Creditor Control of Corporate Acquisitions

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November 2018

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, with the effect concentrated among firms with weak external governance. We conclude that creditors provide valuable corporate governance that benefits shareholders by reducing managerial agency costs.

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

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1. Introduction

The separation of ownership and control incentivizes managers to take privately beneficial actions at the expense of firm value. Although shareholders and creditors can use control rights to discipline managers, their actions may exacerbate incentive conflicts among the suppliers of finance. Indeed, Jensen and Meckling (1976) argue that differences in cash flow rights can lead to shareholder-creditor conflict about firm risk and potential wealth transfers. In this paper, we study corporate acquisitions through the lens of these incentive conflicts to examine how creditor control rights impact borrower investment decisions and shareholder value.

We begin by showing that creditors have the ability and incentive to influence borrower acquisition decisions even beyond their role as a provider of deal financing. In a random sample of 500 loan agreements to public U.S. nonfinancial firms between 1997 and 2015, we find that nearly 75% of loan contracts restrict borrower acquisition activity. Eight percent of these contracts fully prohibit acquisitions without lender approval and an additional two-thirds prohibit acquisitions that do not meet certain criteria. Although strictness varies with borrower quality, we find that acquisition restrictions are widespread across borrowers, lenders, and economic cycles.

Prompted by this evidence, we next examine whether and how increases in creditor control rights affect borrower acquisition decisions, using financial covenant violations as a source of variation in creditor control. Our empirical strategy offers two advantages compared to a direct analysis of acquisition restrictions. First, covenant violations account for multiple channels through which creditors can influence corporate policies. We conjecture that lenders use bargaining power conveyed by a violation to influence acquisitions by tightening restrictions,

reducing credit access, and through behind-the-scenes negotiations.[1] Second, 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.

Tracking the evolution of acquisition restrictions for a random sample of firms, we find that over 24% 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 their credit agreement after a financial 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."[2] This evidence confirms that creditors tighten their control over borrower acquisitions after a covenant violation.

Using a sample of 7,191 acquisitions combined with covenant violation data for 176,378 firm-quarter observations between 1997 and 2015, we find that acquisition activity declines significantly 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 confirms the findings of Chava and Roberts (2008) and Nini et al. (2012), who show that firms reduce capital expenditure and cash acquisitions after a violation.[3]

Our key innovation is using detailed data on mergers and acquisitions (M&A) to infer which types of deals creditors prevent and assess the shareholder value implications. Although

[1] Covenant violations constitute an event of technical default and grant creditors the right to immediately call the loan. Prior research shows that creditors use this opportunity to reduce the availability of credit (Roberts and Sufi, 2009) and influence firm policies through behind-the-scenes negotiations (Ferreira, Ferreira, and Mariano, 2018).
[2] Lee's 2008 10-K: https://www.sec.gov/Archives/edgar/data/58361/000119312508262419/d10k.htm
[3] Our results are also consistent with Denis and Wang (2014), who show that borrowers' investment policies are strongly associated with covenant renegotiations.

creditors undoubtedly act to protect their own interest, their actions likely have spillover effects on shareholders. Agency theory offers ambiguous predictions regarding these effects. On one hand, creditor actions may conflict with shareholder interests due to differences in cash flow rights. On the other hand, both creditors and equity holders share the incentive to limit acquisitions that are motivated by managerial agency problems. If creditors use their control rights primarily to discipline managers, we expect that creditor actions will confer spillover benefits on shareholders.

We find that creditors use their control rights to prevent acquisitions expected to destroy firm value. Our estimates imply that the likelihood of announcing a shareholder value-destroying acquisition falls by over 35% after a covenant violation. Conversely, we find no evidence that creditors limit shareholder value-increasing acquisitions. 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 cumulative abnormal returns (CARs) than deals by similar non-violators. Moreover, violators are more likely to withdraw a bid if they do experience a negative announcement return, suggesting that creditors intervene in poor acquisition attempts by rescinding deal financing or through behind-the-scenes negotiations.

To bolster the interpretation that creditor control rights provide spillover benefits to equity holders, we test whether our results vary with the strength of external governance mechanisms. If equity holders already prevent managers from pursuing private benefits, we expect to observe smaller creditor effects in well governed firms. We form proxies for external governance based on prior research that shows agency costs are most prevalent among firms without blockholders and firms operating in uncompetitive industries (Shleifer and Vishny, 1986; Giroud and Mueller, 2010, 2011). Consistent with our hypothesis, the decline in value-destroying acquisitions and increase in acquirer returns is concentrated among firms with weak external governance. These results suggest

a synergy between creditor and equity governance.

Like all M&A research that uses announcement returns as a measure deal of quality, we acknowledge that CARs are an imperfect proxy and may contain noise stemming from deal anticipation or information about the standalone value of the acquirer. Therefore, we examine target characteristics as an additional test for shareholder-creditor conflict. Amihud and Lev (1981) argue that managers with career concerns have the incentive to engage in risk-reducing activities, such as diversifying acquisitions, even if they lower shareholder value. If creditors share this incentive because of their concave payoff structure, they may use their control rights to encourage firms to "play it safe" by limiting risky yet productive deals or by encouraging diversifying acquisitions.[4] However, our evidence refutes this hypothesis. First, more than 40% of our sample loan agreements explicitly *prohibit* diversifying acquisitions. Second, we find that firms in violation of a covenant are less likely to acquire a firm outside of their industry. This evidence is consistent with the view that diversification benefits managers at the expense of firm value and that creditors actively take steps to limit such deals.

Throughout the paper, we highlight how identification challenges might affect our inferences. For example, violators may earn higher CARs if acquisition announcements signal the relaxation of financial constraints. However, this alternative explanation is unlikely to drive our results for three reasons. First, we note that the typical violator is far from insolvent and that violations reflect a downward change in performance rather than a low absolute level. Second, we show that violators and matched non-violators experience similar balance sheet changes around acquisitions, suggesting that violators are not disproportionately acquiring "cash cows" to relieve

[4] Lewellen (1971) argues that diversifying acquisitions benefit creditors by reducing default risk if underlying assets are not perfectly correlated. Gormley and Matsa (2011) suggest that the joint incentive of creditors and managers to "play it safe" may amplify managerial agency costs, resulting in more diversifying deals.

financial constraints. Finally, although we find that bank-financed deals earn higher average CARs (as in Bharadwaj and Shivdasani, 2003), violators are less likely to finance an acquisition with a bank loan and earn higher CARs even when we control for the source of deal financing.

This paper adds to the literature that uses M&A as a setting to study incentive conflicts among managers and the suppliers of finance. Though many papers examine the mechanisms that equity holders use to improve acquisition outcomes,[5] little is known about the role that creditors play in M&A beyond deal financing. We contribute by showing that creditors use control rights to influence borrower acquisition decisions. In doing so, we add several novel insights to the growing literature that argues creditors influence corporate decisions even outside the zone of insolvency.[6]

First, an open question in this literature is whether creditors curtail specific investments or simply restrict credit access and allow managers to maintain discretion within expenditure limits. We show that creditors impose granular restrictions on specific investments and tighten these restrictions after a covenant violation. The frequency with which lenders prohibit acquisitions without their consent implies that they play an active role in large investment decisions.

Second, prior research is unable to discern the riskiness of foregone investment after a covenant violation. Exploiting acquisitions as a discrete observable investment decision, we find no evidence that creditors force borrowers to inefficiently reduce firm risk. Instead, we find that creditors restrict borrowers from acquiring targets outside of their line of business, suggesting that creditors view diversification as costly. On the surface, this finding contrasts with Acharya, Amihud, and Litov (2011), who show that firms located in countries with stronger creditor rights

[5] This literature identifies shareholder rights and voting (Masulis, Wang, and Xie, 2007; Li, Liu, and Wu, 2018), 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) as important determinants of acquirer returns.

[6] Extant literature shows that financial covenant violations are associated with declines in a broad range of investment and financial policies, including capital expenditure, leverage, and employment (Chava and Roberts, 2008; Roberts and Sufi, 2009; Falato and Liang, 2016).

in bankruptcy engage in more value-reducing diversifying acquisition. However, these findings can be reconciled under Acharya et al.'s interpretation that their results reflect the choice of self-interested managers to ensure job security by trading off corporate value for lower risk. Our analysis suggests that creditors seek to limit this value destruction using their available set of control rights. More broadly, our results contribute to the literature that argues diversification is driven by managerial agency problems and lowers firm value (Morck, Shleifer, and Vishny, 1990; Denis, Denis, and Sarin, 1997; Gormley and Matsa, 2011, 2016).

Finally, prior research draws mixed conclusions about the shareholder value implications of creditor influence. Güner, Malmendier, and Tate (2008) argue that conflicting interests cause firms to make worse investment decisions when a banker joins the board. Ertan and Karolyi (2016) show that stock prices are negatively correlated with the estimated likelihood of a covenant violation and conclude that shareholders expect creditors 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.[7] We complement these studies by analyzing short-run stock price reactions to large corporate events and find that creditors use control rights to limit value-destroying acquisitions. In sum, our evidence is consistent with economic models that show state-contingent creditor control rights can produce spillover benefits for shareholders by reducing managerial agency costs.[8]

[7] Nini et al. (2012) examine stock returns over two years following a violation and find that violators earn excess returns relative to a four-factor asset pricing model. Ersahin et al. (2017) use establishment-level data to show that covenant violators close relatively unproductive plants.

[8] For example, the incomplete contracting models of Aghion and Bolton (1992) and Dewatripont and Tirole (1994) show that debt contracts can be written such that decision rights optimally shift to creditors when private benefits are likely to lead to inefficient outcomes.

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.[9] In this section, we provide empirical support for the second two channels by showing that creditors restrict borrower acquisitions and tighten these restrictions in renegotiations after a covenant violation.

2.1 Acquisition restrictions in loan contracts

As discussed in Wight, Cooke, and Gray (2009), the standard private credit agreement includes negative covenants that limit certain borrower activities without explicit permission from lenders. 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 500 loan contracts from U.S. nonfinancial firms between 1997 and 2015.[10]

For each contract, we search the set of negative covenants for evidence that creditors restrict the 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 partial restrictions: (i) a prohibition of deals above a certain size ("Expenditure limit"), (ii) a prohibition on deals that would cause the borrower to violate their existing covenants on a pro forma basis ("Pro forma compliance"), (iii) a prohibition on deals that do not meet a non-covenant

[9] Roberts (2015) shows that the typical loan is renegotiated about every 9 months and the outcome of the renegotiation reflects changes in the borrower's financial health.

[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 500 contracts to minimize collection costs while providing a relatively large sample.

financial test ("Financial test"), and (iv) a prohibition on deals 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 acquisition, and the target must be in a similar line of business.[12]

Table 1 summarizes results for our 500 contracts and shows that acquisition restrictions are widespread across borrowers, lenders, and economic cycles. We find that 8% of private credit agreements fully restrict borrower acquisition activity and an additional two-thirds impose a partial restriction. In addition to being pervasive, Table 1 shows that acquisition 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 restriction, but even the largest and safest firms in our sample often face a partial restriction on acquisitions. Restrictions are more common for loans originated during the 2001-2002 and 2008-2009 recessions, but they are also used extensively during economic expansions. Larger lenders tend to impose fewer restrictions but, in unreported analyses, we find that this reflects matching of large borrowers and

[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

large lenders. Although we believe that creditors can also influence acquisitions as a provider of financing and through behind-the-scenes negotiations, the evidence in Table 1 suggests that contractual restrictions serve as a powerful tool to control borrower acquisition decisions.

2.2 Changes in acquisition restrictions following covenant violations

Our research design, which we describe more fully in Section 3, uses financial covenant violations to identify periods of heightened creditor control. The hypothesis underlying our approach 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 with the SEC 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

[13] We extend the Nini et al. (2012) covenant violation dataset through 2015 and match on the full set of controls in Table 4 Column (2), which we describe 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.

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

Table 2 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 restriction. 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 2, supporting the validity of our matching procedure.

The middle panel displays the frequency that 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 restrictions with a 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 definition, the bottom row reports that over 24% of contracts tighten within six months of violation. For comparison, only 4% of non-violators experience tightening. The 20% difference is economically significant

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

and implies that covenant violations lead to a sharp increase in creditor control of corporate acquisitions.

3. Empirical design

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, creditors can use control rights to influence acquisitions through multiple channels. Analysis of contractual restrictions alone would fail to account for creditors' role as a provider of deal financing and their ability to affect borrowers through behind-the-scenes negotiations. 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. But, 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 the experience of firms that are not in violation of a financial covenant as a comparison group to estimate the counterfactual outcome for covenant violators. Covenant violations, of course, are not randomly assigned to firms. By construction, violations 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 that are associated with acquisition outcomes. Throughout the analysis, we highlight how these factors could affect inferences and take the following 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 regressions standard to the acquisition literature to account for observable differences between violators and non-violators. Following Moeller, Schlingemann, 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]

We also 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

[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.

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 any independent relationship between the variables and acquisition decisions. We also include one year lags to control for firm conditions when the loan contracts were negotiated and to proxy for the unobserved covenant thresholds. Together, 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

$$\begin{aligned} y_{i,t} = \beta \cdot Violation_{i,t} + \theta_1 \cdot AcquirerControls_{i,t-1} + \theta_2 \cdot CovenantControls_{i,t-1} \\ + \theta_3 \cdot HigherOrderCovenantControls_{i,t-1} + \theta_4 \cdot CovenantControls_{i,t-5} \\ + \theta_5 \cdot DealControls_{i,t} + Industry_i + Year_t + \varepsilon_{i,t} \end{aligned} \quad (1)$$

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 in the acquisition literature 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% tails in all analyses.

We draw our M&A sample from the Securities Data Company (SDC) Platinum Mergers

[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.

and Acquisitions database. Following prior research (e.g. Moeller et al., 2004; Masulis et al., 2007), we filter out 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 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 creditor control

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. Moreover, this window corresponds with the six-month average bidding process in Boone and Mulherin (2007). Thus, our analyses test whether creditors intervene in potential acquisitions that are on the near-term horizon. In robustness tests reported in the Internet Appendix, we verify that our results are robust to using a one-year trailing indicator.

4.3 Sample characteristics

Panel A of Table 3 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

[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.

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. Over 40% of firms in their sample report at least one covenant violation between 1997 and 2008. Although the propensity to violate is negatively related to firm size, the fraction of firms that ever report a violation is substantial across the distribution. This fraction ranges from 25% (for firms with greater than $5 billion in assets) to 44% (for firms with less than $100 million in assets).

Panel B of Table 3 presents acquirer characteristics split by violation status. Violators are smaller and have experienced weaker performance than non-violators, though the typical violator is far from insolvent. The median violating acquirer breached 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, 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 examine how covenant violations affect acquisition behavior, focusing on the frequency of acquisitions and stock price reaction to deal announcements. These analyses test whether creditors use their control rights to influence borrower acquisitions and examine whether they do so in a way that conflicts or is congruent with shareholder preferences.

5.1 Acquisition activity

Figure 1 plots two measures of acquisition activity around a covenant violation. The graphs reveal that firms are one-half as likely to make an acquisition while in violation of a financial covenant and show that total acquisition expenditure falls by about one-third. These findings are consistent with prior research that documents a decline in investment activity following a covenant violation and extends the findings of Nini et al. (2012) to a large set of acquisitions made with all forms of payment.

As noted in Section 4, firms in violation of a covenant differ from firms that have not recently breached a threshold. Thus, it is possible that unobserved differences between violators and non-violators could produce a spurious relation between covenant violations and acquisition activity. For example, small firms are more likely to violate a covenant and less likely to make an acquisition. However, size and other factors do not vary over short horizons so, if constant unobserved firm characteristics drive our results, we should see a similar effect for firms immediately pre-violation. 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 regressions in the form of Equation (1) to control for known factors that influence acquisition decisions. The effect of a covenant violation remains large and statistically significant after controlling for observables (Panel A, Table 4). The estimate in Column (2) implies that firms in violation of a covenant are 1.2% less likely to announce an acquisition compared to similar non-violators. This effect is large relative to the unconditional likelihood of a deal in 3.9% of firm-quarters. Columns (3) and (4) examine acquisition expenditure scaled by lagged assets as an alternative measure of acquisition activity. Acquisition expenditure is the total

deal consideration announced per firm-quarter and takes a value of zero if a firm does not undertake a deal in that quarter. This alternative measure produces similar inferences, as we continue to find that acquisition activity falls when firms are in violation of a covenant.

5.2 Which acquisitions do creditors prevent?

Positive NPV acquisitions create value for both shareholders and debtholders, so we do not expect creditors to prevent all acquisitions.[19] Instead, we expect creditors to constrain unproductive acquisitions that may negatively impact the borrower's credit risk. The intuition for this hypothesis is developed by incomplete contracting theory (Aghion and Bolton, 1992) and agency theory (Jensen and Meckling, 1976; Jensen 1986). If creditors primarily use control rights to stop bad investments motivated by managerial agency problems, we expect that acquisitions announced by violators will earn higher announcement returns.

On the contrary, Amihud and Lev (1981) argue that managers with career concerns have the incentive to engage in risk-reducing activities. If creditors share this incentive because of their concave payoff structure, we hypothesize that covenant violations will lead to deals that are privately optimal for managers and creditors but destroy shareholder value. Indeed, Gormley and Matsa (2011) show that risk-aversion leads managers to make value-destroying acquisitions in response to an increase in liability risk and conjecture that "a high amount of financial leverage that moderates managerial agency problems in normal times may amplify another managerial agency conflict when the firm encounters an adverse shock." Under this alternative, we expect that acquisitions announced by violators will earn lower announcement returns.

We test these hypotheses by examining the probability of announcing a shareholder value-destroying or value-enhancing deal. We classify deals as value-destroying (enhancing) if an

[19] It is possible that a positive NPV acquisition increases firm risk substantially enough that it reduces creditor value. In such cases, equity holders should be willing to compensate creditors to permit the acquisition.

acquirer earns a 3-day CAR that is more than one standard deviation below (above) our sample mean CAR.[20] This approach is similar to Paul (2007) and Chen, Harford, and Li (2007), who test whether directors and institutional monitors can prevent value-destroying acquisitions. 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 deals from ever being announced.

Panel B of Table 4 reports 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 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.

5.3 Acquirer announcement returns

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

[20] 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.

Masulis et al. (2007) and estimate regression models that control for these differences. Table 5 shows that effect of a covenant violation on acquirer returns remains large and statistically significant after controlling for confounding factors. Coefficients on control variables in Column (1) compare closely with those in related studies. For example, acquirer returns are inversely related to firm size and market-to-book ratio, consistent with Moeller et al. (2004).

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).[21] 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 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.

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

[21] 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.

equity issuance leads to lower announcement returns (Travlos 1987).[22] 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.

5.4 Deal completion

Thus far, we have shown that the likelihood of announcing a value-destroying deal falls by over 35% of the sample mean when firms are in violation of a covenant and that shareholders earn over 1.5% higher returns for deals that creditors permit. Yet, it is unlikely that creditors can perfectly predict which acquisitions will create value. If creditors learn additional information about deal quality after announcement, we expect that firms in violation of a covenant will be more likely to withdraw a bid. Results in Table 6 support this conjecture. Covenant violators are 3.1% less likely to complete an acquisition, particularly with negative announcement returns. The economic magnitude of this effect is large, given that the unconditional frequency of bid withdrawal is 5%. This result suggests that creditors intervene in poor acquisition attempts by rescinding deal financing or pressuring management to call off the deal.

5.5 Heterogeneity with respect to external 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 governance mechanisms do not ensure that managers maximize shareholder value. Chava, Kumar, and Warga (2010) argue that managerial agency risk is an important determinant of creditor restrictions. Thus, if creditor control benefits shareholders by complementing other governance mechanisms,

[22] In unreported robustness checks, we control for interactions between method of payment and target listing status because Chang (1998) and Fuller et al. (2002) find that all-stock acquisitions of private targets earn higher returns, and Eckbo, Makaew, and Thorburn (2017) find that acquirer returns are negatively related with equity payment for public targets. Our results are nearly identical under this alternate specification.

we expect creditor effects to be concentrated among poorly governed firms.

We investigate this hypothesis by testing the relation between covenant violations and acquirer returns on subsamples split by governance characteristics. The goal of each split is to proxy for "weakness" in a particular form of governance. We recognize that our governance splits are not perfect, and we do not assume that "weak" governance is necessarily value-reducing for all firms. As shown in Coles, Daniel, and Naveen (2008), one size of corporate governance rarely fits all firms. Nevertheless, we believe that consistent evidence across well-known proxies for corporate governance supports the plausibility of our interpretation.

Table 7 presents the results. Columns (1) and (2) report ordinary least squares regressions estimated on subsamples split by the presence of a blockholder that owns at least 10% of shares outstanding. Since institutional shareholders have the financial incentive to monitor management (Shleifer and Vishny, 1986), creditor effects should be concentrated among firms without a blockholder. Consistent with this hypothesis, Columns (1) and (2) show that the effect of a covenant violation is significant for firms with weak institutional monitoring and insignificant for firms with a blockholder. However, the difference between subsamples is not significant at conventional levels in either specification. This insignificance is not surprising given the noise in our proxy for shareholder monitoring. Further, we do not expect covenant violations to have the opposite effect on firms with strong equity-governance; we merely hypothesize that creditor effects should be most prevalent among firms with weak shareholder monitoring.[23]

Columns (3) and (4) provide further indication that our findings are related to managerial agency problems. Giroud and Mueller (2010, 2011) demonstrate that firms in noncompetitive

[23] We also split the sample into "dictator" and "democracy" firms using the classification in Harford, Humphery-Jenner, and Powell (2012). We find that the effect is concentrated among firms with weak shareholder rights, but do not report the results because data necessary to construct the G-index is only available prior to 2008.

industries benefit more from strong shareholder governance. Our estimates highlight a similar relation between industry competition and creditor control rights; creditor effects are stronger for firms in the top tercile of industry concentration than for those in the bottom. In sum, results in Table 7 suggest that creditor governance can serve as a substitute for traditional equity governance to limit managerial agency problems.

6. Additional evidence

Results thus far suggest that creditors provide valuable corporate governance that benefits shareholders by reducing managerial agency costs. Although we believe that M&A announcements provide the best possible empirical setting to assess the impact of creditor control rights, we recognize that CARs are an imperfect measure of deal quality. In this section, we examine target characteristics as an alternative measure of acquisition outcomes and explore the robustness of our conclusions to alternative explanations.

6.1 Target characteristics

Harford et al. (2012) show that entrenched managers destroy value by disproportionately engaging in diversifying acquisitions and avoiding private targets. If creditors use their control rights to prevent wealth destruction, we expect covenant violations to reverse these trends. Conversely, if creditors use control rights to engage in wealth transfers, we expect creditors to encourage diversification or limit risky yet possibly productive deals (such as private targets).

We first proxy for deal riskiness using the target's line of business. Columns (1) and (2) of Table 8 Panel A show that acquirers in violation of a financial covenant are 4.7% to 4.9% 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 or reduce risk via diversification.

We next use public listing status as a proxy for target risk. We conjecture that private targets are riskier due to less public information and a lack of market prices to assess value. Here again, we find that acquirers in violation of a covenant do not shy away from risky deals. Estimates in Column (4) suggest that violators are 6.5% more likely to target a private firm than similar non-violators, which represents a 15% increase relative to the unconditional mean.

Finally, we examine the probability of buying a private target entirely with stock. Hansen (1987) suggests that acquirers use stock offers when there is high uncertainty in the target's valuation since the ultimate value of the consideration depends on the realized value of the target. Thus, the interaction of private listing status and equity payment may provide a more precise proxy for deal riskiness. Once again, we find no evidence that creditors prevent borrowers from engaging in risky acquisitions. The effect of a violation on the likelihood of acquiring a private target with all-stock payment is positive and marginally significant in Column (5) and becomes insignificant when we impose the quasi-RDD controls in Column (6). In sum, we find no evidence that violators use acquisitions to inefficiently reduce firm risk.

Although our findings suggest that creditors prevent acquisitions expected to destroy firm value, observable target characteristics do not fully explain the higher acquirer CARs earned by violators relative to non-violators. Panel B 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.[24] These findings suggest that creditors do not impose blanket restrictions to

[24] 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

inefficiently reduce firm risk, but rather monitor the quality of borrower investments and prevent value-destroying acquisitions when they have the ability and incentive to do so.[25]

6.2 Acquirer balance sheet changes

One potential 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. Unfortunately, only a small number of violators acquirer public targets so we have insufficient data to analyze target balance sheets. As an alternative, we examine acquirer balance sheet changes using a propensity matched sample of acquisitions. If it is the case that creditors encourage the acquisition of "cash cows", 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 4, Column 2). Table 9 reports summary statistics for the matched sample around completed acquisitions. 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 pre-acquisition: tangible assets, cash holdings, current ratio, and leverage. Further, Panel B shows that changes in these variables around an acquisition do not significantly differ between violators

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.

[25]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.

and non-violators. These results suggest that violators do not disproportionately avoid risky targets with weak balance sheets or disproportionately acquire "cash cows" to lower firm risk.

6.3 Alternative explanation: financial constraints

One possible alternative explanation for our results is that violators make fewer acquisitions because they are financially constrained and earn higher CARs because M&A announcements signal the relaxation of these constraints. However, since deal financing is one channel through which creditors can influence acquisitions, the difference between financial constraints as a source of endogeneity and financial constraints as a creditor effect is quite subtle. For instance, a creditor effect implies that covenant violations enable creditors to limit borrower acquisitions by reducing their line of credit. An endogenous relation, however, could imply that (i) violators have a different acquisition opportunity set than non-violators absent creditor intervention (e.g., if violators are cash-poor) and/or (ii) a hypothetical acquisition of the same target by a violator and non-violator would produce different acquirer CARs (e.g., if it reveals more positive news about the lender's willingness to extend credit). This distinction is hard to separate empirically, but we take the following steps to assess the scope for these alternatives.

First, we reiterate that acquirers in violation of a covenant are not in severe financial distress and that our regressions control for observable measures of financial health. Indeed, Panel A of Table 9 shows that violators frequently have sufficient liquidity to complete a cash acquisition. Second, analysis of post-acquisition balance sheet changes in Panel B provides no evidence to support the conjecture that violators use acquisitions to escape financial constraints.

Third, we examine how acquirer returns vary with deal financing. If a stronger public signal is responsible for higher announcement returns, we would expect positive CARs to be concentrated in deals where an acquirer received new bank debt. We explore this alternative hypothesis by hand-

collecting financing data from acquirer 10-K's for our propensity matched sample of acquisitions. We code whether the deal is financed with a new bank loan or an amendment increasing the amount of an existing bank loan ("Bank Loan") and whether the acquirer issues another form of debt to finance the deal ("Non-bank Credit").

Panel A of Table 10 shows that violators are less likely to fund an acquisition with new bank debt compared to matched non-violators. This result is consistent with Roberts and Sufi (2009), who show that covenant violations lead to a reduction in credit, and provides supports for one of the channels through which creditor control rights affect acquisition activity. Notably, we find no difference in the propensity to issue non-bank credit, suggesting that violators do not substitute corporate bonds for banks loans.

Panel B shows that CARs are indeed higher for acquirers that obtain a new bank loan. These results are 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, Column (4) shows that the effect of a covenant violation is unchanged after controlling for debt financing. These results highlight that creditors influence M&A outcomes beyond their role as a provider of deal financing. We conclude that the relaxation of financial constraints is not the sole source of positive announcement returns for acquirers in violation of a covenant.

6.4 Alternative empirical specifications

Previous research proposes several empirical strategies to identify the effect of a covenant violation. In the Internet Appendix, we discuss trade-offs of employing the quasi-RDD of Roberts and Sufi (2009) and Nini et al. (2012) versus the sharp regression discontinuity design of Chava and Roberts (2008). We probe the robustness of our results to alternative identification strategies, covenant violation definitions, and CAR calculations. Though our main results are similar across

specifications, we conclude that statistical power considerations favor using the quasi-RDD because of the small number of firms that announce an acquisition while in violation of a financial covenant.

7. Conclusion

An extensive M&A literature argues that acquisition decisions are susceptible to a variety of conflicts of interest (see Betton, Eckbo, and Thorburn (2008) for a review). 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 following a covenant violation. In a random sample of private credit agreements, we find that 8% of loan contracts fully prohibit acquisitions without lender approval and an additional two-thirds prohibit acquisitions that do not meet certain criteria. This contractual 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 rights, 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, with the effect concentrated among firms with weak external governance. We find no evidence that creditors use their control rights to inefficiently reduce firm risk. Strikingly, more than 40% of our sample loan agreements explicitly prohibit diversifying acquisitions. We conclude that creditors provide valuable corporate governance that benefits shareholders by reducing managerial agency costs.

Although there are strong theoretical foundations for potential conflicts between debt and equity holders, our empirical results are consistent with Fama and Miller (1972, pp. 180), who conjecture, “From a practical viewpoint, however, situations of potential conflict between bondholders and shareholders … are probably unimportant. In general, investment opportunities that increase a firm's market value by more than their cost both increase the value of the firm's shares and strengthen the firm’s future ability to meet its current bond commitments.” Likewise, our findings suggest that creditors and equity holders share congruent preferences to limit investments that would otherwise decrease a firm’s market value.

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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. Acquisition expenditure is the total deal consideration announced during a firm-quarter, scaled by lagged assets. Bars represent group means and lines denote 90% confidence intervals.

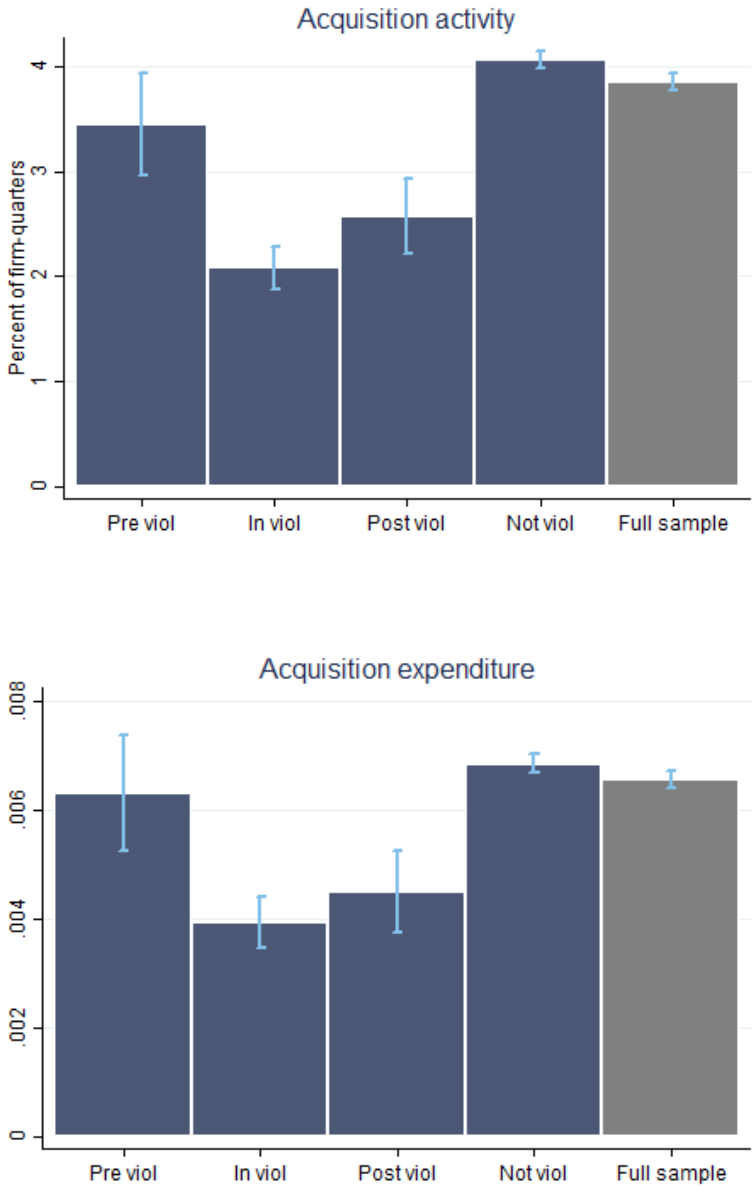

Figure 2: Acquirer announcement returns. This figure displays acquirer announcement returns by financial covenant violation status for a sample of 7,191 mergers and acquisitions announced by 2,907 U.S. nonfinancial firms from 1997 to 2015. We classify firms as "in viol" for two quarters after they report 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.

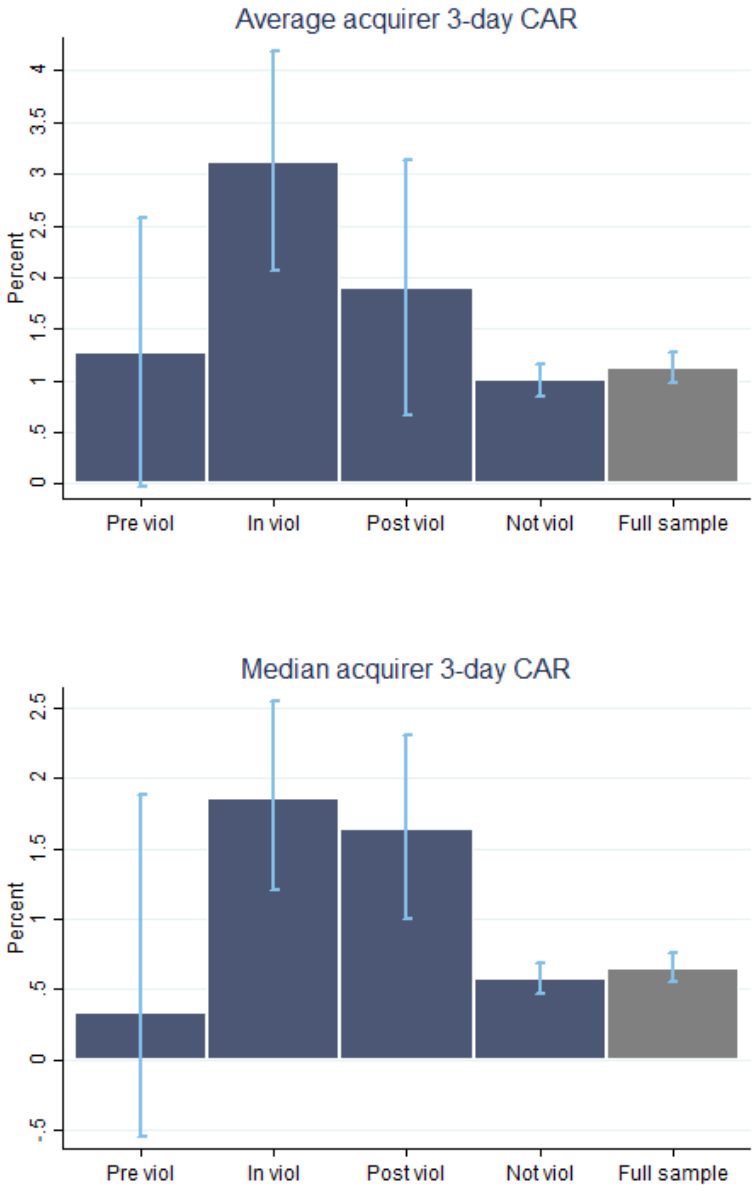

Table 1: Frequency of acquisition restrictions. This table reports the percentage of private credit agreements that contain an acquisition restriction using a random sample of 500 contracts from U.S. nonfinancial firms in EDGAR and Compustat between 1997 and 2015. A "Full Restriction" prohibits all acquisitions 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.

					Partial Restrictions			
	N	**Full Restriction**	**Partial Restriction**	**No Restriction**	**Expenditure Limit**	**Pro Forma Compliance**	**Financial Test**	**Prohibit Diversifying**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All credit agreements	500	8%	66%	26%	40%	44%	24%	41%
By borrower size								
Less than $500M	109	20%	64%	16%	47%	39%	20%	39%
$500M to $5,000M	238	4%	84%	12%	53%	60%	34%	53%
Greater than $5,000M	153	5%	41%	54%	14%	24%	10%	23%
By borrower rating								
No credit rating	242	11%	76%	13%	50%	54%	28%	48%
Speculative-grade rating	124	5%	84%	11%	53%	56%	36%	51%
Investment-grade rating	134	4%	32%	64%	10%	16%	5%	18%
By business cycle								
Recession	80	10%	56%	34%	35%	35%	20%	34%
Expansion	420	7%	68%	25%	41%	46%	25%	42%
By lender								
Top 5 lender	348	6%	65%	29%	39%	44%	24%	39%
Other lender	152	13%	69%	18%	43%	45%	24%	45%

Table 2: 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 4 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 N = 106	**Non-Violators** N = 94	**Difference**	***p*-value**
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 3: 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
Blockholder (0/1)	0.352	0.478	0.000	0.000	1.000	5,586
HHI	0.153	0.143	0.058	0.105	0.193	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

Table 3: 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 4: 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. Acquisition activity is an indicator that equals one if an acquisition is announced during a firm-quarter, and zero otherwise. Acquisition expenditure is the total deal consideration announced during a firm-quarter, scaled by lagged assets. 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.

Panel A: Acquisition activity

	Acquisition activity		**Acquisition expenditure**	
	(1)	(2)	(3)	(4)
Financial covenant violation	-0.016***	-0.012***	-0.002***	-0.001***
	(0.002)	(0.002)	(0.000)	(0.000)
Size	0.001***	0.001***	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Stock price runup	0.011***	0.010***	0.003***	0.002***
	(0.001)	(0.001)	(0.000)	(0.000)
Market-to-book ratio	0.000	0.041***	0.001***	0.008***
	(0.000)	(0.003)	(0.000)	(0.001)
Operating cash flow / assets	0.032***	0.030***	0.005***	0.004***
	(0.003)	(0.004)	(0.001)	(0.001)
Leverage ratio	0.009***	0.112***	0.000	0.013***
	(0.003)	(0.023)	(0.001)	(0.005)
Interest expense / assets		-0.044		0.056
		(0.233)		(0.047)
Net worth / assets		0.051***		0.010***
		(0.008)		(0.002)
Current ratio		0.003**		0.001**
		(0.001)		(0.000)
Lagged & higher-order controls	No	Yes	No	Yes
Industry & year fixed effects	Yes	Yes	Yes	Yes
Observations	176,378	176,378	176,378	176,378
Adjusted R-squared	0.009	0.012	0.007	0.010
Unconditional mean	0.039	0.039	0.007	0.007
% Δ relative to mean	-40.542	-29.976	-28.715	-18.413

Panel B: Acquisition quality

	Value-destroying acquisition	Value-neutral acquisition	Value-enhancing acquisition
	(1)	(2)	(3)
Financial covenant violation	-0.003***	-0.009***	-0.000
	(0.001)	(0.001)	(0.001)
Size	0.000***	0.000***	-0.000
	(0.000)	(0.000)	(0.000)
Stock price runup	0.002***	0.006***	0.003***
	(0.000)	(0.001)	(0.001)
Market-to-book ratio	0.007***	0.028***	0.006***
	(0.001)	(0.003)	(0.001)
Operating cash flow / assets	0.004**	0.020***	0.006***
	(0.002)	(0.003)	(0.002)
Leverage ratio	0.027***	0.071***	0.016*
	(0.009)	(0.018)	(0.009)
Interest expense / assets	-0.050	-0.001	0.043
	(0.088)	(0.180)	(0.091)
Net worth / assets	0.010***	0.036***	0.007**
	(0.003)	(0.006)	(0.003)
Current ratio	0.000	0.002*	0.001**
	(0.001)	(0.001)	(0.001)
Lagged & higher-order controls	Yes	Yes	Yes
Industry & year fixed effects	Yes	Yes	Yes
Observations	176,378	176,378	176,378
Adjusted R-squared	0.004	0.008	0.002
Unconditional mean	0.008	0.025	0.007
% Δ relative to mean	-37.719	-35.608	-2.332

Table 5: 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 6: Deal completion. This table reports OLS estimates of the effect of a financial covenant violation on deal completion. The sample consists of 7,191 deals announced by 2,907 U.S. nonfinancial firms from 1997 to 2015. The dependent variable is an indicator that equals one if an announced acquisition is completed, and zero otherwise. Column (1) reports estimates from a regression on the full sample of M&A announcements. Columns (2) and (3) report estimates from regressions on the subsample of announcements that earned negative and positive CARs, respectively. 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 Completed		
	Full Sample	*Negative CAR Sample*	*Positive CAR Sample*
	(1)	(2)	(3)
Financial covenant violation	-0.031*	-0.057**	-0.017
	(0.017)	(0.029)	(0.020)
Size	-0.001***	-0.001	-0.001**
	(0.000)	(0.001)	(0.001)
Stock price runup	0.020***	0.024**	0.019***
	(0.006)	(0.011)	(0.007)
Market-to-book ratio	-0.010	-0.015	-0.007
	(0.019)	(0.029)	(0.023)
Operating cash flow / assets	0.035	0.086	-0.010
	(0.044)	(0.068)	(0.060)
Leverage ratio	0.045	-0.058	0.124
	(0.120)	(0.197)	(0.158)
Interest expense / assets	-0.446	2.664	-2.346
	(1.225)	(1.989)	(1.511)
Net worth / assets	0.143**	0.194**	0.091
	(0.067)	(0.083)	(0.101)
Current ratio	0.008	0.017	-0.001
	(0.009)	(0.014)	(0.011)
Lagged & higher-order controls	Yes	Yes	Yes
Industry & year fixed effects	Yes	Yes	Yes
Observations	7,191	3,178	4,013
Adjusted R-squared	0.022	0.030	0.017
p-value of difference		0.255	

Table 7: Heterogeneity with respect to 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 4 and 5 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 shareholder monitoring by splitting the sample based on the presence of a 10% blockholder. In Columns (3) and (4), we proxy for the disciplining effect of product market competition by sorting firms into the top and bottom HHI terciles. 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 (%)**
	(1)	(2)	(3)	(4)
	No blockholder		*Top HHI tercile*	
Financial covenant violation	-0.003***	1.748**	-0.003**	2.617**
	(0.001)	(0.848)	(0.001)	(1.197)
Observations	91,385	3,622	69,087	2,776
Adjusted R-squared	0.004	0.032	0.003	0.038
	Blockholder		*Bottom HHI tercile*	
Financial covenant violation	-0.001	1.386	0.001	-0.374
	(0.001)	(1.580)	(0.002)	(1.324)
Observations	49,813	1,964	51,321	2,036
Adjusted R-squared	0.005	0.029	0.004	0.035
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	0.325	0.838	0.000	0.088

Table 8: 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		**Private target**		**Private all-stock**	
	(1)	(2)	(3)	(4)	(5)	(6)
Financial covenant violation	-0.049*	-0.047*	0.067**	0.065**	0.029*	0.024
	(0.027)	(0.028)	(0.031)	(0.031)	(0.017)	(0.017)
Size	-0.001	-0.001	-0.009***	-0.008***	-0.001***	-0.001***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Stock price runup	0.012	0.018	0.017	0.016	0.001	0.005
	(0.011)	(0.013)	(0.013)	(0.015)	(0.008)	(0.008)
Market-to-book ratio	0.003	0.034	0.010*	0.033	0.023***	0.010
	(0.005)	(0.041)	(0.005)	(0.043)	(0.004)	(0.024)
Operating cash flow / assets	-0.067*	0.052	-0.094**	-0.144	-0.180***	-0.188***
	(0.040)	(0.085)	(0.045)	(0.095)	(0.031)	(0.056)
Leverage ratio	0.010	0.507*	-0.180***	0.073	-0.052***	0.016
	(0.038)	(0.269)	(0.035)	(0.282)	(0.015)	(0.145)
Interest expense / assets		4.694*		-2.319		1.533
		(2.814)		(2.805)		(1.406)
Net worth / assets		-0.014		0.175		-0.154*
		(0.146)		(0.140)		(0.089)
Current ratio		-0.004		-0.003		-0.008
		(0.018)		(0.019)		(0.009)
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	0.094	0.101	0.079	0.092

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 9: 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 4 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 10: Acquisition financing. This table presents the role of debt financing 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 4 Column (2). After the match, we eliminate deals for which we are unable to hand-collect financing data from the acquirer's 10-K. This process yields a sample of 524 deals by 449 acquirers. Panel A reports the fraction of deals that are financed with debt. Bank loan is an indicator that equals one if the deal is financed with either a new bank loan or an amendment increasing the amount of an existing bank loan, and zero otherwise. Non-bank credit is an indicator that equals one if the acquirer issues another form of debt to finance the deal, and zero otherwise. Panel B reports OLS estimates of the effect of debt financing on acquirer CARs. 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 debt financing

	All (N=524)	**In Violation** (N=282)	**Not in Viol** (N=242)	***p*-value of difference**
Bank loan (0/1)	0.313	0.270	0.364	0.021
Non-bank credit (0/1)	0.122	0.131	0.112	0.495
Bank loan or Non-bank credit (0/1)	0.376	0.344	0.413	0.103

Panel B: Debt financing and acquirer returns

	Acquirer 3-day CAR (%)			
	(1)	(2)	(3)	(4)
Financial covenant violation	2.341**			2.482***
	(0.915)			(0.917)
Bank loan		1.576*		1.735*
		(0.924)		(0.919)
Non-bank credit			1.588	1.136
			(1.256)	(1.264)
Intercept	0.738	1.505***	1.804***	-0.019
	(0.598)	(0.580)	(0.487)	(0.724)
Observations	524	524	524	524
Adjusted R-squared	0.013	0.005	0.003	0.021

Appendix: Variable definitions. CCM denotes the CRSP-Compustat Merged Database. TFN denotes the Thomson Reuters s34 Master File. 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
Acquisition expenditure	SDC & CCM	Total deal consideration announced during a firm-quarter, scaled by lagged total assets (*atq*)
Blockholder	TFN	Indicator that equals one if an institutional investor owns more than 10% of shares outstanding, 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
HHI	CCM	Sales based Herfindahl-Hirschman Index calculated at the 3-digit SIC level following Giroud and Mueller (2010)
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*)
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]

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
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

November 2018

1. 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 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.

In Table A.1, we assess the robustness of our results under a range of alternate specifications. Panel A 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*). *Full Sample* is our full sample of firm-quarters used in the main analysis. *Propensity Matched Sample* is our one-to-one propensity matched sample constructed using the full set of controls in Table 4 Column (2). *Dealscan Sample* is the subsample of firm-quarters with covenant data available in Dealscan. *Dealscan Sample [+|- 20]* restricts the *Dealscan Sample* to the subset of firm-quarters that have a financial ratio within +|- 20% of the relevant covenant threshold. *CR (2008) Sample* is the subsample of firm-quarters with a current ratio, total net worth, or tangible net worth covenant in Dealscan. Finally, *CR (2008) Sample [+|- 20]* restricts the *CR (2008) Sample* to the subset of firm-quarters within +|- 20% of a relevant covenant threshold.

Panels B, C, and D present OLS estimates of the effect of a financial covenant violation on acquisition behavior. Columns (1)-(3) of Panel B reproduce estimates from the quasi-regression discontinuity specification used in our main analysis. Columns (4)-(6) of Panel B report estimates from the same empirical model, but using the *Propensity Matched Sample*. Panel C reports estimates from the same empirical model, except it includes controls for the distance to the tightest covenant threshold observed in Dealscan instead of the quasi-regression discontinuity 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).[26]

[26] 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.

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 (as documented in Panel A).

2. 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 2 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, 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.2 we show that our main results are robust to classifying firms as "in violation" for four quarters after they report a financial covenant violation.

3. Alternate acquirer CAR specifications

Table A.3 reports robustness tests for our acquirer CAR results. All regression models include the full set of controls, Fama-French 48 industry fixed effects, and year fixed effects, as in Column (2) of Table 5, but we alter the specification in each row to check robustness. We find 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, iv) using value-weighted rather than equal-weighted CARs, and v) repeating the analysis on our propensity matched sample.

Table A.1: Alternate empirical specifications. This table presents robustness checks of our results using a range of alternate specifications. Panel A presents the frequency of firm-quarters with an acquisition announced by violators and non-violator. Panels B, C, and D present ordinary least squares estimates of the effect of a covenant violation on acquisition behavior using several alternative specifications. Internet Appendix Section 1 provides a detailed description of each of these specifications. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Panel A: Acquisition frequency

	Total		Value-destroying		Value-neutral		Value-enhancing	
	#	%	#	%	#	%	#	%
Full 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
Propensity Matched 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 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 Sample [+|- 20]								
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
CR (2008) 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
CR (2008) Sample [+|- 20]								
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

Panel B: Quasi-RDD

	Full Sample			*Propensity Matched Sample*		
	Acquisition	**Value-destroying**	**Value-enhancing**	**Acquisition**	**Value-destroying**	**Value-enhancing**
	(1)	(2)	(3)	(4)	(5)	(6)
Financial covenant violation	-0.012***	-0.003***	-0.000	-0.008***	-0.002***	0.000
	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Acquirer controls	Yes	Yes	Yes	Yes	Yes	Yes
Quasi-RDD covenant controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry & year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	176,378	176,378	176,378	23,830	23,830	23,830
Adjusted R-squared	0.012	0.004	0.002	0.013	0.004	0.001

Panel C: Dealscan sample RDD

	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

	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.2: 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 4, except that we use a one-year violation indicator. Panel B presents OLS estimates from regressions that are identical to Table 5, 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

Table A.3: Alternate CAR specifications. This table reports robustness tests for our acquirer CAR results. All regression models include the full set of controls, Fama-French 48 industry fixed effects, and year fixed effects, as in Column (2) of Table 5. Row (1) presents the main specification, but with standard errors double clustered on firm and year. Row (2) reports the main specification estimated on the subsample of completed acquisitions. Rows (3)–(5) present the main specification with alternate acquirer CAR calculations. Row (6) reports the difference in average CARs between violators and non-violators in the propensity matched sample. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

	Acquirer CAR (%)
(1) Double cluster on firm and year	1.758***
	(0.620)
(2) Subsample of completed deals	1.704**
	(0.681)
(3) 5-day equal weighted CARs	1.744**
	(0.747)
(4) 3-day value weighted CARs	2.117**
	(0.966)
(5) 5-day value weighted CARs	2.094**
	(1.066)
(6) Propensity matched sample	2.250**
	(0.890)