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Rent extraction amid borrowers' adversity: evidence from activist short sellers' attacks

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Abstract

Finance theory suggests that the privileged information that traditional banks obtain about borrowers through monitoring creates opportunities for banks to impose informational hold-up costs on such borrowers. Because a surge in borrower risk increases banks' hold-up power, banks with information monopoly should be able to increase their rates beyond the level explained by borrower risk alone. We test this theory using the setting of activist short sellers' public allegations—a setting that increases borrower risk and restricts borrower access to public financing sources—and find, on average, that, after controlling for both *ex ante* and *ex post* changes in borrower credit risk, banks increase loan pricing following activist short sellers' allegations. Our loan pricing results not explained by changes in borrower credit risk are consistent with banks extorting borrowers during times of adversity.

Keywords: rent extraction; private debt; loan pricing; information; activist short sellers.
JEL classifications: G21; G10; G41.

1. Introduction

An ongoing lending relationship affords a lending bank the opportunity to gain privileged access to private information about the borrower—an information trove that is unavailable to nonlenders (e.g., prospective lenders)—and this evolving information asymmetry between the relationship lender and prospective lenders gives the relationship lender an information monopoly (Diamond 1991; Rajan 1992; Houston and James 1996; Marquez 2002). This information monopoly creates a unique situation where prospective lenders face an adverse selection problem that deters competition for a potential lending relationship with the borrower, increasing the borrower's cost of switching lenders as a result[1] and presenting opportunities for the relationship lender to extract rents from the informationally captured borrower (Diamond 1991; Rajan 1992; Schenone 2010). This study exploits the setting of activist short sellers' allegations against borrowers to investigate whether

[1] As Santos and Winton (2008, 1315) and Hale and Santos (2009, 185) noted, "if the borrower seeks to switch to a new funding source, it is pegged as a lemon regardless of its true financial condition."

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relationship banks exploit information monopoly by charging higher interest rates than is conventionally justified by changes in borrowers' credit risk alone.

Since the 2007–2009 financial crisis, activist short sellers have emerged and produced radically different, forensic-type negative research reports about public firms (Ljungqvist and Qian 2016; Brendel and Ryans 2021). In these reports, activist short sellers publicly explain their reasons for shorting a stock to convince long investors to sell their shares, thus benefiting from their short position (Paugam, Stolowy, and Gendron 2021). Anecdotes suggest that the adverse information released in these reports impacts not only equity investors but also other stakeholders, such as banks and regulators. For example, after the attack of Hindenburg Research on Gautam Adani's conglomerate in 2023, which saw the prices of the conglomerate's multiple stocks tumble significantly, India's central bank was concerned with the adverse impact of this short seller attack on credit market stability, asking domestic lending banks to provide details about their exposure to the attacked firm (Reuters 2023). Furthermore, in the same article by Reuters, it was noted that the financial market appeared to punish lenders exposed to the attacked firm.[2] While these allegations can be *costly* to banks, do they also provide banks with the opportunity to *benefit* through, for example, opportunistic rent seeking from attacked borrowers? Specifically, following activist short sellers' allegations, do banks adjust loan rates beyond and above the level justified by changes in borrower risk alone?

The setting of activist short sellers' allegations presents us with the unique opportunity to potentially detect the presence of information rents (i.e., informational hold-up costs) for the following reasons. *First*, activist short sellers' allegations have been documented to reveal novel, negative information about borrowers' credit quality (Ljungqvist and Qian 2016; Brendel and Ryans 2021), which potentially alters (i.e., increases) borrowers' credit risk (Hendershott, Kozhan, and Raman 2020; see, e.g., Griffin, Hong, and Kim 2016; Lleshaj and Kocian 2021; Ho, Lin, and Lin 2022).[3] As an increase in borrowers' credit risk in general increases banks' hold-up power, it should make it easier for a relationship lender to extort a borrower (Rajan 1992; Santos and Winton 2008). *Second*, short selling activities have been shown to restrict borrowers' access to public financing sources (Grullon, Michenaud, and Weston 2015; Wong and Zhao 2017; Meng et al. 2020; van Binsbergen, Han, and Lopez-Lira 2023). This restriction increases a borrower's switching cost, making it difficult for the borrower to switch lenders and therefore affording the relationship lender the opportunity to extract rent (Schenone 2010).

To test our conjecture that banks may extort borrowers following activist short sellers' allegations, we analyze activist short sellers' attacks against US borrowing firms and focus on banks' pricing of new loans made to attacked firms with existing lending relationships over the period of 2008–2018. In all our tests, we use a staggered difference-in-differences (DiD) research design and adopt entropy balancing (Hainmueller 2012) to examine whether, within the same lead arranger, the price of new loans to attacked firms (relative

[2] In the Reuters article, the French bank Société Générale, for example, stated that markets were "overpricing" the risk to Indian lenders of their exposure to the Adani Group and that a selloff in banking shares seemed overdone. Standard asset (equity or credit) pricing models (see, e.g., Duffie and Lando 2001) suggest that capital providers consider *default* and *information* risk when supplying capital. In the anecdote above showing that banks' own investors price banks' exposure to attacked firms, we consider uncertainty as perceived by the lenders' own market (unrelated to lenders' exposure to the attacked firms' potential default likelihood) as a source of *information risk*. Given the above, throughout this article, we consider short sellers' allegations as potentially reflecting or inducing changes in default risk and/or information risk.

[3] The extent of novelty of information contained in these allegations may be challenged by the following factors, which could mute the extent to which borrower credit risk is impacted: (1) through their access to privileged information advantage about borrowers, banks may have been already aware of some—if not all—of the problems described by activist short sellers; (2) some activist short sellers are accused of engaging in spreading false rumors about attacked firms (Cohodes 2020; Mitts 2020); and (3) attacked firms generally attempt to contradict activist short sellers' allegations, often responding to activist short sellers through public denials, press releases, conference calls, internal investigations, or lawsuit threats (Brendel and Ryans 2021).

to nonattacked firms) increases by more than the level that is justifiable by borrower credit risk, following activist short sellers' attacks.

All else being equal, we find that an increase in all-in-drawn spreads in excess of the LIBOR rate on loans made to attacked firms (after controlling for both *ex ante* and *ex post* changes in credit risk) is associated with the publication of activist short sellers' attacks, relative to those for loans made to non-attacked firms. In terms of *static* effects, the increase in loan spreads that we document amounts to an 8 percent increase in loan spreads, equivalent to a 16-basis-point increase over the mean, on average. Regarding *dynamic* effects, we find that this impact on attacked firms emerges only after the allegation events and persists for a few more years, with no significant differences in loan spreads during the pre-allegation periods.[4] Interestingly, we also observe a noticeable "initially-declining but later-increasing" pattern in the effect of allegations on loan spreads that is explained by the prolonged nature of SEC regulatory actions. Specifically, allegations initially lead to a sharp increase in loan pricing, followed by a decline and subsequent stagnation, before rising dramatically in the later periods. This declining phase up to the stagnation point coincides with the period of SEC investigations, whereas the post-stagnation surge in loan spreads aligns with the aftermath of SEC enforcement actions. Notably, we find that loan spread increases are more than twice as large, on average, after the SEC substantiates allegations made by activist short sellers.

Given that our loan pricing results are incremental to controlling for changes in credit risk (implying that short sellers' allegations have an impact on loan pricing, beyond their effect through an increase in credit risk), we interpret this evidence as banks exploiting the adverse impact of activist short sellers' attacks on attacked firms' financing options to extract more value from attacked firms when granting new loans.[5]

As an alternative strategy for testing the rent extraction theory, we directly adopt two measures of economic rent, which are essentially continuous variables reflecting high interest rates that are unexplained by borrower credit risk. Specifically, we measure this variable as the difference between the interest rate charged on loans made to a focal firm and the average interest rate charged on loans (of the same type and purpose and of similar maturity and credit allocation) made to similar-sized peer firms with similar risk profiles. Using the same DiD setup, we indeed find that our proxies for economic rent are positively associated with short sellers' allegations. In further support of the rent extraction story, we also find that activist short sellers' allegations are negatively associated with the likelihood that borrowers switch lenders when they seek new financing. Finally, in a cross-sectional test that considers the interaction of high interest rates and lender switching by borrowers, we find evidence that, following activist short sellers' allegations, attacked borrowing firms that do not switch (who switch) lenders are extorted (not extorted) by banks. These observations point to rent extraction as an economic mechanism underlying our findings.

In addition, we explore heterogeneity in allegations to provide insights into which short sellers' allegation(s) lead(s) banks to increase loan prices the most versus the least, following activist short sellers' attacks. There is ongoing skepticism about the activities of some activist short sellers, with concerns that certain allegations may be misleading or driven by questionable incentives (Cohodes 2020; Mitts 2020; Herbst-Bayliss 2021).[6] While making an allegation is one thing, whether capital allocators—such as banks—perceive it as credible is another matter entirely. One way to assess the significance of these allegations in the eyes of credit providers is to examine how the broader capital market reacts to them. If investors treat an activist short seller's claims as consequential, credit allocators might do the same.

4 This timing evidence alleviates concerns that documented associations are rather driven by some preallegation factors.

5 This interpretation is in line with earlier studies on the impact of informational monopoly on loan pricing (Santos and Winton 2008; Hale and Santos 2009).

6 See a rebuttal by Block (2022).

Accordingly, to address the above issue, we differentiate between allegations that trigger an adverse market reaction and those that do not. Specifically, we define the variable *PRICE_DROP*, which is an indicator variable that equals 1 if the short seller's campaign results in a negative return, as indicated by our data provider, and 0 otherwise.[7] Our findings reveal that allegations that cause stock price decreases have twice the impact of those that do not. Focusing on the subset of allegations associated with a price drop, we further classify them on the basis of their perceived nature or form. We distinguish between allegations that are fraudulent or particularly serious and those that are not. Specifically, "fraudulent/serious" allegations are allegations that fall into one of the following eight categories: (1) accounting fraud, (2) misleading accounting, (3) major business fraud, (4) other illegal activities, (5) pyramid schemes, (6) ineffective products, (7) invalid patents, and (8) medical effectiveness concerns. Allegations that do not fit into these categories—such as claims of excessive leverage, stock promotion, anticipated dividend cuts, market bubbles, or general overvaluation—are labeled "nonfraudulent/nonserious." The results indicate that "fraudulent/serious" allegations within the "price drop" category have 1.5 times the impact of "nonfraudulent/nonserious" allegations within the same "price drop" category. These findings reinforce the notion that banks act more opportunistically when borrowers face allegations that pose greater reputational or financial risk.

Our baseline results are robust to a battery of sensitivity tests and analyses to alleviate endogeneity concerns. Specifically, we find qualitatively similar evidence to our baseline findings when we, for example, adopt (1) alternative DiD designs to address the inherent econometric problems associated with two-way fixed effect (TWFE) models; (2) alternative fixed effect structures to further alleviate concerns about potential endogeneity; (3) alternative propensity score matching (PSM) methods to further account for potential heterogeneity in observable characteristics between attacked and non-attacked firms; and (4) alternative windows around treatment to address sample imbalance related to the treated group.

Our study makes the following contributions to the literature. First, we contribute to the agency conflict stream of the debt contracting literature. This literature maintains that banks' interventions can exacerbate value-impairing conflicts of interest between shareholders and debtholders (Jensen and Meckling 1976; Rajan 1992; Houston and James 1996; Gorton and Kahn 2000). For example, banks can make use of their information advantage and negotiation power to extract information monopoly rent from borrowers (Diamond 1991; Rajan 1992; Houston and James 1996). Banks can also arrange loan covenants to restrict borrowing firms' risk taking by requiring that borrowers maintain sufficient liquidity that satisfies their self-interest at the expense of shareholders (Liu and Mauer 2011; Nini, Smith, and Sufi 2012). We contribute to this stream of studies by providing evidence that, beyond lenders' concerns about increases in borrowers' credit risk, banks can also capitalize on borrowers' adversity to opportunistically seek rent from borrowers. While a preponderance of bank loan pricing studies have exploited the mechanism of credit risk, whether default or information risk, to explain why banks charge higher or lower loan spreads,[8] there is far less evidence to date on banks' opportunistic use of loan spreads to extract rent from borrowers. Essentially, despite the pervasiveness of theoretical works on bank rent extraction, there is little empirical evidence to date in the loan contracting setting that tests the rent extraction story.

[7] We follow Ahn, Bushman, and Patatoukas (2024) and use the immediate price drop upon the release of short sellers reports as a measure of the severity of short sellers' allegations.

[8] The literature on this subject is abundant (Graham, Li, and Qiu 2008; Bharath et al. 2011; see, e.g., Bharath, Sunder, and Sunder 2008; Chava, Livdan, and Purnanandam 2009; Campello et al. 2011; Costello and Wittenberg-Moerman 2011; Lin et al. 2011; Valta 2012; Hertzel and Officer 2012; Aslan and Kumar 2012; Houston et al. 2014; Kim, Song, and Stratopoulos 2018; Kim, Song, and Zhang 2011; Cen et al. 2016; Campello and Gao 2017; Bushman, Williams, and Wittenberg-Moerman 2017).

Our study therefore adds to the few studies that directly test the rent extraction story (e.g., Santos and Winton 2008; Hale and Santos 2009; Schenone 2010). Unlike prior rent extraction studies, we provide novel evidence that, in times of borrower adversity (induced by *third-party allegations about firms* rather than some industry-specific or nationwide adverse shock), relationship banks leverage the adversity created by the allegations to extort affected borrowers. In this context, our evidence shows that the bank–firm relationship is not always beneficial to borrowing firms (see, e.g., Boot, Grfenbaum, and Thakor 1993; Petersen and Rajan 1994, 1995).

Second, we contribute to studies examining whether and how short selling influences debt markets. While past studies document that short interest provides relevant information about default risk for public debt (or bond) investors (Hendershott, Kozhan, and Raman 2020), little is known about whether activist short selling impacts banks, which provide *private* debt and tend to have superior information-gathering and processing abilities than other capital providers do. Griffin, Hong, and Kim (2016) showed that equity short interest transmits, to credit default swap (CDS) investors, relevant information about credit spread not incorporated into prices through other channels.[9] It should be noted, however, that *activist* short sellers differ from other short sellers captured by short interest or short selling threats (Chang, Lin, and Ma 2019; Hope, Hu, and Zhao 2017) in that the former release negative information about attacked firms *directly* and *publicly* to market participants, whereas other short sellers use information *privately* to take short positions and release such information *indirectly* to the market via their trading. Moreover, activist short sellers impact attacked firms' reputation and financing options through the negative publicity of their attacks, which are frequently discussed in the media (Paugam, Stolowy, and Gendron 2021). Our study provides useful insights into a hitherto under-researched question of whether banks react to such public negative information revealed by activist short sellers and how they exploit the negative publicity surrounding short attacks.

Third, our study also contributes to the growing literature concerning the extent of the information advantage that activist short sellers have over other market participants (Chen 2016; Ljungqvist and Qian 2016; Wong and Zhao 2017; Black 2018; Brendel and Ryans 2021; Paugam, Stolowy, and Gendron 2021; Zhao 2020; Stolowy, Paugam, and Gendron 2022) and to the regulatory debate about the costs and benefits of activist short sellers' attacks. While prior research has shown that activist short sellers' attacks contribute to price discovery and disseminate new information to equity investors (Ljungqvist and Qian 2016; Paugam, Stolowy, and Gendron 2021), this line of research does not examine the impact on other important capital providers, such as banks, which are also informed market participants. (Activist) short sellers reduce attacked firms' ability to access public sources of financing (Grullon, Michenaud, and Weston 2015; Wong and Zhao 2017; Meng et al. 2020; van Binsbergen, Han, and Lopez-Lira 2023). Given this impact, our study shows that while short sellers' allegations also impose costs on lenders through increasing their exposure to credit risk, they also provide opportunities for exposed lenders to economically benefit from attacked borrowers' adversity. In this context, we expand the knowledge base on the costs and benefits of activist short selling.

2. Research design

2.1 Data and sample

We collect activist short seller allegation events from 2010 to 2018 from the Activist Insight Shorts database (previously called "Activist Short Research"). These attacks relate

[9] Lleshaj and Kocian (2021) used public short sale announcements to also show that CDS spreads increase following such announcements. Ho, Lin, and Lin (2022) found that the relaxation of short-sale constraints following regulation SHO allows banks to filter out low-quality borrowers.

to 1,507 campaigns.[10] These allegations span a spectrum of issues, some of which have the following flags in the database: "accounting fraud," "misleading accounting," "major business fraud," "other illegal activities," "bubble," "competitive pressures," "industry issues," "product ineffectiveness," "overvaluation," "overlevered," "patent expiration/invalid," "stock promotion," etc. Using these allegation event dates as a reference for forming a baseline sample consisting of attacked and non-attacked firms, we initiate the construction of our baseline sample for existing loans and that for new loans. In so doing, we first extract all US-domiciled/listed firms' financial data from Compustat North America for the period of 2008–2018.[11] We call this our *initial* sample.

We merge firm–year fundamental data in our initial sample with LPC's Dealscan traditional bank loan data at the loan facility/tranche level (not the firm level)[12] to arrive at a *Penultimate "I"* sample of 2,482 firms reporting 8,890 facility–year non-missing observations for the period of 2008–2018.[13] Because our research investigation requires us to ensure that borrowing firms had existing loans before activist short sellers' attacks, we construct a *Penultimate "II"* sample of 2,377 firms (8,524 facility–year loan observations) by keeping only firms with loans in both the pre- and post-allegation periods. In this sample, the number of unique campaigns decreases to 237 campaigns (down from 372 in the *Penultimate "I"* sample) because the firms included in Dealscan represent a small fraction of the firms included in Compustat.[14]

Because rent extraction relies on an ongoing lending relationship, we further ensure that the same borrower–lender relationship(s) exist(s) before and after the short selling attack events.[15] Thus, we adopt, as our foundation sample, a restricted sample of only 3,119 new loans made to 1,216 borrowers in each period for which the borrower–lender relationship(s) observed in the current period is (are) the same relationship(s) observed in any of the previous period(s).[16] This ensures that both the treated group and the control group (i.e., the never-treated group) contain only loan observations for which ongoing lending relationship(s) exist(s). This strict approach should allow for examining by how much, relative to the control group, rent extraction in the treated group is different between before versus after activist short sellers' allegations. We label this sample the *final, pre-matched (loan–year)* sample. We provide details of how we construct this sample in Appendix A.

Adopting this restrictive sample of loans for which ongoing lending relationship(s) exist(s) also allows for the adoption of Lead Lender fixed effects (hereafter, "Lender FEs") as our

[10] Some of these campaigns consist of multiple reports published per attacked firm and can comprise of different allegations with potentially different consequences per attacked firm. Thus, our sample of activist short sellers' campaigns contains multiple observations per firm–year, allowing us to exploit variation in the consequences that each allegation type exerts. This notwithstanding, in subsequent robustness checks, we only keep the first campaign per attacked firm, and find qualitatively similar results.

[11] We do not start the sample at 2010 (the first year of allegation events in our sample), but rather at 2008 in order to have two pre-periods (i.e., years 2008 and 2009) for year 2010 allegation events.

[12] The unit of analysis in our Dealscan-based loan data is a loan facility/tranche. Consequently, when merged with borrowers' fundamental data, the number of loan observations in the merged sample is typically the same irrespective of whether merged borrowers' fundamental data are available at annual vs. semiannual vs. quarterly intervals.

[13] Following prior studies (e.g., Graham, Li, and Qiu 2008), we do not delete financial firms from our sample.

[14] In our study, for example, one can confirm this by simply comparing the number of loan facility observations in our Penultimate "I" Dealscan dataset (i.e., 8,890 observations) with the number of observations in the raw Compustat US-firm financial dataset (i.e., 88,006 firm–year observations). From this comparison, one can see clearly that only approximately 10 percent of observations in the Compustat US-firm financial dataset is mapped to observations in the Dealscan dataset (this is why bank loan studies typically have a small number of observations).

[15] Our sample is restricted to only borrower-lending relationships with the main lead lenders.

[16] Specifically, we exclude loan observations of both treated and control firms without existing borrower–lender relationship in any of the past eight years. Note that eight years is chosen because this is the distance between the last allegation event (year 2018) and the 1st ever allegation event (year 2010) in our sample: we want to ensure that loans made at the last allegation event date (the last later-treated units) are never compared with loans at prior allegation event dates (the earlier-treated units) that did not have the same lender as in the current period.

primary unit fixed effect; this allows us to compare, within the same lead lender, the economic rent extracted in the treated group (relative to the control group) before the allegations with that after the allegations.

On the basis of the foundation sample (i.e., the final, pre-matched [loan–year] sample), we construct an entropy-balanced sample of 2,732 loan facility years (associated with 1,088 unique borrowing firms), which is the sample we use for all estimations.[17] We call this sample our *final, matched (loan–year)* sample. Entropy balancing ensures optimal sample balancing by identifying weights for the control sample to equalize the distribution of covariates across the treatment and control samples. It achieves better matching than traditional PSM methods do and allows for retaining sufficient test power by keeping the same number of observations as the pre-matched sample. In the subsection that follows this one, we describe how we implement this matching.

Because our study involves conducting other tests at the *firm–year* level (such as tests on SEC regulatory actions and loan renegotiation), as well as tests at the *firm-day* level (such as the test on CDS pricing), we retrieve the list of borrowers in the final, matched (loan–year) sample above and construct two other samples of panel data for each of these borrowing firms using the same sample period 2008–2018. For the firm–year test category, the above process results in a *final, pre-matched (firm–year)* sample of 10,875 observations (1,088 firms), which, after entropy balancing, further reduces to a *matched (firm–year)* sample of 8,289 observations (1,019 firms). For the firm-day test category, the above process results in a *final, pre-matched (firm-day)* sample of 543,641 daily observations (335 firms), which, after entropy balancing, also reduces to a *matched (firm-day)* sample of 402,004 observations (306 firms).

To mitigate the influence of outliers, in all the samples above, we winsorize all the continuous variables at the 1 percent and 99 percent levels.

2.2 Empirical model

To determine if activist short seller allegation events provide banks with an opportunity to extort borrowers, we regress loan spreads on the DiD indicator of activist short sellers' allegations, loan characteristics, and borrower characteristics (including measures of borrower credit risk) and fixed effects (hereafter, "FE"). We specify several measures of borrower credit risk as additional controls in our primary design following related studies (see, e.g., Santos and Winton 2008; Hale and Santos 2009) because doing so allows us to determine whether activist short sellers' allegations affect the pricing of new loans over and beyond their impact on borrower credit risk. Observing any incremental effect over and above borrower credit risk would therefore allow for directly interpreting the observed effect as evidence of rent extraction. Specifically, we formulate our baseline regression as follows:

$$\begin{aligned} LOG\_AIS_{f,i,t} = \alpha_1 + \alpha_2 POST\_ATTACK_{i,t} + \alpha_{3-K} CONTROLS_{i,t/f,i,t} + \text{Lender FEs} \\ + \text{Industry FEs} + \text{Year FEs} + \varepsilon \end{aligned} \tag{1}$$

where the subscripts f, i, and t refer to the loan facility/tranche, borrowing firm, and reporting period, respectively, and the regression clusters standard errors at the lender level and reports heteroskedasticity-consistent standard errors.

In the above specification in equation (1), *LOG_AIS*, the dependent variable, is the natural logarithm of all-in-drawn spread (AIS); AIS refers to loan interest rates on Dealscan all-in-drawn loan in basis points (including any upfront fees and annual fees) in excess of the LIBOR rate. The key variable of interest, *POST_ATTACK*, is an indicator variable that

[17] Our foundation sample of 3,119 loan facility-year observations further reduces to 2,732 observations, because, one of our important measures of credit risk—the distance-to-default measure—has only 2,732 observations in the foundation sample. This explains why, after entropy balancing based on all covariates, we end up with 2,732 observations.

equals 1 if a loan is activated in periods following an activist short seller's attack and 0 otherwise. This variable captures the change in the outcome variable of attacked firms before and after short sellers' allegations, relative to that of non-attacked firms.

CONTROLS encompasses all loan- and borrower-level controls and determinants of loan spreads. These control variables include loan attributes (i.e., loan size, maturity, number of prior loan deals, performance pricing, loan type, and loan purpose) and borrower characteristics (i.e., size, profitability, tangibility, leverage, growth prospects, borrower information risk, and borrower default risk). While Lender FEs fully control for fixed differences between treated and control units in a DiD setup (see, e.g., Bertrand and Mullainathan 1998, 15), it also generally controls for the potential confounding influence of (1) unobservable *time-invariant* bank-related determinants of the cost of debt (e.g., the reputation, contracting style and market power of lending banks) and (2) factors related to borrower–lender relationships, as this FE captures "firm–bank pairings" (Campello and Gao 2017, 113). Industry and Year FEs are also included to control for differences in loan contract terms across industries and over time, respectively (Graham, Li, and Qiu 2008; Kim, Song, and Zhang 2011). We do not specify macrolevel credit risk determinants (i.e., term spread and credit spread) in our regression based on a US-only sample because there is no cross-sectional difference across firms in these variables, but rather a time series difference that is subsumed by Year Fes.[18]

Regarding borrower-level controls, profitability (i.e., *ROA*) is the ratio of earnings before interest, taxes, depreciation and amortization (*EBITDA*) divided by total assets; firm size (i.e., *LOG_ASSETS*) is defined as the natural logarithm of total assets in millions of US dollars; *LEVERAGE* is defined as the sum of debt in current liabilities and long-term debt scaled by total assets; *TANGIBILITY* is defined as gross property, plant and equipment divided by total assets; Tobin's Q (i.e., *Q*) is the sum of market value of equity and book value of debt scaled by total assets; *Z_SCORE* is Altman's (1968) Z score; *DISTANCE-TO-DEFAULT* is the first principal component based on two measures of distance-to-default–*EDF* and *BSMProb*, where: *EDF* is the expected default frequency estimated from the KMV-Merton-based method of Bharath and Shumway (2008), and *BSMProb* is the probability of bankruptcy estimated from the Black–Scholes–Merton (BSM) option-pricing model following the method in Hillegeist et al. (2004); *ABS_ACCRUALS* is the absolute value of firm–year total accruals scaled by lagged total assets; *MISSTATEMENT* is an indicator set equal to 1 if a borrowing firm misstated its financial statements in a current year (identified through future-period restatements), and 0 otherwise; *AVERAGE_CDS_SPREADS* is the annual average of all daily CDS spreads with 5-year maturity from the Markit database; and *DOWNGRADE* is an indicator that equals 1 if a borrower receives a downgrade on its Standard & Poor's entity credit rating and 0 otherwise. Consistent with the debt contracting literature (e.g., Graham, Li, and Qiu 2008; Santos and Winton 2008; Hale and Santos 2009; Schenone 2010; Valta 2012; Lin et al. 2013), we adopt *ROA*, *LEVERAGE*, *Z_SCORE* and *DISTANCE-TO-DEFAULT* as our *ex ante* proxies for *default risk*, whereas we adopt *DOWNGRADE* and *AVERAGE_CDS_SPREADS* as our *ex post* proxies of *default risk*. Similarly, following the information risk stream of the debt literature (e.g., Duffie and Lando 2001; Lambert, Leuz, and Verrecchia 2007; Kim, Song, and Stratopoulos 2018; Kim, Song, and Zhang 2011), we adopt *ABS_ACCRUALS* and *MISSTATEMENT* as our *ex ante* and *ex post* proxies for *information risk*, respectively.

Regarding loan facility attribute controls, *LOG_LOAN_SIZE* is the natural logarithm of the US dollar loan facility amount; *LOG_MATURITY* is the natural logarithm of the number of months to maturity of a loan facility; *LOAN TYPE FE* is a vector of indicator

[18] Indeed, this econometric intuition is likely the reason why US bank loan studies specifying such macrolevel credit risk determinants do not additionally specify Year FEs (see, e.g., Graham, Li, and Qiu 2008; Campello and Gao 2017).

variables that assume the value of 1 for a given loan type (e.g., term loan, revolver, 364-day facility), and 0 otherwise; *LOAN PURPOSE FE* is a vector of indicator variables that assume the value of 1 for a given loan facility purpose (e.g., corporate purposes, debt repayment, working capital, CP backup, takeover, acquisition line, and leverage buyout offers), and 0 otherwise.

We note here that, as mentioned before, we entropy-balance the sample before estimating equation (1). We implement entropy balancing as outlined below. Like in any matching method, one must adopt, in the logistic propensity score covariate model or other preprocessing matching scheme, some dichotomous indicator variant of the original regressor used in the prediction model to identify treated and control units (see, e.g., Smith 2016). Thus, we use our ever-treated DiD indicator *ATTACK*, to separate ever-treated units from never-treated units. We then use the entropy-balance reweighting scheme to reweight the data from the control units (i.e., observations with $ATTACK = 0$) to match the first, second and third moments computed from the data of the treated units (i.e., observations with $ATTACK = 1$), adopting all variables contained in the vector *CONTROLS* (including Industry and Time FEs) as covariates.[19] The entropy balancing procedure then orthogonalizes the treatment indicator (*ATTACK*) with respect to the covariate moments that are included in the reweighting.

The key variable of interest in equation (1) is the DiD estimator of *POST_ATTACK*. With respect to our empirical prediction, after accounting for borrower credit risk and all other controls in equation (1), a significantly positive coefficient on *POST_ATTACK* (α_2) would be consistent with our hypothesis that banks seek informational rents following activist short sellers' allegations.

3. Results

3.1 Descriptive results

Table 1 shows the distribution of the main variables of interest across all samples by SIC industry classification. For example, in the final, pre-matched (loan–year) sample, there appears to be substantial variation in the outcome variable (i.e., loan spreads) across industries, thus necessitating the use of Industry FEs to control for this heterogeneity in the outcome variable. Although not tabulated for brevity, we also observe substantial variation in loan spreads across years; thus, specifying Year FEs in the estimations should address the concern that time trends, rather than short sellers' attacks, drive the regression results. Panel A of Table 2 tabulates the descriptive statistics of the key variables in our final, pre-matched (loan–year) sample. The mean (median) AIS of 196.19 (175) basis points, for example, is consistent with the summary value reported in prior bank loan contracting studies (see, e.g., Hertzel and Officer 2012). We also find that 21.4 percent of loan tranches are associated with borrowers that have been the subject of activist short sellers' attacks at any point in time during our sample period. Many control variables across both samples (e.g., *LOG_MATURITY, LOG_LOAN_SIZE, PERFORMANCE_PRICING_IND*, *ROA*, *LOG_ASSETS*, *LEVERAGE*, *TANGIBILITY, Q*, and *MISSTATEMENT*) also have means similar to those reported in prior studies (Chava, Livdan, and Purnanandam 2009; Kim, Song, and Zhang 2011; Valta 2012; Kim, Song, and Stratopoulos 2018; Kim, Wiedman, and Zhu 2023). In Panel A, we also report that 79.4 percent of the final, matched (loan–year) sample has missing CDS data. This statistic is consistent with previous studies; for example, Kim, Wiedman, and Zhu (2023) reported a similar statistic of 82.2 percent as the percentage of missing CDS data in their final sample. In the same table, we

[19] We do not include Lender FEs as a covariate in the entropy balancing preprocessing scheme because adopting a complex FE structure (such as Lender FEs) simply does not allow the reweighting optimization solution to converge.

Table 1. Distribution and statistics by industry.

		Loan–year sample				Firm–year sample				Firm–day sample			
Obs	Industry name	#Firms	#Obs	*ATTACK*	*LOG_AIS*	#Firms	#Obs	*ATTACK*	*RENEG_IND*	#Firms	#Obs	*ATTACK*	*DAILY CDS SPREAD* (%)
1	Chemicals	47	110	0.055	4.997	46	457	0.013	0.123	21	36,670	0.00	1.157
2	Computers	145	387	0.390	5.193	133	1,272	0.232	0.137	28	38,370	0.260	2.567
3	Extractive	102	240	0.125	5.249	77	770	0.113	0.186	28	43,952	0.119	1.906
4	Financial	24	55	0.091	4.967	24	231	0.095	0.139	6	5,986	0.00	0.972
5	Food	47	136	0.309	4.985	43	421	0.183	0.135	13	27,055	0.190	0.948
6	Insurance/Real Estate	16	60	0.267	5.255	15	159	0.101	0.208	3	4,726	0.250	1.910
7	Manf: Electrical Eqpt.	33	69	0.145	5.115	32	305	0.141	0.111	5	6,311	0.00	1.241
8	Manf: Instruments	51	104	0.125	4.920	50	480	0.069	0.110	13	15,962	0.064	0.904
9	Manf: Machinery	50	143	0.357	4.967	49	523	0.109	0.132	11	17,695	0.192	1.445
10	Manf: Metal	36	81	0.012	5.219	32	338	0.021	0.130	17	18,941	0.102	2.010
11	Manf: Misc.	8	24	0.458	5.018	6	62	0.355	0.097	3	6,400	0.804	1.193
12	Manf: Rubber/glass/etc.	25	62	0.145	5.070	24	258	0.128	0.136	7	8,352	0.192	3.400
13	Manf: Transport Eqpt	39	144	0.403	5.192	35	406	0.232	0.222	10	20,617	0.00	5.906
14	Mining/Construction	40	98	0.245	5.329	32	322	0.193	0.155	4	7,761	0.370	1.577
15	Others	7	8	0.000	5.441	5	37	0.00	0.108	1	632	0.00	5.770
16	Pharmaceuticals	28	77	0.519	5.061	27	244	0.295	0.090	12	16,616	0.258	0.459
17	Retail: Misc.	81	211	0.318	5.126	67	688	0.199	0.134	24	50,122	0.246	3.065
18	Retail: Restaurant	23	48	0.083	5.054	20	195	0.056	0.103	5	6,117	0.544	1.116
19	Retail: Wholesale	56	169	0.290	5.246	52	576	0.170	0.139	12	20,705	0.053	1.887
20	Services	119	285	0.140	5.301	104	1,038	0.102	0.166	28	46,986	0.045	2.977
21	Textiles/Print/Public.	67	148	0.108	5.205	57	570	0.093	0.151	27	32,623	0.015	2.846
22	Transportation	87	198	0.116	5.355	78	738	0.066	0.161	20	34,154	0.00	2.497
23	Utilities	85	262	0.000	4.894	80	785	0.000	0.110	37	76,888	0.000	1.412
		1,216	3,119			1,088	10,875			335	543,641		

Table 2. Descriptive statistics (loan–year sample).

Panel A: Pre-matched sample

Variable	*N*	Mean	Median	Std Dev	10th Pctl	75th Pctl	90th Pctl
ATTACK	3,119	0.214	0.000	0.410	0.000	0.000	1.000
POST_ATTACK	3,119	0.110	0.000	0.313	0.000	0.000	1.000
AIS	3,119	196.191	175.000	108.138	100.000	245.000	325.000
LOG_AIS	3,119	5.148	5.165	0.531	4.605	5.501	5.784
LOG_MATURITY	3,119	3.891	4.094	0.490	3.178	4.094	4.111
LOG_LOAN_SIZE	3,119	19.783	19.807	1.272	18.133	20.723	21.416
LOG_NO_OF_PRIOR_DEALS	3,119	0.076	0.000	0.247	0.000	0.000	0.000
PERFORMANCE_PRICING_IND	3,119	0.273	0.000	0.446	0.000	1.000	1.000
LOG_N_FINANCIAL_COVENANT	3,119	0.172	0.000	0.375	0.000	0.000	0.693
LOG_N_GENERAL_COVENANT	3,119	0.528	0.693	0.579	0.000	0.693	1.609
ROA	3,119	0.127	0.119	0.077	0.063	0.159	0.217
LOG_ASSETS	3,119	8.379	8.295	1.503	6.516	9.373	10.472
LEVERAGE	3,119	0.110	−0.026	0.974	−0.998	0.587	1.341
TANGIBILITY	3,119	0.564	0.433	0.437	0.085	0.883	1.181
Q	3,119	2.112	1.821	1.038	1.236	2.431	3.265
Z_SCORE	3,119	1.755	1.670	1.235	0.480	2.495	3.314
DISTANCE-TO-DEFAULT	2,732	0.027	0.000	0.113	0.000	0.000	0.018
ABS_ACCRUALS	3,119	0.056	0.044	0.055	0.009	0.071	0.113
MISSTATEMENT	3,119	0.177	0.000	0.382	0.000	0.000	1.000
AVERAGE_CDS_SPREAD	3,119	0.333	0.000	1.185	0.000	0.000	1.007
DOWNGRADE	3,119	0.039	0.000	0.193	0.000	0.000	0.000
MISSING_CDS_SPREADS	3,119	0.794	1.000	0.404	0.000	1.000	1.000
MISSING_DOWNGRADE	3,119	0.860	1.000	0.347	0.000	1.000	1.000

Panel B: Matched sample

BEFORE: without weighting

	TREATED = 1			*TREATED = 0*		
Variable	mean	variance	skewness	mean	variance	skewness
LOG_MATURITY	3.938	0.219	−2.146	3.887	0.239	−2.272
LOG_LOAN_SIZE	20.060	1.529	−0.465	19.760	1.544	−0.549

(continued)

Table 2. (continued)

Panel B: Matched sample

BEFORE: without weighting

	TREATED = 1			*TREATED* = 0		
Variable	mean	variance	skewness	mean	variance	skewness
LOG_NO_OF_PRIOR_DEALS	0.107	0.084	2.531	0.061	0.050	3.619
PERFORMANCE_PRICING_IND	0.271	0.198	1.030	0.287	0.205	0.943
ROA	0.131	0.004	0.859	0.133	0.005	0.265
LOG_ASSETS	8.789	2.047	0.104	8.354	2.122	0.199
LEVERAGE	0.467	1.213	1.091	−0.045	0.685	0.884
TANGIBILITY	0.348	0.115	1.474	0.593	0.177	0.679
Q	2.417	1.093	2.069	2.110	0.936	2.516
Z_SCORE	1.631	0.989	0.812	1.897	1.422	0.252
DISTANCE-TO-DEFAULT	0.043	0.022	3.783	0.023	0.010	5.601
ABS_ACCRUALS	0.059	0.004	3.517	0.051	0.002	2.574
MISSTATEMENT	0.222	0.173	1.341	0.168	0.140	1.773
AVERAGE_CDS_SPREAD	0.228	0.323	4.232	0.336	0.827	4.243
DOWNGRADE	0.023	0.023	6.343	0.032	0.031	5.365
MISSING_CDS_SPREADS	0.767	0.179	−1.263	0.779	0.172	−1.345
MISSING_DOWNGRADE	0.889	0.099	−2.481	0.865	0.117	−2.131

AFTER: with weighting

	TREATED = 1			*TREATED* = 0		
		#obs = 605			#obs = 2,127	
Variable	mean	variance	skewness	mean	variance	skewness
LOG_MATURITY	3.938	0.219	−2.146	3.937	0.219	−2.140
LOG_LOAN_SIZE	20.060	1.529	−0.465	20.050	1.528	−0.453
LOG_NO_OF_PRIOR_DEALS	0.107	0.084	2.531	0.107	0.084	2.531
PERFORMANCE_PRICING_IND	0.271	0.198	1.030	0.271	0.198	1.028
ROA	0.131	0.004	0.859	0.131	0.004	0.858
LOG_ASSETS	8.789	2.047	0.104	8.787	2.047	0.108
LEVERAGE	0.467	1.213	1.091	0.467	1.212	1.091

(continued)

Table 2. (continued)

AFTER: with weighting

Variable	*TREATED = 1*			*TREATED = 0*		
		#obs = 605			#obs = 2,127	
	mean	variance	skewness	mean	variance	skewness
TANGIBILITY	0.348	0.115	1.474	0.348	0.115	1.474
Q	2.417	1.093	2.069	2.416	1.092	2.070
Z_SCORE	1.631	0.989	0.812	1.630	0.989	0.811
DISTANCE-TO-DEFAULT	0.043	0.022	3.783	0.043	0.022	3.784
ABS_ACCRUALS	0.059	0.004	3.517	0.059	0.004	3.518
MISSTATEMENT	0.222	0.173	1.341	0.222	0.173	1.340
AVERAGE_CDS_SPREAD	0.228	0.323	4.232	0.228	0.323	4.239
DOWNGRADE	0.023	0.023	6.343	0.023	0.023	6.341
MISSING_CDS_SPREADS	0.767	0.179	−1.263	0.767	0.179	−1.261
MISSING_DOWNGRADE	0.889	0.099	−2.481	0.889	0.099	−2.478

also show that 86 percent of loan–years lack credit rating data; this statistic is also reasonable compared with that reported in prior studies (see, e.g., Bharath et al. 2011).

Untabulated Pearson correlations between variable pairings (available upon request) show that the covariates are not strongly correlated with each other, suggesting that our regressions are unlikely to suffer from multicollinearity problems. Given the importance of credit risk covariates, we carefully inspect and confirm that our credit risk proxies are not collinear; thus, we follow prior studies (see, e.g., Houston et al. 2014) and include all of these proxies in the same regression.

Panel B of Table 2 shows the descriptive statistics of the covariates used for entropy balancing. As the tabulations show, the imbalance in the covariates that is observed across the treated and control units in the sample is eliminated after entropy balancing. Essentially, treated and control units are almost identical in the distributions of covariates; that is, the first, second, and third moments of each covariate are similar (if not the same) across both the treated and control groups.

3.2 Regression results

3.2.1 Primary findings—the impact of short sellers' attacks on rent extraction

Table 3 tabulates the regression results for the association between activist short sellers' attacks and loan spreads, which are based on the baseline sample consisting of firms with activist short sellers' attacks (i.e., the treatment group $ATTACK = 1$) plus firms with no attacks (i.e., the control group $ATTACK = 0$). Panel A presents the results of the estimations using our baseline final, matched (loan–year) sample, whereas Panel B reports the results that restrict the window around treatment. Panel C then tabulates the results using the final, pre-matched (loan–year) sample to allow for comparing the results with and without entropy balancing. To understand how the relationship between activist short sellers' allegations and the cost of debt evolves with the stepwise inclusion of different determinants of credit spread, the tabulation in Panel A has five columns. Column 1 controls for only loan characteristics. Column 2 controls for both loan characteristics and borrower characteristics (excluding measures of borrower credit risk). Column 3 then adds *ex ante* measures of credit risk to the previously included controls, whereas Column 4 adds *ex post* measures of credit risk. Finally, Column 5 specifies all control variables (including both *ex ante* and *ex post* measures of credit risk).

To highlight the importance of control variables in influencing credit pricing, independent of activist short sellers' allegations, we begin by discussing coefficients on all control variables. We base our discussions on the results reported in Column 5 of Table 3, Panel A (and Columns 1–5 of Table 3, Panel C), allowing for the estimates to be compared in the presence versus absence of entropy balancing. We begin with loan-level determinants of credit pricing, where we find, across both Panels A and C, evidence that loans with shorter maturities and larger loan amounts are, overall, associated with a lower cost of debt, all else equal. These findings are consistent with theories presented in prior studies (e.g., Graham, Li, and Qiu 2008; Valta 2012; Campello and Gao 2017). For example, the negative association between loan size and loan spreads aligns with the economic idea that economies of scale in bank lending allow borrowers savings in the cost of accessing credit. We then move to borrower-level determinants of credit pricing; consistent with theories posited in the abovementioned prior loan contracting studies, we find that profitable firms, large firms, low-leverage firms, high-asset tangibility firms, high-Tobin's Q firms, high-Z score firms (i.e., financially healthy firms), high-accounting quality firms (i.e., non-misreporting firms), and low-CDS spread firms tend to borrow at a lower cost of debt, all else equal.

Somewhat inconsistent, rather than conflicting, evidence across both panels is that the coefficient on *DISTANCE-TO-DEFAULT* is negative after entropy balancing (Panel A), whereas it is positive before entropy balancing (Panel C). We had expected, according to

Table 3. Short seller allegations and cost of debt.
These tables present OLS regression estimates of short sellers' allegations on bank loan spreads, controlling for borrower credit risk and other loan- and borrower-level characteristics. Panel A uses the matched (entropy-balanced) sample, Panel B restricts treatment windows by dropping loans outside [−T,+T], and Panel C uses the pre-matched sample. Heteroskedasticity-adjusted t-statistics are in parentheses, with clustering at the lender level and a "lender + industry + year" FE structure. Since many ex post default risk measures are missing, we follow (e.g., Byoun 2008, p. 3078) by setting them to 0 and adding a missing value indicator, included but not reported for brevity. The missing value indicator per measure, which we specify in the regressions, is expected to control for any measurement error associated with the adjustment we had made to each measure of default risk. The sample size in Panel A, for example, decreases from 2,732 to 2,692 because imposing the Lender FEs automatically drop singleton observations to address the concern that "maintaining singleton groups in linear regressions where fixed effects are nested within clusters can overstate statistical significance and lead to incorrect inference" (Correia 2015) (this caveat also applies to Panels B and C). Asterisk demarcations *, **, and *** represent statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively. All the variables are defined in Appendix B.

Panel A: Matched (entropy-balanced) sample

	(1)	(2)	(3)	(4)	(5)
	dep. var.: *LOG_AIS*				
_cons	8.192***	8.385***	8.285***	8.307***	8.330***
	(28.10)	(27.24)	(38.67)	(23.05)	(31.35)
POST_ATTACK	**0.098*** **	**0.081** **	**0.090*** **	**0.066** **	**0.077*** **
	(3.77)	(2.52)	(3.54)	(2.14)	(3.01)
Time-Varying Loan Characteristics:					
LOG_MATURITY	0.157***	0.132***	0.133***	0.129***	0.125***
	(3.85)	(3.35)	(3.07)	(3.07)	(2.78)
LOG_LOAN_SIZE	−0.182***	−0.155***	−0.148***	−0.162***	−0.155***
	(−13.06)	(−7.89)	(−8.52)	(−7.03)	(−7.65)
LOG_NO_OF_PRIOR_DEALS	−0.118***	−0.135***	−0.252***	−0.141***	−0.247***
	(−2.86)	(−3.57)	(−3.36)	(−3.19)	(−3.32)
PERFORMANCE_PRICING_IND	−0.037**	−0.028	−0.025	−0.035*	−0.029*
	(−2.23)	(−1.66)	(−1.45)	(−1.92)	(−1.86)
Time-Varying Borrower Characteristics:					
LOG_ASSETS		−0.043***	−0.043***	−0.024	−0.029*
		(−3.29)	(−3.24)	(−1.36)	(−1.77)
TANGIBILITY		−0.068	−0.060	−0.063	−0.060
		(−1.22)	(−1.53)	(−1.20)	(−1.54)

(continued)

Table 3. (continued)

Panel A: Matched (entropy-balanced) sample					
	(1)	(2)	(3)	(4)	(5)
	dep. var.: ***LOG_AIS***				
Q		–0.091***	–0.075***	–0.084***	–0.069***
		(–5.84)	(–4.75)	(–5.25)	(–4.03)
Ex Ante Measures of Credit Risk:					
Default risk: *ROA*			–0.085		0.072
			(–0.34)		(0.28)
LEVERAGE			0.104***		0.097***
			(3.24)		(3.42)
Z_SCORE			–0.062**		–0.067***
			(–2.45)		(–2.73)
DISTANCE-TO-DEFAULT			–0.134*		–0.103*
			(–1.97)		(–1.78)
Information risk: *ABS_ACCRUALS*			–0.176		–0.235
			(–0.66)		(–0.87)
Ex Post Measures of Credit Risk:					
Default risk: *DOWNGRADE*				–0.082	–0.150**
				(–1.13)	(–2.05)
AVERAGE_CDS_SPREADS				0.109**	0.085**
				(2.55)	(2.51)
Information risk: *MISSTATEMENT*				0.049**	0.068***
				(2.30)	(3.72)
Time-Invariant Loan Characteristics:					
Loan Type FEs	Yes	Yes	Yes	Yes	Yes
Loan Purpose FEs	Yes	Yes	Yes	Yes	Yes

(continued)

Table 3. (continued)

Panel A: Matched (entropy-balanced) sample

	(1)	(2)	(3)	(4)	(5)
	dep. var.: ***LOG_AIS***				
Main FE structure:					
Lender FEs	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
N	2692	2692	2692	2692	2692
Adjusted R^2	0.639	0.658	0.685	0.666	0.692

Panel B: Restricting the window around the treatment

	(1)	(2)	(3)	(4)
	[−5,+5]	[−4,+4]	[−3,+3]	[−2,+2]
	dep. var.: ***LOG_AIS***			
POST_ATTACK	**0.071*****	**0.063*****	**0.052****	**0.051****
	(3.37)	(3.21)	(2.60)	(2.52)
All Control Variables?				
Time-Invariant Loan Characteristics:	Yes	Yes	Yes	Yes
Loan Type FEs	Yes	Yes	Yes	Yes
Loan Purpose FEs	Yes	Yes	Yes	Yes
Main FE structure:				
Lender FEs	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes

(continued)

Table 3. (continued)

Panel B: Restricting the window around the treatment

	(1)	(2)	(3)	(4)
	[−5,+5]	[−4,+4]	[−3,+3]	[−2,+2]
	dep. var.: *LOG_AIS*			
Year FEs	Yes	Yes	Yes	Yes
N	2655	2594	2527	2433
Adjusted R^2	0.688	0.693	0.694	0.706

Panel C: Pre-matched sample + restricting the window around the treatment

	(1)	(2)	(3)	(4)	(5)
	Full	**Drop treated observations outside of the window:**			
	Sample dep. var.: *LOG_AIS*	[−5,+5]	[−4,+4]	[−3,+3]	[−2,+2]
_cons	7.957***	7.914***	7.943***	7.905***	7.864***
	(38.51)	(41.34)	(42.05)	(41.39)	(38.69)
POST_ATTACK	**0.081***	**0.078***	**0.067***	**0.057***	**0.049**
	(3.51)	(3.60)	(3.08)	(2.65)	(2.15)
Time-Varying Loan Characteristics:					
LOG_MATURITY	0.073***	0.075***	0.068***	0.063***	0.060***
	(2.73)	(2.98)	(3.36)	(3.23)	(2.75)
LOG_LOAN_SIZE	–0.127***	–0.125***	–0.125***	–0.121***	–0.118***
	(–9.80)	(–9.37)	(–9.73)	(–8.94)	(–9.37)
LOG_NO_OF_PRIOR_DEALS	–0.162***	–0.138***	–0.139***	–0.117**	–0.080
	(–3.88)	(–2.79)	(–3.03)	(–1.99)	(–1.65)
PERFORMANCE_PRICING_IND	–0.013	–0.008	–0.001	0.000	0.005
	(–1.41)	(–0.71)	(–0.09)	(0.04)	(0.42)
Time-Varying Borrower Characteristics:					
LOG_ASSETS	–0.040***	–0.041***	–0.042***	–0.044***	–0.046***
	(–2.73)	(–2.74)	(–2.94)	(–3.05)	(–3.42)
TANGIBILITY	–0.036*	–0.038*	–0.046**	–0.053***	–0.055***
	(–1.94)	(–1.98)	(–2.41)	(–2.70)	(–2.70)

(continued)

Table 3. (continued)

Panel C: Pre-matched sample + restricting the window around the treatment					
	(1)	(2)	(3)	(4)	(5)
	Full	Drop treated observations outside of the window:			
	Sample dep. var.: *LOG_AIS*	[−5,+5]	[−4,+4]	[−3,+3]	[−2,+2]
Q	–0.062***	–0.066***	–0.067***	–0.069***	–0.070***
	(–4.24)	(–4.36)	(–4.68)	(–4.67)	(–4.91)
Ex Ante Measures of Credit Risk:					
Default risk: *ROA*	–0.330	–0.316	–0.291	–0.319*	–0.390*
	(–1.56)	(–1.53)	(–1.50)	(–1.68)	(–1.84)
LEVERAGE	0.085***	0.084***	0.081***	0.080***	0.082***
	(5.09)	(5.53)	(5.43)	(5.26)	(5.21)
Z_SCORE	–0.069***	–0.070***	–0.071***	–0.069***	–0.066***
	(–5.70)	(–5.75)	(–5.97)	(–6.03)	(–5.03)
DISTANCE-TO-DEFAULT	0.102	0.106	0.175*	0.180*	0.185
	(0.82)	(0.87)	(1.79)	(1.78)	(1.60)
Information risk: *ABS_ACCRUALS*	–0.071	–0.041	0.004	0.187	0.238
	(–0.29)	(–0.17)	(0.02)	(0.85)	(0.98)
Ex Post Measures of Credit Risk:					
Default risk: *DOWNGRADE*	–0.028	–0.030	–0.019	–0.023	–0.020
	(–0.97)	(–1.09)	(–0.64)	(–0.74)	(–0.68)
AVERAGE_CDS_SPREADS	0.076***	0.075***	0.075***	0.073***	0.070***
	(4.92)	(4.81)	(4.65)	(4.53)	(4.83)
Information risk: *MISSTATEMENT*	0.008	0.005	0.001	–0.004	–0.004
	(0.48)	(0.29)	(0.06)	(–0.26)	(–0.24)
Time-Invariant Loan Characteristics:					
Loan Type FEs & Loan Purpose FEs	Yes	Yes	Yes	Yes	Yes
Main FE structure:					
Lender FEs, Year FEs, & Industry FEs	Yes	Yes	Yes	Yes	Yes
N	2,692	2,655	2,594	2,527	2,433
Adjusted R^2	0.560	0.558	0.556	0.554	0.551

finance theory, to see a positive coefficient for this variable in both panels, but we only observe this in the pre-matched sample. A fundamental reason why we may be observing a negative coefficient on this variable after entropy balancing is that, before entropy balancing, the pre-matched sample has variation in distance-to-default, which naturally allows the expected positive correlation with loan spreads to emerge. However, entropy balancing explicitly matches on the basis of the distance-to-default and other credit risk covariates (among other covariates), forcing the treated and control groups to have nearly identical distributions of these variables. As a result, there is little variation left in the distance-to-default measure across observations, which may possibly mute or even reverse the correlation with loan spreads owing to remaining differences in unobserved factors.

Credit pricing studies argue that changes in the cost of credit should be conventionally justified by changes in borrower credit risk alone (e.g., Santos and Winton 2008; Hale and Santos 2009). However, theories suggest that credit risk consists of not only default risk but also information risk (see, e.g., Duffie and Lando 2001; Easley, Hvidkjaer, and O'Hara 2002; Easley and O'Hara 2004; Lambert, Leuz, and Verrecchia 2007). Therefore, it is important to pay special attention to the separate roles of default risk and information risk—the two components of credit risk—in our setting. Our aim here is to carefully assess whether various proxies for default risk and information risk, *ex ante* or *ex post*, significantly explain away banks' adjustments to the cost of credit, following activist short sellers' allegations.

We turn our attention back to the results tabulated in Table 3, Panels A and C, where we separate *ex ante* from *ex post* proxies of credit risk. We observe that, after banks had adjusted loan spreads upward for increases in default risk (i.e., $\Delta ROA < 0$, $\Delta LEVERAGE > 0$, $\Delta Z\_SCORE < 0$, $\Delta DISTANCE\text{-}TO\text{-}DEFAULT > 0$; $\Delta AVERAGE\_CDS\_SPREAD > 0$), as well as for increases in information risk (i.e., $MISSTATEMENT = 1$) that were concurrent with activist short sellers' allegations, they continued to increase the cost of credit.[20] The evidence presented shows that, after controlling for loan- and borrower-level characteristics (including several measures of credit risk) and FEs, the relationship between activist short sellers' attacks and the outcome variable (i.e., loan spreads) is positive and significant at the 5 percent level or better. For example, in Table 3, Panel A, the estimated coefficient of 0.077 ($t = 3.01$) in Column 5 is an increase in loan spreads by 8 percent (i.e., equivalent to a 16-basis-point increase over the mean) following activist short sellers' attacks.[21] In Columns 1–4 of Table 3, Panel B (and Columns 2–5 of Table 3, Panel C), we restrict the windows around treatment by dropping loan observations of treated firms that fall outside the window [−T,+T], where T = 2, 3, 4, or 5. In these more restricted balanced treatments, we continue to observe a positive association between activist short sellers' allegations and bank loan spreads (even after controlling for multiple measures of credit risk).

Taken together, these findings indicate that, following activist short sellers' allegations, banks increased the cost of credit beyond and above the level justified by changes in borrower risk alone, all else equal. This behavior seems to be consistent with the rent extraction story, where we posit that banks could seize the opportunity presented by borrowers' adversity—induced by activist short sellers' allegations—to hold up such borrowers for

[20] Given the importance of credit rating in general in predicting default risk, in unreported tests (available upon request), we follow prior studies and adopt *RATING_SCORE* instead of *DOWNGRADE* as an *ex post* proxy of default risk, alternatively adopting this variable as either a continuous variable or FE. We code *RATING_SCORE* from 1 (AAA) to 30 (D), assigning the number 31 "to all observations without a credit rating" (see, e.g., Valta 2012, footnote 14). We confirm that we continue to observe that banks increase interest rates by more than is justified by increases in default risk if we specify this alternative proxy as a *continuous* control variable or *FE*.

[21] Because the main independent variable is an indicator whereas the dependent variable is a log-based measure, the coefficient of 0.077 implies that the logarithm of all-in-drawn spreads (AIS) will be 0.077 higher according to the model; this means that the actual value of AIS will be multiplied by $\exp(0.077) \approx 1.08$, corresponding to an 8 percent increase.

high interest rates. Therefore, we conclude that borrowers bear informational hold-up costs in bank loan contracting following activist short sellers' allegations.

3.2.2 Dynamic estimation

Given that reputation, once soiled, tends to require a considerable amount of effort and time ("months or even years") to repair or rebuild (Chakravarthy, deHaan, and Rajgopal 2014, 1336), we seek to understand whether the rent extraction effect we document is short- or long-lived. On the basis of insights from the above study and to better track potential effects, we follow existing DiD studies that have event years commencing near the beginning of the sample period and adopt 6 years before and 6 years after event years (see, e.g., Silvers 2021, section 3.5). This allows us to decompose the treatment indicator (*POST_ATTACK*) into thirteen granular, dynamic treatment indicators: *POST_ATTACK* $(T \pm N)$, where $0 \leq N \leq 6$ years. Following a recent approach to dynamic estimation (see, e.g., Heese and Pérez-Cavazos 2019, 701–703; Lang et al. 2020, 148–149), we choose time $T-1$ as the benchmark period, so the coefficient of the specific dynamic treatment indicator at $T-1$ is restricted to zero.[22]

We then estimate the dynamic model using the Sun and Abraham (2021) interaction weighted estimates (to address the concern that treatment effect heterogeneity in a staggered DiD setting, if not accounted for, biases estimates) and display the results in figures 1 and 2. Figure 1 displays the results in event time based on "all" allegations in the full, matched (loan–year) sample, whereas figure 2 graphs the results of estimations in event time based on only allegations in the same matched sample that the capital market believes to be consequential. Specifically, figure 2 is based on a sample of entropy-matched never-treated units plus treated units associated with allegations for which the data provider on a short seller allegation campaign (i.e., the Activist Insight Shorts database) has indicated that the activist campaign return is negative. We code such consequential allegations as *PRICE_DROP* = 1 (hereafter, we refer to these allegations as "price drop" allegations).

On the basis of the evidence presented, we show that the rent extraction effect of activist short sellers' allegations on loan spreads kicks in, following the allegation event, but not before it. Specifically, based on the visual evidence presented in figures 1 and 2—where we fixate on "all" allegations and "price drop" allegations, respectively, we observe, overall, that allegation effects commence either in the 1st or 2nd year following the allegation but then appears to persist more strongly, especially in later years following the allegations. The evidence on persistence in effects is, first, consistent with our earlier-mentioned assertion that it takes time to rebuild a damaged reputation. Second, we interpret the delayed persistence in effects to be consistent with banks reacting more strongly to the impact of reputation-damaging events when such allegations have been later validated by credible gatekeepers such as regulators. See, for example, the evidence that, following short sellers' attacks, "the SEC takes longer to complete their investigation and issue an order" (Brendel and Ryans 2021).[23] Blackburne et al. (2021) also documented that SEC investigations take on average 3.2 years to complete. Taken together, the dynamic pattern of effects of "all" and "price drop" allegations seems to align with the idea that banks opportunistically extort borrowers over the prolonged period that bounds SEC regulatory actions. In the next few paragraphs, we will formally test the story of the regulator initiating investigations that may or may not lead to an eventual substantiation of short sellers' claims.

Given the widely known SEC constraint of limited investigative resources, one would expect the regulator to focus its investigations more on firms with allegations that are more serious/consequential. Thus, to set the stage for later examining whether SEC regulatory

22 The idea in these approaches is to estimate how the outcome variable before and after the event compares to the benchmark period (i.e., period $T-1$ in our case).

23 There is anecdotal evidence in support of this. For example, in an April 2022 university workshop that invited a renowned activist short seller Daniel Yu (founder of Gotham City Research), the activist short seller explained that, in his experience, the SEC validates short sellers' allegations three or more years later.

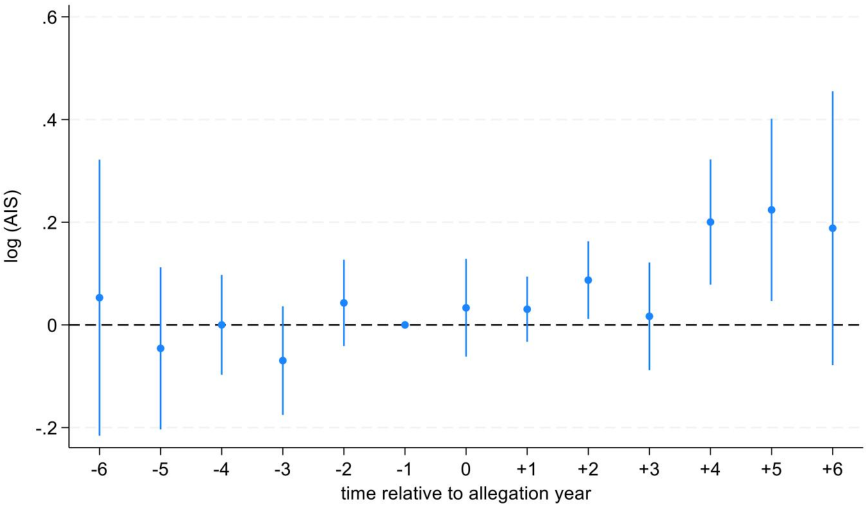

Figure 1. Dynamic effects—"all" allegations. This figure graphs in event time, the coefficient estimates from the Sun and Abraham (2021) setup for the dynamic effect of activist short sellers' allegations on bank loan spreads. In the estimation, time t−1 is used as the benchmark period, so the coefficient of *POST_ATTACK (T−1)* is restricted to 0. The estimation is based on "all" allegations. This figure shows an increase in loan pricing after activist short sellers' allegations. The allegation effects commence in the 2nd year following the allegation but then persist more strongly, especially in later years.

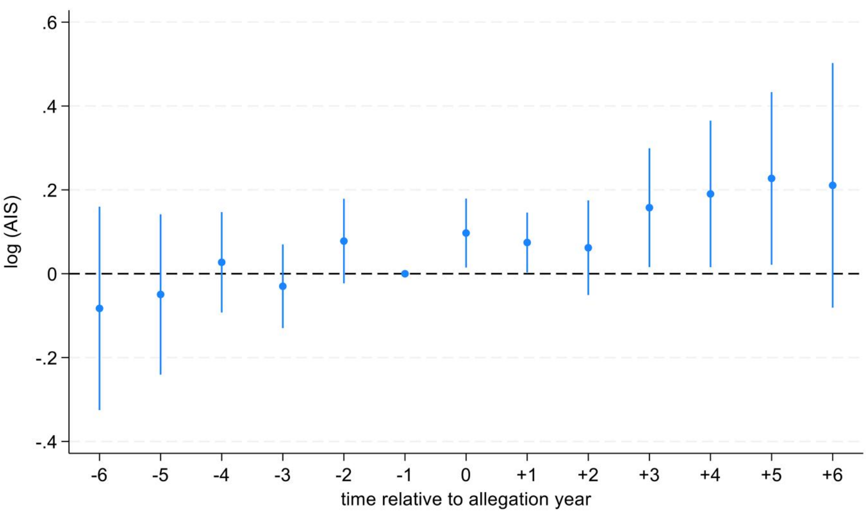

Figure 2. Dynamic effects—"price drop" allegations. This figure graphs in event time, the coefficient estimates from the Sun and Abraham (2021) setup for the dynamic effect of activist short sellers' allegations on bank loan spreads. In the estimation, time t−1 is used as the benchmark period, so the coefficient of *POST_ATTACK (T−1)* is restricted to 0. The estimation is based on "price drop" allegations only. The allegation effects commence in 1st year following the allegation then persist more strongly, especially in later years.

actions follow short seller allegations, we discuss more, the results of the dynamic estimation of "price drop" allegations displayed as figure 2. Unlike the visual evidence in figure 1 where effects commence in the second year following the allegation, we observe an even more immediate effect of "price drop" allegations in figure 2. The results in figure 2, however, continue to show the evidence of persistence in effects that we also observed in figure 1.[24] An interesting observation from the "price drop" estimation is the noticeable "initially-declining, but later-increasing" dynamic effect of allegations on loan spreads that we spot in the post-allegation window. Specifically, based on the precise economic estimates (untabulated), we observe an immediate increase in the loan price by 9.7 percent (P value $< .05$) in year T, a further increase of 7.4 percent (P value $< .05$) in year T + 1, no significant effect in year T + 2 (P value $> .10$), a resumption of increase in loan spreads by 15.8 percent (P value $< .05$) in year T + 3, a further increase of 19 percent (P value $< .05$) in year T + 4, another growth of 22.7 percent (P value $< .05$) in year T + 5 and a marginally significant increase of 21.1 percent (P value $> .10$). Here, the observed pattern clearly shows an immediate effect, followed by a next-period decrease in the magnitude of the effect, which then stagnates but picks up again but in a more dramatic fashion.

To delve into the underlying reasons for the "initially-declining, but later-increasing" pattern of allegation effects, we conduct two analyses involving SEC regulatory actions. Specifically, we test whether allegations predict SEC investigations, as well as SEC enforcement actions such as accounting and auditing enforcement releases (AAERs). We obtained raw data on all closed SEC investigations from January 1, 2000, to August 2, 2017, courtesy of Blackburne et al. (2021), who generously shared their data on previously undisclosed SEC investigations with us. We also collect data on AAERs from the repository maintained at the University of Southern California (see Dechow et al. 2011). Our tests here are motivated by anecdotal evidence that SEC investigations and enforcement actions follow short sellers' allegations. For example, on November 20, 2024, the activist short seller Hindenburg Research posted on its official social media account alerting its followers (with the intent to signal) that their January 20, 2023 fraud allegations against the Adani group now, after almost 2 years, have been substantiated by the SEC and the DOJ via enforcement actions against the Adani group.

In Table 4, we utilize our final, matched (firm–year) sample—not the loan–year sample—and report the results of the regression of SEC investigation commencement (Columns 1–4)/SEC enforcement action (Columns 5–8) on activist short sellers' allegations. For each form of regulatory action, we use alternative time-based measurements indicating whether regulatory actions occurred at time T, T + 1, T + 2, or T + 3. Because we use a firm–year sample, we specify firm-level controls as covariates and adopt a Firm + Year FE structure, clustering standard errors at the firm level. We find that the SEC commences investigations immediately and in the subsequent year (by a likelihood of 4 and 2.8 percent, respectively) following activist short sellers' allegations; however, we do not find that investigations commence 2 or 3 years after such allegations are made by short sellers. We then observe intriguing evidence that the SEC, on average, issues AAERs against attacked firms (by a likelihood of 0.5 percent) in the third year following the allegations; we do not document any evidence of AAERs in the years before time T + 3.[25] This AAER effect that we observe seems to align well with the end of the SEC investigation period (see, e.g., the evidence in Blackburne et al. (2021) that SEC investigations, on average, take 3 years to end).

24 In unreported tests, we do not observe this pattern for the "no price drop" allegation types. Furthermore, using this allegation types results in a violation of the parallel trends assumption, as we observe both positive and negative effects in the pre-allegation period. As such a violation challenges identification under DiD, we cannot learn much from this estimation and have thus elected to drop this test altogether in our article.

25 The effect of a 0.005 increase in the likelihood of issuing AAERs is economically meaningful when one considers the occurrence of AAERs in our sample. Indeed, as Dechow et al. (2011, 54) alluded, "misstatements resulting in SEC enforcement actions are rare events", representing "less than half of one percent of the firm–years available on COMPUSTAT." We find exactly this to be the case in our sample, whereby in Panel A of Supplementary Appendix Table OA1, we report the mean value of AAERs to be 0.004—that is, less than one percent of our total sample have AAERs. An increase in the likelihood of AAERs at time T + 3 by 0.005 therefore corresponds to a 100-plus percentage-point increase over our sample mean, which is economically significant.

Table 4. Timing of SEC regulatory actions and short seller allegations.

This table reports OLS regression estimates for the effect of short sellers' allegations on SEC investigation commencement versus SEC enforcement action, controlling for borrower-level characteristics. The table reports heteroskedasticity-adjusted t statistics in parentheses, observations are clustered at the borrowing firm level, and the regression adopts a "firm + year" fixed effect ("FE") structure. The asterisks *, **, and *** represent statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively. All the control variables are defined in Appendix B.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	INVESTIGATE (T)	***INVESTIGATE (T+1)***	***INVESTIGATE (T+2)***	***INVESTIGATE (T+3)***	***AAER (T)***	***AAER (T+1)***	***AAER (T+2)***	***AAER (T+3)***
POST_ATTACK	**0.040**** (2.09)	**0.028***** (3.18)	**0.010** (0.42)	**–0.005** (–0.16)	**–0.018** (–0.70)	**–0.010** (–1.08)	**–0.003** (–0.48)	**0.005*** (1.70)
INVESTIGATE (T)		–0.087** (–2.06)				0.011 (1.26)		
INVESTIGATE (T/T+1)			–0.178*** (–4.22)				0.010 (1.02)	
INVESTIGATE (T/T+1/T+2)				–0.144*** (–4.33)				0.014 (1.10)
AAER (T)						0.299** (2.56)		
AAER (T/T+1)							0.088 (1.63)	
AAER (T/T+1/T+2)								–0.139 (–1.58)
All firm-level controls included?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Main FE structure:								
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	8234	7196	6171	5204	7233	6378	5491	4643
Adjusted R^2	0.006	0.018	0.108	0.094	0.451	0.532	0.405	0.548

We then juxtapose our SEC investigations and enforcement action results with the earlier-observed "initially-declining, but later-increasing" loan pricing effect we observed in figure 2. On the basis of our observations, we posit that the declining part of the pattern up to the stagnation point coincides with the SEC investigation period (time T to T + 3), whereas the post-stagnation part of the pattern coincides with the period following SEC enforcement actions (time T + 3 to T + 6). The adoption of the framework suggested above may cause one to ask why the effects observed at the declining phase of the dynamic pattern are weaker than those observed at the increasing phase of the dynamic pattern. We propose two potential arguments that may explain the weak or declining allegation effects that we document during the SEC investigation periods.

First, while it is possible for banks to extract rent during investigations (subsequent to attacks) because attacked firms are vulnerable, classic finance theory on rent extraction also points out, however, that rent extraction occurs if the attacked firm's external financing options are restricted—that is, if their switching cost is high. However, external funders' knowledge of the investigation must be what drives the switching cost. Given that the SEC's investigative process is shrouded in secrecy—only the SEC staff, high-level managers of the company being investigated, and outside counsel are typically aware of active investigations (Blackburne et al. 2021)—the attacked firms may obtain information about SEC investigations, but external funders (public and private) do not have access to this information because the SEC keeps it secret. At best, the private lender will therefore learn about the investigation only from the borrower if the borrower chooses to disclose it. Given this, it is not far-fetched to argue that the borrower may choose not to disclose it if they are confident that such an investigation may not lead to an eventual enforcement action against them (see point below).[26]

Second, the literature on the SEC regulatory process suggests that "a vast majority of investigations do not lead to significant negative outcomes such as AAERs" (see, e.g., Bozanic, Down, and Williams 2024).[27] For example, while 11–12 percent of observations are associated with ongoing SEC investigations (Blackburne et al. 2021; Bozanic, Down, and Williams 2024), only 3 percent of observations are associated with SEC enforcement actions related to AAERs and insider trading (see Bozanic, Down, and Williams 2024). In our (pre-matched [firm–year]) sample, without coding post-event observations as 1, we find that only 1.8 (0.4) percent of observations are associated with a just-commenced SEC investigation (AAER) event.[28] Considering the above, banks may not completely expect the SEC investigation to translate into an enforcement action, even if they are made aware by the borrower that the latter is being investigated.

Finally, our dynamic estimation of the effect of activist short sellers' attacks also allows us to detect the presence or absence of pre-event effects; that is, whether banks' reactions

[26] The widely held position in the literature is that banks' private access to borrowers automatically gives them access to all kinds of privileged information (including privileged information about ongoing non-public investigations involving the firm). However, based on the specific insight from the institutional setting of SEC regulatory actions suggesting that the knowledge of SEC investigations at the alleged firm is generally kept a "secret," we are of the view that it is plausible that banks may not *privately* learn of allegations if borrowers choose not to disclose such information to them. Had banks privately obtained access to such information from borrowers during the SEC investigation period, one would have observed larger effects in the investigation period compared to aftermath of the SEC enforcement action, which is where the information becomes public. Indeed, we rather observe relatively larger effects after enforcement actions were publicly taken by the SEC against alleged borrowers. This larger effects in later-periods coinciding with the public issue of AAERs is also strengthened by one important condition that, according to information monopoly theories, should facilitate more rent extraction by banks: the *public* substantiation of allegations and imposition of penalties by the regulator should *further increase borrowers' switching cost* and should therefore make it easy for relationship lenders to extort the alleged borrowers. Essentially, when the SEC publicly takes enforcement actions against alleged borrowers, the ability of the alleged borrowers to switch from the private relationship lender to outside *public* lenders is constrained.

[27] The article is not publicly available, but a coauthor presented it at the submitting author's institution and authorized its citation. Only the abstract of an earlier version is online; the link is provided in the references.

[28] Unlike prior studies cited above that code the entire investigation period as 1, we code only the first firm–year of the SEC investigation as 1. This allows us to test whether short-seller allegations predict its initiation.

are determined by other events occurring just before the activist short seller allegation event. In our estimations in Panels A and B, we do not find any effect prior to the attack dates (i.e., this is consistent with the parallel trends condition), suggesting that what has been documented post-attacks is unlikely to be driven by the presence of pre-existing trends in loan spreads between attacked and non-attacked firms. This effect is also consistent with banks not anticipating the timing of short sellers' allegations.

3.2.3 Direct constructs of economic rent

We now adopt a direct construct of economic rent instead of relying on a regression setup to separate out the amount of premium contained in loan spreads (see Flammer 2021). Intuitively, for rent extraction to occur, one must be able to observe that at the loan facility level, a syndicate of lenders charges a focal borrowing firm a substantially higher interest rate relative to the rate they charge on loans made to peer borrowing firms with similar risk profiles. By directly comparing borrowing firms with similar levels of risk but with divergent interest rates, one could difference away or explain away default risk and information risk explanations of loan spreads.

Empirically, we achieve this by adopting a measure of risk-adjusted interest rates, where we fixate on the interest rate charged on the loan made to a focal borrowing firm at time T and find a benchmark interest rate (also at time T) that could be thought of as what is charged, on average, on loans made to the focal firms' peers with similar risk profiles. To determine the benchmark interest rate, we compute, for loans of the same type and having the same purpose, an industry–year mean *LOG_AIS* (excluding the current observation itself) of borrowing firms in the same two-digit SIC industry and in the same quartile distribution of (1) *firm characteristics* (firm size [*LOG_ASSETS_USD*], default risk [*DISTANCE-TO-DEFAULT*], and information risk [*ABS_ACCRUALS*]); and (2) *loan attributes* (loan maturity [*LOG_MATURITY*] and loan amount [*LOG_LOAN_SIZE*]). While this joint sorting requirement results in a significant loss of observations in our final sample (decreasing from 2,732 to approximately 600 observations), this joint sorting procedure is essential for identifying loans made to firms that are of similar risk profiles. Sorting by industry and size alone is standard and would preserve a large number of observations, but this does not adjust for default and information risk influences on interest rates; controlling for both default and information risks is crucial to make inference on the rent extraction story. Our *ex ante* continuous measure of default risk *DISTANCE-TO-DEFAULT* should therefore allow us to capture the likelihood that a firm is likely to be in financial distress and therefore may default on its financial obligations. Additionally, incorporating our *ex ante* measure of information risk *ABS_ACCRUALS* (Kim, Song, and Zhang 2011; Kim, Song, and Stratopoulos 2018) should allow for capturing the information risk that lenders often encounter when basing their lending decisions on noisy or biased accounting information.

We then compute our risk-adjusted excess interest rate in each firm–year as the difference between *LOG_AIS* and the above-computed industry–year benchmark *LOG_AIS*. We label our risk-adjusted excess interest rate *EXCESS_LOG_AIS_I*. That said, banks may have taken pre-attack risk-mitigating actions at *previous* loan originations, and these activities may have implications for their assessment of the *current* risk profile of borrowers. These past activities can include, for example, requiring collateral or, more restrictively, using financial and general covenants as monitoring devices for curbing borrowers' future riskiness. Another could involve banks (especially those that are more active in the credit derivatives market) taking an insurance policy against borrowers' future riskiness via directly buying a CDS (see, e.g., Kim et al. 2018).[29] Thus, a potential concern with relying exclusively on *EXCESS_LOG_AIS_I* as the single proxy for economic rent could be that our proxies for default risk, which were used as

[29] "CDSs enable lenders to hedge their credit risk" (Kim et al. 2018, 953–954); therefore, CDS protection taken in preattack periods enables lenders to insure themselves against borrowers' potential future defaults.

an input in calculating the amount of rent extracted, may not adequately describe the credit risk profile of borrowers. To alleviate this concern, we create another variant of the rent extraction variable *EXCESS_LOG_AIS_II*, which is measured in exactly the same way as *EXCESS_LOG_AIS_I* except that it considers the following additional pre-attack risk-mitigating sorting variables: $SECURED_{T+1,T+5}$ (existence of collateral requirements in the past 5 years), $FIN\_COV_{T+1,T+5}$ (existence of a financial covenant restriction in the past 5 years), $GEN\_COV_{T+1,T+5}$ (existence of a general covenant restriction in the past 5 years), or $CDS_{T+1,T+5}$ (existence of a CDS over the borrower's debt stock[s] in the past 5 years).

In Columns 1 and 2 of Table 5, we adopt these direct constructs of economic rent as dependent variables and rerun our baseline regression in equation (1) in two estimations. Columns 1 and 2 show that the coefficient of *POST_ATTACK* is positive and significant, with magnitudes of 0.095 (t value = 2.90) and 0.103 (t value = 2.42), respectively. We interpret these effects in such a way that relationship banks tend to increase their risk-adjusted loan spreads by 10–10.8 percent, on average, in response to short sellers' allegations.[30] The economic magnitudes here, after explaining away default and information risk explanations of the cost of debt, fall within a range of estimates that are similar to those in our baseline regression in Table 3. Given that our outcome variables are adjusted for both default and information risks, the results in Table 5 lend further support to the rent extraction story.

3.2.4 Test of the conditions that increase bank hold-up power

3.2.4.1 Increase in credit risk

Rajan's (1992) theoretical work proposed that hold-up problems should increase as borrower risk increases. In this subsection, we formally test this prediction in our setting by examining whether banks perceive activist short sellers' allegations as reflecting significant increases in credit risk. To this end, we follow the debt literature and adopt a very granular proxy of credit risk, that is, daily CDS spreads with 5-year maturity from the Markit database. From this database, we collect data for only the list of firms included in our Dealscan sample and for the period of 2008–2017.[31] (Recall that we had previously labeled this our final, pre-matched [firm-day] sample, which we then entropy-balanced to construct a matched [firm-day] sample.) The use of this sample with daily CDS data allows us to attribute any effect from CDS spread tests to these firms. We adopt 5-year spreads instead of other maturity spreads because these contracts are the most liquid and tend to dominate the CDS market (Jorion and Zhang 2007; Berndt 2015; Lee, Naranjo, and Velioglu 2018; Agca et al. 2022).[32] The granularity of the CDS data allows us to specify an important fixed effect related to default in CDS contracts, that is, Credit Rating FEs. These FEs allow us to control for the concern that CDS investors may simply be reacting to changes in credit quality as perceived by rating agencies but not specifically to activist short sellers' allegations.

We then adopt the following TWFE specification, where we are able to impose a "Borrowing Firm FE" structure:

$$CDS\_SPREAD\ (\%)_{\mathrm{i,t}} = \alpha_1 + \alpha_2 POST\_ATTACK_{\mathrm{i,t}} + \alpha_{3-\mathrm{K}}\mathrm{Control}_{\mathrm{i,t}} + \mathrm{Firm\ FEs} + \mathrm{Time\ FEs} + \mathrm{Credit\ Rating\ FEs} + \varepsilon \tag{2}$$

In the above model, the subscripts i and t are the borrowing firm referenced in CDS contracts and the specific date or period of observing the CDS spread, respectively. In

[30] Economic magnitude is calculated as before, that is, the coefficient 0.095 is interpreted as an increase in economic rent by [exp(0.095) – 1] = 0.0997 = 10 percent.

[31] A Markit sample period of 2008–2017 is used instead of the Dealscan sample period of 2008–2018 because in Markit, the CDS panel data structure in 2018 and onward is different from that in prior years.

[32] Nonetheless, in unreported tests (available upon request), we adopt alternative maturity spreads in the baseline model and find qualitatively similar results.

Table 5. Direct measures of economic rents.

This table reports the OLS regression estimates for the effect of short sellers' allegations on economic rent, controlling for loan- and borrower-level characteristics. The table reports heteroskedasticity-adjusted t statistics in parentheses; observations are clustered at the lender level, and the regression adopts a "lender + industry + year" FE structure. The asterisks *, **, and *** represent statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively. All the variables are defined in Appendix B.

	(1)	(2)
dep. var.:	*EXCESS_LOG_AIS_I*	*EXCESS_LOG_AIS_II*
POST_ATTACK	0.095***	0.103**
	(2.90)	(2.43)
All Controls Included?	Yes	Yes
Lender FEs	Yes	Yes
Year FEs	Yes	Yes
Industry FEs	Yes	Yes
Loan Type FEs	Yes	Yes
Loan Purpose FEs	Yes	Yes
N	563	658
Adjusted R^2	0.160	0.714

Columns 1 and 2 of Table 6, we report estimated results for the above DiD model using daily staggered treatment (i.e., assignment of CDS observations into groups). In Table 6, Column 2 shows that, after controlling for all factors, on average, an increase in daily CDS spreads of approximately sixty-three basis points is associated with activist short sellers' attacks. Banks are themselves one of the major groups of CDS investors, along with, for example, other institutional investors (e.g., hedge funds) and retail investors who invest in swaps through exchange-traded funds and mutual funds. Hence, our CDS pricing effect can be interpreted in such a way that both banks and nonbank CDS investors react actively to allegation-related credit risks in our sample. This, therefore, leads us to reason that banks and other debt market participants likely consider the information content of short sellers' reports. Stated another way, they behave as if the short seller report reflects an increase in the credit risk of borrowing firms referenced in CDS contracts. This increase in borrower credit risk should strengthen relationship banks' negotiation power and encourage banks to hold up borrowers for higher interest rates.

3.2.4.2 Increase in borrower switching costs

Another prediction in information monopoly theories is that, for rent extraction to occur, it should be difficult for borrowers to switch lenders when they are seeking new financing. Indeed, existing studies show that attacked firms have fewer financing options after short sellers' attacks (e.g., Grullon, Michenaud, and Weston 2015; Meng et al. 2020; van Binsbergen, Han, and Lopez-Lira 2023), which makes it difficult for them to find new sources of financing outside of existing lending relationships. As a result, relative to non-attacked firms, attacked firms are less likely to switch lenders following short sellers' attacks.

We test this prediction by replacing the dependent variable in our baseline model in equation (1) with measure(s) of lender switching. We capture the extent to which borrowers switch versus stay using two proxies. The first is *N_BORROWER_SWITCHED_TO*, which we measure as the number of lead lenders (including the main lead lender) that a borrower "switches to" in a given year. Because there are often multiple lead lenders per loan facility, we also adopt a second proxy *BORROWER_SWITCH_EXCEEDS_STAY*, which is an indicator variable that equals 1 if the number of lead lenders that a borrower "switches" exceeds the number it "stays with" in a given year and 0 otherwise. For this analysis, we cannot use the final, matched [loan–year] sample because borrowers in the sample strictly stayed with the main lead

Table 6. Short seller allegations and *ex post* credit risk.

This table reports the regression estimates for the effect of short seller allegations on the Markit database's daily CDS 5-year spreads (in percent). The table reports heteroskedasticity-adjusted *t* statistics in parentheses; observations are clustered at the firm level, and the regression adopts a "Firm + Time" fixed effect ("FE") structure. The logit sample period of 2008–2017 is used instead of 2008–2018 because the CDS structure from 2018 onward is different from that in prior years. Note that the sample size decreases from 402,004 to 376,685 because imposing Firm FEs automatically decreases singleton observations to address the concern that "maintaining singleton groups in linear regressions where fixed effects are nested within clusters can overstate statistical significance and lead to incorrect inference" (Correia 2015). The asterisks *, **, and *** represent statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively. All the variables are defined in Appendix B.

	(1)	(2)
dep. var.:	*DAILY CDS SPREAD* (%)	
_cons	1.505***	–30.102**
	(20.03)	(–2.09)
POST_ATTACK	**1.449****	**0.632***
	(2.46)	(1.73)
ROA		–3.969
		(–0.96)
LOG_ASSETS		2.438**
		(2.08)
LEVERAGE		7.518***
		(3.11)
TANGIBILITY		7.340***
		(2.98)
Q		–0.333
		(–1.18)
Z_SCORE		0.671*
		(1.73)
DISTANCE-TO-DEFAULT		0.479
		(0.52)
ABS_ACCRUALS		1.943
		(0.75)
Credit Rating FEs	Yes	Yes
Firm FEs	Yes	Yes
Date (i.e., Year–Month–Day) FEs	Yes	Yes
N	376685	376685
Adjusted R^2	0.663	0.719

lender, who has the most significant bargaining power in negotiating syndicated loans with a borrower.[33] Thus, there is no borrower switching among the main lead arrangers in this sample. We have thus elected to perform the "borrower switching behavior" test using the entropy-balanced version of one of our initial sample of 8,524 loan–year observations that allows for observing borrowers switching lenders (recalling that, we had previously labeled this sample as our Penultimate "II" sample).

Furthermore, while all of the tests using our final, matched (loan–year) sample adopt a Lender FE structure, we cannot use this same FE structure for testing the switching behavior in our Penultimate "II" sample; this is because significant rent extraction occurs at the

33 The main lead arranger is often the largest loan contributor and has the most influence over pricing, covenants, and other key terms. It typically has greater access to the borrower's private information, including forecasts, strategic plans, and risk assessments. When additional financing, refinancing, or waivers are needed, the main lead arranger acts as the key gatekeeper and can demand higher fees, stricter covenants, or better collateral. This suggests it holds the most significant holdup power compared to other co-arrangers.

level of the main lead arranger. As such, there will be no within-lender variation in borrowers to exploit; essentially, the likelihood of switching the main lead arranger equals 0 for all observations in an estimation that adopts Lender FEs. Following this concern, the next best FE candidate would be Borrowing Firm FEs. However, as is the norm in the bank loan contracting literature, the data structure of Dealscan's bank loan data does not allow for the exploitation of adequate within-firm variation in estimations involving equation (1) in particular; basically, in our sample period, bank loan tranche/facility observations per firm are infrequent (see, e.g., discussions in Campello and Gao 2017, 113). Thus, we elected to adopt our ever-treated indicator *ATTACK* in place of Borrowing Firm FEs, as the role of both measures in a DiD design is to control for fixed differences between treated and control units (see, e.g., Bertrand and Mullainathan 1998, 15).

In Columns 1 and 2 of Table 7, Panel A, we adopt, as the dependent variable, the two aforementioned proxies for borrowing switching, *N_BORROWER_SWITCHED_TO* and *BORROWER_SWITCH_EXCEEDS_STAY*. We find that the coefficient of *POST_ATTACK* is negative and significant across both columns, suggesting that, relative to non-attacked firms, attacked borrowers are less likely to switch banks following activist short seller attacks. In economic terms, the evidence in Column 1 (2) shows, following activist short sellers' attacks, a decrease in the number (likelihood) of lender switches by attacked firms by approximately 0.79 lenders (7.7 percent), relative to non-attacked firms.[34]

A shortfall with the evidence presented above on borrowers' switching behavior is that it is not estimated jointly with the incidence of bank extortion. That is, we find that attacked borrowing firms are less likely to switch lenders, and we also separately find that borrowers are offered higher-interest loans following activist short seller allegations. To provide credibility to our rent extraction story, we now consider the interaction between both of the abovementioned conditions in a single estimation but still use our Penultimate II sample of 8,524 observations. Specifically, in Panel B of Table 7, we regress our direct proxies of economic rent—recomputed in this much larger sample (compared with the smaller sample used before for the economic rent test)—on the DiD variable *POST_ATTACK*, partitioning the sample by two indicators of borrower switching lenders.[35] The first indicator of borrowers switching lenders "*SWITCHED_IND*" is the transformed version of our previous continuous variable *N_SWITCHED*. Specifically, *SWITCHED_IND* is coded 1 if N_*SWITCHED* > 0 and 0 otherwise. Our second indicator of borrowers switching lenders is the previous proxy we had adopted—*BORROWER_SWITCH_EXCEEDS_STAY*.

As presented in Panel B of Table 7, the estimated results across both measures of borrower switching show that the coefficient on *POST_ATTACK* for borrowing firms that switch lenders (i.e., *SWITCHED_IND* = 1 or *BORROWER_SWITCH_EXCEEDS_STAY* = 1) is insignificant, whereas that for borrowing firms that stay with lenders (i.e., *SWITCHED_IND* = 0 or *BORROWER_SWITCH_EXCEEDS_STAY* = 0) is positive and significant at the 10 percent level or better. These findings are consistent with the rent extraction story; specifically, we provide evidence that, following activist short sellers' allegations, attacked borrowing firms that do not switch (who switch) lenders are extorted (not extorted) by banks.

3.2.5 Disparate treatment of allegation types

In this subsection, we exploit heterogeneity across allegation categories or types to assess which types of negative narratives disseminated by short sellers are most versus least consequential. There is ongoing debate over the credibility of activist short sellers, with some critics arguing that their claims may be misleading or influenced by questionable incentives

[34] As before, the coefficients are directly interpreted as economic magnitudes.

[35] We perform the sample split regressions using the state-of-the-art method that robustly estimates sample splits in a single regression to allow for heterogeneous covariate slopes, where each covariate has a different slope coefficient depending on the partitioning variable (Correia 2017). As a consequence, unlike old methods, a single statistic of "R-square," as well as "N" (i.e., number of observations) is outputted in an estimation.

Table 7. Short seller allegations and borrower stay or switch behavior.
This table reports the regression estimates for the effect of short seller allegations on borrower stay or switch behavior, using Poisson pseudolikelihood regression with multiple levels of fixed effects (Column 1) and a linear probability model (Column 2). The table reports heteroskedasticity-adjusted *t* statistics in parentheses; observations are clustered at the firm level, and the regression adopts an "industry + year" fixed effect ("FE") structure. Because many observations are missing for the ex post default risk measures in the final sample, to preserve test power in regressions, we follow prior studies (e.g., Byoun 2008, 3078) and set missing values to 0 while simultaneously creating a missing value indicator for each measure, which we include—but, for brevity, do not report—alongside the counterpart measure as a control variable in regressions. The missing value indicator per measure, which we specify in regressions, is expected to control for any measurement error associated with the adjustment we had made to each measure of default risk. The asterisks *, **, and *** represent statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively. The outputted regression sample size drops from the original 8,524 observations (see Appendix A) to 6,192 (6,558) in column 1 (2) because: (1) the *DISTANCE-TO-DEFAULT* measure is non-missing for 6,568 observations; and (2) the statistical package (STATA) tends to drop observations where FEs cause multicollinearity. All the variables are defined in Appendix B.

Panel A: Predicting switch/stay behavior

	(1)	(2)
	N_BORROWER_SWITCHED_TO	*BORROWER_SWITCH_EXCEEDS_STAY (1/0)*
_cons	–13.024***	–0.227
	(–4.66)	(–1.52)
ATTACK	0.731***	0.069***
	(3.26)	(3.42)
POST_ATTACK	**–0.787****	**–0.077*****
	(–2.14)	(–3.13)
LOG_MATURITY	0.605**	0.005
	(2.55)	(0.23)
LOG_LOAN_SIZE	0.105	0.006
	(1.23)	(0.62)
LOG_1_PLUS_NO_OF_PRIOR_DEALS	0.503**	0.012
	(2.50)	(0.27)
PERFORMANCE_PRICING_IND	–0.211	–0.019
	(–0.98)	(–1.13)
ROA	4.782**	–0.040
	(2.02)	(–0.35)
LOG_ASSETS_USD	0.441***	0.014*
	(3.14)	(1.76)
LEVERAGE	0.407***	0.038***
	(2.78)	(3.54)

(continued)

Table 7. (continued)

Panel A: Predicting switch/stay behavior

	(1)	(2)
	N_BORROWER_SWITCHED_TO	*BORROWER_SWITCH_EXCEEDS_STAY (1/0)*
TANGIBILITY	0.361	0.023
	(0.73)	(0.68)
Q	−0.235	−0.008
	(−1.18)	(−1.21)
Z_SCORE	−0.258	−0.008
	(−1.57)	(−1.03)
DISTANCE-TO-DEFAULT	−0.178	0.014
	(−0.28)	(0.22)
ABS_ACCRUALS	0.242	0.107
	(0.14)	(0.66)
MISSTATEMENT	0.306	0.010
	(1.25)	(0.42)
AVERAGE_CDS_SPREAD	−0.113	−0.016
	(−1.02)	(−1.22)
DOWNGRADE	0.124	0.011
	(0.21)	(0.24)
Loan Type FEs	Yes	Yes
Loan Purpose FEs	Yes	Yes
Industry FEs	Yes	Yes
Year FEs	Yes	Yes
N	6192	6558
Adjusted R^2	0.410	0.168

Panel B: Economic rent conditioned on switch versus stay behavior

	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	dep. var: *EXCESS_LOG_AIS_I*				**dep. var:** *EXCESS_LOG_AIS_II*			
Partitioning variable:	*SWITCHED_IND*		*BORROWER_ SWITCH_EXCEEDS_STAY*		*SWITCHED_IND*		*BORROWER_ SWITCH_EXCEEDS_STAY*	
	= 0	= 1	= 0	= 1	= 0	= 1	= 0	= 1
ATTACK	0.012	−0.194**	−0.001	−0.157	−0.008	−0.210**	−0.020	−0.158
	(0.34)	(−2.08)	(−0.002)	(−1.46)	(−0.244)	(−2.09)	(−0.59)	(−1.43)

(continued)

Table 7. (continued)

Panel B: Economic rent conditioned on switch versus stay behavior

	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	dep. var: ***EXCESS_LOG_AIS_I***				**dep. var:** ***EXCESS_LOG_AIS_II***			
Partitioning variable:	*SWITCHED_IND*		*BORROWER_ SWITCH_EXCEEDS_STAY*		*SWITCHED_IND*		*BORROWER_ SWITCH_EXCEEDS_STAY*	
	= 0	= 1	= 0	= 1	= 0	= 1	= 0	= 1
POST_ATTACK	**0.064***	**0.022**	**0.070***	**–0.103**	**0.098****	**0.025**	**0.100****	**–0.089**
	(1.71)	(0.32)	(1.94)	(–1.00)	(2.43)	(0.36)	(2.54)	(–0.85)
All variables included?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan Type FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan Purpose FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	3213		3213		3272		3272	
Adjusted R-square	0.266		0.265		0.368		0.366	

(Cohodes 2020; Mitts 2020; Herbst-Bayliss 2021). However, the mere act of making an allegation does not necessarily mean that capital allocators, such as banks, will consider it credible or impactful. Thus, a potential way of identifying whether allegations may be noteworthy to credit allocators such as banks is to glean whether the capital market in general believes allegations made in activist short sellers' reports to be consequential. Accordingly, we use the capital market's belief to separate most versus least consequential allegations. Specifically, we code the variable *PRICE_DROP* as 1 if the data provider on short seller allegation reports specifies that the campaign return is negative and 0 otherwise.

Separating allegations using this variable, in Columns 1 and 2 of Table 8, Panel A, we find that "price drop" allegation effects are twice as large as effects of allegations that do not lead to price drops. Zooming in on "price drop" allegations, we further distinguish allegation types by creating two groups that separate allegations by whether they are outright fraudulent/serious or not. Table 8, Panel B provides the distribution of allegation types in our treatment group. Specifically, the variable *FRAUD/SERIOUS* is coded 1 if the short seller allegation is related to the following eight different types: (1) accounting fraud; (2) misleading accounting; (3) major business fraud; (4) other illegal; (5) pyramid scheme; (6) ineffective product; (7) invalid patent; and (8) medical effectiveness. For all other allegations that do not fall under any of the above (i.e., allegations such as overlevered, stock promotion, dividend cut coming, bubble, other overvaluation, etc.), the variable *FRAUD/SERIOUS* is coded 0. As displayed in Columns 3 and 4 of Table 8, Panel A, we find that "price drop" allegations that are of a fraudulent/serious nature lead to effects that are 1.5 times the effect of "price drop" allegations that are not of a fraudulent/serious nature.[36] Overall, these findings lend support to the view that banks are more opportunistic when exposed to relatively "more damaging" allegations about their relationship borrowers.

3.2.6 Endogeneity and robustness checks

In our study, loan observations are not randomly assigned to groups of attacked versus non-attacked firms, so activist short sellers' attacks may not be exogenous to lending decisions. Zhao (2020) showed that certain firm attributes predict the likelihood of such attacks. For example, activist short sellers could time their attack exactly one quarter before a firm negotiates a more expensive loan. It is also possible that some omitted variables excluded from the baseline specification as well as from the alternative FE specifications could jointly determine both activist short sellers' attacks and loan spreads. Thus, to alleviate concerns about potential endogeneity, we perform a number of sensitivity checks below.

3.2.6.1 CDS-only loans

Our interpretation of the baseline results as rent extraction relies critically on the assumption that default risk is adequately controlled for. While our tests incorporate CDS spreads (with missing values coded as zero), the absence of CDS data for some firms may affect the accuracy of our default risk controls. Moreover, borrowers with CDS coverage differ systematically from those without, raising concerns about sample heterogeneity. Although entropy balancing helps mitigate this issue, it does not eliminate it entirely. Thus, to check the robustness of our findings, we condition our tests only on firms that have CDS data. Such an analysis would allow us to inherently control for all observed and unobserved reasons why a CDS contract is written over a borrower's debt stock; essentially, we directly difference away these confounders by comparing treated and control units affected by the same

[36] That said, while economically meaningful, the difference in effects between disparate groups (Column 1 versus 2 and Column 3 versus 4) is not statistically significant. For example, as reported in the lower part of Table 8, Panel A, our chi-square test of the difference in coefficients between Columns 1 and 2 (Columns 3 and 4) generates a *P* value of .46 (.74).

Table 8. Severity/types of allegations.

This table reports OLS regression estimates for the effect of short sellers' allegations on bank loan spreads, controlling for borrower credit risk among other loan- and borrower-level characteristics. The table reports heteroskedasticity-adjusted t statistics in parentheses; observations are clustered at the lender level, and the regression adopts a "lender + industry + year" FE structure. The asterisks *, **, and *** represent statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively. All the variables are defined in Appendix B.

Panel A: Estimation

	(1)	(2)	(3)	(4)
	PRICE DROP = 1	*PRICE DROP* = 0	*PRICE DROP*= 1 & *FRAUD/SERIOUS* = 1	*PRICE DROP* = 1 & *FRAUD/SERIOUS* = 0
	dep. var.: *LOG_AIS*			
POST_ATTACK	**0.124***** (4.79)	**0.067**** (2.07)	**0.171**** (2.36)	**0.105***** (4.36)
All Controls Included?	Yes	Yes	Yes	Yes
Lender FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes
Loan Type FEs	Yes	Yes	Yes	Yes
Loan Purpose FEs	Yes	Yes	Yes	Yes
Difference in coefficients (p value):				
(1)–(2)	0.464			
(3)–(4)	0.741			
N	2502	2274	2220	2367
Adjusted R^2	0.710	0.717	0.757	0.721

Panel B: Distribution of types of allegations

Primary allegation	Freq.	Percent	Cum.
Accounting Fraud	22	3.64	3.64
Bubble	31	5.12	8.76
Competitive Pressures	13	2.15	10.91
Dividend Cut Coming	2	0.33	11.24
Industry Issues	162	26.78	38.02
Ineffective Roll-Up	84	13.88	51.9
Major Business Fraud	10	1.65	53.55

(continued)

Table 8. (continued)

Panel B: Distribution of types of allegations

Primary allegation	Freq.	Percent	Cum.
Misleading Accounting	102	16.86	70.41
Other Illegal	3	0.5	70.91
Other Overvaluation	60	9.92	80.83
Over-Levered	36	5.95	86.78
Patent Expiration	13	2.15	88.93
Patent Invalid	22	3.64	92.56
Product Ineffective	6	0.99	93.55
Pyramid Scheme	22	3.64	97.19
Stock Promotion	1	0.17	97.36
Upcoming Earnings Miss	16	2.64	100
Total	605	100	

confounder. Supplementary Appendix Table OA2 presents results based on this restricted sample of 576 loans within our final matched (loan–year) sample. The findings remain consistent with our baseline estimates, reinforcing our interpretation that banks extort borrowers during periods of adversity.

3.2.6.2 Alternative DiD methods

Our primary static effect model employs the traditional TWFE approach to implement our staggered DiD analysis. However, recent studies have documented potential shortcomings of the TWFE approach in the presence of staggering events. Specifically, these problems are (1) the "bad controls/comparisons" problem, where so-called "bad" units can be used as a comparison group for treated units, and (2) the related "sign-flip" problem, where DiD treatment effect estimates can actually obtain the opposite sign of the true average treatment effect on the treated (Baker, Larcker and Wang 2022, 371, 384, 386, 390). To check whether our TWFE static estimates do not suffer from these problems, we implement recently developed solutions such as the stacked DiD design (see Baker, Larcker and Wang 2022) and the Sun and Abraham (2021) interaction-weighted design. In Supplementary Appendix Table OA3, we implement these alternative designs and continue to find results that are similar to our baseline results.

3.2.6.3 Alternative FE models

Loan contracting terms are equilibrium outcomes, influenced jointly by both the lender and the borrower. However, our baseline tests control for borrower and loan characteristics but do not control for time-varying lender characteristics—only the time-invariant attributes of the lender are controlled for. Thus, we alternatively specify Lender × Year FEs in place of Lender FEs in equation (1) to control for *all time-varying* lender-level determinants of loan spreads. We also alternatively augment Lender FEs with Industry × Year FEs (instead of the Year FEs used in our baseline model) to allow for controlling for all omitted, *time-varying* industry-level and nationwide confounders such as recessions/expansions (e.g., Santos and Winton 2008; Hertzel and Officer 2012). In all of these estimations (see Supplementary Appendix Table OA4), we continue to observe a positive nexus between activist short sellers' attacks and loan spreads. Our results are therefore robust to controlling for potential omitted factors and reverse causality associated with them.

3.2.6.4 Alternative matching methods

To show that our results are not driven by our choice of matching scheme, we perform four other alternative matching procedures: kernel matching (ridge matching, caliper $= 0.005$), kernel matching (local linear matching, caliper $= 0.001$), 1-to-1 matching without replacement, and caliper matching (0.001). We implement each of these procedures as follows: we first use a logistic regression that regresses the probability of being in an attacked or non-attacked group (i.e., $ATTACK = 1$ or 0) on all covariates and fixed effects specified in our baseline model (equation (1)) to compute propensity scores (i.e., the predicted likelihood of being in the attacked sample).[37] Then, under each of these matching techniques, we match the observations in the attacked and non-attacked groups on the basis of the computed propensity scores, and then, for each attacked observation, choose the closest non-attacked observation.

Supplementary Appendix figure OA1 provides graphs, for example, for the kernel matching (ridge matching, caliper $= 0.005$) setup, which allows for visual inspection of whether covariate balance between attacked and matched non-attacked groups has been achieved. Panel A of Supplementary Appendix figure OA1 shows that, prior to matching, the propensity scores of the attacked and non-attacked groups diverged, whereas after

[37] As in the entropy balancing scheme, we do not adopt Lender FEs as a covariate in the propensity score model, because it does not allow our maximum likelihood estimation to converge.

matching, the propensity scores of the attacked and non-attacked groups aligned. Furthermore, Panel B of Supplementary Appendix figure OA1 indicates that the standardized mean difference (a first moment) between the two groups for almost all of the covariates is equal to or very close to 0, whereas the variance ratio of the difference in covariates (a second moment) for half of the cases is also close to 1. The above findings seem to suggest that the kernel matching scheme achieves more covariate balance on the first moments than it does on the second moments. We also observe similar patterns for the other three matching methods. We conclude that these methods are inferior to the entropy balancing scheme, which achieves covariate balance in the first, second, and third moments of the distribution of the covariates (see Table 2, Panel B). Nevertheless, because these methods on their own also help alleviate endogeneity issues, we re-estimate the baseline model using four newly constructed PSM samples that correspond to the methods. We report the estimated results in Supplementary Appendix Table OA5 and document effects that are qualitatively similar to our baseline results, implying that the choice of a matching scheme is not the driver of our results.

3.2.6.5 First-time allegation events versus multiple allegation events

In constructing our baseline sample, we identified "attacked" firm-loan observations on the basis of multiple allegation events; that is, if, in a given year, a firm is faced with multiple activist short sellers' allegations of potentially varying consequences, loan observations for that year were assigned to each of these allegation events. Consequently, loan observations repeat per firm–year for scenarios where there are multiple allegations in a given firm–year. In the Supplementary Appendix, instead of using multiple allegation events to identify assignment into the "attacked" group, we use the first allegation event per firm–year. This new approach of assignment into groups therefore reduces our baseline sample from 2,732 to 2,498 unique loan facility/tranche observations.

In Supplementary Appendix Table OA6, we re-estimate our baseline model and tabulate the results. The evidence presented is consistent with our baseline finding of a positive nexus between activist short sellers' allegations and loan spreads. An interesting observation, however, is that the economic magnitude of effects here (0.050 in Supplementary Table OA6) is 2.7 percent lower than that documented using multiple allegation events (0.077 in Column 5 of Table 3, Panel A). The reduction in economic magnitude could be attributed to the fact that, in retaining only the first allegation event per firm–year, we may have thrown out more consequential allegation events arriving a few days/weeks/months after but within the same year as the first allegation event.

3.2.7 Additional analyses

3.2.7.1 Non-price loan terms

In addition to price terms, rent extraction can occur through non-price loan terms, such as loan covenants and performance pricing provisions. Such terms enhance banks' ability to access borrower private information, enabling them to extract rents. For example, Carrizosa and Ryan (2017) show that lenders can use covenants to require projected financial statements for future periods and monthly historical financial statements from borrowers and trade on borrower private information in the secondary market. We therefore test whether, after controlling for borrower credit risk, one can observe changes in these non-price terms after allegations are made by short sellers.

We adopt the logarithm of the number of *total* covenants (*LOG_N_COVENANT*), the logarithm of the number of *financial* covenants (*LOG_N_FINANCIAL COVENANT*), the logarithm of the number of *general* covenants (*LOG_N_GENERAL COVENANT*), and the presence of performance pricing provisions (*PERFORMANCE_ PRICING_IND*) as separate dependent variables in equation (1) and test whether banks are inclined to adjust these non-price terms of new loans following short seller attacks. Supplementary

Appendix Table OA7 presents the results of OLS estimations that selectively adopt one of these non-price terms as the dependent variable. Our findings reveal no significant associations between short sellers' attacks and these non-price terms in new loan contracts. The absence of significant changes in non-price terms post-attack suggests that banks primarily capitalize on the rent-seeking opportunity afforded by short sellers' allegations through price adjustments in new loan contracts rather than altering the nonprice terms of loans. The focus on price adjustments alone as the means by which the bank extorts the borrower during bad times could be explained by one of three arguments below or a combination thereof.

First, nonprice terms such as restrictive covenants can overburden the borrower (especially during times of adversity), potentially pushing them into financial distress or default. This is counterproductive for the bank, as it increases the likelihood of loan losses. *Second*, while switching costs are higher during adversity, they are not infinite. If the bank pushes too hard with non-price terms, the borrower may still choose to switch lenders, especially if the terms are perceived as unfair. For example, prior studies have documented that borrowers are more likely to switch lenders following an episode of covenant enforcement when the lender chooses to enforce: (1) based on income-seeking incentives (Bird et al. 2022) versus (2) without specific rent-seeking motives in mind (Bird et al. 2023). In particular, Bird et al. (2023) also found that the strict enforcement of covenants can increase the likelihood of borrowers seeking alternative financing sources, even when switching costs are elevated.[38] They interpreted their findings as "borrowers being disgruntled by incremental enforcement of covenant violations" (p. 18). *Third*, non-price terms such as covenants require ongoing monitoring and enforcement, which can be costly for the bank, especially during the period of allegations. Increasing loan spreads is a simpler and more cost-effective way to extract rents. For example, during the 2008 financial crisis, when the firm "General Growth Properties" faced severe liquidity issues and struggled to refinance its debts (followed by a 98 percent decline in stock price after it had failed to meet its debt obligations), some lenders ultimately waived certain covenants and provided temporary relief to avoid triggering a full default that would have forced them into expensive bankruptcy proceedings.[39]

3.2.7.2 Loan renegotiation

In addition to new loan contracting, loan renegotiation is another viable avenue that banks could use to extract informational rents from borrowers (e.g., Rajan 1992; Nikolaev 2018). For example, a firm in distress may request an extension of loan maturity, but the bank, in agreeing to this request, can charge a hefty restructuring fee and a new "commitment fee" for continued access to credit. In the presence of the incentive to avoid reporting loan losses, the bank can also extend a loan's maturity instead of classifying it as a bad debt and further increase collateral requirements, extracting more security from the borrower. Therefore, we test whether, after controlling for changes in borrower credit risk, banks renegotiate existing loan covenants following short sellers' allegations.

We scrutinize the renegotiation of existing loans during years T and T + 1, along with any adverse renegotiation of price and nonprice terms. Following the approach of prior studies examining the determinants of loan renegotiation (e.g., Nikolaev 2018), we perform a firm–year analysis. To this end, we first retrieve our final, matched (firm–year) sample and then merge this with loan renegotiation data in the *loan amendment file* of the Loan Pricing Corporation's (hereafter, "LPC") Dealscan database. Our first renegotiation proxy is an indicator variable *RENEG_IND* that equals 1 if an existing loan contract is renegotiated in a particular year and 0 otherwise. Our second renegotiation proxy is a

[38] "The value of the relationship to the lender should then be higher for borrowers with fewer, or more costly, alternative sources of financing" (p. 3).

[39] https://www.clm.com/wp-content/uploads/2020/11/6557479_3.pdf (Last accessed: May 31, 2025)

continuous variable *LOG_N_LOAN_FAC_RENEG*, which is defined as the logarithm of the number of loan facilities renegotiated per firm–year in the presence of renegotiation and takes a value of 0 in the absence of renegotiation. In Supplementary Appendix Table OA8, Panel A, we first estimate the impact of allegations on the likelihood of renegotiation and the number of loan facilities renegotiated at time T or T + 1. As shown in Columns (1)–(4), we find no evidence that banks renegotiate existing loans following activist short sellers' allegations.

In addition to the measures adopted above, banks can amend existing loan contracts to make them more unfavorable to borrowers by increasing loan spreads, increasing loan security or collateral requirements, decreasing the maturity period (i.e., asking lenders to pay back the loan more quickly), or even reducing the amount of undrawn credit. Therefore, on the basis of textual analyses of the "comment" section of the LPC's facility amendment file (see, e.g., approach in Chu 2021), we identify and code each of the aforementioned unfavorable changes in loan contracts. There is typically less variability in each of these adverse unidirectional changes in contract terms when a sample contains both renegotiation and non-renegotiation firm–years (see, e.g., Chu 2021, Table 2); therefore, we alleviate this concern by constructing a composite amendment indicator *RENEG_IND (UNFAV. TERMS)* that equals 1 if, in a given firm–year during which period a loan renegotiation occurs, a borrower experiences either an increase in loan spreads, a decrease in maturity, or a decrease in the amount of credit; it equals 0 otherwise.[40] Following Chu (2021), we also decompose *RENEG_IND (UNFAV. TERMS)* into its components: *PRICE_INCREASE*, *MATURITY_DECREASE*, and *CREDIT_DECREASE*.

In Panel B of Supplementary Appendix Table OA8, we repeat our renegotiation test by estimating, this time round, the effect of allegations on *RENEG_IND (UNFAV. TERMS)* and its constituent parts. On the basis of the evidence displayed in both panels, we do not find evidence that banks adversely alter price and non-price terms in renegotiated loan agreements. Given the popular view in the literature that renegotiations typically occur when there is a violation of covenant(s), an anticipation of a covenant violation, or a default (see Roberts and Sufi 2009), we interpret the lack of our results on renegotiation as short seller allegations being a less likely reason why, on average, borrowers default or breach covenant(s) in existing loan agreements.

4. Conclusion

This study compares loan spreads for borrowers with prior lending relationships before and after the release of highly negative public reports by activist short sellers. We find that, after controlling for loan and borrower characteristics, banks increase loan spreads beyond and above the level justified by borrower credit risk, following activist short sellers' allegations. This evidence is consistent with classic finance theory on the information hold-up problem, which posits that relationship banks can use their information monopoly to extort borrowers.

We differ from existing empirical works on rent extraction in the following sense. We provide novel evidence that, in times of borrowers' adversity triggered by *others' allegations* (rather than some industry-specific or economy-wide adverse shock), relationship banks—who are generally believed to know their borrowers quite well and are therefore best positioned to corroborate such allegations about borrowers (see, e.g., Boot, Grfenbaum, and Thakor 1993; Petersen and Rajan 1994, 1995)—seem to leverage the adversity created by the allegations to extort affected borrowers rather than to offer them some form of (at least temporary) reprieve.

[40] Our choice of changes in these loan features is motivated by prior research; specifically, prior renegotiation studies (e.g., Roberts and Sufi 2009; Chu 2021) have shown loan features such as amount, maturity, and pricing to be the elements of existing loan contracts that are often subject to *large* changes during loan renegotiations.

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Supplementary material

Supplementary material is available at *Review of Finance* online.

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Data availability

All data used in this article are available from third-party vendors as identified in the article (see Section 2).

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Appendix

Appendix A: Sample construction

	#Obs	#Firms
LOAN–YEAR SAMPLE:		
Penultimate I sample: facility–year loan observations of treated + control firms (i.e., never-treated firms) during the period 2008–2018 (after merging data from multiple databases: Dealscan, Compustat, CRSP, S&P Capital IQ, Markit and Audit Analytics)	8,890	2,482
less loan observations of treated firms without existing loans before activist short sellers' allegations	(366)	(105)
Penultimate II sample: loan observations of treated firms with loans in both pre- and post-allegation periods + control firms	8,524	2,377
less loan observations of both treated and control firms without existing borrower-lender relationship in any of the past eight years	(5,405)	(1,161)
Final, pre-matched sample: loan observations of treated firms with same borrower-lender relationship(s) in both pre- and post-allegation periods + loan observations of control firms that in each period has had the same borrower-lender pair as in any of the previous eight years	3,119	1,216
Final, matched (loan–year) sample: after entropy-balance matching	2,732	1,088
OTHER (FIRM–YEAR) SAMPLE FORMED FROM ABOVE:		
Final, pre-matched (firm–year) sample: Firm–year panel data associated with firms in "final, matched (loan–year) sample"	10,875	1,088
Final, matched (firm–year) sample: after entropy-balance matching	8,289	1,019
OTHER (FIRM-DAY) SAMPLE FORMED FROM ABOVE:		
Final, pre-matched (firm-day) sample: Firm-day panel data associated with firms in "final, matched (loan–year) sample"	543,641	335
Final, matched (firm-day) sample: after entropy-balance matching	402,004	306

Appendix B: Variable definitions

Variable	Definition	Data source
Loan (facility–year) variables		
LOG_AIS	The natural logarithm of the *AIS* loan spreads in basis points in excess of the LIBOR rate (including any upfront fees associated with draw downs and annual fees associated with repayments to lenders).	LPC's Dealscan
EXCESS_LOG_AIS_I	The risk-adjusted excess interest rate in each loan-firm–year, calculated as the difference between *LOG_AIS* and the industry–year benchmark *LOG_AIS*. To determine the benchmark interest rate, we compute, for loans of the same type and having the same purpose, an industry–year mean *LOG_AIS* (excluding the current observation itself) of borrowing firms in the same	LPC's Dealscan, Compustat, CRSP, S&P Capital IQ, Markit

(continued)

(continued)

Variable	Definition	Data source
	two-digit SIC industry and in the same quartile distribution of (1) *firm characteristics* (firm size [*LOG_ASSETS_USD*], default risk [*DISTANCE-TO-DEFAULT*] and information risk [*ABS_ACCRUALS*]); and (2) *loan attributes* (loan maturity [*LOG_MATURITY*] and loan amount [*LOG_LOAN_SIZE*]).	
EXCESS_LOG_AIS_II	The risk-adjusted excess interest rate in each loan-firm–year, measured in exactly the same way as *EXCESS_LOG_AIS_I* except that it considers the following additional pre-allegation risk-mitigating sorting variables: $SECURED_{T+1,T+5}$ (existence of collateral requirements in the past 5 years), $FIN\_COV_{T+1,T+5}$ (existence of a financial covenant restriction in the past 5 years), $GEN\_COV_{T+1,\ T+5}$ (existence of a general covenant restriction in the past 5 years), or $CDS_{T+1,T+5}$ (existence of a CDS over the borrower's debt stock[s] in the past 5 years).	LPC's Dealscan, Compustat, CRSP, S&P Capital IQ, Markit
LOG_N_FINANCIAL_COVENANT	The natural log of the number of financial covenants specified in a loan contract.	LPC's Dealscan
LOG_N_GENERAL_COVENANT	The natural log of the number of general covenants specified in a loan contract.	LPC's Dealscan
LOG_MATURITY	The natural logarithm of loan facility's maturity period (in months) between loan issue date and maturity date.	LPC's Dealscan
LOG_LOAN SIZE	The natural logarithm of the US dollar loan (facility) amount.	LPC's Dealscan
LOG_NO_OF_PRIOR_DEALS	The natural log of 1 plus the number of previous loan deals between a borrower and the lead arrangers for the current deal during the past 12 years.	LPC's Dealscan
PERFORMANCE_PRICING_IND	An indicator variable equal to 1 if the loan contract specifies performance pricing provisions, and 0 otherwise.	LPC's Dealscan
LOAN TYPE INDICATORS	An indicator variable that assumes the value of 1 for a given loan type (e.g., term loan, revolver, 364-day facility), and 0 otherwise.	LPC's Dealscan
LOAN PURPOSE INDICATORS	An indicator variable that assumes the value of 1 for a given loan facility purpose (e.g., corporate purposes, debt repayment, working capital, CP backup, takeover, acquisition line, and leverage buyout offers), and 0 otherwise.	LPC's Dealscan
Allegations variables		
PRICE_DROP	An indicator variable that takes a value of 1 if the short seller's campaign results in a negative return, and 0 otherwise.	Activist Insight Shorts database

(continued)

(continued)

Variable	Definition	Data source
FRAUD/SERIOUS	An indicator variable coded as 1 if the short seller allegation is related to the following eight different types: (1) accounting fraud; (2) misleading accounting; (3) major business fraud; (4) other illegal; (5) pyramid scheme; (6) ineffective product; (7) invalid patent; and (8) medical effectiveness. The remaining allegations that do not fall under any of the above (that is, allegations such as overlevered, stock promotion, dividend cut coming, bubble, other overvaluation, etc.) are coded as = 0.	Activist Insight Shorts database and our computation
Renegotiation (firm–year) variables		
RENEGOTIATE_IND	A dummy equal to 1 if in a firm–year, any of a firm's existing loan contracts is renegotiated; 0 otherwise.	LPC's Dealscan (Loan Amendment file)
N_LOAN_FAC_ RENEGOTIATED	The number of loan facilities renegotiated per firm–year.	LPC's Dealscan (Loan Amendment file)
TERMS_NOW_ UNFAVORABLE	A dummy equal to 1 if in a firm–year, any of a firm's existing loan contracts is adversely renegotiated by either increasing loan spreads, increasing loan security, decreasing loan maturity, or decreasing credit supply; 0 otherwise.	LPC's Dealscan (Loan Amendment file)
PRICE_INCREASE	A dummy equal to 1 if in a firm–year, any of a firm's existing loan contracts is adversely renegotiated by increasing loan spreads; 0 otherwise.	LPC's Dealscan (Loan Amendment file)
MATURITY_DECREASE	A dummy equal to 1 if in a firm–year, any of a firm's existing loan contracts is adversely renegotiated by decreasing loan maturity; 0 otherwise.	LPC's Dealscan (Loan Amendment file)
LOAN_AMOUNT_ DECREASE	A dummy equal to 1 if in a firm–year, any of a firm's existing loan contracts is adversely renegotiated by decreasing credit supply; 0 otherwise.	LPC's Dealscan (Loan Amendment file)
SECURITY_INCREASE	A dummy equal to 1 if in a firm–year, any of a firm's existing loan contracts is adversely renegotiated by increasing loan security; 0 otherwise.	LPC's Dealscan (Loan Amendment file)
CDS (firm-day) variables		
DAILY CDS SPREAD (%)	A firm's daily CDS spreads with 5-year maturity.	Markit
DID variables		
ATTACK	A dummy equal to 1 for all loan/firm-periods in the sample associated with firms (i.e., borrowers) that have been targets in activist short seller campaigns, and 0 otherwise.	Activist Insight Shorts database

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Variable	Definition	Data source
POST_ATTACK	An indicator variable (specific only to the treated group) that takes the value of 1 for all periods starting from the event date, and 0 for all pre-event periods. Note that because there is staggering of events, all control firms (i.e., never-targeted firms in sample) are automatically assigned *POST_ATTACK* = 0 so that *ATTACK* × *POST_ATTACK* and *POST_ATTACK* are essentially the same.	Activist Insight Shorts database
Firm–year characteristics		
ROA	Earnings before interest, taxes, depreciation, and amortization (EBITDA) divided by total assets (AT).	Compustat
LOG_ASSETS	The natural logarithm of total assets (AT) in millions of US dollars.	Compustat
LEVERAGE	The sum of long-term debt (DLTT) and debt in current liabilities (DLC) divided by total assets (AT).	Compustat
TANGIBILITY	Gross property, plant, and equipment (PPEGT) divided by total assets (AT).	Compustat
Q	The sum of market value of equity plus book value of debt divided by total assets (AT), where market value of equity equals price per share (PRCCD) times the total number of shares outstanding (CSHOC), and book value of debt equals total assets (AT) minus book value of equity (CSTK).	Compustat
Z_SCORE	Altman's (1968) Z score, calculated as (1.2 working capital [WCAP] +1.4 retained earnings [RE] +3.3 earnings before interest and taxes [EBIT] + 0.999 sales [SALE])/total assets [AT] +0.6 (market value of equity/book value of debt).	Compustat
DISTANCE-TO-DEFAULT	The 1st principal component based on two measures of distance-to-default—*EDF* and *BSMProb*—where (1) *EDF* is the expected default frequency estimated from the KMV-Merton-based method of Bharath and Shumway (2008); and (2) *BSMProb* is the probability of bankruptcy estimated from the BSM option-pricing model following the method in Hillegeist et al. (2004). The composite measure, distance-to-default, is ultimately calculated as the weighted linear combination of the original variables, using estimated eigenvector weights 0.7071 for (EDF) and 0.7071 (*BSMProb*). That is, $DISTANCE\text{-}TO\text{-}DEFAULT = 0.7071 \times EDF + 0.7071 \times BSMProb$.	Compustat & CRSP

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Variable	Definition	Data source
ABS_ACCRUALS	The absolute value of total accruals, calculated as: (Δ[current assets ACT] - Δ[current liabilities LCT]- Δ[cash and short-term investment CHE] + Δ[debt in current liabilities DLC]—depreciation DP)/lag(total assets [AT] (see Dechow, Sloan, and Sweeney 1995).	Compustat
DOWNGRADE	An indicator equal to 1 if a borrower receives a downgrade on its Standard & Poor's (S&P) entity credit rating and 0 otherwise.	S&P Capital IQ
MISSING_ DOWNGRADE	For firm–years that did not originally have an S&P entity credit rating but were set equal to 0, we follow prior studies (e.g., Byoun 2008, 3078) and simultaneously create a missing value indicator, including this variable alongside the main variable DOWNGRADE in regressions. This variable is therefore coded 1 (0) if a firm–year has a missing (non-missing) S&P entity credit rating.	S&P Capital IQ
AVERAGE_CDS_ SPREADS	A firm's annual average of all daily CDS spreads with 5-year maturity and 0 otherwise.	Markit
MISSING_CDS_ SPREADS	For firm–years that did not originally have CDS spreads data but were set equal to 0, we follow prior studies (e.g., Byoun 2008, 3078) and simultaneously create a missing value indicator, including this variable alongside the main variable AVERAGE_CDS_SPREADS in regressions. This variable is therefore coded 1 (0) if a firm–year has a missing (non-missing) CDS spreads.	Markit
MISSTATEMENT	An indicator set equal to 1 if a borrowing firm misstated its financial statements in a current year (identified through future-period restatements) and 0 otherwise.	Audit Analytics

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