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Control Rights and Capital Structure: An Empirical Investigation

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ABSTRACT

We show that incentive conflicts between firms and their creditors have a large impact on corporate debt policy. Net debt issuing activity experiences a sharp and persistent decline following debt covenant violations, when creditors use their acceleration and termination rights to increase interest rates and reduce the availability of credit. The effect of creditor actions on debt policy is strongest when the borrower's alternative sources of finance are costly. In addition, despite the less favorable terms offered by existing creditors, borrowers rarely switch lenders following a violation.

A FUNDAMENTAL QUESTION IN FINANCIAL ECONOMICS is: How do firms choose their financial policies? Extant empirical research on this question has focused primarily on the presence of taxes and bankruptcy costs (e.g., Scott (1976)), information asymmetry (e.g., Myers and Majluf (1984)), and more recently, market timing behavior (e.g., Baker and Wurgler (2002)). However, beginning with Jensen and Meckling (1976), a large body of theoretical research examines how incentive conflicts between managers and external investors affect corporate financial policies. In particular, theoretical research on financial contracting shows that in the presence of incentive conflicts, optimal debt contracts will allocate certain rights to creditors after negative performance in order to help firms secure financing ex ante (e.g., Aghion and Bolton (1992) and Dewatripont and Tirole

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(1994)). Thus, in the context of the capital structure debate, financial contracting theory raises two important empirical questions: First, to what extent do incentive conflicts and creditor rights impact corporate financial policies? And, second, how do incentive conflicts and creditor rights impact corporate financial policies?

The goal of this study is to answer these questions by examining the response of corporate financial policies to covenant violations. Covenant violations provide a unique opportunity for studying incentive conflicts, creditor rights, and financial policy for several reasons. First, the presence of covenants in debt agreements is motivated by their ability to mitigate incentive conflicts between managers and creditors (Smith and Warner (1979)). Second, covenant violations give creditors the right to demand immediate repayment and withhold further credit, thereby providing a potential channel through which the misalignment of incentives can impact financial policy (Tirole (2006)). Third, covenant violations occur frequently (Dichev and Skinner (2002)) and rarely lead to payment default or bankruptcy (Gopalakrishnan and Parkash (1995)), suggesting that violations are a potentially important concern for firms even outside of financial distress. Finally, the discrete nature of a covenant violation enables us to employ a regression discontinuity design that helps identify the effect of violations on financial policies.

Our analysis centers on a novel data set that includes the universe of financial covenant violations reported on firms' annual and quarterly securities and exchange commission (SEC) filings between 1996 and 2005. Among the population of publicly listed firms in the United States, we find that more than one-quarter violate a financial covenant at some point during our sample horizon. This high incidence of covenant violations in the population of publicly listed firms complements previous evidence that documents a similar frequency among firms that utilize private credit agreements (Dichev and Skinner (2002)). More importantly, the high incidence in the general population implies that covenant violations are relevant for a large number of public firms.

Our first set of results shows that, on average, financial policy exhibits a sharp change following a covenant violation. Specifically, net debt issuing activity experiences a large and persistent drop immediately after the violation. In the four quarters before a violation, borrowers have an average net debt issuance scaled by lagged assets of 80 basis points per quarter. In just two quarters after the violation, net debt issuance falls to -25 basis points. In other words, firms move from increasing net debt issuance by 0.8% of assets per quarter to reducing net debt issuance in just two quarters. Further, this decline is persistent, lasting for over 2 years after the violation, and leads to a corresponding decline in leverage of over 3%. While the change to the stock of debt in firms' capital structures is modest relative to the typical unconditional variation in leverage ratios, the change to the flow of debt is large. In fact, the drop in net debt issuance after a covenant violation moves a firm from the 75^{th} percentile of the net debt issuance distribution to the 35^{th} percentile.

To help isolate the impact of the covenant violation, we turn to a firm and period fixed effects regression framework. We show that net debt issuing activity declines by approximately 70 basis points in the quarter immediately after a covenant violation—a marginal effect larger than that of most previously identified capital structure determinants. For example, a two standard deviation change in the size of the firm, the single most powerful predictor of net debt issuing activity, results in only a 52 basis point quarterly decline in net debt issuances. Further, the 70 basis point drop in net debt issuance moves the firm from the 65^{th} to 35^{th} percentile of the within-firm net debt issuance distribution. The persistence of this decline, even after conditioning on fixed effects and traditional control variables, translates into a decline in leverage ratios over the 2 years following the violation that moves firms from the 70^{th} to the 45^{th} percentile of the within-firm leverage distribution. Thus, while covenant violations are responsible for only a modest fraction of cross-sectional variation in leverage ratios, violations lead to significant time-series variation in leverage ratios despite shrinking asset bases that offset the reduction in net debt issuances (Chava and Roberts (2008) and Nini, Smith, and Sufi (2009)).

Both the magnitude and statistical significance of the financing response to covenant violations are robust to a variety of additional tests aimed at ensuring that the estimated response is free from confounding influences, such as changes in investment opportunities or expected bankruptcy costs that may occur around the time of the violation. For example, we incorporate parametric and nonparametric functions of the variables on which financial covenants are often written to account for the possibility that these measures contain information about managers' preferences for issuing debt. We also show that leverage rebalancing (Leary and Roberts (2005)) and mean reversion in leverage ratios (Flannery and Rangan (2006), Kayhan and Titman (2007)) are not behind our findings, as violators with relatively low leverage ratios reduce net debt issuance by more than nonviolators with high leverage ratios. Finally, in order to control for the possible endogeneity of the covenant threshold and further control for differences between violators and nonviolators, we undertake a regression discontinuity design that reveals a nearly identical decline in net debt issuing activity following a covenant violation.

After establishing the average effect of covenant violations on financial policy, our second set of results attempts to understand *how* violations impact financial policy by identifying the underlying mechanism linking the violation to the subsequent policy response. To do so, we first examine heterogeneity in the effect of violations on financial policy. For example, we find that net debt issuances decline by almost 150 basis points following a new covenant violation, which we define as violations for firms that have not violated a covenant in the previous four quarters, whereas subsequent violations are followed by a 30 basis point decline. We also find that the decline in net debt issuances is significantly larger for firms with high leverage and low market-to-book ratios at the time of the violation. Similarly, the decline in net debt issuances is approximately 115 basis points larger for firms lacking a credit rating. These results suggest that (1) the granting of acceleration and termination rights to existing

creditors has an immediate impact on the provision of credit, and (2) the impact of this rights transfer on financial policy is greatest when alternative sources of capital are relatively expensive or limited.

We next examine the SEC filings of a random subsample of violators in order to identify how creditors use the control rights obtained after the violation to influence lending terms. We find that one out of three violators explicitly states that creditors respond to the violation by reducing the credit facility, increasing the interest spread, or demanding additional collateral. Among violators that report creditor action, we find that net debt issuance scaled by lagged assets declines by 418 basis points in just two quarters. Additionally, our examination of SEC filings reveals that only 4% of violators terminate their relationship with existing creditors within two quarters after the violation. In other words, despite the unfavorable terms offered by existing lenders, very few borrowers repay the violated agreement with financing from *other* lenders. Thus, financial covenant violations have a large effect on financing decisions because violators are unable to obtain financing from other lenders at more favorable terms, thereby subjecting borrowers to the disciplinary actions of their existing creditors.

Our primary contribution to the capital structure literature is to show that incentive conflicts in conjunction with the transfer of control rights have a first-order effect on financial policy. In this sense, our study is related to those by Jung, Kim, and Stulz (1996), Berger, Ofek, and Yermack (1997), and Garvey and Hanka (1999), who show that measures of managerial entrenchment or discretion are correlated with security issuance decisions. In contrast to these studies, we focus on the conflict of interest between managers and creditors. Additionally, we quantify the relative magnitude of the effect of incongruent incentives on financial policy, and we show a precise mechanism (covenant violations and the resulting transfer of control rights) through which incentive conflicts impact financial policy. As such, our study confirms several hypotheses from the financial contracting literature (Aghion and Bolton (1992) and Dewatripont and Tirole (1994)), which has received little attention from empirical capital structure studies.

Our findings are also related to a growing body of research showing that the supply of capital has an important effect on firm financial policy (Faulkender and Petersen (2006), Leary (2006), Sufi (2009a), Lemmon and Roberts (2007)). Unique to our study is the finding that debt covenant violations enable a firm's existing creditors to address incentive conflicts by moderating the supply of capital. More precisely, we show that borrowers rarely switch lenders after a violation, and as a result, changes in the willingness to supply credit by existing lenders lead to significant effects on financial policy.

Finally, our findings are related to empirical research examining covenant violations and the resolution of technical default (e.g., Smith and Warner (1979), Beneish and Press (1993, 1995), Chen and Wei (1993), Sweeney (1994), Chava and Roberts (2008), Nini, Smith, and Sufi (2009), Sufi (2009b)). While these studies show that existing creditors use their acceleration right to extract amendment fees, reduce unused credit availability, increase interest rates, and influence investment, we are the first to explore how covenant violations fit

into the broader capital structure debate.[1] In particular, we are the first to show that covenant violations—via the allocation of control rights—are associated with a large and persistent decline in the flow of debt and a corresponding decline in leverage ratios. Additionally, we are the first to focus attention on the identification of this effect using a novel empirical technique aimed at showing that the estimated financial response is a consequence of creditor actions following the covenant violation (see Smith (1993)).

The remainder of the paper proceeds as follows. Section I describes our data, presenting summary statistics in the process. Section II lays the theoretical foundation and motivation for our study. Sections III and IV present the results. Section V concludes.

I. Data

A. Sample Construction

We begin with all nonfinancial Compustat firm-quarter observations from 1996 to 2005. We choose 1996 as the start year for our sample construction to coincide with the imposition of the SEC's requirement that all firms submit their filings electronically, a feature that we require to measure covenant violations among the population of publicly traded firms. To ensure the continuity of our sample across all of our study, we condition on the presence of both period t and $t-1$ data for all of the variables considered in our analysis.[2] (All variables used in this study are formally defined in the Appendix.) To mitigate the impact of data errors and outliers on our analysis, we Winsorize all variables at the 5^{th} and 95^{th} percentiles, though our results are largely unaffected if we Winsorize at the 1^{st} and 99^{th} percentiles. Finally, because our primary analysis relies on within-firm variation, we include only firms for which there are at least four consecutive quarters of available data. In concert, these criteria reduce the sample from 176,993 firm-quarter observations to 135,736 firm-quarter observations.

We supplement the Compustat data with information on financial covenant violations collected directly from 10-K and 10-Q SEC filings. These data are available given SEC Regulation S-X, which requires that "any breach of a covenant of a[n] . . . indenture or agreement which . . . exist[s] at the date of the most recent balance sheet being filed and which has not been subsequently cured, shall be stated in the notes to the financial statements" (SEC (1988), as quoted by Beneish and Press (1993, p. 236)). As Sufi (2009b) notes, the SEC has reinforced this requirement in recent interpretations: "companies that are,

[1] The fact that covenant violations lead to reduced credit and increased interest rates from existing lenders does not necessarily mean that covenant violations affect capital structure. For example, firms could switch to a new lender after the violation, or firms could reduce net debt and net equity issuances in equal proportion. Our analysis quantifies the magnitude and persistence of the costs associated with covenant violations via their impact on financial policy.

[2] More precisely, we require for each firm-quarter observation nonmissing data for both the contemporaneous and lagged value of total assets, total sales, tangible assets, total debt, net worth, cash holdings, net working capital, EBITDA, cash flow, net income, interest expense, market-to-book ratio, book value of equity, and market value of equity.

or are reasonably likely to be, in breach of such covenants must disclose material information about that breach and analyze the impact on the company if material (SEC (2003))."

In order to extract these data, we first match all *Compustat* quarterly observations to their respective 10-Q or 10-K filing based on the IRS identification number. We then use a Perl program to search the filings for one of 20 terms (see the Appendix). Each time the program finds a term, it prints the 10 lines before and after the term in a separate document. We check each passage to ensure that the existence of the term reflects a financial covenant violation. Thus, each firm-quarter observation in our sample either is or is not in violation of a covenant.

As Dichev and Skinner (2002) note, financial covenant violations that are reported by firms in their SEC filings likely represent situations in which they were unable to obtain an amendment or waiver to cure the violation by the end of the reporting period. While this is in general correct, it is important to note that many of the violations reported in SEC filings are violations that are waived before the reporting period ends. In these cases, the firm voluntarily reports that it was in violation during the reporting period even though it has cured the violation by the end of the reporting period. One potential concern is that the reported violations tracked in our data represent, on average, more serious violations than violations that could be cured before the end of the reporting period. However, a comparison of observable measures of credit quality and investment around the initial reported covenant violation in our sample versus the initial violation in previous studies reveals very similar patterns. For example, cash flow and capital expenditures show patterns around the first reported violation in our sample that are almost identical to those found in studies by Dichev and Skinner (2002) and Chava and Roberts (2008), which suggests that initial reported violations in our sample correspond closely to initial actual violations. Further, in our robustness tests, we explicitly address this concern with a subsample of firms for which we can observe both reported and unreported violations.

B. Summary Statistics

Although the SEC requires firms to report unresolved financial covenant violations, it does not require firms to detail exactly which covenant has been violated. To give a sense of the types of financial covenants employed in private credit agreements, we examine the financial covenants contained in a subsample of 3,603 private credit agreements entered into by 1,894 of the firms in our sample.[3] Almost 97% of these credit agreements contain at least one financial covenant, which can be broadly categorized by the accounting measures on which they are based: debt to cash flow (58%), debt to balance sheet

[3] A table corresponding to this information is available in an Internet Appendix available at: http://www.afajof.org/supplements.asp. For more details on these private credit agreements and how they were obtained, see Nini, Smith, and Sufi (2009). There are slightly fewer observations in Table I than in Nini, Smith, and Sufi (2009) given that some agreements detail financial covenants in an attached exhibit that is not included in the SEC filing.

items (29%), coverage ratios (74%), net worth (45%), liquidity (15%), and cash flow (13%). Among these agreements, 79% contain a financial covenant that restricts a ratio with debt in the numerator, and 74% contain a covenant that restricts a coverage ratio limiting the amount of interest payments. Overall, almost 90% of the credit agreements contain either an explicit or implicit restriction on the borrower's total debt, highlighting the importance of financial covenants in the borrower's capital structure determination.

Panel A of Table I reveals that covenant violations are common. Over one-quarter of the firms in our sample experience a financial covenant violation at some point between 1996 and 2005. Among firms with an average leverage ratio of at least 5%, the percentage of covenant violators increases to 30%. Panel A also shows that firms across all industries violate financial covenants with similar proportions, with the possible exception of firms in Trade-Wholesale. Firms with and without a corporate credit rating violate covenants at approximately similar rates as well. However, smaller firms are significantly more likely to violate financial covenants than larger firms; firms with total assets less than $100 million are almost 20 percentage points more likely to violate a financial covenant than firms with total assets over $5 billion.

Panel B presents the 1-year probabilities of violating a financial covenant in our sample based on the S&P corporate credit rating. Firms rated "A" or better have a 1-year probability of violating a covenant of 1%, while firms rated BB have a 7% probability. Relative to the 1-year *payment* default probabilities reported by S&P, the probabilities of a covenant violation are significantly larger in every rating category except firms rated "CCC" or worse, which includes some firms that have already defaulted on a payment. The difference in the probabilities is particularly large for firms rated "BB" or better. Thus, even firms that are unlikely to default on payments face a nontrivial probability of violating a financial covenant.

Table II presents the summary statistics for our outcome variables (net security issuance and book leverage), our "covenant control variables," and "other control variables." For presentation purposes, we focus our attention on net debt issuance computed from the change in balance sheet debt and net equity issuance computed from the statement of cash flows. However, we also examine net debt issuance computed from the statement of cash flows and net equity issuance computed from the split-adjusted change in shares outstanding (Fama and French (2005)). The results are qualitatively similar and therefore not reported.

The covenant control variables include many of the accounting ratios on which financial covenants are written. As such, they provide a means to control for variation in accounting variables that are correlated with both the violation event and the propensity to issue debt. The third group, other control variables, contains several additional control variables suggested by the empirical capital structure literature (e.g., Frank and Goyal (2005)) as being relevant for financial policy. Overall, the means and medians, after annualizing flow variables, coincide with those found in previous studies investigating capital structure (e.g., Frank and Goyal (2003) and Mackay and Phillips (2005)).

Table I
Covenant Violations

Panel A of this table presents the percentage of firms that report a financial covenant violation in 10-K or 10-Q SEC filings at some point between 1996 and 2005. Panel B reports the 1-year probability of a financial covenant violation and a payment default according to S&P. S&P 1-year cumulative default probabilities are equal-weighted averages over ratings to get the probability for the broad rating class. The sample includes 6,381 firms and 135,736 firm-quarter observations.

Panel A: Fraction of Firms That Violate Financial Covenant

	Percentage of Firms Reporting Violation
Totals	
Total sample	25.6%
Firms with average book leverage ratio greater than 0.05	30.0%
By industry	
Agriculture, minerals, construction	28.6%
Manufacturing	25.4%
Transportation, communication, and utilities	25.2%
Trade—wholesale	34.8%
Trade—retail	23.3%
Services	24.6%
By size (book assets)	
Less than $100M	28.8%
$100M to $250M	28.8%
$250M to $500M	25.0%
$500M to $1,000M	21.7%
$1,000M to $2,500M	18.7%
$2,500M to $5,000M	17.8%
Greater than $5,000M	10.6%
Borrower does not have credit rating	26.6%
Borrower has credit rating	22.3%

Panel B: 1-Year Probabilities of Default by Credit Rating

	1-Year Probability of Covenant Violation	S&P 1-Year Cumulative Default Probability
A or better	1.0%	0.0%
BBB	3.1%	0.2%
BB	6.8%	0.9%
B	9.4%	7.2%
CCC or worse	18.4%	21.9%
Unrated	10.0%	

II. The Consequences of Covenant Violations: Practice and Theory

A. Financial Covenants and Creditor's Rights

Before discussing the theoretical motivation for why covenant violations might impact firms' financial policies, we first clarify what happens when a financial covenant is violated. Provisions in the contract grant creditors the

Table II
Summary Statistics

This table presents summary statistics for the unbalanced panel of 6,381 firms from 1996 to 2005 (135,736 firm-quarters). Net debt issuance and net equity issuance are scaled by lagged assets.

	Mean	Median	*SD*
Capital structure variables			
Net debt issuance (basis points)	50.5	0.0	400.8
Net equity issuance (basis points)	39.8	0.4	166.8
Book $\text{debt}_t/\text{assets}_t$	0.228	0.182	0.221
Covenant control variables			
Net $\text{worth}_t/\text{assets}_t$	0.495	0.518	0.287
Net working $\text{capital}_t/\text{assets}_t$	0.254	0.235	0.271
$\text{Cash}_t/\text{assets}_t$	0.199	0.092	0.231
$\text{EBITDA}_t/\text{assets}_{t-1}$	0.006	0.026	0.068
Cash $\text{flow}_t/\text{assets}_{t-1}$	−0.007	0.017	0.074
Net $\text{income}_t/\text{assets}_{t-1}$	−0.022	0.006	0.077
Interest $\text{expense}_t/\text{assets}_{t-1}$	0.005	0.003	0.006
Other control variables			
Market-to-book ratio_t	2.338	1.572	1.947
Tangible $\text{assets}_t/\text{assets}_t$	0.270	0.194	0.230
$\text{Ln}(\text{assets}_t)$	4.900	4.910	2.384

right to immediately accelerate outstanding amounts in response to a violation, also known as a technical default. In addition, a violation gives creditors the right to terminate any unused portion of lines of credit or revolving credit facilities. As an illustrative example, the loan contract between Digitas Inc. and Fleet National Bank, originated on July 25, 2000, contains the following clauses, which are standard in most private credit agreements.[4]

> 14.1. Events of Default and Acceleration. If any of the following events . . . shall occur: (c) the Borrower shall fail to comply with any of its covenants contained in [the section describing financial covenants]; . . . Then . . . [Fleet] may . . . by notice in writing to the Borrower declare all amounts owing with respect to this Credit Agreement, the Revolving Credit Notes and the other Loan Documents and all Reimbursement Obligations to be, and they shall thereupon forthwith become, immediately due and payable without presentment, demand, protest or other notice of any kind . . .
>
> 14.2. Termination of Commitments. If any one or more of the Events of Default . . . shall occur, any unused portion of the credit hereunder shall forthwith terminate and each of the Banks shall be relieved of all further obligations to make Revolving Credit Loans to the Borrower and the Agent shall be relieved of all further obligations to issue, extend or renew Letters of Credit.

[4] Excerpt is from the 10-Q SEC filing of Digitas Inc. for September 30, 2000 available at http://www.sec.gov/Archives/edgar/data/1100885/000091205700049556/0000912057-00-049556.txt.

While private credit agreements give creditors the right to accelerate outstanding balances in response to technical defaults, extant research suggests that most technical defaults lead to renegotiation and waivers of the violation, as opposed to acceleration of the loan (e.g., Gopalakrishnan and Parkash (1995), Chen and Wei (1993), Beneish and Press (1993)). However, extant research also finds that creditors use their acceleration right to extract amendment fees, reduce unused credit availability, increase interest rates, increase reporting requirements, increase collateral requirements, and restrict corporate investment (Gopalakrishnan and Parkash (1995), Chen and Wei (1993), Sufi (2009b), Chava and Roberts (2008), and Nini, Smith, and Sufi (2009)). Thus, accompanying covenant violations are a wide range of actions undertaken by creditors, which are largely removed from acceleration of the loan or bankruptcy.

B. Theory and Hypothesis Development

Why would covenant violations affect capital structure? To answer this question and motivate our empirical analysis, we focus on theoretical research in which covenants play a crucial role in mitigating incentive conflicts between managers and external investors. The foundation of this literature is Jensen and Meckling (1976), who analyze how risk-shifting tendencies of managers acting on behalf of shareholders influence debt contracts. Given incentive conflicts introduced by managers' convex payoff functions, creditors will attempt to mitigate risk-shifting through covenants restricting firm investment and financial policy.

Covenants emerge endogenously in recent theoretical research that derives debt with control rights as an optimal financial contract. For example, Aghion and Bolton (1992) use an incomplete contracting framework in which a wealth-constrained owner-manager seeks capital to finance projects that produce both cash profits and managerial private benefits. In their model, origination contracts allocate a decision right that depends on an imperfect state signal. When the signal indicates that managerial private benefits are likely to distort the manager into inefficient decisions, the decision right is transferred to creditors (see also Zender (1991)).

Dewatripont and Tirole (1994) assume the existence of an ex ante managerial moral hazard problem, and they find that optimal financial contracts with concave cash flow rights encourage debt-holders to interfere with firm policy after signs of poor performance. Creditor interference serves as a managerial disciplining device, and therefore helps mitigate moral hazard problems. In their model, a noisy signal correlated with firm performance is contractible, and creditors interfere with firm policy conditional on negative realizations of the signal.

Although the allocation of control rights is an important aspect of these models, creditor "control" does not necessarily entail creditors literally replacing managers as decision-makers. To the contrary, control rights in the Dewatripont and Tirole (1994) and Aghion and Bolton (1992) frameworks refer to *limited* rights given to creditors after negative performance. For example, in

Dewatripont and Tirole (1994), creditors may obtain the right to force reorganization, divest, choose a conservative option, or stop a specific project. Covenant violations are a close empirical analog to these models on two dimensions. First, creditors receive termination and acceleration rights following negative performance (i.e., violating a covenant). Second, the acceleration and termination rights that creditors obtain after a violation are limited rights that allow creditors to protect the value of their claim. However, the rights do not give creditors the ability to make financing decisions or run the firm.[5]

Likewise, a covenant violation by itself does not generate changes in financial policy. Instead, a violation is the impetus that leads to changes in financial policy because of the accompanying transfer of control rights and creditor intervention. For example, the covenant violation gives creditors the opportunity to examine the firm more carefully, and, even more importantly, the control rights associated with the violation give creditors the ability to influence financial policy if changes in circumstances warrant such intervention. Consequently, our analysis is comprised of two sections: The first identifies the response of financial policy to covenant violations, and the second identifies the underlying economic mechanism behind this relation.

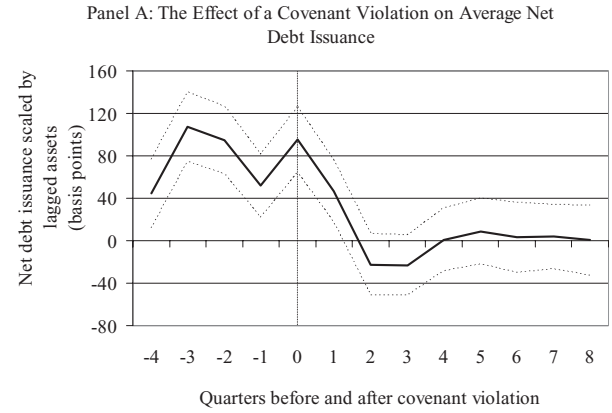
III. The Effect of Covenant Violations on Financing Decisions

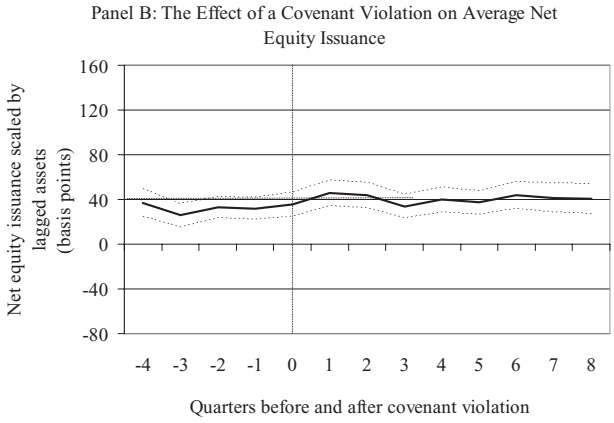
A. Graphical Analysis

We begin our analysis of whether covenant violations affect financing decisions by illustrating the relationship between violations and net debt issuance, net equity issuance, and leverage ratios. Panels A to C of Figure 1 present the unconditional averages for these outcomes in event time, where time zero is the quarter of the violation. In order to clearly identify the effect of a violation on financial policy, we require that the firm not experience another violation in the 12 quarter window surrounding the event. That is, from four quarters prior to the violation until eight quarters after the violation, the firm experiences only one violation indicated in the figures by period zero. While the analysis present in the figures is isolated to a subsample, the regression analysis beginning in the next section examines all violations, and the analysis in Section IV explicitly differentiates between initial and subsequent violations. Additionally, to ensure that our results are not an artifact of a changing sample composition, we focus on the subsample of firms surviving for at least 2 years after the violation. However, the results are qualitatively similar if we remove this requirement.

Panel A examines the effect of covenant violations on the flow of debt capital as measured by net debt issuance scaled by lagged assets. Before the violation, there is no discernable trend in net debt issuance. Immediately following the violation, firms experience a sharp decrease in net debt issuance. By the

[5] Kaplan and Strömberg (2003) find that venture capital contracts often allocate board seats and voting rights to VCs following negative performance, which they interpret as support for the Aghion and Bolton (1992) framework. However, in our setting, creditors do not receive board seats or voting rights after covenant violations.





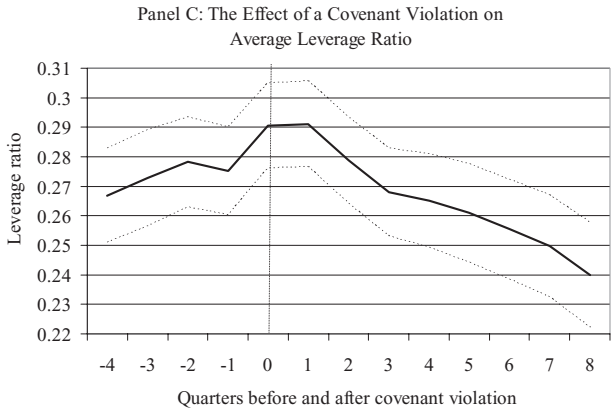


Figure 1. The effect of a covenant violation on capital structure. Panels A, B, and C of Figure 1 show the effect of a covenant violation on net debt issuance, net equity issuance, and the leverage ratio, respectively. The dotted lines define the 90% confidence interval for the mean. The sample includes borrowers that violate a covenant only once (at time $t = 0$) during the 12-quarter window.

second-quarter after the covenant violation, net debt issuance activity falls from +95 basis points to −23 basis points, for an effect of almost 120 basis points in just two quarters. This decline is statistically significant at all conventional levels; it is also economically large, corresponding to an annualized decline in the net flow of debt equal to almost 2.5% of lagged assets. This sharp drop in net debt issuance after the violation moves the average violator from the 75th to the 35th percentile of the unconditional distribution. This change in net debt issuance policy also shows persistence. Even 2 years later, net debt issuance is significantly lower than it was in the five quarters up to and including the quarter of the covenant violation.

While these magnitudes are large, it is important to emphasize that the analysis in Figure 1 likely underestimates the effect of the violation on financial policy given that we cannot measure unused capacity under bank revolving credit facilities. Sufi (2009b) finds a reduction in unused lines of credit scaled by assets of 360 basis points in the year after a covenant violation, which implies a 90 basis point reduction per quarter. Firms generally do not detail their unused revolving credit capacity at the quarterly frequency, which is why we focus on the actual flow of debt.

Panel B presents the results for net equity issuances. Unlike net debt issuances, there is no discernible change in net equity issuances right after the covenant violation. There is some evidence of an increasing trend following the violation; however, it is statistically weak and economically small.

Panel C examines the effect of the violation on book leverage ratios.[6] By the fifth-quarter after the violation, firm leverage is statistically significantly lower than in the quarter of the violation. In fact, by the eighth-quarter after the covenant violation, firm leverage is statistically significantly lower than that in the quarter *before* the covenant violation. From the first to the eighth-quarter after the violation, violators reduce their leverage by 5%—a significant decline relative to the typical within-firm variation in leverage ratios but a modest decline when compared to the typical cross-sectional variation in leverage ratios.[7]

Ultimately, these findings suggest that covenant violations are not responsible for much of the total variation in leverage ratios, the majority of which is comprised of between-firm variation corresponding to differences in average leverage ratios across firms (Lemmon, Roberts, and Zender (2008)). Loosely speaking, a relatively high levered firm is unlikely to become a relatively low levered firm because of a covenant violation, in part because of a contracting asset base. However, violations do generate large changes in net debt issuances (i.e., the flow of debt) and, potentially, large changes in leverage ratios relative

[6] The effect of covenant violations on market leverage ratios (unreported) is almost identical.

[7] The violation moves a firm from the 72nd to 45th percentile of the within-firm distribution of leverage ratios, but only from the 66th to the 58th percentile of the overall leverage distribution—less than one decile. One reason for the muted effect on leverage ratios is that covenant violations lead to significant reductions in assets. Specifically, negative cash flow shocks (Sufi (2009b)), asset sales (Beneish and Press (1993)), and reduced capital expenditures (Chava and Roberts (2008)) following covenant violations work to reduce assets and increase leverage, thereby partially offsetting the effects of reduced net debt issuances.

to the typical within-firm variation. The analysis below investigates these effects more closely.

B. Identification and Empirical Strategy

While the results in Panels A and C of Figure 1 are consistent with a significant effect of violations on financial policy, the extent to which the observed change is due to the covenant violation, as opposed to changes in managers' preferences for debt, is unclear. In particular, our primary identification concern is that changes in the firm surrounding a covenant violation would also bring about a change in financial policy *absent* the covenant violation. For example, managers may alter their financial policies in response to changes in the variables on which covenants are written. Alternatively, managers may alter their financial policies in response to changes in other firm characteristics that occur contemporaneously with the violation. Therefore, the goal of our empirical strategy is to show that managers would *not* have altered their financial policies in the same manner had the covenant violation *not* occurred.

To illustrate this issue, consider the following hypothetical example in which a credit agreement contains a covenant restricting the borrower's debt to EBITDA ratio to remain below 3.0. Suppose then that we demonstrate that net debt issuance is lower for the firm when debt to EBITDA is above 3.0. In this example, we should be cautious in asserting that net debt issuance is lower because the firm violates a covenant given that increases in the debt to EBITDA ratio are also likely correlated with changes in managerial preferences over debt policies. For example, an increase in the debt to EBITDA ratio may also be associated with an increase in the probability of bankruptcy or a decline in expected taxes. According to a tax-bankruptcy cost tradeoff theory, both effects would lead managers to prefer less debt. Therefore, without controlling for variation in the debt to EBITDA ratio and other factors associated with financial policy, our estimate of the impact of covenant violations ignores the fact that managers may have reduced net debt issuance even in the absence of the violation.

To disentangle the effect of the covenant violation from changes in financial policy that would have otherwise occurred, we estimate the impact of the covenant violation by focusing only on discontinuous changes in financial policy occurring at the covenant threshold. We apply this discontinuity approach by including as right-hand side variables a covenant violation indicator variable along with linear, nonlinear, and step functions of the underlying variables on which covenants are written.[8] With the inclusion of these functions, the point estimate on the covenant violation indicator variable is identified under the assumption that managerial preferences over financial policies are not discontinuous *exactly* at the covenant threshold. This assumption is valid as long as managers, in the absence of financial covenants, would not have chosen the

[8] For a graphical analysis of the regression discontinuity design, see the Internet Appendix.

exact same ratios and levels of the ratios as creditors to determine financial policy.

Both anecdotal and empirical evidence suggest that this assumption is valid. First, discussions with commercial lenders indicate that covenant restrictions are often highly contested during the pre-origination negotiations, which suggests that covenants are not simply placed at the managerial chosen threshold.[9] Second, extant research shows that interest rates are lower when loan contracts contain more covenants (Bradley and Roberts (2004)), which implies that covenants must be valuable for the creditor. Given the forgone interest payments, it is unlikely that creditors place covenants at thresholds that managers would have used themselves in the absence of the covenants. Nonetheless, we undertake a variety of robustness tests below to further ensure that this assumption is valid and that the estimated financing response is properly identified.

C. Isolating the Impact of Violations on Net Debt Issuance

For the empirical analysis, we construct a matrix of right-hand side variables, X, consisting of 16 variables on which covenants are written. The matrix includes 12 noninteraction (i.e., level) covenant controls and four interaction terms.[10] We include these interactions given that many covenants are written on combinations of the underlying variables (debt to EBITDA, for example). The choice of these controls is based on the most common financial covenants employed in private credit agreements. Following previous empirical capital structure studies (e.g., Rajan and Zingales (1995)), the matrix X also includes the lagged natural logarithm of assets, the lagged tangible to total assets ratio, the lagged market-to-book ratio, and a lagged indicator variable identifying whether the firm has an S&P credit rating.[11] Given this matrix X, we estimate the following firm fixed effects specification:

[9] We are particularly grateful for discussions with Rob Ragsdale, formerly of First Union; Terri Lins, formerly of Barclays, FleetBoston, and First Union/Wachovia; Horace Zona formerly of UBS, Toronto Dominion, and currently with First Union/Wachovia; Steven Roberts, formerly with Toronto Dominion; and Rich Walden, Rick Gabriel, and Doug Antonossi of JP Morgan Chase & Co.

[10] The level variables are: lagged book debt to assets ratio, lagged net worth to assets ratio, lagged cash to assets ratio, lagged and current EBITDA to lagged assets ratio, lagged and current cash flow to lagged assets ratio, lagged and current net income to lagged assets ratio, and lagged and current interest expense to lagged assets ratio. The interaction terms are: lagged debt to assets ratio interacted with lagged cash flow to lagged assets ratio, lagged debt to assets ratio interacted with lagged EBITDA to lagged assets ratio, lagged debt to assets ratio interacted with lagged net worth to assets ratio, and lagged EBITDA to lagged assets ratio interacted with lagged interest expense to lagged assets ratio.

[11] Unreported analysis incorporating the median industry leverage ratio (Frank and Goyal (2003)), cash flow volatility, the marginal tax rate (Graham (1996)), and credit ratings (Kisgen (2006)) produce qualitatively similar findings. In addition, the inclusion of interacted one-digit industry by quarter fixed effects does not affect our core estimates and does not raise the adjusted-R^2; as a result, we do not include them in the analysis.

$$\frac{D_{i,t} - D_{i,t-1}}{A_{i,t-1}} = \alpha_i + \sum_{f=1}^{4} \theta_f + \sum_{t=1996q3}^{2005q2} \delta_t + \beta_0 * Violation_{i,t} + \beta_1 * Violation_{i,t-1} + \Gamma * f(X_{i,t-1}, X_{i,t}) + \eta_{it}, \quad (1)$$

where $f(X)$ corresponds to a vector of functions of the variables on which covenants are written, and all other variables discussed above. While our general specification contains both lagged and contemporaneous control variables, removal of all contemporaneous controls has a negligible effect on our results.

Column (1) in Panel A of Table III presents the estimation results from the baseline firm fixed effects specification with only fiscal quarter and calendar year-quarter indicator variables as controls (i.e., restricting $\Gamma = 0$). The results show that, on average, net debt issuance falls from eight basis points above the firm mean (*Covenant violation*$_t$) to 62 basis points below in the quarter immediately after the covenant violation (*Covenant violation*$_{t-1}$), a decline of 70 basis points. The standard errors in parentheses imply a t-statistic of eight, even after removing firm fixed effects and accounting for within-firm correlation (Petersen (2009)). The specification reported in column (2) adds the 12 noninteraction covenant control variables. The adjusted-R^2 increases more than threefold to over 18% relative to the baseline fixed effects specification. However, the magnitude of the covenant violation coefficient declines only moderately and is still statistically large. The specification reported in column (3) includes the four interaction terms, which have little impact on the adjusted-R^2 or estimated covenant violation coefficient.

Finally, column (4) presents the results for a kitchen sink specification including the following controls: the 16 covenant control variables (level and interaction terms), higher-order polynomial terms (squared and cubic terms) for each of the 16 covenant controls, and quintile indicator variables for each of the 16 covenant controls. To be clear, the last set of controls consists of 80 (5×16) indicator variables, where each indicator variable equals one if the firm-quarter observation falls in the relevant quintile of the covenant control distribution. The adjusted-R^2 of the regression increases by more than three times that of the regression reported in column (1), suggesting that these additional controls have significant predictive power. However, even with this extensive set of over 120 covenant control variables, the covenant violation coefficient estimate is largely unaffected, remaining at –50 basis points with a t-statistic of almost 7.0.

In Panel B, we present estimates from the first difference analog to the fixed effects specification in equation (1). More precisely, the specifications reported in Panel B examine the change in net debt issuance for a given firm as a function of covenant violations, after controlling for changes in covenant control variables. We report the first difference specification for two reasons. First, fixed effects and first difference estimators are both consistent under standard exogeneity assumptions (see Wooldridge (2002), pp. 284–285), and so a comparison of the estimates is useful in assessing whether the estimation in equation (1) is

properly specified. Second, the graphical analysis in Figure 1 shows a sharp and persistent decline in net debt issuance immediately following a covenant violation, which suggests that a first difference specification may more accurately capture the effect of the violation on net debt issuance. As Panel B shows, the estimates from the first difference specification are similar to the fixed effects estimates in Panel A.

Table III

Covenant Violations and Net Debt Issuance

This table presents coefficient estimates of firm fixed effects regressions (Panel A) and first difference regressions (Panel B) of net debt issuance on covenant violation indicators and control variables. The specifications reported in columns (2)–(4) of Panel A include lagged natural logarithm of total assets, the lagged tangible assets to total assets ratio, the lagged market-to-book ratio, and a lagged "has S&P rating" indicator as control variables. In addition, the specification in column (2) of Panel A includes the 11 *covenant control variables*: the lagged book debt to assets ratio, the lagged net worth to assets ratio, the lagged cash to assets ratio, the lagged and current EBITDA to lagged assets ratio, the lagged and current cash flow to lagged assets ratio, the lagged and current net income to lagged assets ratio, and the lagged and current interest expense to lagged assets ratio. Column (3) of Panel A includes the covenant control variables in addition to four *covenant control interaction variables*: the lagged debt to assets ratio interacted with the lagged cash flow to lagged assets ratio, the lagged debt to assets ratio interacted with the lagged EBITDA to lagged assets ratio, the lagged debt to assets ratio interacted with the lagged net worth to assets ratio, and the lagged EBITDA to lagged assets ratio interacted with the lagged interest expense to lagged assets ratio. Column (4) of Panel A includes all covenant control variables and covenant control interaction variables, these variables squared and to the third power, and five quantile indicator variables for each of the controls. Columns (1)–(4) of Panel B include the first differenced analogs to control variables in Panel A, with the exception of measures using debt, which are differences lagged two quarters instead of one-quarter to avoid spurious correlations. All specifications include calendar year-quarter indicator variables and fiscal quarter indicator variables. Standard errors are reported in parentheses and are clustered by firm.

Panel A: Fixed Effects

Dependent Variable: Net debt issuance$_t$/assets$_{t-1}$ (Basis Points)				
	(1)	(2)	(3)	(4)
Covenant violation$_t$	8.4	3.2	2.2	3.2
	(8.1)	(7.6)	(7.7)	(7.6)
Covenant violation$_{t-1}$	−62.2**	−50.3**	−54.3**	−50.3**
	(7.8)	(7.2)	(7.2)	(7.2)
Covenant control variables	None	Covenant control variables	Covenant control variables, covenant interaction control variables	Control variables, control variables squared, control variables to the third power, and quintile indicators for each control
Number of firm-quarters	135,736	135,736	135,736	135,736
Number of firms	6,381	6,381	6,381	6,381
R^2	0.051	0.183	0.187	0.204

(*continued*)

Table III—*Continued*

Panel B: First Differences				
Dependent Variable: Change in Net debt issuance$_t$/assets$_{t-1}$ (Basis Points)				
	(1)	(2)	(3)	(4)
Covenant violation$_t$	9.2	−3.3	−2.3	2.2
	(11.0)	(10.1)	(10.1)	(10.0)
Covenant violation$_{t-1}$	−44.9**	−60.4**	−59.7**	−50.3**
	(11.2)	(10.3)	(10.3)	(10.3)
Covenant control variables	None	Covenant control variables	Covenant control variables, covenant interaction control variables	Control variables, control variables squared, control variables to the third power, and quintile indicators for each control
Number of firm-quarters	123,557	123,557	123,557	123,557
Number of firms	6,345	6,345	6,345	6,345
R^2	0.003	0.139	0.140	0.159

** Statistically distinct from zero at the 1% level.

In Table IV, we examine the long-run evolution of net debt issuance and leverage ratios after a covenant violation. The regression specifications in columns (1) and (2) of Table IV are identical to the specifications reported in columns (1) and (4) of Table III, respectively, but for the inclusion of covenant violation indicators for eight quarters after the covenant violation. Because of this specification change that requires data for eight lags, the sample for Table IV is smaller than that used in Table III.

Column (1) presents the long-run estimation results from the baseline firm fixed effects specification with only fiscal quarter and calendar year-quarter indicator variables as additional controls. Consistent with the results in Figure 1, the estimates show that net debt issuance drops sharply in the two quarters after the covenant violation, and remains statistically significantly lower than the firm mean even eight quarters after the violation. Column (2) includes the comprehensive set of control variables described in Table III; the short-run and long-run effects are qualitatively similar, with only slightly smaller magnitudes. Thus, the estimates presented in columns (1) and (2) indicate a sharp and persistent decline in net debt issuing activity, even after including the additional controls for variables on which covenants are written.

The results reported in columns (3) and (4) of Table IV demonstrate the long-run effect of the decline in net debt issuance on leverage ratios. Column (3) presents estimates from a specification including only firm, calendar year-quarter, and fiscal quarter indicator variables as controls, and shows that leverage ratios gradually decline in response to the covenant violation—consistent with the pattern exhibited in Panel C of Figure 1. By six quarters after the violation, the leverage ratio is not statistically distinct from the long-run firm average at a meaningful confidence level. The coefficient estimates reported in

Table IV
Long-Run Effect of Covenant Violations

This table presents coefficient estimates from firm fixed effects regressions of net debt issuances (columns (1) and (2)) and the leverage ratio (columns (3) and (4)) on covenant violation indicator variables and control variables. Column (2) contains identical control variables as column (4) of Table III. Column (4) contains the lagged logarithm of total assets, the lagged market-to-book ratio, the lagged tangible to assets ratio, the current and lagged EBITDA to lagged assets ratio, the current and lagged cash flow to lagged assets ratio, the current and lagged net income to lagged assets ratio, and a "has S&P rating" indicator variable. All specifications include calendar year-quarter indicator variables and fiscal quarter indicator variables. Standard errors are reported in parentheses and are clustered by firm.

	Dependent Variable: Net Debt Issuance$_t$/Assets$_{t-1}$ (Basis Points)		Dependent Variable: Leverage Ratio (Basis Points)	
	(1)	(2)	(3)	(4)
Covenant violation$_t$	19.6*	11.3	290.7**	214.4**
	(8.7)	(8.3)	(28.8)	(28.4)
Covenant violation$_{t-1}$	−32.8**	−28.8**	222.9**	157.7**
	(8.8)	(8.3)	(24.9)	(24.6)
Covenant violation$_{t-2}$	−51.6**	−43.1**	128.4**	106.9**
	(8.5)	(8.0)	(22.8)	(22.4)
Covenant violation$_{t-3}$	−27.6**	−21.8**	125.3**	107.0**
	(8.8)	(8.3)	(22.7)	(22.4)
Covenant violation$_{t-4}$	−26.6**	−22.6**	56.3*	43.4*
	(9.0)	(8.5)	(22.3)	(21.6)
Covenant violation$_{t-5}$	−41.5**	−34.0**	69.3**	60.2**
	(8.9)	(8.4)	(22.5)	(21.9)
Covenant violation$_{t-6}$	−27.1**	−25.5**	40.7	27.1
	(9.1)	(8.6)	(21.6)	(21.0)
Covenant violation$_{t-7}$	−17.9*	−17.2*	9.4	2.1
	(8.7)	(8.2)	(23.0)	(22.4)
Covenant violation$_{t-8}$	−30.6**	−33.6**	−22.9	−21.8
	(9.1)	(8.6)	(27.8)	(27.0)
Control variables	None	All covenant control variables from Table III, column (4)	None	Leverage control variables (listed above)
Number of firm-quarters	92,862	92,862	92,862	92,862
Number of firms	5,654	5,654	5,654	5,654
R^2	0.110	0.215	0.790	0.798

*,** Statistically distinct from zero at the 5% and 1% level, respectively.

column (4) are from a specification that includes controls commonly used in the capital structure literature (lagged natural logarithm of assets, lagged asset tangibility, lagged market-to-book, lagged "has S&P rating" indicator, and current and lagged EBITDA, cash flow, and net income scaled by lagged assets). The results are similar.[12]

[12] We implicitly account for the dynamic properties of leverage by allowing for serial correlation in the within-firm error structure (Lemmon, Roberts, and Zender (2008)). Further, unreported analysis of leverage defined by subtracting cash from debt produces qualitatively similar results.

To gauge the economic significance of these results, we conduct two exercises. First, we examine how a covenant violation moves a firm in the net debt issuance and leverage distributions. Given the inclusion of firm fixed effects in the regression analysis that produces the point estimates, we use the within-firm distributions (i.e., the distribution after removing firm fixed effects) as a benchmark. The 70 basis point drop reported in column (1) of Table III, Panel A moves a firm from the 65th to the 35th percentile of the distribution, and the 54 basis point drop in column (4) moves a firm from the 65th to the 40th percentile of the distribution—a full quartile of the conditional distribution. For leverage, the 314 (290.7 – (–22.9)) basis point drop reported in column (3) of Table IV moves a firm from the 72nd to the 45th percentile of the distribution, and the 236 basis point drop in column (4) moves a firm from the 69th to the 45th percentile of the distribution—again a full quartile.

Our second exercise is to compare the effect of a violation to the marginal effect of traditional determinants of financial policy. To do so, we first estimate the effect of covenant violations on net debt issuances and leverage ratios using only traditional control variables found in the capital structure literature (e.g., Rajan and Zingales (1995), Baker and Wurgler (2002), and Frank and Goyal (2005)), as well as firm and period fixed effects. Panel A of Table V presents the parameter estimates and within-firm standard deviations of each right-hand side variable. The last column presents the product of the parameter and two times the standard deviation for the purpose of our comparisons. As illustrated by the table, the marginal impact of a covenant violation is substantially larger than every other control variable, even after allowing for relatively large changes in the underlying variable.

To perform an analogous comparison for leverage, we turn to a more general finite distributed lag model. More specifically, we lag each control variable eight periods in the regression specification. To ease the interpretation of our results, we estimate the model in first difference form. We then compute the long-run multiplier for each control variable as the sum of the eight estimated slope coefficients. This measure estimates the impact of a one-unit change to the control variable on the long-run firm-specific leverage ratio. As before, we multiply each control variables' estimate by twice the corresponding standard deviation in order to examine how a large change in the corresponding variable affects the firm's long-run leverage ratio.

Panel B of Table V presents the results, which show that the long-run impact of a covenant violation on firms' leverage ratios is greater than the long-run impact of most previously identified determinants. Specifically, from the quarter of the violation to the eight quarters after the violation, firms reduce their leverage ratio by an average of 235 basis points. The next most significant predictor is firm profitability. A two-standard deviation increase in this variable leads to a 223 basis point decline in the firm's long-run leverage ratio. These results demonstrate that the decline in the leverage ratio that follows a covenant violation is large relative to the impact of other variables previously examined in the capital structure literature. However, we emphasize that the comparison presented in Panel B of Table V is focused on the *long-run* effects. The short-run

or immediate effect on leverage of most previously identified determinants is significantly larger than that of a covenant violation. Covenant violations are associated with a gradual but persistent effect on leverage ratios because of the persistent decline in net debt issuances, whereas shocks to most previously identified control variables appear to have an immediate effect that tends to decay over time (e.g., Kayhan and Titman (2007)).

Table V
Economic Magnitudes

Panel A presents a comparison of the economic magnitude of a covenant violation and other variables on net debt issuance. Column (1) presents coefficient estimates of firm fixed effects regressions of net debt issuance on covenant violations and controls. The specification includes calendar year-quarter indicator variables and fiscal quarter indicator variables. Standard errors are reported in parentheses and are clustered by firm. Column (2) reports the within-firm standard deviation of the right-hand side variables, and column (3) reports the effect of a two standard deviation change in the right-hand side variable on net debt issuance. Panel B presents a comparison of the magnitude effects of a covenant violation and other variables on the long-run leverage ratio. Column (1) presents the long-run effect of each variable on the leverage ratio. The basis for the long-run effect is a first difference regression that includes eight lags for each differenced variable in addition to calendar year-quarter indicator variables and fiscal quarter indicator variables. For all of the variables except for the covenant violation indicator variable, the long-run multiplier is calculated by adding the coefficient estimates on the eight lags of the differenced variable in question. For the covenant violation indicator variable, the long-run multiplier is the coefficient estimate on the eight-quarter lag term minus the coefficient estimate on the contemporaneous term. Column (2) reports the within-firm standard deviation of the right-hand side variables, and column (3) reports the effect of a two standard deviation change in the right-hand side variable on the long-run leverage ratio.

Panel A: Net Debt Issuances

Dependent Variable: Net Debt Issuance$_t$/Assets$_{t-1}$ (Basis Points)			
	(1) Coefficient Estimates	(2) Within-Firm Standard Deviation of RHS Variable	(3) Estimate $*$ 2 *SD* Increase in RHS Variable
Covenant violation$_{t-1}$	−66.2**		
	(7.6)		
Ln(Assets$_{t-1}$)	−51.6**	0.535	−55.2
	(3.6)		
(EBITDA/assets)$_{t-1}$	−455.5**	0.036	−32.8
	(45.5)		
Market-to-book$_{t-1}$	9.3**	1.191	22.2
	(1.1)		
(Tangible assets/assets)$_{t-1}$	154.4**	0.071	21.9
	(24.5)		
Industry median leverage$_{t-1}$	−531.2**	0.023	−24.4
	(59.4)		
Number of firm-quarters	135,736		
Number of firms	6,381		
R^2	0.107		

(*continued*)

Table V—*Continued*

Panel B: Leverage Ratios			
Dependent Variable: Leverage Ratio (Basis Points)			
	(1) Long-Run Multiplier	(2) Within-Firm Standard Deviation of RHS Variable	(3) Estimate ∗ 2 *SD* Increase in RHS Variable
Covenant violation	−235.2**		
Ln(Assets)	−36.0	0.235	−16.9
(EBITDA/assets)	−3,096.9**	0.036	−223.0
Market-to-book	−62.1**	0.830	−103.1
(Tangible assets/assets)	679.1*	0.042	57.0
Industry median leverage	−491.6	0.010	−9.8
Number of firm-quarters	73,411		
Number of firms	5,118		
R^2	0.064		

In Panel A: *,** statistically distinct from zero at the 5% and 1% level, respectively.
In Panel B: *,** estimate of long-run effect statistically distinct from zero at the 5% and 1% level, respectively.

D. Robustness

D.1. Managerial Rebalancing of Leverage Ratios

In this section, we examine whether the estimated effect of the covenant violation on net debt issuance simply reflects managerial rebalancing of leverage ratios. Previous research suggests that managers dynamically rebalance their leverage ratios (Leary and Roberts (2005)) and many managers explicitly report having a target range for the debt to equity ratio (Graham and Harvey (2001)). Given that covenant violations occur when leverage ratios are high, the concern is that managers are reacting to the higher leverage ratio, and there is no direct effect of the violation itself. The previous results in Table III largely mitigate this concern by showing that the magnitude of the effect of covenant violations on net debt issuance is robust to both parametric and nonparametric controls for the lagged leverage ratio. Nonetheless, we investigate this issue explicitly here because of its importance for our identification strategy.

We first examine the change in net debt issuance for covenant violators versus nonviolators across the leverage distribution. In Panel A of Table VI, the sample is split into quartiles based on the level of the leverage ratio in period $t-1$. Importantly, the quartiles are constructed using the entire sample containing both violators and nonviolators. The first column shows a rebalancing effect among nonviolators, albeit a nonmonotonic effect. Firms in higher lagged leverage quartiles have smaller increases in net debt issuance, which is consistent with the rebalancing evidence in previous studies. Column (2) shows that the net debt issuance of covenant violators is lower in every quartile of the

Table VI

Covenant Violations versus Leverage Rebalancing

This table presents evidence on covenant violations and managerial leverage rebalancing. The sample includes firms that have an average book leverage ratio of 0.05 or greater for the sample. In Panel A, firm-quarter observations at time t are separated into quartiles based on the leverage ratio at $t-1$. In Panel B, firm-quarter observations at time t are separated into quartiles based on the debt to EBITDA ratio at $t-1$, and observations with negative EBITDA are excluded. For each quartile, the mean net debt issuance scaled by lagged assets at time t is reported for firms that violate and do not violate a covenant at time $t-1$. Panel C presents fixed effects regression coefficient estimates where the specification in column (1) includes an interaction between the lagged covenant violation indicator variable and the lagged leverage ratio and the specification in column (2) includes an interaction between the lagged covenant violation indicator variable and the lagged debt to EBITDA ratio. Standard errors are reported in parentheses and are clustered by firm.

Panel A: Leverage Ratio

	Mean Net Debt Issuance Scaled by Lagged Assets (Basis Points)$_t$	
	No Covenant Violation$_{t-1}$	Covenant Violation$_{t-1}$
Leverage Quartile 1	106	99
Leverage Quartile 2	56	14^{++}
Leverage Quartile 3	39	-16^{++}
Leverage Quartile 4	69	-27^{++}

Panel B: Debt to EBITDA Ratio

	Mean Net Debt Issuance Scaled by Lagged Assets (Basis Points)$_t$	
	No Covenant Violation$_{t-1}$	Covenant Violation$_{t-1}$
Debt to EBITDA Quartile 1	91	57
Debt to EBITDA Quartile 2	40	-30^{++}
Debt to EBITDA Quartile 3	25	-23^{++}
Debt to EBITDA Quartile 4	32	-20^{++}

Panel C: Fixed Effects Regressions

Dependent Variable: Net Debt Issuance$_t$/Assets$_{t-1}$ (Basis Points)		
	(1)	(2)
Leverage ratio$_{t-1}$	-500.0^{**}	
	(22.4)	
Leverage ratio$_{t-1}$ * Violation$_{t-1}$	-135.1^{**}	
	(21.6)	
Debt to EBITDA$_{t-1}$		-19.5^{**}
		(1.2)
Debt to EBITDA$_{t-1}$ * Violation$_{t-1}$		-4.0^{*}
		(1.6)
Number of firm-quarters	104,383	78,643
Number of firms	4,765	4,272
R^2	0.116	0.105

$^{++}$ Statistically distinct from "no covenant violation" at the 1% level.

*,** Statistically distinct from zero at the 5% and 1% level, respectively.

distribution of lagged leverage ratios. In fact, covenant violators in the second quartile have an average net debt issuance that is lower than that of nonviolators in the highest leverage quartile, a difference that is statistically distinct from zero at the 5% level. If managerial rebalancing is the only effect, then it is unlikely that violators in lower leverage quartiles would be reducing net debt issuance by more than nonviolators in higher leverage quartiles.

In Panel B, the sample is split into quartiles based on the debt to EBITDA ratio. Column (2) shows that violators have lower net debt issuance relative to nonviolators in every quartile of the debt to EBITDA ratio. As in Panel A, covenant violators with relatively low debt to EBITDA ratios have lower net debt issuance than nonviolators with high debt to EBITDA ratios. For example, violators in the second quartile have net debt issuance of –30 basis points while nonviolators in the fourth quartile have net debt issuance of +32 basis points, a difference that is statistically distinct from zero at the 1% level. If one interprets the debt to EBITDA ratio as a measure of financial health, the results in Panel C suggest that financially healthy violators reduce net debt issuance by more than financially unhealthy nonviolators, implying that financial distress alone (i.e., in the absence of a covenant violation) is not responsible for the reduction in net debt issuance that we observe.

Finally, Panel C examines the rebalancing alternative in a regression context. The specification in column (1) is identical to the specification reported in column (1) of Table III, except for the inclusion of the lagged leverage ratio and the interaction of the lagged leverage ratio with the lagged covenant violation indicator variable. As the coefficient estimate on the lagged leverage ratio indicates, firms reduce net debt issuance when leverage ratios are high. This finding coincides with the rebalancing found in previous empirical capital structure studies. However, the coefficient estimate on the interaction term indicates that covenant violators reduce net debt issuance by significantly more than nonviolators in response to high leverage ratios. In fact, net debt issuance decreases by an additional 27% relative to the baseline rebalancing effect implied by the lagged leverage coefficient.

Column (2) reports the results from a similar specification with the debt to EBITDA ratio. These results reinforce the interpretation that rebalancing alone is not responsible for our results. Firms with higher debt to EBITDA ratios reduce net debt issuance, but the effect among violators is significantly stronger. The response of net debt issuance by violators to higher debt to EBITDA ratios is 20% stronger than the response by nonviolators.[13]

The coefficient estimates in Panel C also provide a useful interpretation of magnitudes. Relative to managerial rebalancing, the estimates suggest that a covenant violation is associated with a reduction in net debt issuance that is 20 to 30% larger in response to higher debt levels. That is, covenant violations,

[13] The differences in coefficient estimates across the leverage and debt to EBITDA ratio specifications are due to scale—normalizing by the relative standard deviations leads to economically similar effects.

on average, lead to a reduction in net debt issuance that is significantly larger than we would otherwise observe.[14]

D.2. Regression Discontinuity Using Dealscan Sample

In this section, we isolate the analysis to a sample of loans for which we know the covenant thresholds, as well as any changes (or "buildup") in those thresholds. Such an analysis alleviates two potential concerns associated with the preceding results: (1) the exact covenant threshold is unknown and (2) we observe only reported covenant violations. The analysis and sample are similar to those found in Chava and Roberts (2008). To avoid any redundancy and manage the length of our study, we purposely keep the discussion of the data and methodology brief, referring the reader to their study for further details.

The sample consists of all loan-quarter observations satisfying the following criteria. First, the loan and borrower must lie in the intersection of the Dealscan and Compustat databases to ensure the availability of loan and accounting information. Second, the loan must contain either a current ratio or net worth covenant to ensure an accurate measurement of the relevant accounting variable.[15] The final sample is a panel of firm-quarter observations in which each observation either is or is not in violation of a covenant. To determine whether a firm is or is not in violation, we compare the firm's actual accounting measure (i.e., current ratio or net worth) to the covenant threshold implied by the terms of the contract.

Our empirical strategy in this section can be viewed as a refinement of that discussed above in Section III.C, because this subsample allows us to incorporate the precise distance to the covenant threshold into our regression specification. Formally, our empirical strategy in this section is a regression discontinuity design in which the function mapping the distance between the underlying accounting variable and the covenant threshold is discontinuous. Specifically, our treatment variable, *Violation*, is defined as

$$Violation_{it} = \begin{cases} 1 & \text{if } z_{it} - z_{it}^{0} < 0 \\ 0 & \text{otherwise,} \end{cases} \tag{2}$$

where z is the observed current ratio (or net worth), z^0 is the covenant threshold, and i and t index firms and quarters, respectively.

Our empirical model for this section is similar to that in the previous section,

$$\frac{D_{it} - D_{it-1}}{A_{it-1}} = \alpha_i + \sum_{f=1}^{4} \theta_f + \sum_{1994q1}^{2004q1} \delta_t + \beta_0 Violation_{it-1} + \beta_1' X_{it-1} + \eta_{it}, \tag{3}$$

[14] Results available in the Internet Appendix also show that violators across the change in EBITDA/book assets ratio distribution experience lower net debt issuance than non-violators.

[15] Covenants restricting the debt to EBITDA ratio, for example, create a problem when trying to measure this ratio with Compustat accounting data since "debt" can refer to any component of a firm's debt structure including: long-term, short-term, senior, junior, secured, total, funded, etc.

where all variables are defined above. The parameter of interest is β_0, which represents the impact of a covenant violation on firm i's net debt issuing activity. As discussed earlier, the appeal of the regression discontinuity approach is that the effect of the violation is consistently estimated under very mild assumptions. Specifically, the identifying assumption is that the error term, η_{it}, does not exhibit precisely the same discontinuity as the violation (Hahn, Todd, and Van der Klaauw (2001)). See Rauh (2006) for another application in corporate finance.

The estimation results, available in the Internet Appendix,[16] reveal a decline in net debt issuance of 60 basis points following the violation—almost identical to that found above for the whole sample. Importantly, this result is robust to the inclusion of traditional determinants of capital structure decisions, as well as smooth functions of the underlying distance to the covenant threshold. Additionally, estimation of equation (3) on the subsample of firm-quarter observations that are "close" to the point of discontinuity reveals a similar 60 basis point decline in net debt issuances following the violation. Overall, the similarity of our results in the Dealscan sample with those found in the primary sample mitigates concerns over endogeneity of the covenant threshold and self-reported violations.

D.3. Avoiding Covenant Violations

One consideration associated with covenant violations is the impact that they might have on the ex ante actions of managers, who may attempt to avoid violating a covenant through accounting manipulation or other activities (e.g., Dichev and Skinner (2002) and Dyreng (2007)). We consider this issue by examining the impact of incorporating into our regressions measures of abnormal accruals, which, despite being somewhat noisy (Dechow, Sloan, and Sweeney (1995)), have "the potential to reveal subtle manipulation strategies related to revenue and expense recognition" (DeFond and Jiambalvo (1994, p. 149)). We examine several different measures including: abnormal total accruals (DeFond and Jiambalvo (1994)), abnormal working capital accruals (DeFond and Jiambalvo (1994)), and abnormal current accruals (Teoh, Welch, and Wong (1998) and Bharath, Sunder, and Sunder (2008)). The results, not reported, reveal a marginally significant correlation with financial policy but, more importantly, reveal nearly identical estimates of the impact of covenant violations on net debt issuance.

Beyond ensuring the robustness of our inferences, these results are reassuring for two additional reasons. First, the notion that managers can consistently fool commercial bank lenders through accounting manipulation is questionable. CFOs are required to submit periodic covenant compliance reports that discuss in great detail the computation of and adherence to each financial covenant. Additionally, creditors have significant experience in originating and monitoring loans and are well aware of possible accounting manipulations. Indeed, most every loan contract spells out in detail the precise accounting conventions

[16] An Internet Appendix for this article is online in the "Supplements and Datasets" section at http://www.afajof.org/supplements.asp.

to be used in the computation of the covenants' accounting ratios (Taylor and Sansone (2007)). Finally, the lending process is a repeated game, which lessens the attractiveness of any borrower deception because of the risk to future financing needs.

Second, the survey results of Graham, Harvey, and Rajgopal (2005) suggest that managers are more likely to take real actions, such as cutting investment, to meet these goals, as opposed to manipulating accounting statements. In our context, the fact that some managers may cut investment to avoid a violation will tend to work against finding evidence of a significant effect of covenant violations on net debt issuing activity. The reason is as follows: Managers are more likely to take action to avoid a violation when violating is costly. Consequently, the observed violations are precisely those situations in which managers believe that it is relatively less costly to violate. For example, it is less costly for managers with poor investment opportunities to cut investment in order to avoid the violation. Therefore, observed covenant violators have better investment opportunities, on average, than the unobserved sample of true violators. Given the better investment opportunities, banks are less likely to increase the cost of credit or limit access to credit. Thus, the possibility that some managers take ex ante actions to avoid violations suggests that our estimates of the impact of covenant violations on financial policy may be conservative.

IV. How Do Violations Affect Financial Policy?

A. *Initial versus Subsequent Violations*

Covenant violations occur in clusters. For example, in our sample, a firm is almost 20 percentage points more likely to violate a financial covenant if it violated a covenant in the previous four quarters. As a result, it is important to take into account how the sequence of violations affects security issuance decisions. To do so, we estimate the regression

$$\Delta\left(\frac{D_{it}-D_{it-1}}{A_{it-1}}\right)=\sum_{f=1}^{4}\theta_f+\sum_{t=1996q3}^{2005q2}\delta_t+\sum_{j=0}^{8}\beta_j I(InitialViolation_{it+j})+\sum_{j=0}^{8}\gamma_j I(SubViolation_{it+j})+f(\Delta X_{it},\Delta X_{it-1})+\eta_{it},\qquad(4)$$

which separates the effect of initial versus subsequent violations on the change in net debt issuance policy. We focus on a first difference specification to ease the interpretation of our results, though results from a firm fixed effects specification produce identical inferences. The estimate for β_1 captures the effect of an initial covenant violation in the previous quarter on the change in net debt issuance, and the estimate for γ_1 captures the incremental *additional* effect of a subsequent violation in the previous quarter on net debt issuance. An initial violation is defined to be a violation for a firm that has not violated a covenant in the previous four quarters, although lengthening this window to the previous eight quarters does not affect the results. All other violations

are considered subsequent violations. We include eight lags of both initial and subsequent violations to capture the full time series of the effect. The matrix of control variables X includes all covenant control variables in column (3) of Table III, Panel B.

Column (1) of Table VII presents estimates of the effect of initial ("New") violations on the change in net debt issuance (i.e., with the parameter vector γ restricted to zero). The estimates show that initial violations have a strong effect on the change in net debt issuances: In the first-quarter after the initial covenant violation, net debt issuance is 81 basis points lower than the previous quarter. In the second quarter after the initial violation, net debt issuance declines by an additional 66 basis points. The cumulative effect of initial violations is almost 150 basis points in the two quarters after the initial violation. The effect of initial violations is quickly realized, as is evident from the smaller effects of the third through eighth lags.

Column (2) presents estimates from the full specification including subsequent violations. Unlike the initial covenant violations, subsequent violations have a much smaller incremental effect on net debt issuances. The largest effect occurs in the quarter immediately after a subsequent violation, where net debt issuance declines by almost 30 basis points. However, this estimate is not statistically significant at a reasonable confidence level. The effect of initial violations on net debt issuance in the two quarters after the violation remains statistically significant and economically large.

Because there is a large decline in net debt issuance in the two quarters after an initial violation, we examine whether subsequent violations have an incremental effect on net debt issuance if they occur during these two quarters. Column (3) shows that the coefficient estimate on $new\ violation_{t-1} * subsequent\ violation_t$ is –44.7, which implies that net debt issuance falls by an additional 44.7 basis points in the quarter after an initial covenant violation if the borrower again violates a covenant. However, the estimate is not statistically significant at a reasonable confidence level. Subsequent violations in the second-quarter after the violation also do not have a statistically significant incremental effect on net debt issuance.

Overall, the estimates in Table VII demonstrate that the sharpest decline in net debt issuance occurs after the initial violation as opposed to subsequent violations. This result suggests that there are significant changes in financial policy after creditors initially obtain acceleration and termination rights, a finding that is consistent with models by Aghion and Bolton (1992) and Dewatripont and Tirole (1994). These results support the interpretation that the provision of control rights leads to a change in net debt issuance policy that would not have otherwise occurred.[17]

[17] In unreported analysis, we perform a similar examination of net equity issuances. The results reveal no significant differences in the response of net equity issuances to initial versus subsequent violations, suggesting that the results in Figure 1 are not masking heterogeneity in the equity policy response to covenant violations.

Table VII

Initial versus Subsequent Covenant Violations

This table presents coefficient estimates from first difference regressions of the change in net debt issuances on new versus subsequent covenant violation indicator variables. A new violation is defined to be a violation in which there is no violation by the same firm in the previous four quarters. All other violations are subsequent violations. All specifications include calendar year-quarter indicator variables and fiscal quarter indicator variables, and all control variables in Column (3) of Table III, Panel B. Standard errors are reported in parentheses and are clustered by firm.

Dependent Variable: Change in [Net Debt Issuance$_t$/Assets$_{t-1}$ (Basis Points)]				
	(1)	(2)		(3)
New violation$_t$	17.1	16.2	New violation$_t$	17.1
	(15.9)	(15.9)		(15.9)
New violation$_{t-1}$	−80.7**	−81.1**	New violation$_{t-1}$	−64.0**
	(16.9)	(18.0)		(21.7)
New violation$_{t-2}$	−66.4**	−56.4**	New violation$_{t-1}$ ∗ Subsequent violation$_t$	−44.7
	(16.1)	(16.6)		(34.5)
New violation$_{t-3}$	0.6	8.3	New violation$_{t-2}$	−68.3**
	(15.6)	(16.1)		(20.3)
New violation$_{t-4}$	−19.0	−12.2	New violation$_{t-2}$ ∗ Subsequent violation$_{t-1}$	−27.7
	(15.9)	(16.6)		(36.8)
New violation$_{t-5}$	−42.5**	−30.1	New violation$_{t-2}$ ∗ Subsequent violation$_t$	45.7
	(16.4)	(17.1)		(40.4)
New violation$_{t-6}$	−10.0	0.4	New violation$_{t-3}$	0.6
	(16.0)	(17.1)		(15.6)
New violation$_{t-7}$	−12.4	−3.8	New violation$_{t-4}$	−19.0
	(15.8)	(17.3)		(15.9)
New violation$_{t-8}$	5.0	6.5	New violation$_{t-5}$	−42.5**
	(16.5)	(17.7)		(16.4)
Subsequent violation$_t$		−1.8	New violation$_{t-6}$	−10.1
		(15.0)		(16.0)
Subsequent violation$_{t-1}$		−28.8	New violation$_{t-7}$	−12.4
		(17.9)		(15.8)
Subsequent violation$_{t-2}$		−1.5	New violation$_{t-8}$	4.9
		(17.9)		(16.5)
Subsequent violation$_{t-3}$		−4.8		
		(17.8)		
Subsequent violation$_{t-4}$		−9.4		
		(17.5)		
Subsequent violation$_{t-5}$		−11.7		
		(17.4)		
Subsequent violation$_{t-6}$		1.2		
		(17.9)		
Subsequent violation$_{t-7}$		18.3		
		(19.4)		
Subsequent violation$_{t-8}$		−17.0		
		(15.6)		
Number of firm-quarters	88,485	88,485		88,485
Number of firms	5,609	5,609		5,609
R^2	0.131	0.131		0.131

*,** Statistically distinct from zero at the 5% and 1% level, respectively.

B. Cross-sectional Variation in the Financing Response to Covenant Violations

In this section, we explore cross-sectional variation in the effect of violations. Figure 1 and Table VII demonstrate that the primary drop in net debt issuance occurs in the two quarters immediately after an initial covenant violation. We therefore focus on the drop in these two quarters when examining cross-sectional variation, though using only the first-quarter following the violation produces qualitatively similar results. More specifically, the analysis in Table VIII limits the sample to the 1,593 initial violations in our sample and examines what firm characteristics affect the magnitude of the drop in net debt issuance in the two quarters after the violation. The general specification takes the following form.

Table VIII

Cross-sectional Heterogeneity of the Effect of Covenant Violation

This table examines the cross-sectional heterogeneity of the effect of a covenant violation on net debt issuance. The sample is isolated to 1,593 observations where firms experience a new covenant violation. The dependent variable is the change in net debt issuance from the quarter of a new violation to two quarters after a new violation. A new violation is defined to be a violation in which there is no violation by the same firm in the previous four quarters. The independent variables are measures of firm characteristics at the time of the violation. Standard errors are clustered by firm.

Dependent Variable: Change in [Net Debt Issuance$_t$/Assets$_{t-1}$ (Basis Points)] in Two Quarters after New Violation				
	(1)	(2)	(3)	(4)
Constant	−126.1**	−106.2**	−47.8	15.6
	(15.3)	(18.2)	(64.3)	(70.2)
Subsequent violation$_{t-1}$		−58.9	−52.2	−47.9
		(32.5)	(32.3)	(32.4)
Cash/assets$_{t-2}$			−49.4	−52.9
			(105.4)	(105.1)
Leverage ratio$_{t-2}$			−440.4**	−483.1**
			(87.9)	(89.0)
Market-to-book$_{t-2}$			29.0*	28.1*
			(13.2)	(13.2)
EBITDA/assets$_{t-2}$			212.8	259.0
			(316.9)	(317.8)
(Tangible assets/assets)$_{t-2}$			105.9	106.2
			(73.0)	(73.0)
Ln(assets)$_{t-2}$			−0.2	−14.7
			(9.4)	(11.6)
Has S&P credit rating$_{t-2}$				114.5*
				(53.1)
Number of observations	1,593	1,593	1,593	1,593
Number of firms	1,332	1,332	1,332	1,332
R^2	0.000	0.002	0.026	0.028

*,** Statistically distinct from zero at the 5% and 1% level, respectively.

$$\left(\frac{D_{it} - D_{it-1}}{A_{it-1}}\right) - \left(\frac{D_{it-2} - D_{it-3}}{A_{it-3}}\right) = \alpha + \beta X_{it-1} + \gamma X_{it-2} + \eta_{it}. \quad (5)$$

This specification provides estimates of how firm characteristics in the quarters of and immediately after the violation lead to differential reductions in net debt issuance. Our primary goal is to examine whether the provision of termination and acceleration rights to existing creditors after a violation affects borrowers differentially based on their relative bargaining power. As a result, the matrix X contains information on subsequent violations, financial conditions, and access to alternative sources of capital.

Column (1) shows that firms experience an average reduction in net debt issuance of 126 basis points in the two quarters after the covenant violation, which is consistent with Panel A of Figure 1 and Table VII. In column (2), we explore whether the drop in net debt issuance is larger if a violator subsequently violates a covenant in the quarter after the violation. Consistent with the estimates in Table VII, we find that firms that subsequently violate a covenant experience an additional drop of 59 basis points, but this estimate is statistically significant only at the 7% level.

In column (3), we examine how financial condition at the time of the violation influences the reduction in net debt issuance after the violation. Firms with high leverage ratios experience sharper declines in net debt issuance. The estimate implies that a one-standard deviation increase in the leverage ratio (21%) in the quarter of the violation leads to an additional 92 basis point reduction in net debt issuance, a 75% reduction relative to the mean. Firms with high market-to-book ratios experience a drop in net debt issuance that is somewhat smaller, though still statistically significant. A one-standard deviation increase in the market-to-book ratio (1.3) leads to a change in net debt issuances that is 36 basis points higher, 30% relative to the mean reduction. In column (4), we include an indicator for whether the firm has a credit rating from S&P. We find that on average rated firms experience a reduction in net debt issuance that is 115 basis points smaller than unrated firms, which is almost 100% of the mean effect.

The results in columns (3) and (4) demonstrate that covenant violations lead to a larger reduction in net debt issuances for firms with reduced debt capacity, lower market valuations, and limited access to rated debt instruments. These findings suggest that covenant violations have a strong effect on net debt issuance when the firm cannot easily access alternative sources of capital on more favorable terms.[18] Thus, violations by firms with limited outside options tend to be more severe in terms of their consequences for financial policy.

C. The Actions of Creditors

Finally, we provide additional evidence on the underlying mechanisms through which violations affect financial policy by examining the 10-Q and

[18] In unreported analysis, we also find that firms' financial policy response to covenant violations is amplified when competitors in the same industry are also experiencing violations, though the effect is statistically weak.

10-K filings of a random sample of covenant violators. An examination of the filings is useful given that some firms provide detailed explanations of the outcome of the covenant violation. These explanations provide unique insight into how creditors use their acceleration and termination rights. The drawback of the explanations is that firms voluntarily choose the level of detail to report. The SEC does not provide strict guidelines for the reporting of covenant violations, other than requiring the firm to report the violation and its effect on the business if material. Therefore, the fact that a firm does not explicitly state that a creditor took some action does not imply that the creditor in fact took no action.

To uncover trends in violation outcomes, we examine the SEC filings of a random sample of 500 initial covenant violators. Panel A of Table IX examines the fraction of firms reporting different outcomes (column (1)) and the cumulative change in net debt issuance in the two quarters after the violation conditional on the outcome (column (2)). As column (1) shows, in 32% of the cases, the firm reports in its SEC filings that existing creditors take some action in response to the violation. The most common action is a reduction in the size of the credit facility (24%). Creditors also increase the interest spread (15%) and require additional collateral (7%). In 63% of the violations, the firm reports that the existing creditors granted a waiver for the violation, but no additional action is reported. However, as aforementioned, SEC regulations do not require firms to detail the exact terms of the waiver, so we cannot be sure that no creditor action was taken when there is no reported action.

The last row of column (1) reveals that only 4% of borrowers report terminating the credit facility in the two quarters after the covenant violation. In other words, despite the unfavorable terms offered by existing creditors, very few firms choose to terminate the existing credit facility. This result suggests that most firms are unable to obtain more favorable financing from alternative sources after a covenant violation, which makes financial decisions particularly sensitive to the willingness of existing creditors to supply credit.[19]

Column (2) shows that the drop in net debt issuance is significantly larger in the two quarters after an initial violation for firms that report some creditor action. Specifically, firms that report some action by existing creditors experience a reduction in net debt issuance equal to 418 basis points—or 4.18% of lagged assets—in just two quarters. Firms that report receiving waivers experience an increase in net debt issuance of 29 basis points, which is statistically significantly distinct from firms that report some creditor action. Finally, net debt issuance for borrowers that terminate their existing credit facility decreases by 190 basis points, but this statistic should be interpreted cautiously given that there are only 22 borrowers in this category.

In Panel B, we examine cross-sectional variation in the propensity for creditors to take action based on firm characteristics in the quarter of and after the violation. We do so by estimating a probit regression via maximum likelihood, where the dependent variable is equal to one if the existing creditor takes an

[19] In four cases, the *creditor* terminated the agreement within two quarters after the initial violation. In all four cases, the creditor first took some action (reducing facility and/or increasing the interest spread), and then requested that the borrower find a new lender.

Table IX

The Response of Creditors to Covenant Violations

This table presents evidence from SEC 10-K and 10-Q filings on how creditors respond to new covenant violations. A new violation is defined to be a violation in which there is no violation by the same firm in the previous four quarters. The data reported in this table are for a random sample of 500 covenant violators for whom we examine the filings in the quarter of, the quarter after, and two quarters after the violation. The change in net debt issuance is the change from the quarter of the violation to two quarters after the violation. Panel A examines the outcomes of the violations, and Panel B presents marginal effect estimates from probit specifications relating the probability of creditors taking action to firm characteristics at the time of the violation. We note that borrowers are *not required* to report any actions taken by creditors.

Panel A: Response to Covenant Violations

	(1) Fraction	(2) Change in Net Debt Issuance, Conditional on Outcome
Creditor takes some action	0.322	−418
Reduction in size of credit facility	0.238	−475
Interest rate increased	0.148	−401
Additional collateral required	0.072	−436
Creditor grants waiver, no action reported	0.626	29^{++}
Borrower terminates credit agreement	0.044	−190

Panel B: Probit Estimates

Dependent Variable: Creditor Takes Some Action Following Violation {0,1}			
	(1)	(2)	(3)
Subsequent violation$_{t-1}$	0.15*	0.12**	0.12**
	(0.04)	(0.05)	(0.05)
Cash/assets$_{t-2}$		−0.43**	−0.44**
		(0.16)	(0.16)
Leverage ratio$_{t-2}$		0.43**	0.47**
		(0.12)	(0.12)
Market-to-book$_{t-2}$		−0.03	−0.02
		(0.02)	(0.02)
EBITDA/assets$_{t-2}$		−1.09*	−1.15*
		(0.49)	(0.49)
(Tangible assets/assets)$_{t-2}$		−0.02	−0.02
		(0.10)	(0.10)
Ln(assets)$_{t-2}$		0.00	0.03
		(0.01)	(0.02)
Has S&P credit rating$_{t-2}$			−0.16*
			(0.06)
Number of observations	474	474	474
R^2	0.018	0.074	0.084

$^{++}$ Statistically distinct from "creditor takes some action" at the 1% level.
*,** Statistically distinct from zero at the 5% and 1% level, respectively.

explicit action and zero otherwise. The estimated marginal effects in column (3) show that firms with high cash balances, low leverage ratios, high cash flow, and an S&P credit rating at the time of the violation are less likely to experience an unfavorable creditor action. These estimates suggest that existing creditors

are less likely to reduce credit, increase interest spreads, or require additional collateral for firms with additional debt capacity and greater access to alternative sources of capital.

In unreported analysis, we also investigate the long-run effects of a covenant violation in our random sample.[20] We find that among firms that report creditor action following covenant violations, net debt issuance drops by almost 140 basis points as a fraction of lagged assets in the two quarters following the violation. Additionally, the decline persists over the subsequent 2 years, which results in a 270 basis point decline in leverage ratios over the 2 years following the violation. In contrast, the drop in net debt issuance and leverage for violators that do *not* report creditor action is much smaller and not statistically distinct from zero at a meaningful confidence level.

Overall, the results in this subsection provide important insights into the mechanism through which violations affect financial policy. A large fraction of firms explain in their SEC filings that existing creditors react to the covenant violation by reducing the size of the credit facility, increasing the interest spread, and requiring additional collateral. In addition, very few borrowers terminate their existing agreement, which suggests that borrowers are unable to obtain more favorable terms from other lenders. Firms with additional debt capacity and greater access to alternative sources of capital are less likely to experience an unfavorable creditor action. Finally, the long-run effect of the violation on financial policy is significantly stronger when firms report creditors taking action in response to the violation. Taken together, these findings suggest that the provision of rights to creditors has a large effect on net debt issuance policy because firms are particularly susceptible to changes in the willingness to supply funds by existing creditors after a violation.[21]

V. Conclusion

This study shows that incentive conflicts play an important role in shaping corporate financial policy. Specifically, we show that financial covenant violations lead to large and persistent declines in net debt issuing activity by providing creditors with limited rights to influence financial policy via changes to the terms of the credit agreement. Consequently, firms' leverage ratios also exhibit a significant decline relative to the typical within-firm variation in leverage ratios; however, covenant violations have a limited impact on the cross-sectional distribution of leverage ratios. Further, we show that the financing response to covenant violations is stronger when (1) existing creditors take various actions (e.g., increase interest rates, reduce allowable borrowings, etc.) to moderate the supply of credit, and (2) borrowers' access to alternative sources of finance is

[20] These results are available in the Internet Appendix.

[21] The case study of L.A. Gear by DeAngelo, DeAngelo, and Wruck (2002) provides additional evidence of the mechanisms documented in our study. They document that violations led to reductions in credit for L.A. Gear after covenant violations, ultimately reducing availability from $360 million to $25 million.

either limited or relatively costly. Thus, covenant violations play a key role in determining the flow of credit to firms because of their ability to allocate control rights to creditors in a state-contingent manner.

While the focus of this study has been on identifying and quantifying the role of incentive conflicts and control rights in shaping financial policy, we believe that the perspective taken here can potentially shed light on several unresolved issues in capital structure. For example, recent research (e.g., Molina (2005), Almeida and Philippon (2006), and Korteweg (2006)) has focused on alternative measures of bankruptcy costs to help explain debt conservatism (Graham (2000)). Similarly, numerous theoretical and empirical studies assume that firms' aversion to high leverage is driven by expected bankruptcy costs (e.g., Bradley, Jarrell, and Kim (1984), Fischer, Heinkel, and Zechner (1989), Leland (1994), Hovakimian (2006)). While a focus on improving the measurement of bankruptcy costs may yield more realistic patterns for capital structure, CFOs rank bankruptcy cost considerations *seventh* in terms of their importance in debt financing decisions (Graham and Harvey (2001)).

Alternatively, CFOs rank maintenance of financial flexibility as the most important reason for limiting debt financing. We believe that a consideration of contractual restrictions and creditor rights outside of bankruptcy may help explain observed capital structures, and may provide an explanation that is more in line with survey evidence and recent theory on the importance of financial flexibility (DeAngelo and DeAngelo (2006)). In particular, our findings suggest that firms may appear ex ante conservative given the expected consequences associated with potential covenant violations. In other words, knowing ex ante that debt contracts impose significant restrictions on corporate behavior and violation of those restrictions impose significant consequences, managers may decide to rely less than they otherwise would on debt financing. We look forward to future research that pursues these possibilities.

Appendix: Variable Definitions and Covenant Violation Search Terms

This appendix details the variable construction for analysis of the Compustat sample. All cash flow statement variables are first disaggregated into quarterly flows.

Total Sales = item 2

Total Assets = item 44

Book Debt = item 51 + item 45

Net Equity Issuance = (item 84 – item 93)/lagged item 44*Net Equity Issuance* = (shrout(t) ∗ cfacshr(t) – shrout(t–1) ∗ cfacshr(t–1)) ∗ (prc(t)/cfacpr(t) + prc(t–1)/cfacpr(t–1)) [*CRSP* definition]

Net Debt Issuance = (book debt – lagged book debt)/lagged item 44

Net Debt Issuance = (data86 – data92)/lagged item 44 [Statement of cash flows definition]

Market Value of Equity = item 14 ∗ item 61

Book Value of Equity = item 44 – (item 54 + annual item 10) + item52

Tangible Assets = item 42
Net Worth = item 44 – item 54
Cash = item 36
Net Working Capital = item 40 – item 49
EBITDA = item 21
Cash Flow = item 8 + item 5
Net Income = item 69
Interest Expense = item 22
Abnormal Total Accruals = based on the study by DeFond and Jiambalvo (1994). Total accruals are first constructed from the statement of cash flows as the difference between cash flow (item 76, adjusted for aggregation) and net cash flow from operating activities (item 108, adjusted for aggregation), normalized by the start-of-period assets. For each firm, this measure is then regressed against 1/assets (item 44), the change in operating income (item 21) normalized by start-of-period assets (item 44), and tangible assets (item 42) normalized by start-of-period assets (item 44). The residuals from these regressions form the abnormal total accruals.

Abnormal Working Capital Accruals = identical to *Abnormal Total Accruals* but for the use of working capital accruals, defined as the change in inventory (item 38), plus the change in accounts receivable (item 37), plus the change in other current assets (item 39), less the change in accounts payable (item 46), less the change in income taxes payable (item 47), less the change in other current liabilities (item 48).

Abnormal Current Accruals = is an annual measure using annual Compustat data and is based on the study by Teoh, Welch, and Wong (1998), and whose derivation follows closely that found in Bharath, Sunder, and Sunder (2008). Total current accruals are first constructed from the statement of cash flows (Hribar and Collins (2002)) as the sum of minus the change in accounts receivables, the change in inventory, the change in accounts payables, the change in taxes payable, and the change in other current assets. Total current accruals are then normalized by last period's total assets and regressed on two variables: (1) the inverse of last period's total assets and (2) the change in sales normalized by last period's total assets. The regression is run separately for each year and each of the Fama and French 38 industry groups. The parameter estimates from these regressions are then used to compute the normal current accruals for each firm in a particular industry-year as the predicted values from the regression. One modification, however, is that the second regressor from the regression is replaced by the difference between the change in sales and the change in accounts receivables normalized by the start-of-period total assets for the computation of normal current accruals. The difference between the actual current accruals and the normal current accruals are abnormal current accruals.

The covenant violation search terms are as follows:

"in violation of covenant," "in violation of a covenant," "in default of covenant," "in default of a covenant," "in technical violation of covenant," "in technical violation of a covenant," "in violation of financial covenant," "in violation of a

financial covenant," "in default of financial covenant," "in default of a financial covenant," "in technical violation of financial covenant," "in technical violation of a financial covenant," "in technical default of financial covenant," "in technical default of a financial covenant," "not in compliance," "out of compliance," "received waiver," "received a waiver," "obtained waiver," "obtained a waiver."

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