cumulative abnormal equity returns around hedge fund activism (e.g. Brav et al. (2008) and Brav et al. (2015)). The source of this value accretion to the equity holders has been the subject of scholarly debate. Some studies (e.g. Brav et al. (2015)) provide evidence that there is a significant operational improvement for targeted firms once an activist gets involved. Their findings suggest that the stock market incorporates these anticipated improvements in the share price at the time of the announcement. However, Klein and Zur (2011) report that there is a significant wealth loss for the bondholders of the targeted firms around the date of activist involvement and argue that their findings are consistent with wealth transfer from creditors to shareholders. To investigate potential expropriation of debtholders we exploit the heterogeneity in equity returns around the date of announcement of activist involvement. We divide our sample of targeting events in two groups based on the cumulative abnormal returns (CAR) around the date of Schedule 13D filing by the activist hedge fund using the [-10,+10] day announcement window return.[20] We denote the first group of targets as *High CAR* firms following Krishnan et al. (2016), who define these as targets with stock announcement returns in excess of

[20]We use the 21-day window CAR as it has been used in prior studies of hedge fund activism (Krishnan et al., 2016). In our robustness tests we use different event windows and find that the results remain robust to the choice of event window length.

10%. We classify the remaining activism events as *Low CAR* (i. e. the 21-day window announcement return is equal to or below 10%). For our sample, a 10% CAR in the 21-day window around the event corresponds to the 65^{th} percentile of the distribution. Thus, almost one-third of our sample experiences an announcement window CAR of more than 10%.

We first compare the *Low CAR* and *High CAR* target subsamples by providing summary statistics and performing a univariate test for differences in firm characteristics for these two groups. Our results are reported in Table 11. We find that on average, there is no significant difference between target firms in these two groups for the period immediately before being targeted by an activist. As reported in Panel C, across a broad spectrum of firm attributes such as size (book value), growth, market to book ratio, and profitability, we find that the differences are statistically insignificant.[21] This suggests that the cumulative abnormal return around the activism event is unlikely to be correlated to any specific set of firm attributes. The lack of meaningful differences in firm characteristics between *High CAR* and *Low CAR* targets suggests that pre-activism firm-level characteristics are not the driving factor for announcement-related CAR. We also examine the cumulative abnormal return for different stated purposes of activism, and classify each event's purpose following the taxonomy of Greenwood and Schor (2009). We find that different activist purposes are associated with similar CARs. Results are unreported for brevity. Next, we turn our attention to loan characteristics before and after activism.

We compare the loan terms for loans taken prior to being targeted and for loans obtained after an activist's involvement for the Low CAR and High CAR subsamples. These results are reported in Table 12. Panel A focuses on Low CAR targets, i.e. firms that did not experience a large positive announcement reaction to their share price when an activist involvement was reported. The first column lists the average loan characteristics for all loans that were obtained up to three years before the date of Schedule 13D filing. The second column reports the same information for loans obtained in the three years immediately after the announcement of activist involvement. As shown in columns 3 and 4, there is little change in spread charged on the loans, loan size, loan maturity, and collateral requirement. We repeat this analysis for the High CAR firms and report our results in Panel B. An analysis of changes in loan characteristics for this group highlights some significant differences.

[21] In Panel C, the only noticeable difference we observe is that *High CAR* target firms have a $410 million lower market value, which is significant at the 10%-level.

On average, we find that for the High CAR firms, the post-activism loans carry a spread that is almost 47 basis points higher compared to loans in the pre-activism period. This implies a 20% increase from the pre-activist period loan spreads. The increase is both statistically significant (at the one percent level) as well as economically significant. We also find that there is a significantly higher likelihood that lenders demand collateral for their loans after activist involvement compared to the period before the firm becomes a target (75% versus 65%). While these first differences for pre- and post-activist involvement show interesting patterns, we also want to test the difference in these differences for our two groups (High and Low CAR respectively). To test whether the change in loan spreads for High CAR firms is significantly different from the change in loan spread for Low CAR firms, we perform a difference-in-differences test. Panel C reports the average change in loan characteristics, i.e. post-activism minus pre-activism for Low CAR and High CAR firms in columns 1 and 2. Columns 3 and 4 show the difference of the differences and the t-statistic for significance respectively. We find that loans issued to High CAR firms post activism carry significantly higher loan spreads compared to loans issued before activism and that this difference is also unique to High CAR target firms. Our results provide the first indication of potential expropriation of debtholder wealth, suggesting that lenders are cautious when shareholders obtain high cumulative abnormal returns directly around the activism event.

While the univariate tests provide strong evidence that the spreads charged to High CAR targets is significantly higher, these tests do not take into account other firm and loan characteristics. To control for these characteristics, we run the following multivariate regression:

$$\begin{aligned}(Loan\ Term)_{i,j,t} &= \alpha + \beta_1(Post\ Activism)_{i,j,t} + \beta_2(High\ CAR)_j \\ &+ \beta_3(Post\ Activism)_{i,j,t} \times (High\ CAR)_j \\ &+ \gamma(BC)_{j,t} + \delta(LC)_{i,t} + \epsilon_{i,j,t} \end{aligned} \tag{3}$$

where *Loan Term* is one of four loan terms: *Log(Loan Spread)*, *Collateral*, *Log(Maturity)*, or *Loan Size/Assets*, all defined as before. *Post Activism* is a dummy equal to one if the loan is issued after

activism and zero otherwise. *High CAR* is a dummy equal to one if the target firm's 21-day activism announcement return is higher than 10%, and zero otherwise. The coefficient of the interaction term between *Post Activism* and *High CAR* (β_3) captures the evolution of loan terms for the period before and after activism across the High CAR firms and Low CAR firms. The difference-in-differences approach ensures that the estimates remain unbiased from any time-invariant differences between High CAR and Low CAR firms. All regressions include calendar year, borrower industry (1-digit SIC), loan purpose, and loan type fixed effects. To account for any change in credit risk, we also incorporate dummy variables for the S&P domestic long term issuer credit rating (Compustat item *splticrm*) in our regressions.

The results reported in Table 13 confirm the univariate results reported earlier. The coefficient on $(Post\ Activism)_{i,j,t} \times (High\ CAR)_j$ is positive and significant at the one percent level for the log(loan spread) regression (column 1). Thus, after controlling for various firm and loan characteristics, the loan spread for High CAR targets is over 8.50% higher compared to Low CAR firms.[22] The coefficient on *Post Activism* is not significant, again confirming the univariate results that Low CAR targets do not see significant change in loan spread after being targeted. These results suggest that target firms pay higher spreads only when the target firm experiences a high cumulative abnormal equity return.[23] These results confirm that lenders adjust their loan spreads when their borrower is targeted, but that the heterogeneity in equity returns around activism is an important consideration. Our results are consistent with the wealth expropriation explanation proposed by Klein and Zur (2011). In addition, we find evidence that wary lenders look to protect themselves when we examine other loan terms. For example, the results reported in column 2 of Table 13 show that High CAR targets are significantly more likely to obtain loans requiring collateral. Loans issued post activism to High CAR target firms have a 6.77% higher probability of requiring collateral compared to loans issued to Low CAR target firms, holding all other covariates at the sample mean. Thus, lenders appear to adjust collateral requirements in response to an activist's involvement. In column 3, we fail to find any significant difference in loan maturity for the High CAR targets. The last column provides limited support for wealth expropriation argument. On average, the relative loan size to High CAR targets is lower (significant at the 10% level) when compared to the Low

[22] Based on the difference between coefficient for $(Post\ Activism)_{i,j,t} \times (High\ CAR)_j$ and $(Post\ Activism)_{i,j,t}$

[23] For robustness, we replace High CAR with the continuous cumulative abnormal return. Our results remain largely unchanged.

CAR group. Taken together, the results described in Table 13 suggest that significant changes in loan terms in response to an activist targeting are limited to those firms that experience high stock returns around the announcement date of activist involvement. In this subgroup, the change in loan terms is centered on a significant increase in loan spread, a larger likelihood of obtaining loans that require collateral, and somewhat smaller loan size.

We also examine whether High CAR targets experience a significant improvement (or decline) in operating performance post activism, i.e. whether shareholders are able to distinguish future operational outperformance. We focus on the subsample of High CAR targets and test if the operating performance in the post-targeting period differs significantly when compared to the performance in the most recent year before hedge fund activism. We focus on four commonly used operating performance measures: Return on Assets (ROA), EBITDA margin, Leverage ratio, and Cash holding as percentage of assets. We find little evidence that the operating performance of high CAR targets changes significantly in the post-activism period. We repeat this analysis for longer time windows, and compare performance in the year prior to activism (Year -1) to performance two years (Year +2) and three years (Year +3) after the year of targeting (Year 0). Again we find little evidence of performance improvement. Thus, we fail to find strong evidence for the hypothesis that shareholders bid up the target's stock price around the date of activism in anticipation of future performance improvements. The results are reported in the Internet Appendix.

4.5. Robustness

4.5.1. Purpose of Target Firm Borrowing and Performance Improvement

It may be argued that firms targeted by an activist hedge fund are more likely to deploy capital to new investments in an effort to improve performance. The risk associated with such investments may increase firms' credit risk, and hence firms' cost of borrowing. To explore this potential channel, we examine whether the target firm's level of investment expenditure changes significantly around activism.

We compare investment expenditures in the year after activism (Year +1) to investment expenditures in the year immediately prior to activism (Year -1). Our focus is on two types of investment measures: Research and Development Expenses (RND), and Capital Expenditures (Capex), both scaled by lagged assets. As changes to investment expenditures may be linked to changes in subsequent performance, we also examine two operating performance measures: Return on Assets

(ROA), and the EBITDA margin. We are unable to document meaningful changes to R&D expenditure for targets around activism. However, capital expenditure shows significant *decline* after the arrival of the activist. Average Capex as a percentage of lagged assets is 6.94% in Year -1 and it drops to 5.72% in Year +1. This difference is significant at the one percent level. Return on Assets (ROA) also shows a significant *decrease* by 1.52 percentage points in the year immediately following activism. We find little evidence of changes to the target's EBITDA margin.

We re-estimate these values for longer periods, comparing sample means in the year prior to activism (Year -1) to those two years (Year +2) and three years (Year +3) after the year of activism. The ratio of Capex to lagged assets continues to be significantly lower compared to Year -1. However, the ROA rises monotonically and by the third year after activism (Year +3) there is no statistically significant difference in ROA level when compared to the year immediately prior to activism (Year -1).

Overall, it appears that target firms decrease their level of production expansion (Capex) post activism. In addition, we find that targets retain a similar level of R&D expenditures post activism compared to the year before activism. These two findings suggest that it is unlikely post-activism loans are used for new investments. The results are reported in the Internet Appendix.

4.5.2. Balanced Sample

Our difference-in-differences estimation reported in Table 13 is based on all loans (made in the six-year window [-3, +3]) to firms that were targeted by an activist hedge fund. Some of these firms obtained a loan only in the three-year period before the activist involvement and did not borrow in the period following the activist involvement. However, firms with only loans before activism and none after may differ significantly from target firms that obtained loans both before and after activism. This may introduce selection bias in our standard difference-in-differences results even after controlling for firm characteristics. To address this concern, we create a more restricted sample of targets to examine how an activist's campaign affects the loan terms. We start by focusing on the loan origination records for all the target firms in our sample and only retain those firms for which we observe at least one loan origination in the three-year period before the activist announcement *and* at least one loan origination in the three-year period after the activist involvement. We denote this subsample as the "balanced sample." We re-estimate our regression model described in equation 3 for this balanced sample. Our results confirm the positive

relationship between High CAR target firms and subsequent higher loan spreads. Further, the balanced sample results show that announcement of activists' targeting for firms experiencing high CARs appear to have an even larger impact on loan spreads. The coefficient for the interaction term $(Post\ Activism) \times (High\ CAR)$ is 0.128 (significant at the 1% level). The coefficient is almost 40% larger than for the interaction terms in the unbalanced sample and suggests that high CAR target firms pay 14% higher loan spreads. We report these results in the Internet Appendix.

4.5.3. Choice of CAR Event Window

Following Krishnan et al. (2016), we have reported our results based on a 21-day event window. While the 21-day window is most frequently used, previous studies also use alternative event windows around activism. For example, Brav et al. (2008) use a symmetrical 41-day window, while Klein and Zur (2011) use an 11-day window. To check the robustness of our results, we re-estimate all our tests using five alternative equity return event windows. We calculate the cumulative abnormal returns (CAR) for the symmetrical 3-, 5-, 7-, 11-, and 41-day event windows, and use these CARs to assess the impact on loan spreads post activism. As before, for each event window, we use the 10% CAR as a threshold to define an event as High CAR. We find that the coefficient for the interaction term $(Post\ Activism) \times (High\ CAR)$ is positive and significant at the 1%-level across all alternative event windows. Thus, the increased loan spreads for target firms with high equity returns is robust to the choice of the event window. The results are reported in the Internet Appendix.

We also check if there is a reversal in cumulative abnormal return after activism. If high CAR simply reflects stock price overreaction, this should reverse itself in the post-event window. We examine different long-horizon time windows beyond the 21-day event window (including 100, 140, 160 days after activism) and find little evidence that the announcement CARs are an overreaction. We find that, on average, the targets that generated high CAR around the activist hedge fund intervention also had a positive cumulative abnormal return over longer post-event window periods. Thus, these firms do not exhibit a reversal in stock price. Firms with low CAR around activism, on the other hand, had an insignificant return over the same post-event time period.

4.5.4. Balanced Sample and Multiple CAR Event Windows

To mirror the robustness results reported in section 4.5.2 in which we restrict our analysis to the "balanced sample" (targets that obtain at least one loan both before and after the activist

involvement) we repeat our event window robustness tests for the balanced sample. Effectively we combine the two robustness tests described above. The results reported in the Internet Appendix show that similar to the results for the 21-day window, the coefficient on the interaction term $(Post\ Activism) \times (High\ CAR)$ continues to be statistically significant and much larger compared to the unbalanced sample.

4.5.5. Threshold in Defining High CAR - Low CAR

In our robustness tests for different event windows, we defined *High CAR* as a dummy variable that takes the value one if the returns around the announcement date exceed 10%. However, stocks may not move as much over shorter event windows compared to longer windows, potentially affecting the segregation of our sample along the High CAR-Low CAR dimension. We address this concern by using an alternative definition of High CAR based on the cross-sectional distribution of announcement CAR for each window. We assign a value of one to *High CAR* if the equity cumulative abnormal return exceeds the 75^{th} percentile, and assign a value of zero otherwise. The results based on this alternative definition of High CAR are reported in the Internet Appendix. The positive and significant relationship between high stock returns around activist targeting announcement and loan spread continues to be statistically significant across all event windows. This confirms our earlier results that lenders are significantly more likely to charge a higher loan spread if the activist targeting of their borrower is accompanied by a large equity return.

5. Conclusion

A number of recent studies have documented that when firms are targeted by activist hedge funds, there is a significant change in the financial policies and operating performance of the targets. We contribute to this growing literature by focusing on how the arrival of a hedge fund activist affects the lending contract terms for a firm in which an activist hedge fund has acquired a substantial equity holding. Our starting point is the set of two well-established and widely reported results. First, the announcement of hedge fund activism, on average, generates a large positive cumulative abnormal returns (CAR) for the targets. Second, there is a large cross-sectional variation in the announcement CARs with a substantial proportion of targets reporting negative announcement returns. We exploit this variation to test if lenders consider the activist targeting of their borrower

to be beneficial or harmful to their interests. This is an important issue as the positive announcement returns for shareholders have been interpreted both as favorable (activists improve operating performance) and detrimental (activists appropriate wealth from creditors to shareholders) for the lenders. We first show that, on average, lenders charge significantly higher loan spreads and are more likely to demand collateral for the loans made to targeted firms *after* the activist's initial involvement. We confirm this finding using a variety of robustness tests. Our main result is based on a simple argument that if wealth expropriation creates the bulk of shareholder wealth, then both an increase in loan rates and a greater demand for collateral should be concentrated in the firms that experience the largest announcement-related equity returns. We show that this is indeed the case. The increase in loan spreads is almost exclusively limited to those targets for which the activist announcement CAR is the largest. We also find that the high CAR targets are significantly more likely to post collateral for their loans. Thus, we provide empirical evidence that lenders take into account potential wealth expropriation when they issue new loans to targeted firms.

Our findings also point to some new avenues of future research, two of which we describe here. Most of the existing studies of hedge fund activism, including our paper, have focused on the impact of such an intervention on the target firm's shareholders and debtholders. Arguably, activist investors also have a major impact on other stakeholders in the targeted firm. These stakeholders include employees, customers, and suppliers. Conceivably, the financial and operational changes precipitated by activist hedge funds can also have a lasting impact on the communities in which their targets are located. A number of popular press stories have highlighted these concerns.[24] How hedge fund activism impacts the myriad stakeholders of their targets is an interesting research question. Such a broad examination, however, is beyond the scope of our paper.

Second, we do not explore the impact of actions undertaken by "non-activist investors." The focus of our paper is the impact of activist investors on debt contracting of the firm. Thus, we focus exclusively on the "voice" channel of corporate governance. In this setting, a significant shareholder (i.e. the activist) pushes for changes via monitoring and intervening in the operating and financial strategy of the firm. However, the "exit" channel may also lead to significant impact

[24]For example, a 2016 story in the Atlantic magazine describes the impact of hedge-fund investor Nelson Peltz and his company Trian Fund Management on DuPont Chemicals. The article states "So-called activist investors are increasingly gaining control of legacy corporations, forcing them to trim payrolls and downsize research operations—and, quite possibly, damaging the entire economy." (Available at `https://www.theatlantic.com/business/archive/2016/11/activist-investors/506330/`)

on managerial decision making. If the shareholders disagree with the strategic choices of their firms' managers they can simply sell (i.e. exit) their stake. Arguably, if enough shareholders exit, the resulting decline in share price can provide the managerial discipline via the market for corporate control. When passive investors (in contrast to the activist investors that we examine in this paper) undertake large equity block transactions (buying as well as selling) they are required to report the transaction details to the SEC in Form 13G. Future work that explores the impact of such block transactions of passive investors on the corporate governance of a firm can provide valuable insights.

Appendix A : Variable definitions

Variable	Description
Activism-related	
Post Activism	A dummy variable equal to one if the loan was issued after the hedge fund activism event, and zero otherwise.
High CAR	Dummy variable equal to one for target firms with cumulative abnormal stock returns in excess of 10% in a 21-day window around the event date.
Low CAR	Dummy variable equal to one for target firms with cumulative abnormal stock returns below or equal to 10% in a 21-day window around the event date.
Target Firm	Dummy variable equal to one for target firms, and equal to zero for matched control firms.
Firm-level [Compustat code]	
Assets	The book value of assets in millions of real year 2000 dollars of the firm. [at]
Log(Assets)	The natural logarithm of Assets.
Asset growth	The year over year growth rate in the book value of firm assets. [(at_t - at_{t-1})/at_{t-1}]
Sales	Sales in millions of real year 2000 dollars of the firm. [sale]
Market Value	Market capitalization in millions of real year 2000 dollars. [csho*prcc_f]
Return on Assets (ROA)	EBITDA/assets. [oibdp/at]
EBITDA/Sales	EBITDA/sales. [oibdp/sale]
Total Payout Yield	The sum of dividends and stock repurchases, scaled by market value. (Common dividend + preferred dividend + share repurchases) / (Market Value) [(dvc+dvp+prstkc) / (csho*prcc_f)]
Cash / Assets	Cash divided by the book value of assets. [che/at]
Leverage	The sum of short-term and long-term debt, scaled by the book value of assets. [(dlc+dltt)/at]

Capex / Assets	Capital expenditures divided by the book value of assets. [capx/at]
Market-to-Book	The market value of assets divided by the book value of assets. [(at-ceq+csho*prcc_f)/at]
Coverage	The ratio of EBITDA to interest expenses. [oibdp/xint]
Tangibility	The ratio of NPPE to the book value of assets. [ppent/at]
Asset Maturity	The weighted average of maturity of current assets and Net PPE. [(act/(act+ppent))*(act/cogs) + (ppent/(act+ppent))*(ppent/dp)]
Log(Asset Maturity)	The natural logarithm of Asset Maturity.
Regulated	A dummy variable equal to one for firms in the utilities industry (SIC codes 49), and zero otherwise.
Credit rating	The S&P Long-Term Issuer Credit Rating [splticrm] converted into a numeric value following the pattern AAA = 22, AA+ = 21, and so on.
Number of Analysts	The natural logarithm of (1 + the number of analysts). The number of analysts represents the sum of all analysts with a one-year ahead EPS forecast in the IBES database for the first month in each fiscal year.
Forecast Dispersion	The standard deviation of the one-year ahead EPS forecast (IBES database) for the first month in each fiscal year.
Amihud illiquidity	The annual average ratio of the daily absolute stock return to the (dollar) trading volume on that day.
Loan-level	
Loan Spread	The all-in-spread drawn (AISD), which equals the coupon spread over LIBOR on the drawn amount plus the annual fee in basis points.
Log(Loan Spread)	The natural logarithm of the all-in-spread drawn (AISD).
Loan Size	The loan facility size in millions of real year 2000 dollars.
Log(Loan Size)	The natural logarithm of Loan Size.

Loan Size / Assets	The loan facility size in millions of real year 2000 dollars, divided by the firm's book value of assets in millions of real year 2000 dollars.
Maturity	The length in months between facility activation date and maturity date.
Log(Maturity)	The natural logarithm of Maturity.
Number of lenders	The total number of lenders in the facility lending pool.
Collateral	A dummy variable equal to one if the facility required the posting of collateral, and zero otherwise.
Loan Concentration	The ratio of the current loan facility amount to the sum of existing debt and the amount of the loan facility.
Total Covenants	The sum of General and Financial Covenants.
Financial Covenants	The sum total number of financial covenants, which are based on accounting ratios.
General Covenants	The sum total number of sweeps (asset, debt, equity, and insurance), and dividend restrictions.
REL(Amount)	The ratio of the dollar value of facilities with the current lead bank in the last five years, divided by the total dollar value of facilities borrowed by the firm in the last five years.

Appendix B : Sample exhibit of a Schedule 13D Item 4

Name of reporting Persons	Jana Partners LLC
Name of issuer	ConAgra Foods
CUSIP Number	205887102
Date of event	June 8, 2015
Date of filing	June 18, 2015

Item 4. **PURPOSE OF TRANSACTION**

The Reporting Persons acquired the Shares because they believe the Shares are undervalued and represent an attractive investment opportunity. JANA believes that the Issuer has significantly underperformed in shareholder value creation. Most significantly, the acquisition of Ralcorp Inc. in January 2013, which in JANA's opinion was the most significant recent strategic decision made by the Issuer's Board of Directors (the "Board"), has been followed by disappointing performance for shareholders, repeated guidance misses, negative revisions to long term earnings targets, no dividend per share growth, and operating performance challenges. Issues with this acquisition led to the Issuer taking a $1.3 billion impairment on March 26, 2015, after which JANA began purchasing the Shares and analyzing opportunities for improved shareholder value creation. JANA believes that in the period since the Ralcorp acquisition, the Board has failed to adequately address the shareholder value destruction and persistent underperformance that followed the Ralcorp acquisition.

JANA is prepared, if necessary, to nominate Messrs. Alford, Lawrence and Rosenstein (collectively, the "Potential Nominees") for election to the Issuer's board of directors and to participate in the solicitation of proxies in support of the Potential Nominees. JANA believes that the Potential Nominees possess the necessary expertise, experience and focus on shareholder value to help the Board evaluate and address opportunities for shareholder value creation, including but not limited to: undertaking a strategic review of the Issuer's strategy and corporate structure to determine if its businesses are optimally positioned to succeed, as well as various potential alternative transactions and structures; addressing the Issuer's operational performance and cost structure; and optimizing the Issuer's capital allocation policies and capital structure. JANA has requested, however, that the Issuer delay the June 21, 2015, deadline to submit notice of stockholder nominations for the Issuer's

2015 annual meeting of stockholders (the "Annual Meeting") in order to give the Issuer and JANA more time to have collaborative discussions regarding the steps JANA believes the Issuer should take to maximize shareholder value, as well as why JANA believes stockholders would benefit from the addition of the Potential Nominees to the Board.

Figure 1: Target Firm Return on Assets (ROA) Before and After Activism

Figure 1 plots the coefficients β_k, k=-3,...,+3, from the following regression at the firm - year level :

$$ROA_{it} = \alpha_{it} + \sum_{k=-3}^{3} \beta_k d_{it}[t+k] + \gamma Control_{it} + \lambda SIC3 + \lambda t + \epsilon_{it}$$

where ROA_{it} is the return on assets, defined as the ratio of earnings before interest and taxes to total assets. $d_{it}[t+k], k$=-3,...,+3 is a dummy variable equal to one if firm i was or will be targeted by an activist hedge fund in $t+k$ years. t is the year of targeting. $Control_{it}$ are control variables including the logarithm of firm market capitalization and firm age. Firm capitalization is the number of common shares outstanding multiplied by the shares' annual closing price. Firm age is proxied by the number of years since the firm's first appearance in Compustat/CRSP. $\lambda SIC3$ and λt are three-digit SIC and year fixed effects, respectively. The solid line plots the coefficients on $d[t+k]$ dummies, which represent industry-year adjusted ROA. The dotted lines represent 95% confidence intervals.

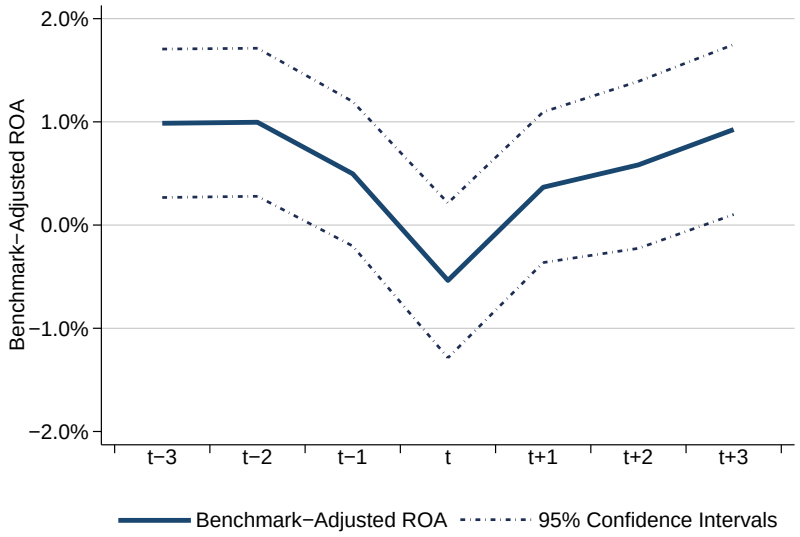

TABLE 1. Sample Description

This table provides an overview of our sample. Panel A outlines the selection criteria for hedge fund events. Sample selection for the initial 3,210 events is as follows. First, we compile a comprehensive list of hedge fund names to identify activist hedge funds. Sample selection criteria are described in detail in section 3.1. Column 1 shows the full sample of Schedule 13D filings. Column 2 describes the sample of events where we are able to find at least one loan issued to the target firm in the [-3 year; +3 year] window around activism. Column 3 shows events, where the target firm obtained at least one loan in the three years before *and* at least one loan after the Schedule 13D filing date.

Panel A: Sample Selection Procedure.

Criterion	**Number of Events**	**Number of Unique Target Firms**
13D files retrieved from SEC's EDGAR tool: 1996 - 2013	3,210	2,418
GVKEY identifier available	3,118	2,330
Exclude if targeted in the previous 5 years	2,461	2,330
Merger with Compustat, drop if not matched	2,015	1,917
Exclude if assets unreported	2,002	1,904
Exclude finance, insurance, and real estate companies - SIC codes 6000 - 6999	1,674	1,588
Size and debt restrictions	1,435	1,363

Panel B: Number of Activist Hedge Fund Events per Year.

Year	Number of Filings	Number of Filings Target Issued Loans at Any Time in the [-3year;+3year] Window Around Activism	Number of Filings Target Issued Loans in the Three Years Prior to and After Activism
1996	55	36	15
1997	103	55	27
1998	73	38	21
1999	49	27	10
2000	60	39	23
2001	50	32	16
2002	63	35	15
2003	70	40	24
2004	72	41	24
2005	107	65	39
2006	130	84	43
2007	149	84	36
2008	127	64	34
2009	48	20	10
2010	70	34	16
2011	68	39	18
2012	69	36	16
2013	72	45	24
Total	1,435	814	411

TABLE 2. Descriptive Statistics - Firm Characteristics

The panels below provide descriptive statistics for our sample. Panel A describes the Compustat universe of firms from 1996 to 2016, excluding all target firm observations. Panel B shows statistics for target firms in the year prior to activism. Panel C displays results for a standard t-test for mean differences between the Compustat universe of firms, and target firm observations in the year prior to activism. Variables are defined in Appendix A. In Panel C, ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: Descriptive statistics for the Compustat universe of firms from 1996 to 2016, excluding all target firm observations.

Variable	Obs.	Mean	St.Dev.	25%	Median	75%
Assets (mln USD)	62,515	5,015	13,005	101.17	489.99	2,669
Asset growth (%, yoy)	62,515	16.49	42.53	-1.88	8.09	22.00
Market Value (mln USD)	62,278	5,134	13,740	87.83	504.68	2,699
Market-to-Book	62,219	1.94	1.49	1.10	1.46	2.19
Return on Assets (%)	62,260	10.27	17.76	5.05	11.96	18.98
EBITDA/Sales (%)	62,260	4.71	65.38	4.03	11.70	21.16
Total Payout Yield (%)	61,895	2.75	4.91	0.00	0.64	3.67
Cash/Assets (%)	62,489	17.46	19.91	2.70	9.44	25.41
Leverage (%)	62,203	32.04	30.53	4.85	28.09	48.27
CAPEX/Assets (%)	61,884	5.93	6.35	1.94	3.93	7.43

Panel B: Descriptive statistics for target firm observations in the year prior to activism.

Variable	Obs.	Mean	St.Dev.	25%	Median	75%
Assets (mln USD)	1,435	1,654	4,951	87.97	292.58	1,066
Asset growth (%, yoy)	1,361	13.93	41.68	-3.03	5.86	17.88
Market Value (mln USD)	1,432	1,250	3,779	69.88	223.40	866.10
Market-to-Book	1,432	1.61	1.01	1.05	1.33	1.85
Return on Assets (%)	1,359	9.31	15.47	3.81	10.53	17.03
EBITDA/Sales (%)	1,431	1.32	71.19	3.04	9.65	16.96
Total Payout Yield (%)	1,427	3.02	6.10	0.00	0.34	3.31
Cash/Assets (%)	1,435	18.68	21.09	2.80	10.08	27.70
Leverage (%)	1,427	31.64	32.56	1.49	25.83	48.58
CAPEX/Assets (%)	1,424	5.69	6.47	1.80	3.49	6.98

continued

Table 2
Continued

Panel C: t-test for differences between the Compustat universe, and target firms in the year prior to activism.

Variable	Compustat Mean	Target Firm Mean	Mean Difference	t-statistic for Mean Difference
Assets (mln USD)	5,015	1,654	-3,361***	-9.77
Asset growth (%, yoy)	16.49	13.93	-2.56**	-2.19
Market Value (mln USD)	5,134	1,250	-3,884***	-10.69
Market-to-Book	1.94	1.61	-0.33***	-8.33
Return on Assets (%)	10.27	9.31	-0.96**	-1.96
EBITDA/Sales (%)	4.71	1.32	-3.39*	-1.94
Total Payout Yield (%)	2.75	3.02	0.27**	1.98
Cash/Assets (%)	17.46	18.68	1.22**	2.31
Leverage (%)	32.04	31.64	-0.40	-0.49
CAPEX/Assets (%)	5.93	5.69	-0.24	-1.44

TABLE 3. Descriptive Statistics - Firm Characteristics for Target Firms With and Without Loans

This table summarizes the characteristics of hedge fund target firms with loans in the [-3 years; +3 years] around activism, and hedge fund target firms without loans in the [-3 years; +3 years] around activism. The table reports values for the year prior to the initial Schedule 13D filing date. Panel A describes hedge fund target firms with at least one loan in the window around activism. Panel B shows statistics for hedge fund target firms without loans around activism. Panel C summarizes the characteristics and displays results for a standard t-test for mean differences between the samples. Variables are defined in Appendix A. In Panel C, ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: Descriptive statistics for target firms with Dealscan match around activism

Variable	Obs.	Mean	St.Dev.	25%	Median	75%
Assets (mln USD)	814	2,575	6,286	213.79	643.87	2,133
Asset growth (%, yoy)	791	14.43	37.99	-2.39	6.51	18.82
Market Value (mln USD)	813	1,847	4,638	124.84	439.75	1,606
Market-to-Book	813	1.49	0.66	1.08	1.29	1.74
Return on Assets (%)	790	12.58	12.94	7.66	12.66	18.51
EBITDA/Sales (%)	812	10.89	36.00	6.13	11.23	19.08
Total Payout Yield (%)	810	3.36	6.19	0.00	0.81	3.84
Cash/Assets (%)	814	10.22	12.63	1.85	5.52	13.67
Leverage (%)	812	40.45	31.62	18.85	37.65	55.65
CAPEX/Assets (%)	808	6.17	6.66	2.09	3.92	7.63

Panel B: Descriptive statistics for target firms without Dealscan match around activism

Variable	Obs.	Mean	St.Dev.	25%	Median	75%
Assets (mln USD)	621	446.57	1,521	47.29	108.10	284.80
Asset growth (%, yoy)	570	13.25	46.34	-4.53	4.91	16.67
Market Value (mln USD)	619	465.38	1,930	41.76	102.77	336.55
Market-to-Book	619	1.76	1.33	1.01	1.37	2.04
Return on Assets (%)	569	4.78	17.44	-2.42	5.88	14.45
EBITDA/Sales (%)	619	-11.24	98.73	-2.81	6.21	14.09
Total Payout Yield (%)	617	2.57	5.95	0.00	0.00	2.41
Cash/Assets (%)	621	29.77	24.55	8.08	24.90	45.93
Leverage (%)	615	20.02	30.07	0.00	5.24	31.80
CAPEX/Assets (%)	616	5.06	6.16	1.47	3.06	6.20

continued

Table 3
Continued

Panel C: t-test for differences between target firms with and without Dealscan match around activism

Variable	Target Firms With Loans	Target Firms Without Loans	Mean Difference	t-statistic for Mean Difference
Assets (mln USD)	2,575	447	-2,128***	-8.26
Asset growth (%, yoy)	14.43	13.25	-1.18	-0.51
Market Value (mln USD)	1,847	465	-1,382***	-6.97
Market-to-Book	1.49	1.76	0.27***	5.05
Return on Assets (%)	12.58	4.78	-7.80***	-9.46
EBITDA/Sales (%)	10.89	-11.24	-22.13***	-5.90
Total Payout Yield (%)	3.36	2.57	-0.79**	-2.42
Cash/Assets (%)	10.22	29.77	19.55***	19.58
Leverage (%)	40.45	20.02	-20.43***	-12.35
CAPEX/Assets (%)	6.17	5.06	-1.11***	-3.20
Total	814	621		

TABLE 4. Ex-Post Performance of Target Firms With and Without Loans

This table reports results for a standard t-test for differences in firm characteristics before and after activism for hedge fund target firms with loans issued in the [-3 years; +3 years] window around activism, and hedge fund target firms without loans issued in the same window. Column (3) reports results for a difference-in-differences estimation. Variables are defined in Appendix A. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Variable	Target Firms with Loans Mean Difference (Year +1) - (Year -1)	Target Firms without Loans Mean Difference (Year +1) - (Year -1)	Mean of Difference-in-Differences	t-statistic for Difference-in-Differences
Assets (mln USD)	26.35	-4.13	-30.48	0.08
Market Value (mln USD)	-32.14	-23.51	8.63	-0.03
Market-to-Book	0.11**	0.16**	0.05	-0.60
Return on Assets (%)	-1.13	-1.12	0.01	0.00
EBITDA/Sales (%)	-1.78	0.54	2.32	-0.66
Total Payout Yield (%)	0.25	0.58	0.33	-0.66
Cash/Assets (%)	0.90	0.06	-0.84	0.60
Leverage (%)	2.30	1.30	-1.00	0.40
CAPEX/Assets (%)	-0.33	-0.37	-0.04	0.08

TABLE 5. Descriptive Statistics - Loan Characteristics

This table shows descriptive statistics for the loans issued to target firms in the [-3 years; +3 years] window around activism. The sample includes all loans issued to target firms from three years prior to three years after the date they were targeted. Variables are defined in Appendix A.

Variable	Obs.	Mean	St.Dev.	25%	Median	75%
Loan spread (basis points above LIBOR)	3,050	239.57	149.45	150.00	225.00	300.00
Loan Size (mln USD)	3,050	324.70	616.58	48.95	139.82	353.85
Loan Size / Assets (%)	3,050	23.68	24.59	7.60	15.87	30.89
Maturity (months)	2,952	49.71	21.47	36.00	60.00	60.00
Number of lenders	3,050	7.26	7.63	2.00	5.00	10.00
Collateral (yes/no)	3,050	0.66	0.47	0.00	1.00	1.00
Number of Total Covenants	1,937	4.49	2.15	3.00	4.00	6.00
Number of Financial Covenants	2,065	1.96	0.94	1.00	2.00	2.00
Number of General Covenants	2,153	2.40	1.76	1.00	2.00	4.00
REL(Amount)	3,050	0.47	0.44	0.00	0.41	1.00

TABLE 6. Loan Characteristics Before and After Hedge Fund Activism

This table reports results from a standard t-test for differences in loan characteristics before and after activism. The sample consists of 3,050 loans issued to target firms from three years prior to three years after the date they were targeted. Variables are defined in Appendix A. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Variable	Before Hedge Fund Activism	After Hedge Fund Activism	Mean Difference	t-statistic for Mean Difference
Loan spread (basis points above LIBOR)	228.88	250.56	21.68***	4.02
Loan Size (mln USD)	325.20	324.19	-1.01	-0.05
Loan Size / Assets (%)	24.02	23.33	-0.69	-0.77
Maturity (months)	49.66	49.76	0.10	0.13
Number of lenders	7.51	7.01	-0.50*	-1.83
Collateral (yes/no)	0.64	0.68	0.04***	2.67
Number of Total Covenants	4.54	4.43	-0.11	-1.05
Number of Financial Covenants	2.03	1.90	-0.13***	-3.18
Number of General Covenants	2.36	2.45	0.09	1.09
REL(Amount)	0.46	0.47	0.01	0.19

TABLE 7. The Effect of Hedge Fund Activism on Target Firm Loan Terms

This table reports coefficient estimates from OLS regressions relating hedge fund activism with target firm loan terms. The Collateral specifications in columns (3) and (4) report coefficient estimates from probit regressions. The sample consists of loans to target firms from three years prior to three years after they were targeted. The dependent variables in this table are Log(Loan Spread), Collateral, Log(Maturity), and Loan Size/Assets. Variables are defined in Appendix A. The regressions include calendar year dummies, industry dummies based on the one-digit SIC code of the target firm, dummies for the stated purpose of the facility, dummies for loan type, and dummies for the long-term credit rating (S&P) of the target firm. t-statistics based on standard errors corrected for heteroscedasticity are in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log(Loan Spread)	Log(Loan Spread)	Collateral	Collateral	Log(Maturity)	Log(Maturity)	Loan Size/Assets	Loan Size/Assets
Post Activism		0.0430**		0.140**		-0.0119		-0.00683
		(2.14)		(2.23)		(-0.53)		(-0.82)
Log(Assets)	-0.0473***	-0.0465***	0.0029	0.0031	-0.0593***	-0.0594***	-0.0553***	-0.0555***
	(-3.68)	(-3.63)	(0.05)	(0.05)	(-4.25)	(-4.26)	(-14.22)	(-14.21)
Leverage	0.00441***	0.00440***	0.0189***	0.0187***	0.000652	0.000657	-0.000119	-0.000117
	(6.63)	(6.60)	(6.88)	(6.80)	(0.82)	(0.83)	(-0.35)	(-0.34)
Market-to-Book	-0.0303	-0.029	0.00846	0.0159	-0.0432**	-0.0435**	0.00989	0.00968
	(-1.65)	(-1.58)	(0.19)	(0.35)	(-2.08)	(-2.09)	(1.35)	(1.32)
Log(Loan Size)	-0.0733***	-0.0726***	-0.256***	-0.252***	0.125***	0.125***		
	(-6.38)	(-6.34)	(-4.70)	(-4.63)	(9.74)	(9.74)		
Log(Maturity)	-0.00443	-0.00357	0.310***	0.312***			0.0563***	0.0561***
	(-0.22)	(-0.18)	(5.58)	(5.61)			(7.93)	(7.91)
REL(Amount)	-0.0438**	-0.0422*	-0.164**	-0.162**	-0.0355	-0.0358	0.0433***	0.0431***
	(-2.02)	(-1.95)	(-2.42)	(-2.40)	(-1.42)	(-1.44)	(4.42)	(4.40)

continued

Table 7
Continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Log(Loan Spread)	Log(Loan Spread)	Collateral	Collateral	Log(Maturity)	Log(Maturity)	Loan Size/Assets	Loan Size/Assets
Coverage	-0.0410**	-0.0400**	-0.109***	-0.105**	0.0152	0.0149	-0.0102	-0.0104
	(-2.56)	(-2.49)	(-2.66)	(-2.57)	(1.04)	(1.03)	(-1.45)	(-1.47)
Return on Assets	-0.0125***	-0.0125***	-0.0141**	-0.0146***	0.00397*	0.00399*	0.00669***	0.00670***
	(-6.12)	(-6.16)	(-2.54)	(-2.64)	(1.85)	(1.86)	(6.48)	(6.50)
Tangibility			-0.236	-0.226				
			(-1.57)	(-1.50)				
Loan Concentration			1.239***	1.228***				
			(4.29)	(4.25)				
Log(Asset Maturity)					0.0306**	0.0302**		
					(2.42)	(2.39)		
Collateral					0.135***	0.135***		
					(4.68)	(4.70)		
Regulated					-0.189***	-0.188***		
					(-3.15)	(-3.12)		
Cons.	6.592***	6.569***	6.980***	6.876***	1.795***	1.798***	0.245***	0.248***
	(28.68)	(28.62)	(9.50)	(9.35)	(7.24)	(7.26)	(2.65)	(2.67)
Obs.	2,699	2,699	2,685	2,685	2,632	2,632	2,699	2,699
(Pseudo) R^2	0.47	0.47	0.26	0.26	0.23	0.23	0.29	0.29

TABLE 8. Effect of Hedge Fund Activism on Target Firm Credit Risk and Information Asymmetry

This table shows results for a t-test in changes to target firms' credit rating and information asymmetry around activism, based on a loan-level data set. The Unbalanced sample includes all target firm loans in our sample for which the variable is available. To be included in the Balanced sample, a target firm requires at least one loan before and one loan after activism for which the variable is available. Variables are defined in Appendix A. Panel A shows results for credit rating. Panel B shows the results for information asymmetry measures. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: Credit Risk

Variable	Before Hedge Fund Activism	After Hedge Fund Activism	Mean Difference	t-statistic for Mean Difference
Credit rating (Unbalanced)	11.20	10.94	-0.26*	-1.87
Credit rating (Balanced)	11.29	10.96	-0.33**	-2.03

Panel B: Information Asymmetry

Variable	Before Hedge Fund Activism	After Hedge Fund Activism	Mean Difference	t-statistic for Mean Difference
Number of Analysts (Unbalanced)	7.49	7.43	-0.06	-0.25
Number of Analysts (Balanced)	8.00	8.11	0.11	0.37
Forecast Dispersion (Unbalanced)	0.30	0.27	-0.03	-0.49
Forecast Dispersion (Balanced)	0.29	0.31	0.03	0.34
Amihud Illiquidity (Unbalanced)	0.49	0.43	-0.06	-1.54
Amihud Illiquidity (Balanced)	0.39	0.33	-0.06	-1.56
Assets (Unbalanced)	3,079.04	2,970.67	-108.37	-0.43
Assets (Balanced)	3,383.43	3,541.56	158.13	0.48
Tangibility (Unbalanced)	0.32	0.30	-0.02**	-2.03
Tangibility (Balanced)	0.32	0.31	-0.01	-1.39

TABLE 9. Matched Sample - Matching Diagnostics

This table reports diagnostics for the matched sample. Panels A and B report results from a t-test for mean differences in loan characteristics between target firms and control firms before, respectively, after activism. Variables are defined in Appendix A. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: t-test for Differences in Loan Characteristics prior to (Pseudo-)Activism

Variable	Mean Control Firms	Mean Target Firms	Mean Difference	t-statistic for Mean Difference
Loan spread (basis points above LIBOR)	199.32	200.24	0.92	0.12
Loan Size (mln USD)	334.11	382.61	48.50	1.05
Maturity (months)	49.37	49.08	-0.29	-0.22
Number of lenders	8.16	7.28	-0.88*	-1.69
Collateral (yes/no)	0.59	0.57	-0.02	-0.59
REL(Amount)	0.49	0.49	0.00	0.08

Panel B: t-test for Differences in Loan Characteristics after (Pseudo-)Activism

Variable	Mean Control Firms	Mean Target Firms	Mean Difference	t-statistic for Mean Difference
Loan spread (basis points above LIBOR)	208.83	245.01	36.18***	5.93
Loan Size (mln USD)	631.40	360.51	-270.89***	-5.61
Maturity (months)	51.09	49.48	-1.61	-1.61
Number of lenders	9.45	7.78	-1.67***	-4.43
Collateral (yes/no)	0.58	0.68	0.10***	4.48
REL(Amount)	0.52	0.51	-0.01	-0.33

TABLE 10. The Effect of Hedge Fund Activism on Target Firm Loan Terms - Matched Sample

This table reports coefficient estimates from OLS regressions relating hedge fund activism with target firm loan terms using a matched sample of non-targeted firms. The Collateral specifications in columns (3) and (4) report coefficient estimates from probit regressions. The sample consists of loans to target firms and loans to matched control firms. The matching procedure is defined in section 4.3. Variables are defined in Appendix A. The regressions include calendar year dummies, industry dummies based on the one-digit SIC code of the target firm, dummies for the stated purpose of the facility, dummies for loan type, and dummies for the long-term credit rating (S&P) of the target firm. *t*-statistics based on standard errors corrected for heteroscedasticity are in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	(1) Log(Loan Spread)	(2) Log(Loan Spread)	(3) Collateral	(4) Collateral	(5) Log(Maturity)	(6) Log(Maturity)	(7) Loan Size/Assets	(8) Loan Size/Assets
Post Activism x Target Firm		0.123***		0.342***		0.0769*		-0.0628***
		(3.00)		(2.69)		(1.81)		(-3.41)
Post Activism		-0.0734**		-0.0424		-0.123***		0.000482
		(-2.46)		(-0.46)		(-4.07)		(0.04)
Target Firm		-0.0235		-0.0934		-0.0566*		0.0421***
		(-0.70)		(-0.89)		(-1.67)		(2.64)
Log(Assets)	-0.0772***	-0.0756***	-0.0256	-0.0275	-0.0583***	-0.0563***	-0.0616***	-0.0618***
	(-5.69)	(-5.60)	(-0.39)	(-0.42)	(-4.20)	(-4.05)	(-15.80)	(-15.75)
Leverage	0.00295***	0.00298***	0.0133***	0.0134***	-0.00049	-0.000448	0.000031	0.000053
	(3.91)	(3.93)	(4.72)	(4.72)	(-0.62)	(-0.57)	(0.09)	(0.16)
Market-to-Book	-0.0589***	-0.0551***	-0.0479	-0.0401	-0.0491**	-0.0486**	0.0283***	0.0280***
	(-3.34)	(-3.18)	(-1.12)	(-0.93)	(-2.55)	(-2.56)	(2.60)	(2.60)
Log(Loan Size)	-0.0644***	-0.0621***	-0.182***	-0.170***	0.139***	0.137***		
	(-5.20)	(-5.06)	(-2.83)	(-2.65)	(10.15)	(9.91)		

continued

Table 10
Continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Log(Loan Spread)	Log(Loan Spread)	Collateral	Collateral	Log(Maturity)	Log(Maturity)	Loan Size/Assets	Loan Size/Assets
Log(Maturity)	-0.00936	-0.012	0.365***	0.373***			0.0695***	0.0676***
	(-0.38)	(-0.49)	(6.09)	(6.21)			(8.84)	(8.50)
REL(Amount)	-0.0777***	-0.0794***	-0.304***	-0.315***	-0.0143	-0.0152	0.0234**	0.0236**
	(-3.17)	(-3.25)	(-4.24)	(-4.37)	(-0.56)	(-0.60)	(2.36)	(2.38)
Coverage	-0.0548***	-0.0557***	-0.277***	-0.278***	0.0353**	0.0314**	-0.00781	-0.00895
	(-2.91)	(-2.98)	(-5.19)	(-5.23)	(2.23)	(1.97)	(-0.88)	(-1.02)
Return on Assets	-0.0118***	-0.0119***	-0.00247	-0.00244	0.00542**	0.00551**	0.00495***	0.00495***
	(-5.02)	(-5.11)	(-0.36)	(-0.36)	(2.44)	(2.47)	(4.15)	(4.19)
Tangibility			-0.311*	-0.316**				
			(-1.96)	(-1.98)				
Loan Concentration			1.440***	1.431***				
			(4.21)	(4.18)				
Log(Asset Maturity)					0.0292**	0.0288**		
					(2.42)	(2.41)		
Collateral					0.124***	0.124***		
					(4.93)	(4.94)		
Regulated					-0.0895*	-0.0850*		
					(-1.78)	(-1.69)		
Cons.	6.909***	6.887***	0.317	0.156	1.392***	1.464***	0.169***	0.154***
	(10.57)	(10.59)	(0.29)	(0.14)	(4.37)	(4.57)	(3.26)	(2.86)
Obs.	2,493	2,493	2,490	2,490	2,431	2,431	2,493	2,493
(Pseudo) R^2	0.51	0.52	0.26	0.26	0.28	0.28	0.34	0.35

TABLE 11. Firm Characteristics for Targets with Heterogeneous Cumulative Abnormal Return (CAR)

This table provides descriptive statistics for the target firm sample in the year prior to targeting, comparing *Low CAR* to *High CAR* target firms. *Low CAR* target firms are target firms with cumulative abnormal stock returns below or equal to 10% in a 21-day window around the event date. *High CAR* target firms are target firms with cumulative abnormal stock returns in excess of 10% in a 21-day window around the event date. Variables are defined in Appendix A. Panel A describes Low CAR target firms. Panel B describes High CAR target firms. Panel C shows results for a standard t-test for mean differences between the two samples. In Panel C, ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: Low CAR Target Firms

Variable	Obs.	Mean	St.Dev.	25%	Median	75%
Assets (mln USD)	911	1,771	5,578	88.83	294.70	1,142
Asset growth (%, yoy)	870	15.34	43.02	-2.37	6.82	18.63
Market Value (mln USD)	908	1,407	4,423	75.94	253.04	952.14
Market-to-Book	908	1.64	0.95	1.09	1.37	1.87
Return on Assets (%)	869	9.56	15.15	4.19	11.39	17.03
EBITDA/Sales (%)	908	0.56	76.41	3.54	10.43	17.76
Total Payout Yield (%)	904	3.02	6.06	0.00	0.42	3.32
Cash/Assets (%)	911	18.68	20.80	2.98	9.91	27.97
Leverage (%)	905	31.03	31.96	1.62	25.81	47.58
CAPEX/Assets (%)	904	5.61	6.54	1.79	3.41	6.67

Panel B: High CAR Target Firms

Variable	Obs.	Mean	St.Dev.	25%	Median	75%
Assets (mln USD)	492	1,484	3,690	94.74	310.64	994.10
Asset growth (%, yoy)	462	11.63	39.58	-4.80	3.80	16.46
Market Value (mln USD)	492	997.32	2,276	63.04	198.09	758.08
Market-to-Book	492	1.55	1.11	1.00	1.24	1.77
Return on Assets (%)	461	8.45	16.17	2.34	9.59	16.83
EBITDA/Sales (%)	491	1.96	62.94	2.16	8.47	15.80
Total Payout Yield (%)	491	3.03	6.15	0.00	0.11	3.19
Cash/Assets (%)	492	19.05	21.79	2.58	10.21	28.42
Leverage (%)	490	31.48	32.20	1.09	25.57	48.76
CAPEX/Assets (%)	488	5.90	6.39	1.87	3.87	7.52

continued

Table 11
Continued

Panel C: t-test for differences in target firm characteristics

Variable	Low CAR Target Firm Mean	High CAR Target Firm Mean	Mean Difference	t-statistic for Mean Difference
Assets (mln USD)	1,771	1,484	-287	-1.03
Asset growth (%, yoy)	15.34	11.63	-3.71	-1.54
Market Value (mln USD)	1,407	997	-410*	-1.92
Market-to-Book	1.64	1.55	-0.09	-1.57
Return on Assets (%)	9.56	8.45	-1.11	-1.25
EBITDA/Sales (%)	0.56	1.96	1.40	0.35
Total Payout Yield (%)	3.02	3.03	0.01	0.03
Cash/Assets (%)	18.68	19.05	0.37	0.31
Leverage (%)	31.03	31.48	0.45	0.25
CAPEX/Assets (%)	5.61	5.90	0.29	0.79

TABLE 12. Loan Characteristics for Targets with Heterogeneous Cumulative Abnormal Return (CAR)

This table reports results for a standard t-test for differences in loan characteristics before and after activism for *Low CAR* and *High CAR* target firms. The sample consists of 3,050 loans issued to target firms from three years prior to three years after the date they were targeted. *Low CAR* target firms are target firms with cumulative abnormal stock returns below or equal to 10% in a 21-day window around the event date. *High CAR* target firms are target firms with cumulative abnormal stock returns in excess of 10% in a 21-day window around the event date. Variables are defined in Appendix A. Panel A describes loans to Low CAR target firms. Panel B describes loans to High CAR target firms. Panel C reports results for a difference-in-differences estimation. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: Low CAR Target Firms

Variable	Before Hedge Fund Activism	After Hedge Fund Activism	Mean Difference	t-statistic for Mean Difference
Loan spread (basis points above LIBOR)	227.21	235.35	8.14	1.21
Loan Size (mln USD)	332.58	323.10	-9.48	-0.33
Loan Size / Assets (%)	23.90	23.60	-0.30	-0.26
Maturity (months)	49.80	49.61	-0.19	-0.19
Number of lenders	7.55	7.15	-0.40	-1.11
Collateral (yes/no)	0.63	0.64	0.01	0.47
REL(Amount)	0.47	0.48	0.01	0.71

Panel B: High CAR Target Firms

Variable	Before Hedge Fund Activism	After Hedge Fund Activism	Mean Difference	t-statistic for Mean Difference
Loan spread (basis points above LIBOR)	228.35	274.96	46.61***	5.16
Loan Size (mln USD)	311.82	328.26	16.44	0.44
Loan Size / Assets (%)	24.17	22.83	-1.34	-0.91
Maturity (months)	49.46	49.37	-0.09	-0.07
Number of lenders	7.65	6.76	-0.89**	-1.98
Collateral (yes/no)	0.65	0.75	0.10***	3.59
REL(Amount)	0.47	0.44	-0.03	-0.93

continued

Table 12
Continued

Panel C: Difference-in-Differences estimations

Variable	Low CAR Mean Difference	High CAR Mean Difference	Mean of Difference-in-Differences	t-statistic for Difference-in-Differences
Loan spread (basis points above LIBOR)	8.14	46.61***	38.47***	3.41
Loan Size (mln USD)	-9.48	16.44	25.92	0.55
Loan Size / Assets (%)	-0.30	-1.34	-1.04	-0.56
Maturity (months)	-0.19	-0.09	0.10	0.06
Number of lenders	-0.40	-0.89**	-0.49	-0.84
Collateral (yes/no)	0.01	0.10***	0.09**	2.49
REL(Amount)	0.01	-0.03	-0.04	-1.17

TABLE 13. The Effect of Cumulative Abnormal Return Heterogeneity on Target Firm Loan Terms

This table reports coefficient estimates from OLS regressions relating hedge fund activism with target firm loan characteristics. The Collateral specifications in columns (3) and (4) report coefficient estimates from probit regressions. The sample consists of loans to target firms from three years prior to three years after they were targeted. The dependent variables in this table are Log(Loan Spread), Collateral, Log(Maturity), and Loan Size/Assets. Variables are defined in Appendix A. The regressions include calendar year dummies, industry dummies based on the one-digit SIC code of the target firm, dummies for the stated purpose of the facility, dummies for loan type, and dummies for the long-term credit rating (S&P) of the target firm. *t*-statistics based on standard errors corrected for heteroscedasticity are in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)
Dependent variable	Log(Loan Spread)	Collateral	Log(Maturity)	Loan Size/Assets
Post Activism x High CAR[21]	0.0916***	0.197**	0.0222	-0.0203*
	(3.40)	(2.14)	(0.76)	(-1.69)
Post Activism	0.00994	0.0767	-0.0289	0.00115
	(0.44)	(1.11)	(-1.12)	(0.12)
Log(Assets)	-0.0479***	0.00117	-0.0628***	-0.0557***
	(-3.69)	(0.02)	(-4.41)	(-13.93)
Leverage	0.00407***	0.0181***	0.000589	-0.000107
	(5.99)	(6.51)	(0.72)	(-0.31)
Market-to-Book	-0.0326*	0.0186	-0.0451**	0.0086
	(-1.78)	(0.41)	(-2.15)	(1.18)
Log(Loan Size)	-0.0719***	-0.244***	0.125***	
	(-6.26)	(-4.48)	(9.64)	
Log(Maturity)	-0.00879	0.306***		0.0570***
	(-0.43)	(5.46)		(7.87)
REL(Amount)	-0.0421*	-0.160**	-0.0430*	0.0417***
	(-1.92)	(-2.33)	(-1.72)	(4.22)
Coverage	-0.0392**	-0.101**	0.0164	-0.0113
	(-2.42)	(-2.45)	(1.11)	(-1.56)

continued

Table 13
Continued

	(1)	(2)	(3)	(4)
Dependent variable	Log(Loan Spread)	Collateral	Log(Maturity)	Loan Size/Assets
Return on Assets	-0.0124***	-0.0147***	0.00416*	0.00686***
	(-6.11)	(-2.65)	(1.91)	(6.57)
Tangibility		-0.201		
		(-1.33)		
Loan Concentration		1.193***		
		(4.13)		
Log(Asset Maturity)			0.0288**	
			(2.27)	
Collateral			0.130***	
			(4.49)	
Regulated			-0.174***	
			(-2.86)	
Cons.	6.639***	6.913***	1.842***	0.234**
	(28.48)	(9.35)	(7.24)	(2.48)
Obs.	2,643	2,629	2,576	2,643
(Pseudo) R^2	0.48	0.26	0.23	0.30

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