Creditor Control of Corporate Acquisitions

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Abstract

We examine the impact of creditor control rights on corporate acquisitions. Nearly 75% of private credit agreements restrict borrower acquisition decisions. Following a covenant violation, creditors use their bargaining power to tighten these restrictions and limit acquisition activity, particularly deals expected to earn negative announcement returns. Firms that do announce an acquisition while in violation of a covenant earn 1.8% higher stock returns, on average, and do not pursue less risky deals. We conclude that creditors use contractual rights and the loan renegotiation process to limit value-destroying acquisitions driven by managerial agency problems.

Keywords: acquisitions, control rights, corporate governance, covenant violations, creditors
JEL Classification: G21, G31, G32, G34

1. Introduction

Covenants in corporate loan agreements grant lenders the ability to influence a wide range of borrower activities. Standard restrictive covenants give lenders veto power over fairly routine financing and investment decisions, and financial covenants provide the opportunity to renegotiate the entire arrangement following poor borrower performance. These control rights undoubtedly facilitate financing ex-ante by protecting creditors from potential expropriation. Strong creditor control rights, however, may lead to inefficiencies ex-post if differences in cash flow rights create conflicts between shareholder and creditors regarding the optimal course of action. In this paper, we study creditor control of corporate acquisitions to better understand the channels through which lenders impact borrower investment decisions and assess the degree of ex-post inefficiencies.

We begin by providing novel evidence that standard restrictive covenants give lenders the ability to influence borrower acquisition decisions. In a random sample of 2,000 private loan agreements to public U.S. nonfinancial firms between 1997 and 2015, we find that nearly three-quarters of loan contracts restrict borrower acquisition activity to some degree. Ten percent of these contracts fully prohibit future acquisitions without lender approval and an additional 64% prohibit deals that do not meet certain criteria. The severity of these restrictions varies with borrower credit quality and expected renegotiation costs, suggesting that lenders carefully consider their ability to influence borrower acquisition decisions during the life of the loan.

The existence of acquisition restrictions in private loan agreements can be rationalized by the model of Gârleanu and Zwiebel (2009). Since loan contracts are incomplete, there is scope for firms to make deals that transfer value away from creditors. Indeed, Billett, King, and Mauer (2004) show that acquirer bondholders experience negative returns, on average, at the announcement of an acquisition. If borrowers have private information about the size of potential

transfers, Gârleanu and Zwiebel (2009) show that the optimal loan contract includes strict covenants that can be relaxed during renegotiation. Thus, acquisition restrictions create value by (i) allowing lenders to delay the due diligence process until a deal is proposed and (ii) discouraging borrowers from proposing deals unlikely to be acceptable to lenders.[1]

Our key innovation is using detailed data on mergers and acquisitions (M&A) to examine how creditors exercise control and assess the effect on borrowers. If renegotiation is costless and borrowers have sufficient bargaining power, creditor control would not prevent deals with positive net present value (NPV). However, inefficiencies in renegotiations, including the potential for lender hold-up as in Rajan (1992), may discourage borrowers from pursuing positive NPV acquisitions that lenders would deem too risky. We assess the degree of these inefficiencies by studying how creditor control of acquisitions impacts *shareholder* wealth, under the hypothesis that creditor-induced underinvestment would result in fewer risky acquisitions and lower shareholder returns, on average.

Using our sample of loan contracts, we find that borrowers fully prohibited from making acquisitions without lender approval complete fewer deals but earn *higher* announcement returns than unrestricted borrowers. Although this evidence suggests that acquisition restrictions are not associated with underinvestment, this inference is limited in two ways. First, unobserved borrower characteristics are likely correlated with both the strength of contractual restrictions and the opportunities for an acquisition, precluding a causal interpretation. Second, Roberts (2015) shows that the typical loan contract is renegotiated every nine months, so restrictions imposed at loan

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[1] Firms can renegotiate a loan contract to receive permission for a prohibited acquisition. For example, when Symmetry Medical Inc. acquired a portion of DePuy Orthopaedics, they reported in the accompanying 8-K filed on 12/14/2007 that they "entered into a Waiver … lenders (i) consented to the acquisition of assets pursuant to an Asset Purchase Agreement among DePuy Orthopaedics, Inc. and certain subsidiaries of the Company ("Acquisition"), as described above in Item 1.01, and (ii) committed to extend additional senior secured credit in the aggregate amount of $60,000,000 (the "Incremental Term Loan"), and modify the terms of the Credit Agreement accordingly."

origination may differ from those faced at the time of an acquisition. To address these issues, we study the loan renegotiation process for the remainder of the analysis, using financial covenant violations as a source of variation in creditor control.

Financial covenant violations present an ideal laboratory to study the channels through which creditors exercise control because they constitute an event of technical default and force loan parties to negotiate. We conjecture that lenders use bargaining power conveyed by a violation to influence acquisitions by tightening contractual restrictions, reducing credit access, and behind-the-scenes negotiations.[2] Tracking the evolution of these restrictions for a random sample of firms, we find that nearly one-quarter of covenant violators become subject to tighter acquisition restrictions within six months, compared to only 4% of matched non-violators during the same period. For example, Lee Enterprises, Inc., amended its credit agreement after a covenant violation in the third quarter of 2008 to "… modify other covenants, including restricting the Company's ability to make additional investments and acquisitions without the consent of its Lenders."[3] Relative to prior research on covenant violations, this result establishes a clear contractual channel that permits creditors to influence borrowers, even after a violation is resolved.

Additionally, financial covenant violations enable us to confront identification concerns related to the non-random assignment of creditor control rights by implementing the quasi-regression discontinuity design of Roberts and Sufi (2009) and Nini, Smith, and Sufi (2012). This approach identifies creditor influence by exploiting the discontinuous increase in creditor control rights at the point of violation. Using a sample of 7,191 acquisitions combined with covenant violation data for 176,378 firm-quarter observations from 1997 to 2015, we find a significant

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[2] Covenant violations grant creditors the right to immediately call a loan. Prior research shows that creditors use this opportunity to reduce the availability of credit (Roberts and Sufi, 2009) and influence firm policies through negotiations (Ferreira, Ferreira, and Mariano, 2018).

[3] Lee's 2008 10-K: https://www.sec.gov/Archives/edgar/data/58361/000119312508262419/d10k.htm

decrease in acquisition activity after a financial covenant violation. Our estimates suggest that covenant violators are 30% less likely to announce an acquisition than similar non-violators. This evidence corroborates Chava and Roberts (2008) and Nini et al. (2012), who show that firms reduce capital expenditures and cash acquisitions after a violation.[4]

We find that creditors primarily use their control rights to prevent deals expected to destroy firm value. Our estimates imply that the likelihood of announcing a shareholder value-destroying deal falls by almost 38% after a covenant violation. Conversely, we find no evidence that creditors limit shareholder value-increasing deals. This censoring shifts the distribution of realized stock returns to the right. M&A announcements by covenant violators earn, on average, 1.8 percentage point higher three-day announcement returns (CARs) than deals by similar non-violators. We also find that violators do not target less risky firms. Violators are significantly less likely to engage in a diversifying acquisition, no less likely to acquire a firm from a relatively risky industry, and more likely to pursue a private target.[5] This evidence suggests creditors use control rights associated with covenant violations to limit overinvestment rather than prevent efficient but risky deals.

Our results indicate that lenders impose granular restrictions on borrower acquisition decisions, tailor these restrictions during the loan renegotiation process, and limit value-destroying deals after a covenant violation. We note, however, that post-violation changes in borrower behavior need not stem entirely from the tightening of contractual restrictions. For example, the same changes might be observed if creditors simply restrict credit access post-violation and allow managers to maintain discretion within expenditure limits or if creditors remain totally passive and

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[4] Relative to these earlier papers, we use acquisition-level data that allows us to examine the amount and type of investments made by violators as well as assess the shareholder value implications using a standard event study. Incorporating non-cash deals is important to assess whether violating firms are simply switching from cash to other forms of payment for acquisitions.

[5] Our findings contrast with Acharya, Amihud, and Litov (2011), who show that firms located in countries with stronger creditor rights in bankruptcy engage in more value-reducing diversifying acquisition.

the violation serves as a wake-up call for borrowers to improve performance.[6]

To isolate the role of contractual restrictions, we hand-collect loan renegotiation data for a sample of acquisitions completed by covenant violators and a matched sample of non-violators. We find that violators are significantly more likely to amend their loan for reasons related to the acquisition. Accounting for the fact that we only observe renegotiations for completed deals, we infer the need to receive explicit lender permission can explain at least half of the observed difference in acquisition rates between violators and non-violators. Moreover, acquirers that have their loan contract amended to permit a deal earn significantly higher CARs, even if they do not raise new credit at the same time. While creditors can constrain borrowers by reducing credit access, our analysis implies that contractual restrictions are the predominant mechanism driving our results.

These results contrast with financial contracting theory that associates creditor control with a bias towards liquidation (Hart, 1995), which in our setting is analogous to creditors preventing risky but positive NPV acquisitions. Rather, our results suggest that the model of Gârleanu and Zwiebel (2009) is the best lens to view both financial covenants and acquisition restrictions. Contractual restrictions are not an absolute transfer of control to creditors but rather a requirement that borrowers seek permission at the time of an acquisition. Excessive liquidation does not occur because borrowers can compensate lenders for an increase in risk that might occur due to the acquisition; via fees, a higher interest rate, revenues from non-lending business, or maintaining a

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[6] Changes in borrower behavior could also be attributable to time-varying firm conditions correlated with violations if they are not fully accounted for by our empirical specification. In the Internet Appendix, we discuss the strengths and weaknesses of each identification strategy used in the literature to measure the effect of a financial covenant violation and show that our results are similar across these strategies.

reputation as a borrower-friendly lender.[7] As a result, creditor control primarily limits value-destroying acquisitions, which the vast M&A literature attributes to unresolved agency conflicts that enable managers to make acquisitions motivated by private benefits. For example, Becht, Polo, and Rossi (2016) show that mandatory shareholder voting in the United Kingdom deters managers from value-reducing acquisitions, suggesting that agency problems are likely unresolved in the United States where shareholder voting can easily be avoided (Li, Liu, and Wu, 2018). Our evidence suggests that creditor veto power provides similar discipline over self-serving and overconfident managers.

Thus, our paper adds to the literature that uses M&A as a setting to study incentive conflicts among managers and suppliers of finance. Though others examine the mechanisms that equity holders use to improve acquisition outcomes,[8] little is known about the role that creditors play beyond deal financing. We contribute by showing that creditors use control rights and the loan renegotiation process to limit value-destroying deals. In doing so, we add to knowledge showing that financial covenant violations are associated with changes in investment and financial policies (e.g. Chava and Roberts, 2008; Roberts and Sufi, 2009; Falato and Liang, 2016). A unique aspect of our setting is that acquisitions are large, verifiable investments directly contracted on; enabling us to highlight a specific contractual mechanism through which creditors have a lasting influence on borrower behavior. Creditors often impose granular restrictions on specific investments and tighten them after a covenant violation. Moreover, we can measure the effect on borrowers by

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[7] In the context of corporate bankruptcy, Gennaioli and Rossi (2012) present a model where multiple debt classes and floating charge collateral can mitigate creditors' bias towards liquidation. In their model, multiple debt classes reduce the liquidation recovery of creditors with control rights and the floating charge provides controlling creditors with some benefits from reorganization, which can rationalize why firms are rarely liquidated. Although the sources of the tradeoff are different in our context, seniority and reputation can also temper creditors bias towards liquidation.

[8] This literature identifies shareholder rights (Masulis, Wang, and Xie, 2007), institutional monitors (Chen, Harford, and Li, 2007; Fich, Harford, and Tran, 2015), boards of directors (Lin, Officer, and Zou, 2011; Cai and Sevilir, 2012; Schmidt, 2015; Field and Mkrtchyan, 2017), and executive compensation (Datta, Iskandar-Datta, and Raman, 2001; Lin, Officer, and Shen, 2018), among others, as important determinants of acquirer returns.

examining target characteristics and shareholder returns. Our findings suggest creditors primarily use control rights to limit overinvestment rather than inefficiently reduce risk.[9]

2. Creditor control of borrower acquisitions

Creditors can influence borrower acquisition decisions through three mechanisms. First, creditors affect acquisitions as a provider of deal financing. Second, lenders can write contracts that restrict borrower behavior during the life of the loan. Finally, lenders can exert influence through the loan renegotiation process. In this section, we examine and provide empirical support for the second two channels by showing that creditors restrict borrower acquisitions and tighten these restrictions in renegotiations after a financial covenant violation.

2.1 Acquisition restrictions in loan contracts

The standard private credit agreement includes negative covenants that limit certain borrower activities without explicit permission from lenders (Wight, Cooke, and Gray (2009)). Common negative covenants limit a borrower's ability to issue debt, grant liens over assets, pay dividends, and make "fundamental changes, asset sales, and acquisitions." Since these restrictions are not recorded in commonly used databases, such as Dealscan, we hand-collect data for a random sample of 2,000 loan contracts from U.S. nonfinancial firms between 1997 and 2015.[10]

For each contract, we search the set of covenants for evidence that creditors restrict a borrower's acquisition decisions. If we find a restriction, we record whether it prohibits all acquisitions without the consent of lenders ("Full restriction") or imposes any of the following

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[9] Early papers on covenant violations emphasize firm costs in terms of worse loan terms and increased incentive to manipulate accounting performance. More recently, Ertan and Karolyi (2016) show that stock prices are negatively correlated with the estimated likelihood of a covenant violation and conclude that shareholders expect increased creditor control to reduce equity value. Nini et al. (2012) and Ersahin, Irani, and Le (2017) draw the opposite conclusion, however, by examining long-run stock returns and firm performance following a violation.

[10] To construct the sample, we match loans in Dealscan to borrower data in Compustat at quarter-end immediately after the loan start date, using an updated version of the link file provided by Chava and Roberts (2008). We then search for the corresponding loan agreement in EDGAR. We choose a random set of 2,000 contracts to minimize collection costs while providing a large enough sample to draw reliable inferences.

partial restrictions; a prohibition of deals: (i) above a certain size ("Expenditure limit"), (ii) that would cause the borrower to violate their existing covenants on a pro forma basis ("Pro forma compliance"), (iii) that do not meet a non-covenant financial test ("Financial test"), and (iv) outside of the borrower's primary line of business ("Prohibit diversifying").

Our hand-collection reveals that deal restrictions are not boilerplate and can substantially restrict borrower decisions. For example, Section 7.1 of Merisel, Inc.'s August 2010 credit agreement imposes a full restriction, stating that "No Borrower shall … enter into any merger, consolidation or other reorganization with or into any other Person or acquire all or a substantial portion of the assets or Equity Interests of any Person."[11] Alternatively, Flowers Foods, Inc., March 2001 credit agreement permits acquisitions but imposes each of the four partial restrictions: the total consideration must be less than $20 million (1.7% of their total assets), the combined entity must be in compliance with financial covenants on a pro forma basis, their existing revolver must have at least $40 million after the deal, and the target must be in a similar line of business.[12]

Panel A in Table 1 summarizes these data and shows that acquisition restrictions are widespread. We find 10% of private credit agreements fully restrict borrower acquisition activity and a further 64% impose a partial restriction. In addition to being pervasive, these restrictions vary with borrower characteristics, suggesting that restrictions are carefully considered in drafting a loan contract. Smaller and riskier borrowers are more likely to face a full acquisition restriction, but even the largest and safest firms in our sample face at least a partial restriction.

We interpret the existence and strength of acquisition restrictions using the model of Gârleanu and Zwiebel (2009), which is based on borrowers having an incentive to undertake actions that transfers value away from creditors. These actions cannot be priced in the original loan

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[11] Merisel: https://www.sec.gov/Archives/edgar/data/724941/000143774910002697/ex10-20.htm
[12] Flowers Foods: https://www.sec.gov/Archives/edgar/data/1128928/000095014401500428/g67886ex10-5.txt

because borrowers have private information about the transfer size. Covenants allow borrowers to commit to not undertake such actions, removing the need to price the actions ex-ante. If a borrower wants to undertake an action during the loan, renegotiations can occur. At the time of renegotiation, the lender becomes informed about the size of the transfer, so efficient decisions are made.

The model predicts that covenants should be more restrictive when the scope for wealth transfers is larger and when the cost of renegotiation is lower. We find that creditors frequently impose expenditure limits and restrict deals that would trigger a deterioration in observable financial metrics, reflecting lenders desire to prevent large wealth transfers. In addition, the most common partial restriction is a prohibition on diversifying acquisitions, which supports the presumption that diversifying deals are primarily a manifestation of managerial agency problems and frequently generate losses (Baumol, 1959; Jensen 1986).

In Panel B of Table 1, we display a series of regressions to explore the determinants of a full prohibition on acquisitions without lender consent. The evidence suggests that borrower size, risk, and information environment are related to the likelihood of a full restriction. In line with the predictions of Gârleanu and Zwiebel (2009), full restrictions are more likely when there is higher potential for debtholder losses. Full restrictions are most prevalent for risky borrowers, measured by profitability and leverage, as well as for smaller and unrated borrowers, which may capture risk or the availability of information about potential expropriation. Borrowers with greater prior M&A activity are less likely to be fully restricted, since the expected costs of renegotiation are higher for these acquisitive borrowers. We also document that full restrictions are significantly more likely for firms that recently violated a financial covenant, a relationship we explore further below.

After controlling for borrower characteristics, we find little evidence that acquisition restrictions vary with the number of lenders, as might be expected if larger syndicates are

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constructed to allow creditors to commit to not renegotiate, as in Bolton and Scharfstein (1996). Nor are restrictions correlated with the presence of collateral, which Rajan and Winton (1995) suggest can provide incentive to gather information about borrowers.

Table 2 provides a comparison of acquisition activity and average announcement returns for firms with a full, partial, and no restriction. We examine the period between origination and maturity of the loan to create an indicator for whether a firm conducts at least one acquisition during this horizon. For each completed deal, we estimate 3-day shareholder announcement CARs. We find that firms with a full restriction are significantly less likely to announce an acquisition but do not interpret this difference as causal because lenders likely design acquisition restrictions based on borrowers' expected opportunity set. For example, firms that make the most acquisitions tend to have a partial restriction, which reflects the tradeoff between protecting lenders and limiting excessive renegotiation. The second row of Table 2 compares acquirer CARs. Although firms with a full restriction undertake the fewest deals, their deal announcements generate the largest shareholder returns. Under the assumption that lenders would not fully restrict firms expected to make better acquisitions, we interpret these higher returns as suggestive evidence that contractual restrictions discipline acquisition decisions and improve realized outcomes.

The evidence in Table 2 shows that lenders set acquisition restrictions as a function of borrower characteristics and sometimes permit deals that were restricted ex-ante, particularly those with higher CARs. There is no evidence that creditors have a bias toward liquidation, as might be expected due to their senior, and often secured, claim (Hart, 1995). Instead, the results support the presumption that a unique aspect of bank debt is the ability to collect information about borrowers and make efficient renegotiation decisions, as in the model of Bolton and Freixas (2000).

2.2 Changes in acquisition restrictions after financial covenant violations

To better isolate the role of creditor control, our research design examines the renegotiation process that accompanies a financial covenant violation. Our underlying hypothesis is that covenant violations discontinuously increase creditor control of borrower acquisition decisions. As a first step to ensure the validity of our empirical design, we test this hypothesis by examining whether creditors tighten acquisition restrictions after a violation.

We begin with a sample of all quarterly observations from U.S. nonfinancial firms in Compustat from 1997 to 2015. We match firms that report a financial covenant violation to the nearest non-violator in the same quarter via a one-to-one propensity match (with replacement) on a set of observable firm characteristics.[13] To cleanly identify contractual changes attributable to a new violation, we eliminate firms that violated a covenant in any of the four quarters prior to the match. We select a random set of 200 observations where both firms have a private credit agreement in EDGAR.[14] Finally, we read the most recent credit agreement prior to the quarter of violation, or pseudo-violation for non-violators, and search for amendments to the original contract or a new contract filed during the six months post-violation or pseudo-violation. This process allows us to measure contractual changes that occur during renegotiations of the original loan.

Shiloh Industries, Inc. represents an example of changes around a violation. In their July 2009 10-Q, Shiloh reported that "the Company is not in compliance with certain of the financial covenants of its Credit Agreement …" As part of a June 30th amendment waiving the violation, it agreed to modify its existing acquisition restrictions, which previously prohibited diversifying deals and required pro forma covenant compliance. The modification resulted in a full restriction of acquisitions, dictating that "… after the Third Amendment Effective Date, no Company shall

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[13] We extend the Nini et al. (2012) covenant violation dataset through 2015 and match on the full set of controls in Table 5 Column (2). We describe our empirical specification in more detail below.

[14] We select a random sample of 200 firms to minimize the cost of reading SEC filings to accurately identify acquisition restrictions and renegotiations. For comparison, Roberts (2015) studies loan renegotiations for 114 firms.

effect an Acquisition without the prior written consent of Agent and the Required Lenders."[15]

Table 3 shows that Shiloh's experience is common. In our sample of violators and matched non-violators, roughly 30% of the original credit agreements fully restrict borrowers from making an acquisition without lender consent and an additional 55% impose one or more partial restrictions. We note that acquisition restrictions in this sample are significantly more stringent than in the sample reported in Table 1 because covenant violators tend to be smaller and of worse credit quality. However, the frequency of ex-ante acquisition restrictions is similar across the set of violators and non-violators in Table 3, supporting the validity of our matching procedure.

The middle panel displays the frequency with which each provision is added after a violation or pseudo-violation. In our sample, creditors add a full restriction for 10% of violating firms, which is significantly higher than the 2% of matched non-violators. Partial restrictions do not appear to increase, on average, because some violators change from no restriction to partial while others replace partial with full restriction. Thus, we calculate the frequency of "tightening" as the fraction of loans that (i) add a full restriction, (ii) reduce the expenditure limit on permitted acquisitions, or (iii) increase the number of partial restrictions. Based on this, the bottom row reports that over 24% of contracts tighten within six months of violation versus only 4% of non-violators. The 20% difference is economically significant and implies that covenant violations lead to a sharp increase in creditor control over corporate acquisitions. Although we believe that creditors can influence acquisitions as a provider of financing and through behind-the-scenes negotiations, the evidence in Table 3 suggests that contractual restrictions serve as a powerful tool to control borrower acquisition decisions.

3. Empirical design

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[15] Shiloh Industries: https://www.sec.gov/Archives/edgar/data/904979/000119312510127756/d10q.htm

One approach to assessing the impact of creditor rights would be to directly test how M&A outcomes vary with contractual restrictions. Inferences from this analysis, however, would be limited for two reasons. First, Roberts (2015) shows that the typical bank loan is renegotiated five times, so acquisition restrictions may change over the life of the loan. Second, creditor effects would be difficult to identify empirically due to non-random assignment of acquisition restrictions. We address these challenges by using financial covenant violations as an indicator of heightened creditor control. Intuitively, our approach can be characterized as using violations as an instrument for creditor control rights. Since we cannot measure control rights directly, we conduct only reduced-form analysis. The evidence discussed in the previous section reveals a strong "first-stage" relationship, validating the relevance of covenant violations in our empirical design.

We use firms that are not in violation of a financial covenant as a comparison group to estimate the counterfactual outcome for violators. Covenant violations, of course, are not randomly assigned. By construction, violations of financial covenants occur when performance declines and accounting ratios breach contractually stated thresholds. Hence, the design of covenants poses a challenge for researchers wishing to identify the effects of creditor control. Our primary concern is that outcomes may be affected by firm characteristics correlated with violations and would occur absent creditor intervention. Omitted variable bias may emerge if violators and non-violators differ along unobserved dimensions associated with deal outcomes. Through our analyses, we highlight how these factors could affect inferences and take steps to address this identification challenge.

First, we visually explore the timing of the effect of a covenant violation. If constant unobserved firm characteristics explain our results, we would expect similar effects among the quarters around a covenant violation. Conversely, if creditor control drives our results, we would expect to see stronger results soon after a violation rather than when firms are further pre- or post-

violation. Evidence that creditor influence wanes as time elapses post-violation would further suggest a causal interpretation and validate our measure of creditor control.

Second, we estimate standard acquisition regressions to account for observable differences between violators and non-violators. Following Moeller, Schlingemann, and Stulz (2004) and Masulis, Wang, and Xie (2007), we control for size, stock price runup, leverage, market-to-book ratio, and operating cash flow. We refer to these variables as *AcquirerControls* through our analysis. We also include controls for relative deal size, completion status, toeholds, diversifying deals, method of payment, target listing status, cross-border deals, hostile deals, and tender offers. We do not include these variables (*DealControls*) in all specifications, however, because we believe that they are best thought of as outcome variables rather than controls.[16]

Third, we follow Roberts and Sufi (2009) and Nini et al. (2012) and implement a quasi-regression discontinuity design (quasi-RDD) to confront identification concerns related to the non-random assignment of violations. The specification exploits the discontinuity at the point of violation by flexibly controlling for continuous functions of the variables on which covenants are written. We refer to this strategy as a "quasi-discontinuity design" because we do not observe the contractual level of each individual covenant and thus cannot precisely compare firms just above/below the covenant threshold. Instead, the specification identifies the effect of a violation by comparing outcomes for violators and non-violators with similar deterioration in performance.

The quasi-RDD specification controls for lagged and higher-order functions of the following variables (*CovenantControls)*: operating cash flow to assets, leverage ratio, interest expense to assets, net worth to assets, current ratio, and market-to-book. We include linear, quadratic, and cubic covenant variables to flexibly control for independent relations between the

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[16] Angrist and Pischke (2009) advise that regressions should not include controls that are themselves affected by the variable of interest. Nevertheless, we include deal controls in some specifications to be consistent with prior literature.

variables and acquisition decisions. We include one-year lags to control for firm conditions when

loan contracts were negotiated and proxy for the unobserved covenant thresholds. These variables

produce expected outcomes following patterns of poor performance and mimic a standard

regression discontinuity design if covenants are written at similar levels for similar firms.

Our broadest empirical specification is

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\begin{aligned}
y_{i,t} = {} & \beta \cdot Violation_{i,t} + \theta_1 \cdot AcquirerControls_{i,t-1} + \theta_2 \cdot CovenantControls_{i,t-1} \\
& + \theta_3 \cdot HigherOrderCovenantControls_{i,t-1} + \theta_4 \cdot CovenantControls_{i,t-5} \\
& + \theta_5 \cdot DealControls_{i,t} + Industry_i + Year_t + \varepsilon_{i,t}
\end{aligned}
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where $y_{i,t}$ is the outcome of interest for firm i in quarter t, $Violation_{i,t}$ is an indicator that equals

one if firm i reported a financial covenant violation during either of the two quarters prior to

quarter t, $Industry_i$ represents industry fixed effects based on Fama-French (1997) 48 industry

classifications, and $Year_t$ is a fixed effect representing the year of quarter t. In all specifications,

we follow standard practice and cluster standard errors by firm to account for potential serial

correlation in residuals (e.g., Masulis et al., 2007; Field and Mkrtchyan, 2017).

4. Data

4.1 Sample construction

We begin our analysis with the universe of U.S. nonfinancial firm-quarter observations in

Compustat from 1997 to 2015. The sample starts in 1997 because the Securities and Exchange

Commission (SEC) did not require electronic filing for all registered firms until the second quarter

of 1996 and we require two quarters of lagged data for our analyses. Following the methodology

of Nini et al. (2012), we employ a text-search algorithm to identify every occurrence of a financial

covenant violation in the universe of 10-K/10-Q filings on EDGAR and manually inspect the

paragraphs around each potential violation to remove false positives.[17] The resulting dataset indicates financial covenant violation status for each firm-quarter through 2015.

Following Nini et al. (2012), we filter the following observations to facilitate a match with SEC filings: firms with average assets less than $10 million (in real 2000 dollars) and firm-quarter observations with missing total assets, total sales, common shares outstanding, closing share price, or calendar quarter information. We also merge each observation with stock price information from the Center for Research in Security Pricing (CRSP) and require that each observation has one year of stock prices, which we use to compute runup prior to acquisitions. Finally, we require that each observation has non-missing values of the *CovenantControls* for the current and prior four quarters. These criteria yield a sample of 176,378 firm-quarter observations from 7,164 U.S. nonfinancial firms from 1997 to 2015. The appendix lists variable definitions. To mitigate the effect of outliers, we winsorize unbounded variables at the 1% and 99% levels in all analyses.

We draw our M&A sample from the Securities Data Company (SDC) Platinum Mergers and Acquisitions database. Following prior research (e.g. Moeller et al., 2004; Masulis et al., 2007), we filter spinoffs, recapitalizations, exchange offers, repurchases, self-tenders, privatizations, transactions valued at less than $1 million or 1% of the acquirer's market value eleven days prior to the announcement, deals where the acquirer controlled more than 50% of the target prior to the announcement or sought less than 100% after completion, and deals that do not involve a public, private, or subsidiary target. These standard filters ensure that deals are large enough to have a material effect on shareholders and creditors. We finalize our M&A sample by dropping transactions with missing 3-day acquirer CARs, method of payment, or target

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[17] Covenant violations must be disclosed in quarterly financial statements in accordance with Regulation S-X. See the appendix to Nini et al. (2012) for more details on the text-search algorithm and manual coding. We extend the Nini et al. (2012) covenant violation dataset to include years 2009 to 2015 and will make it publicly available with this paper.

characteristics. This process yields a sample of 7,191 deals announced by 2,907 U.S. nonfinancial firms from fiscal years 1997 to 2015. We merge these deals into our firm-quarter sample using cusip, ticker, and company name recorded in the CRSP historical stock names file.

4.2 Identifying covenant violations

Chava and Roberts (2008) note that firms generally file compliance reports with creditors on a quarterly basis to coincide with SEC reporting requirements. In practice, we observe whether firms report a covenant violation in each SEC filing that corresponds to a particular quarter-end, but we do not observe exactly when firms breach covenants or negotiate waivers. In an M&A setting, this limitation means that it is impossible to know precisely whether an acquisition occurred before or after control rights were transferred within a given quarter. We address this issue by classifying an observation as "in violation" if the firm reported a financial covenant violation in an SEC filing for either of the prior two fiscal quarters.

An advantage of this approach is that it precludes a reverse causality problem in which an acquisition leads to a covenant violation in the same quarter. Our trailing indicator, however, does not fully abate measurement error common to studies of covenant violations. There may be borrowers that quickly cure a violation and avoid creditor influence over subsequent deals. In other instances, creditors might maintain approval rights over acquisition decisions for a period beyond two quarters. In either case, the measurement of changes in creditor control is imperfect, which creates classical errors-in-variables and biases our analysis against producing significant results. We choose a two-quarter trailing indicator because violations transfer control rights immediately to creditors, so changes in firm behavior should be observed soon after violation. This window also corresponds with the six-month average bidding process in Boone and Mulherin (2007). Thus, our analyses test if creditors intervene in potential acquisitions on the near-term horizon.

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4.3 Sample characteristics

Panel A of Table 4 reports descriptive statistics for our M&A sample. Acquirers tend to be large, profitable firms. The average acquirer has a $5.1 billion market capitalization and a market-to-book ratio of about 2.0. Our sample acquirers have a mean operating cash flow to assets ratio of 0.12 and a mean leverage ratio of 0.26. We estimate market model CARs using CRSP equal-weighted index returns and a one-year estimation window (252 trading days) ending one month (20 trading days) prior to the three-day [-1, +1] event window centered on announcement. The mean acquirer 3-day CAR in our sample is 1.13%. Overall, our descriptive statistics resemble prior M&A studies, particularly those that parallel our sample selection process.[18]

4.4 Comparison of firms by violation status

It is important to first understand which firms violate their financial covenants before we attempt to identify the effect of creditor control rights on acquisition outcomes. Nini et al. (2012) show that covenant violations are common across firms and stress that violations appear to indicate a downward *change* in performance, rather than a low absolute *level* of performance. Panel B of Table 4 shows that this conclusion hold for our sample of acquirers. Violators are smaller and experience weaker performance than non-violators, though the typical violator is far from insolvent. The median violating acquirer breaches a covenant despite maintaining positive operating cash flow and a market-to-book ratio of 1.3. This valuation is nearly twice as high as the 0.75 median market-to-book ratio that Campbell, Hilscher, and Szilagyi (2008) report for their sample of distressed firms. Violators also do not appear to be extremely levered or suffer from serious liquidity shortfalls. The average violator in our M&A sample has a leverage ratio of 0.32,

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[18] For example, Masulis et al. (2007) report that the average acquirer has a $5.6 billion market capitalization and 1.98 market-to-book ratio in their sample from 1990 to 2003. Moeller et al. (2004) detail mean operating ROA of 0.13, leverage of 0.31, and CARs of 1.10% for their sample of deals spanning 1980 to 2001. John, Knyazeva, and Knyazeva (2015) calculate a relative deal size of 24% and find that 91% of bids are completed in their 1985-2009 sample.

a current ratio of 2.03, and a cash to assets ratio of 0.11. These statistics alleviate concerns that financial position alone may determine acquisition policy for violating firms, but also point to the importance of controlling for differences between violators and non-violators.

5. Covenant violations and acquisition behavior

In this section, we assess the ex-post efficiency of creditor control by examining how covenant violations affect borrower acquisitions. Billett et al. (2004) show that bondholders experience negative wealth effects around M&A, on average, creating the incentive for lenders to oppose some deals. If renegotiation is efficient, the Coase (1960) theorem implies that creditor control would only prevent negative NPV deals.[19] However, creditors' concave payoff function and inefficiencies in the renegotiation process may discourage borrowers from pursuing positive NPV acquisitions that lenders deem too risky. We, therefore, assess the impact of creditor control by examining how covenant violations impact the risk and value of acquisitions undertaken.

5.1 Acquisition activity

Figure 1 reveals that firms are half as likely to enter into an acquisition while in violation of a financial covenant. As noted, however, this relation may be driven by differences between violators and non-violators rather than a creditor effect. For example, small firms are more likely to violate a covenant and less likely to acquire. If constant unobserved firm characteristics drive our results, we should see a similar effect for firms immediately pre-violation as size and other factors do not vary over short horizons. The timing of the effect in Figure 1 refutes this alternative explanation, as the effect is strongest when firms are in violation and wanes as time elapses.

It could still be the case that time-varying firm conditions explain the patterns in Figure 1. Thus, we estimate ordinary least squares regressions to control for known factors that influence

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[19] Borrowers are able to compensate lenders through fees or changes in loan terms, including the interest rate, maturity, amount, and collateral.

acquisition decisions (Columns (1) and (2) of Table 5). After controlling for observables, the effect of a covenant violation remains large and statistically significant. The estimate in Column (2) implies that firms in violation are 1.2% less likely to announce an acquisition compared to similar non-violators, which represents a 30% drop relative to the unconditional likelihood.

5.2 Acquirer announcement returns

The post-violation decline in acquisitions could reflect underinvestment in deals that shareholders would approve, benefitting creditors at the expense of shareholders. Conversely, the decline could represent a reduction in overinvestment, which would benefit both creditors and shareholders.[20] We examine which deals creditors oppose by testing the probability a borrower announces a *shareholder* value-destroying or value-enhancing deal. We classify acquisitions as value-destroying (enhancing) if an acquirer earns a 3-day CAR more than one standard deviation below (above) our sample mean CAR.[21] This approach is similar to Paul (2007) and Chen, Harford, and Li (2007), who test if directors and institutional monitors limit value-destroying deals. We differ from their approach by analyzing the unconditional likelihood of announcing a value-destroying deal, rather than conditional on announcing a deal, because we hypothesize that covenant violations enable creditors to prevent some acquisitions from ever being announced.

Columns (3)–(5) of Table 5 report the results. The likelihood of announcing a shareholder value-destroying or value-neutral deal falls by over 35% of the sample mean when firms are in violation of a covenant. Conversely, we find no evidence that creditors limit acquisitions that are expected to create shareholder value. In unreported analyses, we find that our results are robust

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[20] We assume that creditors benefit in either case because they would not use their control rights in a manner that harms their interests.

[21] We use the acquiring firm's stock returns during the event study estimation window to estimate the standard deviation of stock returns. This produces a firm-specific standard deviation that accounts for differences in idiosyncratic volatility that would otherwise affect the probability of extreme outcomes.

to classifying value-destroying (value-enhancing) deals as those with CARs in the bottom (top) quartile of the empirical CAR distribution. These results suggest that creditors provide valuable corporate governance by censoring acquisitions with low expected synergies.

We further quantify the effect of creditor control rights by analyzing 3-day CARs sorted by acquirer violation status. Figure 2 shows that mean and median acquirer CARs are 1.5% to 2% higher for firms in violation of a financial covenant. These plots encourage a causal interpretation by highlighting the timing of the effect. Creditor control is associated with higher stock price reactions for firms in violation, but this effect is not present for firms pre-violation.

Although a comparison of unconditional returns is informative, violators and non-violators differ in ways that are known to affect announcement returns. Therefore, we follow Masulis et al. (2007) and estimate regression models that control for these differences. Table 6 shows that effect of a covenant violation on acquirer returns remains large and statistically significant after controlling for confounding factors.

Despite the inclusion of standard acquirer controls in Column (1), it is possible that omitted variables correlated with violation status influence our coefficients. In particular, stock price runup is a noisy proxy and may not fully capture recent performance deterioration or deal anticipation. To address this concern, we implement the quasi-RDD of Roberts and Sufi (2009) and Nini et al. (2012).[22] Column (2) reports that acquirers in violation of a covenant earn 1.76% higher announcement returns than similar non-violators. Notably, our coefficient of interest

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[22] We also check SDC for rumors prior to deal announcement to address the possibility that acquisitions made by violators may be more of a surprise to the market. We find that the likelihood of a rumor does not statistically differ between violators and non-violators, and observe no evidence that suggests anticipation drives our results.

remains large and significant as we impose more stringent specifications. This stability suggests that inferences from our event study results are unlikely to be biased by omitted variables.[23]

In Columns (3) and (4), we present regressions that include frequently studied M&A deal characteristics. As these variables may themselves be influenced by covenant violations, the coefficient estimates should be interpreted with caution. Nevertheless, these models confirm that the relation between violations and acquirer CARs persists with or without deal controls. Their coefficients also align with previous studies. For example, evidence of higher acquirer CARs for private/subsidiary targets is consistent with Fuller, Netter, and Stegemoller (2002) and the negative coefficient for all-stock deals supports the view that the adverse selection problem in equity issuance leads to lower announcement returns (Travlos 1987). Together with the assumption that creditors do not permit deals that lower the value of their stake, our results show that firms in violation of a covenant make superior acquisitions decisions that increase firm value.

5.3 Target characteristics

As an alternative test whether the decline in acquisition activity reflects inefficient underinvestment, we examine characteristics of target firms. Harford, Humphery-Jenner, and Powell (2012) show that entrenched managers destroy value by disproportionately engaging in diversifying acquisitions and acquiring public targets. If creditors use their control rights to prevent managerial empire building, we expect covenant violations to reverse these trends. Conversely, if creditors use control rights primarily to reduce the firm's risk profile, we expect covenant violations to lead to fewer risky deals.

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[23] In the Internet Appendix, we show that our results are similar across alternative identification strategies used in the literature to measure the effect of a financial covenant violation. We also show that our results are robust to using a one-year trailing covenant violation indicator and alternative CAR calculations.

Columns (1) and (2) of Panel A in Table 7 show that acquirers in violation of a financial covenant are nearly 5% less likely to target a firm outside of their primary Fama-French 12 industry. The economic magnitude of this effect appears important, given that the unconditional probability of a diversifying deal is 28% in our sample. Together with contractual evidence that creditors explicitly prohibit diversifying acquisitions, our results suggest that creditors prefer managers to focus on their core competencies rather than grow their empire via diversification.[24]

Since our sample consists largely of private targets, we cannot use market prices to assess the volatility of acquisition targets. Instead, we proxy for target risk with an indicator that asset volatility is higher in the target's industry than the acquirer's, based on the argument from Billet et al. (2004) that the impact on creditors is worse if a firm announces the acquisition of a target with a higher asset volatility. We find no evidence that creditors prevent borrowers from engaging in such risky acquisitions (Columns (3) and (4)).

Finally, we examine whether creditors oppose acquisitions of private targets under the hypothesis that private targets are riskier due to greater opacity (Moeller, Schlingemann, and Stulz (2007)). Here again, we find that acquirers in violation of a covenant do not shy away from risky deals. Estimates in Column (5) and (6) suggest that violators are at least 6.5% more likely acquire a private firm than similar non-violators, representing a 15% increase relative to the unconditional mean.

Panel B of Table 7 shows that acquirers in violation of a covenant earn significantly higher mean and median CARs for all deal types, except those involving all stock payment or public

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[24] Prior research offers ambiguous predictions regarding the predicted effect of diversifying acquisitions on debt value. For example, Lewellen (1971) and Galai and Masulis (1976) argue that diversifying deals benefit creditors by reducing default risk if underlying assets are not perfectly correlated. However, Baumol (1959) and Jensen (1986) argue that diversifying deals are a common form of managerial empire building and are more likely to generate losses.

targets.[25] Nevertheless, Table 6 shows that the observable characteristics we examine do not fully explain the higher acquirer CARs earned by violators relative to non-violators. These findings suggest that creditors do not enforce blanket restrictions but rather limit overinvestment when they have the ability and incentive to do so.

5.4 Acquirer balance sheet changes

One additional source of disagreement between creditors and shareholders relates to the strength of the target's balance sheet. Compared to equity holders, creditors may prefer targets that have relatively low leverage, high cash holdings, and tangible assets that can serve as collateral. Again, since only a small number of violators acquire public targets, we have insufficient data to analyze this directly. As an alternative, we examine changes in acquirer balance sheets after the acquisition. If it is the case that creditors discourage the acquisition of highly leveraged or cash poor targets, we would expect to see greater improvement in violators' balance sheets relative to non-violators after a completed acquisition.

To test this conjecture, we construct a propensity matched sample of violators and non-violators. We construct the sample by matching acquirers in violation of a financial covenant to the nearest non-violating acquirer via a one-to-one propensity match (with replacement) on the full set of controls in our main specification (Table 5, Column 2). Table 8 reports summary statistics for the matched sample around completed deals. We track balance sheet changes for three years to allow time for the acquirer to fully integrate the target. Panel A shows that the matched sample exhibits no statistically significant differences in four key balance sheet variables

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[25] In follow-on research, Daher and Ismail (2018) show that acquirers with a loan covenant in Dealscan earn higher CARs for public targets. However, this result is difficult to interpret because only 43% of acquirers in their sample have any covenants. Drucker and Puri (2009) note that many loans appear to have no covenants in Dealscan because the data provider often does not record the information even though the loans do have covenants. Indeed, Roberts and Sufi (2009) read actual loan contracts and show that 95% of loans contain at least one financial covenant. In our sample, 390 of 1,644 deals involving public targets are paid entirely with stock. Violators earn significantly higher mean and median CARs for public targets if we exclude these deals.

pre-acquisition: tangible assets, cash holdings, current ratio, and leverage. Further, Panel B shows that changes in these variables around an acquisition do not differ significantly between violators and non-violators. These results suggest violators do not disproportionately avoid risky targets with weak balance sheets or disproportionately acquire "cash cows" to lower firm risk.

5.5 Heterogeneity with respect to competition and governance

The preceding results show that creditors use heightened control rights from covenant violations to prevent value-destroying acquisitions. These results suggest that similar non-violators make acquisitions that are worse, on average, and imply that existing disciplinary mechanisms do not ensure that managers always maximize shareholder value. Since overinvestment may be particularly costly for creditors, Chava, Kumar, and Warga (2010) argue that managerial agency risk is an important determinant of creditor restrictions. Thus, if creditors primarily use control rights to limit overinvestment, we expect creditor effects to be concentrated among firms that lack strong mechanisms to discipline managers.

We investigate this hypothesis in the Internet Appendix by segmenting the sample based on prior research that shows managerial agency costs are prevalent among firms with low product market competition (Hart, 1983), low analyst coverage (Chen, Harford, and Lin, 2015), no institutional blockholder (Shleifer and Vishny, 1986), a busy board (Fich and Shivdasani, 2006), and weak shareholder rights (Harford, Humphrey-Jenner, and Powell, 2012). Consistent with our hypothesis, Table A.1 shows that the decline in value-destroying acquisitions and increase in acquirer returns is concentrated among firms with low competition and weak governance. The difference between subsamples, however, is not significant at conventional levels in most specifications, which can be explained by the noise in our proxies and the possibility that managerial agency problems are not perfectly resolved in our "strong" governance subsamples.

6. The economic mechanism

In this section, we explore the mechanism through which creditors influence acquisition decisions, particularly the role of contractual restrictions. Since a borrower would need to renegotiate their loan contract to permit a restricted acquisition, we search for evidence of such lender consent in the matched sample of acquiring violators and non-violators. We read all loan documents filed with the SEC within one year prior to the acquisition's effective date and classify a document as an acquisition-induced renegotiation if it explicitly mentions the acquisition target. We further record whether these renegotiations provide new credit to the borrower.

For cases in which an acquirer did not receive new credit, we classify the deal as involving a "Renegotiation with no new credit." As an example of such a renegotiation, Clarient, Inc. amended its existing credit agreement on 12/21/2009, the same day it announced the acquisition of Applied Genomics. In the accompanying 8-K filing, Clarient disclosed the "Fourth Amendment and joinder to Credit Agreement" that included the statement "Borrowers have informed Lender that on the date hereof Clarient has entered into the AG Acquisition Documents, pursuant to which Clarient has consummated the AG Acquisition. Borrowers have requested, pursuant to Section 7.01 of the Agreement, that Lender consent to the AG Acquisition and Lender has agreed to provide such consent subject to the terms and conditions hereof." Since the amendment did not include new credit, we consider this a renegotiation with no new credit triggered by the acquisition.

Panel A of Table 9 shows that such renegotiations are common before acquisitions, particularly for firms that recently violated a financial covenant. In our sample, 22.8% of violators and 10.4% of non-violators completed a renegotiation with no new credit before an acquisition, a large and statistically significant difference. Panel A also shows that the frequency of receiving new credit is comparable across violators and non-violators, suggesting that lender monitoring

through new credit is unlikely to explain the observed differences in announcement CARs. However, reduced credit access may still contribute to the decline in acquisition frequency after a violation, as creditors may use the violation as an opportunity to reduce a line of credit.

The evidence in Panel A can be used to infer the extent to which contractual restrictions and subsequent renegotiations can explain the observed decline in acquisition activity after a financial covenant violation. Since we only observe renegotiations for acquisitions that occur, the conditional probability of a renegotiation is given by Bayes' theorem:

$$\Pr(reneg|allowed) = \frac{\Pr(allowed|reneg) \cdot \Pr(reneg)}{\Pr(allowed)}$$

$$= \frac{\Pr(allowed|reneg) \cdot \Pr(reneg)}{\Pr(allowed|reneg) \cdot \Pr(reneg) + 1 - \Pr(reneg)} \tag{1}$$

where *allowed* indicates that an acquisition is permitted, either because it is not restricted by an existing contract or because it is restricted but the lender consented during a renegotiation. The denominator in (1) is the unconditional probability that an acquisition is allowed, assuming the acquisition is allowed with a probability of 1 if a renegotiation is not required. Since $\Pr(allowed|reneg) < \Pr(allowed)$, the conditional probability of observing a renegotiation is biased downward relative to the unconditional probability of a renegotiation, $\Pr(reneg)$.[26]

We next use Equation (1), along with evidence in Tables 3 and 9, to quantify the conditional probability that lenders allow an acquisition, $\Pr(allowed|reneg)$. In the sample of 200 matched violators and non-violators examined in Table 3, roughly 85% of non-violators face at least a partial acquisition restriction. If we assume that these restrictions bind for all acquisitions, then $\Pr^N(reneg) = 0.85$, where the superscript denotes non-violators. Using $\Pr^N(reneg|allowed) = 0.299$ from Table 9, we can solve (1) for $\Pr(allowed|reneg)$ and find

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[26] As long as $\Pr(allowed|reneg) < 1$ and $\Pr(reneg) > 0$, then $\Pr(allowed|reneg) < \Pr(allowed)$.

that lenders rarely allow restricted acquisitions, $\Pr(allowed|reneg) = 0.075$.[27] If we assume the probability that lenders consent to an acquisition is the same for violators, we can use Equation (1) and the estimate $\Pr^V(reneg|allowed) = 0.463$ from Table 9 to solve for $\Pr^V(reneg)$ as 0.92.[28] Table 3 shows that roughly 90% of violators face at least a partial acquisition restriction, consistent with the assumption that partial restrictions always bind.[29]

Alternatively, if we assume that partial restrictions bind with 50% probability for non-violators, then $\Pr^N(reneg) = 0.575$ since 30% of non-violators face a full restriction and 55% face a partial restriction. Repeating the above exercise yields $\Pr(allowed|reneg) = 0.315$, a considerably higher probability of lender consent. For violators, this implies that $\Pr^V(reneg) = 0.732$, which is slightly higher than what would be expected if partial restrictions bind with 50% probability since 40% of violators face a full restriction and 50% face a partial one. Hence, this estimate suggests that partial restrictions are more likely to bind for violators than non-violators, which is reasonable given evidence in Table 3 that shows violators face tighter partial restrictions.

Finally, we use these estimates to assess the impact on completed acquisitions for violators relative to non-violators:

$$\frac{\Pr^V(allowed)}{\Pr^N(allowed)} = \frac{\Pr(allowed|reneg)\cdot\Pr^V(reneg)+1-\Pr^V(reneg)}{\Pr(allowed|reneg)\cdot\Pr^N(reneg)+1-\Pr^N(reneg)} \tag{2}$$

Assuming that partial restrictions always bind, $\Pr(allowed|reneg) = 0.075$, $\Pr^N(reneg) = 0.85$, and $\Pr^V(reneg) = 0.92$, so Equation (2) implies that violators should only complete about 70% as many acquisitions as non-violators, which is about exactly the reduction estimated in

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[27] Rearranging (1) yields $\Pr(allowed|reneg) = \frac{\Pr(reneg|allowed)[1-\cdot\Pr(reneg)]}{[1-\Pr(reneg|allowed)]\cdot\Pr(reneg)}$.

[28] Rearranging (1) yields $\Pr(reneg) = \frac{\Pr(reneg|allowed)}{\Pr(allowed|reneg)+\Pr(reneg|allowed)+\Pr(allowed|reneg)\cdot\Pr(reneg|allowed)}$.

[29] The consistency means we can calibrate $\Pr(allowed|reneg)$ using either data from violators or non-violators.

Table 5.[30] Under the extreme assumption that all partial restrictions fully bind, the prospect of renegotiation can explain the entire drop in realized acquisitions. Conversely, if we assume that partial restrictions bind with 50% probability for non-violators, $\Pr(allowed|reneg) = 0.315$, $\Pr^N(reneg) = 0.575$, and $\Pr^V(reneg) = 0.732$, so Equation (2) implies that violators should only complete about 82% as many acquisitions as non-violators, which is about 60% of the reduction estimated in Table 5. Thus, under the more conservative assumption that partial restrictions bind for 50% of potential acquisitions contemplated by non-violators, the prospect of renegotiation still explains more than half of the observed drop in acquisitions after a financial covenant violation.

In Panel B of Table 9 we examine how announcement CARs vary with loan renegotiations prior to the completion of an acquisition. Our goal is to examine whether the impact of a covenant violation is confounded by the provision of new bank debt, which might signal a relaxation of financial constraints. The regressions in Panel B show that CARs are indeed higher for acquirers that obtain new credit, consistent with Bharadwaj and Shivdasani (2003), who show that deals financed with bank debt have higher CARs in their sample of 115 cash tender offers from 1990 to 1996. However, we find that acquisitions following a renegotiation without new credit also earn significantly higher returns, regardless of whether the firm recently violated a covenant.

The estimated impact of a covenant violation is slightly lower in regressions that include loan renegotiation indicators, since renegotiations are more common for violators. However, the estimated coefficient on covenant violation remains significantly positive, which would be expected even if heightened renegotiation is the only channel that violations affect acquisitions.

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[30] In column 2 of Table 5, the estimated impact of a covenant violation is 1.2%. In the sample used in Table 5, the frequency of an acquisition for non-violators is 4.09%. This yields an estimated impact of 29.3% or a relative acquisition probability of about 70%.

Since the prospect of renegotiation is much higher for violators, the set of acquisitions that happen without a renegotiation is different for violators than non-violators. If acquisition restrictions are set strategically to exclude deals with low expected synergies, the financial covenant indicator will capture this selection effect even when controlling for the occurrence of a renegotiation.

The combined evidence in Table 9 suggests that financial covenant violations give creditors the opportunity to tighten acquisition restrictions that effectively prohibit a large fraction of potential deals. The prospect of renegotiation results in significantly fewer deals, either because borrowers do not ask for permission or ask and are not granted permission. When the borrower does choose to renegotiate and receives consent from the lender, the deal is viewed positively by equity markets. For deals not needing explicit consent from lenders, equity markets view acquisitions announced by recent covenant violators more positively than deals by other firms, suggesting that restrictions are set to permit relatively good deals.

7. Conclusion

As reviewed in Betton, Eckbo, and Thorburn (2008), extensive literature argues that acquisition decisions are susceptible to a variety of conflicts of interest. Considering these incentive conflicts, our paper examines whether and how creditors use control rights to influence corporate acquisitions. We find that creditors impose granular restrictions on borrower acquisition decisions and tighten these restrictions after a financial covenant violation. In a random sample of private credit agreements, we find that 10% of these contracts fully prohibit future acquisitions without lender approval and an additional 64% prohibit deals that do not meet certain criteria. Given the widespread use of private credit (Sufi, 2009), this evidence implies that corporate creditors play an active role in borrower M&A decisions, even beyond deal financing.

Using financial covenant violations as a source of variation in creditor control, we show that creditors limit acquisitions expected to destroy firm value. Conditional on announcing an acquisition, covenant violators experience significantly higher stock price reactions than similar non-violators and do not appear to pursue less risky targets. The frequency with which covenant violators renegotiate loan contracts prior to engaging in these acquisitions implies that contractual restrictions are the predominant mechanism that creditors use to discipline borrowers' acquisition behavior. We conclude that creditors use contractual rights and the loan renegotiation process to limit overinvestment rather than to inefficiently reduce risk.

The effectiveness of covenants and renegotiation that we document can justify the widespread use of restrictive covenants in private loan agreements. In addition to acquisition restrictions, typical covenants place restrictions on collateral, new borrowing, asset sales, payments to investors, and other common activities. These restrictions likely serve a similar purpose; they prevent borrowers from unilaterally taking actions that could harm their lenders and force a renegotiation in the event that borrowers want to undertake a prohibited action. Although we focus on M&A, which is an important corporate activity that has been the subject of considerable prior research, future research may examine how creditor control influences a broader set of corporate activities. Moreover, we provide evidence that creditors exert contractual control beyond periods following a violation of a financial covenant, and future research may examine the extent of this control in the governance of firms.

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Figure 1: Acquisition activity. This figure displays acquisition activity by financial covenant violation status for a sample of 176,378 firm-quarter observations from 7,164 U.S. nonfinancial firms between 1997 and 2015. We classify firms as "in viol" for two quarters after they report a financial covenant violation. We classify firms as "pre viol" and "post viol" for two quarters before and after they are in violation, respectively. We classify the remaining firms as not in violation. Acquisition activity is an indicator that equals one if an acquisition is announced during a firm-quarter, and zero otherwise. Bars represent group means and lines denote 90% confidence intervals.
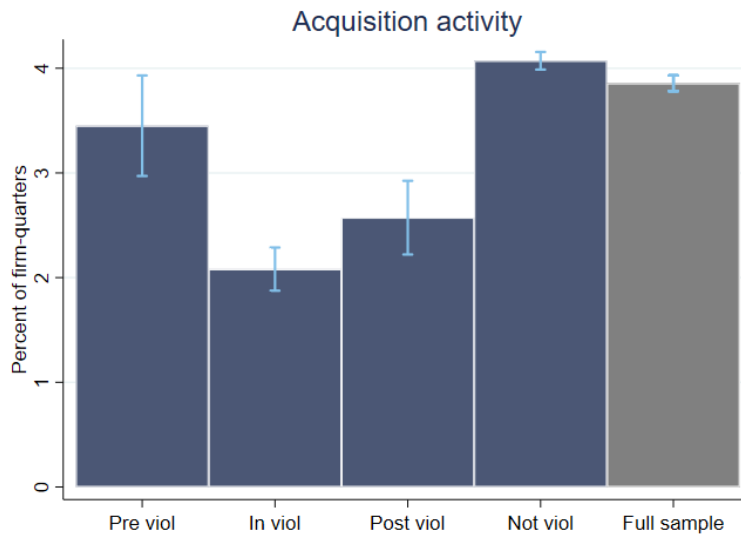
Figure 2: Acquirer announcement returns. This figure displays acquirer announcement returns by financial covenant violation status for a sample of 7,191 mergers and acquisitions announced by 2,907 U.S. nonfinancial firms from 1997 to 2015. We classify firms as "in viol" for two quarters after they report a financial covenant violation. We classify firms as "pre viol" and "post viol" for two quarters before and after they are in violation, respectively. We classify the remaining firms as not in violation. We estimate market model cumulative abnormal returns (CARs) using CRSP equal-weighted index returns and a one-year estimation window (252 trading days) ending one month (20 trading days) before the [-1, +1] event window. Bars represent group means/medians and lines denote 90% confidence intervals.
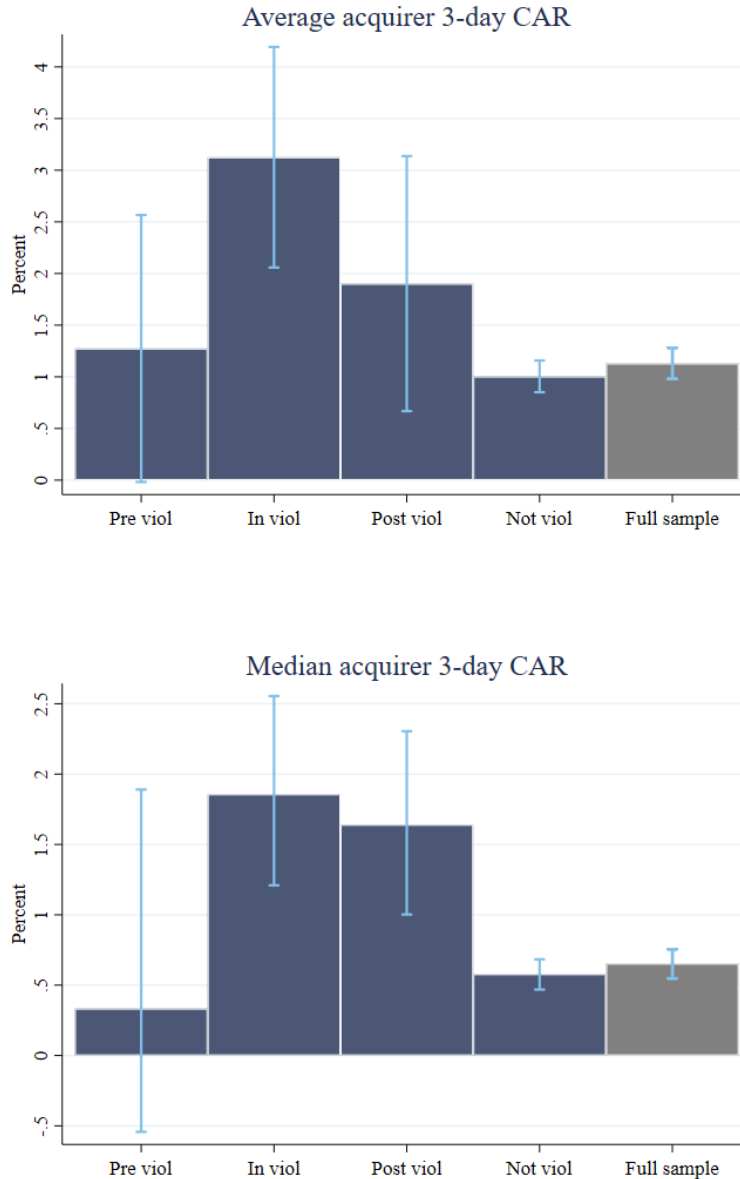
Table 1: Acquisition restrictions. The sample consists of a random set of 2,000 private credit agreements from U.S. nonfinancial firms in EDGAR and Compustat between 1997 and 2015. Panel A reports the percentage of contracts that contain an acquisition restriction. A "Full Restriction" prohibits all acquisitions without the consent of lenders. A "Partial Restriction" imposes at least one of the following conditions: "Expenditure Limit" prohibits deals above a certain size; "Pro Forma Compliance" prohibits deals that would cause the borrower to violate their existing covenants on a pro forma basis; "Financial Test" prohibits deals that would fail a non-covenant financial test; "Prohibit Diversifying" prohibits deals outside of the borrower's primary line of business. We report borrower characteristics from the fiscal quarter of loan origination and classify firm size based on real (2015 dollars) total assets. Top 5 lender status is based on the number of loans originated by the agent during the year. Panel B displays estimates from linear probability models, where the dependent variable equals one if the contract has a Full Restriction. Heteroskedasticity-consistent standard errors clustered by firm are reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Panel A: Frequency of acquisition restrictions

	N	Full Restriction	Partial Restriction	No Restriction	Partial Restrictions			
					Expenditure Limit	Pro Forma Compliance	Financial Test	Prohibit Diversifying
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All credit agreements	2000	10%	64%	26%	37%	39%	26%	40%
By borrower size								
Less than $500M	514	23%	66%	11%	45%	39%	27%	44%
$500M to $5,000M	999	6%	77%	17%	43%	50%	34%	49%
Greater than $5,000M	487	6%	34%	61%	14%	18%	8%	18%
By borrower rating								
No credit rating	958	15%	74%	11%	45%	48%	31%	49%
Speculative-grade rating	519	7%	80%	13%	48%	49%	37%	51%
Investment-grade rating	523	5%	28%	67%	9%	14%	5%	14%
By syndicate size								
Sole lender	262	26%	55%	19%	32%	28%	21%	35%
2 to 12 lenders	1223	9%	71%	21%	43%	46%	30%	46%
Greater than 12 lenders	515	6%	51%	43%	24%	30%	17%	29%
By agent bank								
Top 5 lender	1277	6%	63%	30%	35%	41%	26%	41%
Other lender	723	17%	64%	19%	39%	37%	25%	40%
By loan type								
Senior secured	1170	14%	76%	10%	48%	49%	34%	50%
Senior unsecured	829	5%	46%	49%	20%	26%	14%	27%
Subordinated unsecured	1	0%	100%	0%	100%	0%	0%	0%

Panel B: Determinants of full acquisition restriction

	Full Restriction		
	(1)	(2)	(3)
Borrower risk			
Size	-0.026***	-0.018***	-0.028***
	(0.006)	(0.007)	(0.007)
Operating cash flow / assets	-0.565***	-0.522***	-0.507***
	(0.125)	(0.126)	(0.125)
Leverage ratio	0.077*	0.070*	0.078*
	(0.040)	(0.040)	(0.041)
Unrated	0.033*	0.031*	0.029
	(0.018)	(0.018)	(0.018)
Financial covenant violation	0.112***	0.105**	0.097**
	(0.042)	(0.042)	(0.042)
Borrower investment opportunities			
Market-to-book ratio	0.003	0.003	0.001
	(0.010)	(0.010)	(0.010)
Prior acquisition activity	-0.049***	-0.048***	-0.046***
	(0.012)	(0.012)	(0.012)
Lender characteristics			
Number of lenders		-0.015	
		(0.009)	
Top 5 lender		-0.036**	
		(0.016)	
Loan characteristics			
Maturity			-0.056***
			(0.014)
Number financial covenants			-0.010
			(0.022)
Senior secured			0.014
			(0.016)
Industry & year fixed effects	Yes	Yes	Yes
Observations	2,000	2,000	2,000
Adjusted R-squared	0.123	0.127	0.132

Table 2: Contractual restrictions and acquisition behavior. This table describes borrower acquisition activity between origination and maturity for a random sample of 2,000 private credit agreements from U.S. nonfinancial firms in EDGAR and Compustat between 1997 and 2015. We test differences in means using t-tests and differences in medians using Wilcoxon rank sum tests. The symbols *, **, and *** indicate significant differences at the 10%, 5%, and 1% level, respectively.

	Full Restriction (1)	Partial Restriction (2)	No Restriction (3)	Diff. between (1) and (3)	Diff. between (2) and (3)
Fraction of firms with acquisition	0.079	0.256	0.198	-0.119***	0.058***
Mean acquirer 3-day CAR (%)	3.480	1.407	0.418	3.062**	0.989
Median acquirer 3-day CAR (%)	2.307	0.700	0.519	1.788**	0.181

Table 3: Tightening of acquisition restrictions. This table reports the evolution of acquisition restrictions for a random sample of 106 U.S. nonfinancial firms that violate a financial covenant and 94 matched non-violators. We construct the sample by matching firms that report a covenant violation to the nearest non-violator in the same quarter via a one-to-one propensity match (with replacement) on the full set of controls in Table 5 Column (2) and then selecting a random set of 200 observations where both firms have a credit agreement available in EDGAR. We hand-collect pre-violation acquisition restrictions from these contracts and search for changes in contracts and amendments for six months post-violation. We calculate the frequency of tightening as the fraction of loans that (i) add a full restriction, (ii) reduce the expenditure limit on permitted acquisitions, or (iii) increase the number of partial restrictions.

	Violators	**Non-Violators**	**Difference**	*p*-value
	N = 106	N = 94		
Ex-ante acquisition restrictions				
Full restriction	0.302	0.266	0.036	0.577
Partial restriction:	0.547	0.543	0.004	0.948
Expenditure limit	0.377	0.309	0.068	0.309
Pro forma covenant compliance	0.274	0.277	-0.003	0.962
Financial test	0.274	0.245	0.029	0.644
Prohibit diversifying deals	0.377	0.340	0.037	0.589
No restriction	0.151	0.191	-0.040	0.449
Restriction added post-violation				
Full restriction	0.104	0.021	0.083	0.018
Partial restriction:	0.047	0.011	0.036	0.132
Expenditure limit	0.094	0.021	0.073	0.030
Pro forma covenant compliance	0.057	0.032	0.025	0.403
Financial test	0.066	0.011	0.055	0.046
Prohibit diversifying deals	0.038	0.000	0.038	0.058
Frequency of tightening	0.245	0.043	0.202	0.000

Table 4: Sample description. The firm-quarter sample consists of 176,378 firm-quarter observations from 7,164 U.S. nonfinancial firms with data available in the CRSP-Compustat Merged Database between 1997 and 2015. The mergers and acquisitions (M&A) sample consists of 7,191 deals announced by 2,907 of these firms. We obtain the initial M&A sample from SDC Platinum and filter out spinoffs, recapitalizations, exchange offers, repurchases, self-tenders, privatizations, deals valued at less than $1 million or 1% of the acquirer's market value 11 days prior to the announcement, deals where the acquirer controlled more than 50% of the target prior to the announcement or sought less than 100% after completion, and deals that do not involve a public, private, or subsidiary target. Panel A displays descriptive statistics for the M&A sample. Panel B presents acquirer characteristics split by financial covenant violation status. We test differences in means using *t*-tests and differences in medians using Wilcoxon rank sum tests. The symbols *, **, and *** indicate significant differences at the 10%, 5%, and 1% level, respectively. We winsorize unbounded variables at the 1/99% level throughout the analysis. The appendix lists variable definitions.

Panel A – Descriptive statistics

	Mean	**S.D.**	**Q1**	**Median**	**Q3**	**N**
Acquirer characteristics						
Market value of equity ($B)	5.144	14.588	0.266	0.893	3.087	7,191
Assets ($B)	4.146	9.964	0.216	0.814	2.898	7,191
Stock price runup	0.039	0.527	-0.261	-0.038	0.206	7,191
Market-to-book ratio	2.007	1.327	1.224	1.607	2.261	7,191
Operating cash flow / assets	0.116	0.149	0.077	0.133	0.189	7,191
Leverage ratio	0.256	0.206	0.080	0.239	0.377	7,191
Interest expense / assets	0.019	0.020	0.005	0.014	0.027	7,191
Net worth / assets	0.495	0.228	0.343	0.489	0.660	7,191
Current ratio	2.714	2.436	1.326	1.992	3.070	7,191
Deal characteristics						
Acquirer 3-day CAR (%)	1.130	7.768	-2.485	0.654	4.203	7,191
Deal value ($M)	725.299	2188.975	26.056	93.254	362.034	7,191
Relative deal size	0.292	0.493	0.039	0.104	0.306	7,191
Completed (0/1)	0.948	0.221	1.000	1.000	1.000	7,191
Toehold (%)	0.376	3.531	0.000	0.000	0.000	7,191
Diversifying (0/1)	0.284	0.451	0.000	0.000	1.000	7,191
All-cash (0/1)	0.592	0.492	0.000	1.000	1.000	7,191
All-stock (0/1)	0.113	0.317	0.000	0.000	0.000	7,191
Public target (0/1)	0.229	0.420	0.000	0.000	0.000	7,191
Private target (0/1)	0.449	0.497	0.000	0.000	1.000	7,191
Subsidiary target (0/1)	0.322	0.467	0.000	0.000	1.000	7,191
Cross-border deal (0/1)	0.164	0.370	0.000	0.000	0.000	7,191
Hostile (0/1)	0.008	0.087	0.000	0.000	0.000	7,191
Tender offer (0/1)	0.057	0.232	0.000	0.000	0.000	7,191
Risky target (0/1)	0.324	0.468	0.000	0.000	1.000	7,191

Table 4: Sample description (cont.)

Panel B – Acquirer summary statistics by violation status

	In Violation			Not in Violation		
	Mean	Median	N	Mean	Median	N
Assets ($B)	2.068***	0.230***	285	4.232	0.856	6,906
Stock price runup	0.016	-0.171***	285	0.040	-0.036	6,906
Market-to-book ratio	1.791***	1.325***	285	2.016	1.613	6,906
Operating CF / assets	0.040***	0.076***	285	0.119	0.135	6,906
Leverage ratio	0.324***	0.295***	285	0.253	0.236	6,906
Current ratio	2.030***	1.589***	285	2.743	2.010	6,906
Cash / assets	0.108***	0.048***	285	0.166	0.079	6,906
PP&E / assets	0.277	0.185	285	0.260	0.170	6,906

Table 5: Acquisition behavior. This table reports ordinary least squares (OLS) estimates of the effect of a financial covenant violation on acquisition behavior. The sample consists of 176,378 firm-quarter observations from 7,164 U.S. nonfinancial firms between 1997 and 2015. The dependent variable in Columns (1) and (2) is an indicator that equals one if an acquisition is announced during the firm-quarter, and zero otherwise. The dependent variable in Columns (3)-(5) is an indicator that equals one if a value-destroying, value-neutral, or value-enhancing acquisition is announced during the firm-quarter, and zero otherwise. We classify an acquisition as value-destroying (value-enhancing) if the acquirer's 3-day CAR is more than one standard deviation below (above) the mean. We classify an acquisition as value-neutral otherwise. Our broadest specification regresses the dependent variable on an indicator that equals one if the firm reported a financial covenant violation within the previous two quarters, firm size, stock price runup, covenant controls, four-quarter lags of the covenant controls, the second and third power of the covenant controls, Fama-French 48 industry fixed effects, and year fixed effects. Controls are measured at the previous fiscal quarter end. Heteroskedasticity-consistent standard errors clustered by firm are reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

	Acquisition Activity		Value-destroying	Value-neutral	Value-enhancing
	(1)	(2)	(3)	(4)	(5)
Financial covenant violation	-0.016***	-0.012***	-0.003***	-0.009***	-0.000
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Size	0.001***	0.001***	0.000***	0.000***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Stock price runup	0.011***	0.010***	0.002***	0.006***	0.003***
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Market-to-book ratio	0.000	0.041***	0.007***	0.028***	0.006***
	(0.000)	(0.003)	(0.001)	(0.003)	(0.001)
Operating cash flow / assets	0.032***	0.030***	0.004**	0.020***	0.006***
	(0.003)	(0.004)	(0.002)	(0.003)	(0.002)
Leverage ratio	0.009***	0.112***	0.027***	0.071***	0.016*
	(0.003)	(0.023)	(0.009)	(0.018)	(0.009)
Interest expense / assets		-0.044	-0.050	-0.001	0.043
		(0.233)	(0.088)	(0.180)	(0.091)
Net worth / assets		0.051***	0.010***	0.036***	0.007**
		(0.008)	(0.003)	(0.006)	(0.003)
Current ratio		0.003**	0.000	0.002*	0.001**
		(0.001)	(0.001)	(0.001)	(0.001)
Lagged & higher-order controls	No	Yes	Yes	Yes	Yes
Industry & year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	176,378	176,378	176,378	176,378	176,378
Adjusted R-squared	0.009	0.012	0.004	0.008	0.002
Unconditional mean	0.039	0.039	0.008	0.025	0.007
% Δ relative to mean	-40.542	-29.976	-37.719	-35.608	-2.332

Table 6: Acquirer announcement returns. This table reports OLS estimates of the effect of a financial covenant violation on acquirer announcement returns. The sample consists of 7,191 deals by 2,907 U.S. nonfinancial firms from 1997 to 2015. Heteroskedasticity-consistent standard errors clustered by firm are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level.

	Acquirer 3-day CAR (%)			
	(1)	(2)	(3)	(4)
Financial covenant violation	1.860***	1.758***	1.614**	1.619**
	(0.687)	(0.678)	(0.663)	(0.657)
Size	-0.057***	-0.050***	-0.021***	-0.018**
	(0.007)	(0.007)	(0.007)	(0.008)
Stock price runup	-0.041	-0.158	0.032	-0.021
	(0.241)	(0.287)	(0.238)	(0.283)
Market-to-book ratio	-0.277**	-0.728	-0.144	0.097
	(0.108)	(0.828)	(0.107)	(0.817)
Operating cash flow / assets	-0.711	1.730	-0.207	1.755
	(0.933)	(1.886)	(0.915)	(1.848)
Leverage ratio	0.870	-2.562	0.334	-1.636
	(0.556)	(4.944)	(0.546)	(4.882)
Interest expense / assets		45.513		52.026
		(52.688)		(51.891)
Net worth / assets		-4.084		-4.090
		(3.083)		(2.981)
Current ratio		0.124		0.149
		(0.332)		(0.327)
Relative deal size			2.189***	2.046***
			(0.320)	(0.324)
Completed			0.199	0.184
			(0.542)	(0.538)
Toehold			0.056**	0.053**
			(0.026)	(0.025)
Diversifying			0.331	0.304
			(0.212)	(0.212)
All-cash			-0.114	-0.061
			(0.236)	(0.238)
All-stock			-1.098***	-1.258***
			(0.422)	(0.419)
Private target			2.849***	2.785***
			(0.330)	(0.328)
Subsidiary target			3.441***	3.380***
			(0.329)	(0.327)
Cross-border deal			-0.043	-0.063
			(0.222)	(0.223)
Hostile			-2.159**	-2.183**
			(0.952)	(0.951)
Tender offer			1.739***	1.715***
			(0.419)	(0.419)
Lagged & higher-order controls	No	Yes	No	Yes
Industry & year fixed effects	Yes	Yes	Yes	Yes
Observations	7,191	7,191	7,191	7,191
Adjusted R-squared	0.018	0.025	0.053	0.057

Table 7: Deal characteristics. Panel A displays OLS estimates of the effect of a financial covenant violation on target selection. Panel B presents mean and median CARs split by acquirer violation status and deal type. The sample consists of 7,191 deals announced by 2,907 U.S. nonfinancial firms from 1997 to 2015. We test differences in means using *t*-tests and differences in medians using Wilcoxon rank sum tests. The symbols *, **, and *** indicate significant differences at the 10%, 5%, and 1% level, respectively.

Panel A: Target selection

	Diversifying Target		Risky Target		Private Target	
	(1)	(2)	(3)	(4)	(5)	(6)
Financial covenant violation	-0.049*	-0.047*	-0.007	-0.011	0.067**	0.065**
	(0.027)	(0.028)	(0.029)	(0.029)	(0.031)	(0.031)
Size	-0.001	-0.001	0.001*	0.001	-0.009***	-0.008***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Stock price runup	0.012	0.018	-0.018	-0.018	0.017	0.016
	(0.011)	(0.013)	(0.011)	(0.013)	(0.013)	(0.015)
Market-to-book ratio	0.003	0.034	0.001	-0.029	0.010*	0.033
	(0.005)	(0.041)	(0.005)	(0.044)	(0.005)	(0.043)
Operating cash flow / assets	-0.067*	0.052	-0.006	0.017	-0.094**	-0.144
	(0.040)	(0.085)	(0.040)	(0.090)	(0.045)	(0.095)
Leverage ratio	0.010	0.507*	-0.025	0.671**	-0.180***	0.073
	(0.038)	(0.269)	(0.038)	(0.294)	(0.035)	(0.282)
Interest expense / assets		4.694*		-3.364		-2.319
		(2.814)		(2.884)		(2.805)
Net worth / assets		-0.014		-0.222		0.175
		(0.146)		(0.156)		(0.140)
Current ratio		-0.004		0.018		-0.003
		(0.018)		(0.019)		(0.019)
Lagged & higher-order controls	No	Yes	No	Yes	No	Yes
Industry & year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,191	7,191	7,191	7,191	7,191	7,191
Adjusted R-squared	0.068	0.071	0.061	0.062	0.094	0.101

Panel B: Acquirer CARs by deal type and violation status

	In Violation			Not in Violation		
	Mean	Median	N	Mean	Median	N
Diversifying target	3.856***	2.079	70	1.273	0.692	1,969
Focused target	2.888***	1.695***	215	0.958	0.580	4,937
Public target	0.073	-0.025	58	-0.910	-0.581	1,586
Private target	3.433***	1.643**	144	1.305	0.727	3,084
Subsidiary target	4.725***	3.701***	83	2.083	1.149	2,236
All-cash payment	3.279***	1.925***	116	1.224	0.719	4,139
Mixed payment	4.439***	2.331**	111	1.298	0.767	2,010
All stock payment	0.304	-0.735	58	-0.577	-0.922	757

Table 8: Acquirer balance sheet changes. This table reports balance sheet changes around a propensity matched sample of acquisitions. We construct the sample by matching acquirers in violation of a financial covenant to the nearest non-violating acquirer via a one-to-one propensity match (with replacement) on the full set of controls in Table 5 Column (2). After the match, we drop withdrawn deals, deals made by acquirers that complete more than one acquisition during the calculation window, and deals made by acquirers with insufficient data to calculate post-acquisition changes. This process yields a sample of 414 deals made by 404 acquirers. The symbol Δ denotes the difference from one year pre-acquisition to three years post-acquisition. We test differences in means using *t*-tests and differences in medians using Wilcoxon rank sum tests. The symbols *, **, and *** indicate significant differences at the 10%, 5%, and 1% level, respectively.

Panel A: Pre-acquisition summary statistics

	In Violation			Not in Violation		
	Mean	Median	N	Mean	Median	N
PP&E / assets	0.277	0.177	202	0.309	0.207	212
Cash / assets	0.114	0.055	202	0.128	0.054	212
Current ratio	2.104	1.611	202	2.245	1.778	212
Leverage ratio	0.307	0.267	202	0.314	0.281	212

Panel B: Post-acquisition changes

	In Violation			Not in Violation		
	Mean	Median	N	Mean	Median	N
Δ PP&E / assets	-0.028	-0.012	202	-0.019	-0.015	212
Δ Cash / assets	-0.018	-0.002	202	-0.033	-0.004	212
Δ Current ratio	-0.265	-0.135	202	-0.398	-0.147	212
Δ Leverage ratio	0.038	0.026	202	0.045	0.026	212

Table 9: Renegotiation of contractual restrictions. This table explores the economic mechanism through which creditors exert control for a propensity matched sample of acquisitions. We construct the sample by matching acquirers in violation of a financial covenant to the nearest non-violating acquirer via a one-to-one propensity match (with replacement) on the full set of controls in Table 5 Column (2). After the match, we eliminate deals for which we are unable to hand-collect loan renegotiation data from EDGAR. This process yields a sample of 522 deals by 446 acquirers. Panel A reports the prevalence of loan renegotiations prior to acquisitions. Any amendment is an indicator equal to one if the deal triggered a loan contract renegotiation. New credit is an indicator equal to one if the renegotiation provided additional credit to the borrower. Amendment with no new credit is an indicator equal to one if the renegotiation did not provide new credit. Panel B reports estimates from OLS regressions of announcement CARs on loan renegotiation indicators. Heteroskedasticity-consistent standard errors clustered by firm are reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Panel A: Prevalence of loan renegotiations prior to acquisitions

	All (N=522)	In Violation (N=281)	Not in Viol (N=241)	*p*-value of difference
Amendment with no new credit (0/1)	0.170	0.228	0.104	0.000
New credit (0/1)	0.216	0.235	0.195	0.271
Any renegotiation (0/1)	0.387	0.463	0.299	0.000

Panel B: Loan renegotiations and acquirer returns

	Acquirer 3-day CAR (%)			
	(1)	(2)	(3)	(4)
Financial covenant violation	2.461***	2.196**	2.338**	1.938**
	(0.909)	(0.910)	(0.910)	(0.911)
Amend with no new credit		2.379*		3.277***
		(1.220)		(1.241)
New credit			2.537**	3.266***
			(1.220)	(1.245)
Observations	522	522	522	522
Adjusted R-squared	0.024	0.031	0.034	0.047

Appendix: Variable definitions. CCM denotes the CRSP-Compustat Merged Database. SDC denotes the SDC Platinum Mergers and Acquisition Database.

Panel A: Firm characteristics

Variable	Source	Description
Acquisition activity	SDC	Indicator that equals one if an acquisition is announced during a firm-quarter, and zero otherwise
Book value of equity	CCM	Total assets (*atq*) minus total liabilities (*ltq*) plus deferred taxes and investment tax credits (*txditcq* if available, 0 if missing)
Cash	CCM	Cash holdings (*cheq*)
Current ratio	CCM	Total current assets (*actq*) divided by total current liabilities (*lctq*)
Financial covenant violation	Hand-collected	Indicator that equals one if the firm reported a financial covenant violation within the previous two quarters, and zero otherwise
Interest expense	CCM	Interest expense (*xintq*)
Leverage ratio	CCM	Long-term debt (*dlttq*) plus debt in current liabilities (*dlcq*), divided by total assets
Market value of assets	CCM	Market value of equity minus book value of equity plus total asset
Market value of equity	CCM	Common shares outstanding (*cshoq*) times the fiscal quarter closing price (*prccq*)
Market-to-book ratio	CCM	Ratio of market value to book value of total assets
Net worth	CCM	Stockholder's equity (*seqq*)
Operating cash flow	CCM	Operating income before depreciation (*oibdpq*)
PP&E	CCM	Net property, plant and equipment (*ppentq*)
Prior acquisition activity	SDC & CCM	Log of one plus the number of acquisitions the firm completed in the 3 years before the loan origination date
Size	CCM	Log of average assets (*atq*)
Stock price runup	CCM	Deal sample: Acquirer's buy-and-hold abnormal return (BHAR) over the [-210, -11] window using the CRSP equal-weighted index as market proxy. Firm-quarter sample: BHAR [-4qtr, -1qtr]
Unrated	CCM	Indicator that equals one if firm has an S&P long term issuer credit rating, and zero otherwise

Appendix: Variable definitions (cont.)

Panel B: Deal characteristics

Variable	Source	Description
Acquirer 3-day cumulative abnormal return (CAR)	SDC & CCM	Market model CAR estimated using CRSP equal-weighted index returns and a one-year estimation window (252 trading days) ending one month (20 trading days) before the [-1, +1] event window
Value-destroying acquisition	SDC & CCM	Indicator that equals one if an acquisition announced during the firm-quarter earns a 3-day CAR more than one standard deviation below the mean, and zero otherwise
Value-neutral acquisition	SDC & CCM	Indicator that equals one if an acquisition announced during the firm-quarter earns a 3-day CAR within one standard deviation of the mean, and zero otherwise
Value-enhancing acquisition	SDC & CCM	Indicator that equals one if an acquisition announced during the firm-quarter earns a 3-day CAR more than one standard deviation above the mean, and zero otherwise
All-cash	SDC	Indicator that equals one if the acquisition is paid entirely with cash, and zero otherwise
All-stock	SDC	Indicator that equals one if the acquisition is paid entirely with stock, and zero otherwise
Completed	SDC	Indicator that equals one if an announced acquisition is completed, and zero otherwise
Cross-border deal	SDC	Indicator that equals one if the target is located outside of the U.S., and zero otherwise
Deal value	SDC	Total value paid by the acquirer, excluding fees and expenses
Diversifying	SDC	Indicator that equals one if the primary SIC of the acquirer and target are not in the same Fama-French 12 industry, and zero otherwise
Hostile	SDC	Indicator that equals one if the acquisition is hostile, and zero otherwise
Private target	SDC	Indicator that equals one if the target is private, and zero otherwise
Public target	SDC	Indicator that equals one if the target is public, and zero otherwise
Subsidiary target	SDC	Indicator that equals one if the target is a subsidiary of a public or private firm, and zero otherwise
Relative deal size	SDC & CCM	Deal value scaled by the acquirer's market value of equity 11 trading days prior to the announcement
Risky target	SDC & CCM	Indicator that equals one if average asset volatility is higher in target's industry than acquirer's industry, and zero otherwise
Tender offer	SDC	Indicator that equals one if a tender offer is made, and zero otherwise
Toehold	SDC	Percentage of target's common stock owned by the acquirer prior to deal announcement. Assumed 0 if missing in SDC

Internet Appendix for

Creditor Control of Corporate Acquisitions

January 2020

1. Heterogeneity with respect to competition and governance

The results presented in this paper suggest that creditors intervene in borrower acquisitions to limit overinvestment driven by managerial agency problems. In this section, we provide additional support for this interpretation by testing the relation between covenant violations and acquisition performance in subsamples split by proxies for competition and governance.

The goal of each split is to proxy for "weakness" in a particular mechanism that could potentially mitigate the overinvestment problem. While we expect creditor effects to be concentrated among firms that lack "strong" mechanisms to mitigate the overinvestment problem, we do not necessarily expect statistically significant differences across subsamples with "strong" and "weak" disciplinary mechanisms. For example, it is possible for creditors to have some effect in the "strong" subsample if the governance mechanism does not perfectly mitigate the overinvestment problem. Moreover, our proxies are imperfect and may be endogenous. If shareholders respond to underlying agency problems by improving governance, the "unresolved" agency issues may be fairly similar across the groups.

We begin by splitting the sample into top and bottom HHI terciles because Hart (1983) argues that product market competition reduces slack and can force managers to maximize firm value. Consistent with the idea that competition can temper the overinvestment problem, estimates in Columns (1) and (2) in Table A.1 show that creditors have a larger effect on firms operating in concentrated industries than firms operating in competitive industries.

In a similar vein, we segment the sample based on a myriad of governance measures related to managers' ability to extract private benefits. Estimates reported in Table A.1 show that creditor effects are concentrated among firms with low analyst coverage, no institutional blockholder, a

busy board, and weak shareholder rights.[31] However, the difference between subsamples with "weak" and "strong" governance is not significant at conventional levels in most specifications. Therefore, we interpret the results as suggestive evidence that that creditors intervene in borrower acquisition activity to limit overinvestment driven by unresolved managerial agency problems.

2. Alternate empirical specifications

Our goal is to measure the effect of a financial covenant violation by comparing outcomes for violators to outcomes for similar non-violators. Previous researchers have proposed several identification strategies to accomplish this goal. In our main analysis, we use the "quasi-regression discontinuity design" of Roberts and Sufi (2009) and Nini et al. (2012). The advantage of this strategy is that it enables us to examine the broadest possible sample of firms with the least amount of measurement error, since the SEC mandates disclosure in quarterly financial statements. The drawback of this strategy is that we do not observe the contractual level of each individual covenant and thus cannot precisely compare firms above/below the threshold. Instead, the quasi-RDD specification exploits the discontinuity at the point of violation by flexibly controlling for continuous functions of the variables on which covenants are written and mimics a standard regression-discontinuity design only if covenants are written at similar levels for similar firms. We employ this specification throughout the analysis to ensure that our sample of covenant violators is large enough to detect variation in acquisition behavior.

An alternative approach is to impute violations using covenant data from Dealscan and financial ratios from Compustat. The advantage of this strategy is that it provides a clear counterfactual by comparing firms just above/below covenant thresholds. The drawback of this

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[31] We caution interpretation of the results in our busy board and shareholder rights splits because sample sizes are severely limited by RiskMetrics' focus on S&P 1500 firms and the fact that the G-index can only reliably be constructed until 2006 (see Karpoff, Schonlau, and Wehrly (2017).

strategy is twofold. First, the sample size falls significantly due to data availability in Dealscan.

Second, creditors frequently write covenants on non-GAAP financial measures that cannot reliably

be constructed from accounting data in Compustat. Chava and Roberts (2008) argue that

researchers can minimize this measurement error by focusing exclusively on current ratio and net

worth covenants, since these covenants tend to have more standard definitions. The drawback of

this approach, however, is a further reduction in sample size.

　　With these factors in mind, we assess the robustness of our results under a range of alternate

specifications. We begin by examining sample sizes afforded by each specification. Table A.2

splits the data into various subsamples and presents the frequency of acquisition announcements

by firms that have reported a violation within the previous two quarters (*Violators*) and firms that

have not (*Non-Violators*).

　　Quasi-RDD sample is our full sample of firm-quarters used in the main analysis. *One-to-*

one nearest neighbor sample is a one-to-one nearest neighbor propensity score matched sample of

firm-quarters, constructed using a logit regression to estimate the probability that a firm-quarter is

in violation of a covenant as a function of the full set of covariates in the quasi-RDD specification

(Table 5, Column 2).[32] *Dealscan RDD sample* is the subsample of firm-quarters with covenant

data available in Dealscan. *Dealscan RDD [+/- 20] sample* restricts the *Dealscan RDD sample* to

the subset of firm-quarters that have a financial ratio within +|- 20% of the relevant covenant

threshold. *Chava and Roberts (2008) sample* is the subsample of firm-quarters with a current ratio,

total net worth, or tangible net worth covenant in Dealscan. Finally, *Chava and Roberts (2008)*

[+/- 20] sample restricts the *Chava and Roberts (2008) sample* to the subset of firm-quarters within

+|- 20% of a relevant covenant threshold.

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[32] We match with replacement throughout the analysis to minimize the difference between our treated and control groups. In unreported tests, we find that our results are robust to matching without replacement.

Consistent with our main results, Table A.2 shows that a lower proportion of *Violator* firm-quarters announce a value-destroying acquisition than *Non-Violators* across all samples. However, it also shows that the sample size drops severely when imputing violations using current ratio and net worth covenants.

Table A.3 presents OLS estimates of the effect of a financial covenant violation on acquisition behavior across alternate empirical specifications. Panel A reproduces estimates from the quasi-regression discontinuity specification used in our main analysis. Panel B reports propensity score matching estimates using four matching protocols. To construct the estimates, we use a logit regression to predict whether a firm-quarter is in violation of a covenant conditional on the full set of covariates in the quasi-RDD specification. We then estimate the average treatment effect on the treated (ATT) using the propensity scores and one-to-one nearest neighbor matching in Row (1), five-nearest neighbor matching in Row (2), Gaussian kernel matching in Row (3), and radius matching in Row (4). Panel C reports estimates from the same empirical model as Panel A, except it includes controls for the distance to the tightest covenant threshold observed in Dealscan instead of the quasi-RDD controls. *Distance to threshold* is defined as the difference between tightest covenant threshold and observed financial ratio, divided by the firm-specific standard deviation of the financial ratio. Panel D reports estimates using the regression discontinuity specification of Chava and Roberts (2008).[33] Our results are robust to these alternative specifications, except for the Chava and Roberts (2008) regression discontinuity design. The imprecision of these estimates is due to a lack of acquisition activity among violators in this subsample. For example, Table A.2 shows that only 9 *Violator* firm-quarters in the *Chava and*

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[33] Following Chava and Roberts (2008), we impute violations from current ratio, total net worth, and tangible net worth observed in Compustat and the corresponding threshold in Dealscan. We linearly interpolate dynamic covenant thresholds, drop loans that appear to be in violation at origination, and, in the case of overlapping loans, define the relevant package to be the tighter of the two unless the latter deal corresponds to a refinancing.

Roberts (2008) RDD [+/- 20] sample have an acquisition announcement.

3. Alternate acquirer CAR specifications

Table A.4 reports robustness tests for our acquirer CAR results. In Panel A, we re-estimate the quasi-regression discontinuity specification from our main analysis (Column 2 of Table 6), but we alter the specification in each row to check robustness. Panel A shows that our main results are robust to i) double clustering on firm and year, ii) using only the subsample of completed acquisitions, iii) using 5-day rather than 3-day CARs, and iv) using value-weighted rather than equal-weighted CARs. Panel B reports propensity score matching estimates of the impact of a financial covenant violation on acquirer 3-day CARs. We use a logit regression to estimate the probability that an acquirer is in violation of a covenant as a function of the full set of covariates in the quasi-RDD specification (Table 6, Column 2). We then estimate the average treatment effect on the treated (ATT) given the propensity score using four different matching techniques (one-to-one nearest neighbor, five-nearest neighbors, Gaussian kernel, and radius).[34] Together, the results in Table A.4 support our main finding that acquirers in violation of a financial covenant earn higher announcement returns.

4. Alternate covenant violation indicator

In our main analysis, we classify firms as "in violation" for two quarters after they report a financial covenant violation. We choose this horizon because violation is a discrete event that transfers control rights immediately to borrowers, so changes in firm behavior should be observed soon after. Indeed, Table 3 shows that over 24% of firms that violate a financial covenant become subject to tighter acquisition restrictions within six months. While we expect the effect of this tightening to persist, we focus our analysis on a two-quarter horizon because, as time goes on,

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[34] The one-to-one nearest neighbor matched sample is the same sample used in Tables 9 and 10 of the main analysis.

many control firms will themselves become subject to additional creditor monitoring either as a result of a covenant violation or as they negotiate a new credit agreement upon maturity. Nevertheless, in Table A.5 we show that our main results are robust to classifying firms as "in violation" for four quarters after they report a financial covenant violation.

Table A.1: Heterogeneity with respect to competition and governance. This table displays cross-sectional variation in the effect of a financial covenant violation. The regression specifications are the same as those reported in Tables 5 and 6 except that we split the sample according to governance characteristics measured at the prior fiscal year end. In Columns (1) and (2), we proxy for the disciplining effect of product market competition by sorting firms into top and bottom terciles according to a sales based Herfindahl-Hirschman Index (HHI), calculated at the 3-digit SIC level. In Columns (3) and (4), we proxy for analyst monitoring by sorting firms into top and bottom terciles based on the number of analysts providing an annual EPS estimate in the last I/B/E/S forecast summary before the fiscal year end. In Columns (5) and (6), we proxy for shareholder monitoring by splitting the sample based on the presence of a 10% blockholder in the Thomson Reuters s34 Master File. In Columns (7) and (8), we proxy for director monitoring by splitting the sample based on the presence of a busy board. Following Field and Mkrtchyan, (2017), we classify busy boards as those in which a majority of independent directors serve on three or more boards according to the RiskMetrics Directors database. In Columns (9) and (10), we proxy for shareholder rights by splitting the sample into "dictatorship" and "democracy" firms following Harford, Humphery-Jenner, and Powell (2012). Specifically, we classify a "dictatorship" as a firm with a G-index of at least 10 or a classified board, and "democracy" as a firm with a G-index below 10 and no classified board according to the RiskMetrics Governance Legacy database. Heteroskedasticity-consistent standard errors clustered by firm are reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

	Value-destroying acquisition	Acquirer 3-day CAR	Value-destroying acquisition	Acquirer 3-day CAR	Value-destroying acquisition	Acquirer 3-day CAR
	(1)	(2)	(3)	(4)	(5)	(6)
	Concentrated industry		*Low analyst coverage*		*No blockholder*	
Financial covenant violation	-0.003**	2.617**	-0.001***	2.108*	-0.003***	1.748**
	(0.001)	(1.197)	(0.001)	(1.256)	(0.001)	(0.848)
Observations	69,087	2,776	56,398	1,379	91,385	3,622
Adjusted R-squared	0.003	0.038	0.001	0.023	0.004	0.032
	Competitive industry		*High analyst coverage*		*Blockholder*	
Financial covenant violation	0.001	-0.374	-0.002	-0.938	-0.001	1.386
	(0.002)	(1.324)	(0.002)	(1.150)	(0.001)	(1.580)
Observations	51,321	2,036	58,247	3,176	49,813	1,964
Adjusted R-squared	0.004	0.035	0.004	0.036	0.005	0.029
Acquirer controls	Yes	Yes	Yes	Yes	Yes	Yes
Covenant controls	Yes	Yes	Yes	Yes	Yes	Yes
Lagged & higher-order controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry & year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
p-value of difference	0.073	0.088	0.821	0.065	0.389	0.838

Table A.1 (cont.): Heterogeneity with respect to competition and governance.

	Value-destroying acquisition	Acquirer 3-day CAR (%)	Value-destroying acquisition	Acquirer 3-day CAR (%)
	(7)	(8)	(9)	(10)
	Busy board		*Dictatorship*	
Financial covenant violation	-0.008***	6.149**	-0.003***	1.856**
	(0.002)	(2.804)	(0.001)	(0.722)
Observations	11,090	541	159,515	6,439
Adjusted R-squared	0.004	0.096	0.004	0.023
	No busy board		*Democracy*	
Financial covenant violation	-0.002	-1.132	-0.002	0.674
	(0.002)	(1.124)	(0.003)	(1.903)
Observations	51,853	2,453	14,727	681
Adjusted R-squared	0.004	0.042	0.007	0.039
Acquirer controls	Yes	Yes	Yes	Yes
Covenant controls	Yes	Yes	Yes	Yes
Lagged & higher-order controls	Yes	Yes	Yes	Yes
Industry & year fixed effects	Yes	Yes	Yes	Yes
p-value of difference	0.013	0.010	0.702	0.541

Table A.2: Acquisition frequency across alternate samples. This table presents the frequency of firm-quarters with an acquisition announced by violators and non-violators. Sample sizes, and thus acquisition frequencies, vary across rows based on the empirical specification used to identify treatment (violators) and control (non-violators) firms. Internet Appendix Section 2 provides a detailed description of each of these specifications.

	Total		Value-destroying		Value-neutral		Value-enhancing	
	#	%	#	%	#	%	#	%
Quasi-RDD sample								
Violators	275	2.12	40	0.31	163	1.26	72	0.55
Non-Violators	6,683	4.09	1,380	0.84	4,173	2.55	1,130	0.69
One-to-one nearest neighbor sample								
Violators	275	2.12	40	0.31	163	1.26	72	0.55
Non-Violators	338	3.11	63	0.58	219	2.02	56	0.52
Dealscan RDD sample								
Violators	86	2.19	7	0.18	55	1.40	24	0.61
Non-Violators	2,733	4.89	589	1.05	1,700	3.04	444	0.80
Dealscan RDD [+/- 20] sample								
Violators	30	1.88	2	0.13	19	1.19	9	0.56
Non-Violators	591	4.53	121	0.93	364	2.79	106	0.81
Chava and Roberts (2008) RDD sample								
Violators	37	2.29	3	0.19	27	1.67	7	0.43
Non-Violators	678	4.64	137	0.94	434	2.97	107	0.73
Chava and Roberts (2008) RDD [+/- 20] sample								
Violators	9	1.88	1	0.21	6	1.25	2	0.42
Non-Violators	106	3.70	15	0.52	70	2.44	21	0.73

Table A.3: Acquisition behavior across alternate empirical specifications. This table presents robustness checks of the effect of a financial covenant violation on acquisition behavior. Panel A reproduces ordinary least squares (OLS) estimates from the quasi-regression discontinuity specification in our main analysis (Table 5). Panel B reports propensity score matching estimates using four propensity score matching protocols. We use a logit regression to estimate the probability that a firm-quarter is in violation of a covenant as a function of the full set of covariates in the quasi-RDD specification. We then estimate the average treatment effect on the treated (ATT) using the propensity score and one-to-one nearest neighbor matching in Row (1), five-nearest neighbor matching in Row (2), Gaussian kernel matching in Row (3), and radius matching in Row (4). Panels C and D present OLS estimates using several alternate regression discontinuity specifications. Internet Appendix Section 2 provides a detailed description of each of these specifications. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Panel A: Quasi-RDD estimates

	Acquisition	Value-destroying	Value-enhancing
	(1)	(2)	(3)
Financial covenant violation	-0.012***	-0.003***	-0.000
	(0.002)	(0.001)	(0.001)
Acquirer controls	Yes	Yes	Yes
Quasi-RDD covenant controls	Yes	Yes	Yes
Industry & year fixed effects	Yes	Yes	Yes
Observations	176,378	176,378	176,378
Adjusted R-squared	0.012	0.004	0.002

Panel B: Propensity score matching estimates

Method	Acquisition		Value-destroying		Value-enhancing		
	ATT	SE	ATT	SE	ATT	SE	Obs
(1) One-to-one nearest neighbor	-0.008***	(0.002)	-0.002**	(0.001)	0.001	(0.001)	23,830
(2) Five-nearest neighbors	-0.007***	(0.002)	-0.002***	(0.001)	0.000	(0.001)	52,459
(3) Kernel	-0.011***	(0.002)	-0.003***	(0.001)	-0.000	(0.001)	176,378
(4) Radius	-0.019***	(0.001)	-0.005***	(0.001)	-0.001	(0.001)	176,378

Panel C: Dealscan RDD estimates

	Dealscan Sample			Dealscan Sample [+/- 20]		
	Acquisition	Value-destroying	Value-enhancing	Acquisition	Value-destroying	Value-enhancing
	(1)	(2)	(3)	(4)	(5)	(6)
Financial covenant violation	-0.022***	-0.007***	-0.001	-0.018***	-0.006***	-0.001
	(0.003)	(0.001)	(0.001)	(0.004)	(0.001)	(0.002)
Distance to threshold	0.002	0.001***	0.000	0.006*	0.001	0.001
	(0.001)	(0.000)	(0.000)	(0.003)	(0.001)	(0.001)
Distance to threshold squared	-0.000**	-0.000***	-0.000***	0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Acquirer controls	Yes	Yes	Yes	Yes	Yes	Yes
Quasi-RDD covenant controls	No	No	No	No	No	No
Industry & year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	59,765	59,765	59,765	14,648	14,648	14,648
Adjusted R-squared	0.010	0.004	0.002	0.011	0.007	0.002

Panel D: Chava and Roberts (2008) RDD estimates

	CR (2008) Sample			CR (2008) Sample [+/- 20]		
	Acquisition	Value-destroying	Value-enhancing	Acquisition	Value-destroying	Value-enhancing
	(1)	(2)	(3)	(4)	(5)	(6)
Financial covenant violation	-0.012**	-0.002	0.000	-0.001	0.001	0.001
	(0.006)	(0.002)	(0.003)	(0.008)	(0.004)	(0.004)
Distance to threshold	0.001	0.001	0.000	0.017*	0.002	0.004
	(0.003)	(0.001)	(0.001)	(0.009)	(0.004)	(0.003)
Distance to threshold squared	-0.001	0.000	-0.000	0.008	0.002	-0.001
	(0.001)	(0.000)	(0.000)	(0.005)	(0.002)	(0.001)
Acquirer controls	Yes	Yes	Yes	Yes	Yes	Yes
Quasi-RDD covenant controls	No	No	No	No	No	No
Industry & year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,217	16,217	16,217	3,345	3,345	3,345
Adjusted R-squared	0.013	0.005	0.003	0.013	-0.002	0.009

Table A.4: Acquirer announcement returns across alternate empirical specifications. This table reports robustness tests for our acquirer 3-day CAR (%) results. In Panel A, we re-estimate the quasi-regression discontinuity specification from our main analysis (Column 2 of Table 6), but with the following alterations. Row (1) double clusters standard errors on firm and year. Row (2) uses only the subsample of completed deals. Rows (3)-(5) replace the dependent variable with alternate acquirer CAR calculations. Panel B reports propensity score matching estimates of the impact of a financial covenant violation on acquirer 3-day CARs. We use a logit regression to estimate the probability that an acquirer is in violation of a covenant as a function of the full set of covariates in the quasi-RDD specification. We then estimate the average treatment effect on the treated (ATT) using the propensity score and four alternative matching protocols. We report ATT estimates using one-to-one nearest neighbor matching in Row (1), five-nearest neighbor matching in Row (2), Gaussian kernel matching in Row (3), and radius matching in Row (4). The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Panel A: Quasi-RDD estimates

Method	Coefficient	Standard Error	Observations
(1) Double cluster on firm and year	1.758***	(0.620)	7,191
(2) Subsample of completed deals	1.704**	(0.681)	6,819
(3) 5-day equal weighted CARs	1.744**	(0.747)	7,191
(4) 3-day value weighted CARs	2.117**	(0.966)	7,191
(5) 5-day value weighted CARs	2.094**	(1.066)	7,191

Panel B: Propensity score matching estimates

Method	ATT	Standard Error	Observations
(1) One-to-one nearest neighbor	1.611*	(0.920)	531
(2) Five-nearest neighbors	1.493**	(0.723)	1,269
(3) Kernel	1.418**	(0.666)	6,736
(4) Radius	2.067***	(0.653)	6,736

Table A.5: Alternate covenant violation indicator. This table presents robustness checks of our results using a four-quarter covenant violation indicator. Panel A presents OLS estimates from regressions that are identical to Table 5, except that we use a one-year violation indicator. Panel B presents OLS estimates from regressions that are identical to Table 6, except that we use a one-year violation indicator. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Panel A: Acquisition activity

	Acquisition	Value-destroying acquisition	Value-neutral acquisition	Value-enhancing acquisition
	(1)	(2)	(3)	(4)
Financial covenant violation	-0.012***	-0.003***	-0.009***	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Acquirer Controls	Yes	Yes	Yes	Yes
Quasi-RDD covenant controls	Yes	Yes	Yes	Yes
Industry & year fixed effects	Yes	Yes	Yes	Yes
Observations	176,378	176,378	176,378	176,378
Adjusted R-squared	0.012	0.004	0.008	0.002

Panel B: Acquirer announcement returns

	Acquirer 3-day CAR (%)			
	(1)	(2)	(3)	(4)
Financial covenant violation	1.477***	1.296**	1.283***	1.189**
	(0.510)	(0.503)	(0.496)	(0.491)
Acquirer Controls	Yes	Yes	Yes	Yes
Quasi-RDD covenant controls	No	Yes	No	Yes
Deal Controls	No	No	Yes	Yes
Industry & year fixed effects	Yes	Yes	Yes	Yes
Observations	7,191	7,191	7,191	7,191
Adjusted R-squared	0.018	0.024	0.053	0.057