Control Rights and Capital Structure: An Empirical Investigation*

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Abstract:

We show that creditors use the rights obtained after financial covenant violations to exert control over a large number of financing decisions of solvent firms. After showing that financial covenant violations occur among almost one third of all publicly listed firms, we find that creditors use the threat of accelerating the loan to reduce net debt issuing activity by over 2% of assets per annum immediately following a covenant violation - an effect that is significantly larger than the effect of previously identified determinants of capital structure decisions. Further, this decline is persistent in that net debt issuing activity fails to return to pre-violation levels even after two years, resulting in a significant decline in leverage. As such, our study highlights the role of incentive conflicts and control rights in shaping corporate financial policies outside of bankruptcy.

A fundamental question in financial economics concerns how firms make financing decisions. While there is significant debate about the underlying factors that affect these decisions, there has been little discussion concerning *who* makes these decisions. Indeed, the static tradeoff (Scott (1976)), the pecking order (Myers and Majluf (1984)), and the market timing (Baker and Wurgler (2002)) theories all assume that the manager is the principal decision maker behind financial policy, and that direct creditor interference is irrelevant as long as the firm meets its interest and principal payments (Townsend (1979), Gale and Hellwig (1985), Hart (1995), Hart and Moore (1998)). While these theories have had some success in explaining financial policy, they have also encountered a number of critics suggesting that the capital structure puzzle posed by Myers (1984) is far from solved.[1]

In contrast to the creditor passivity assumed by traditional capital structure theories, there is a strand of the optimal contracting literature which hypothesizes that creditors may exert direct influence over the security issuance decisions of firms, even outside of bankruptcy (e.g., Aghion and Bolton (1992), Dewatripont and Tirole (1994)). Building on the original insight of Jensen and Meckling (1976), these studies show that the existence of managerial agency problems can give rise to the state contingent allocation of control rights as a means of ensuring investors a fair return in expectation. When ex post managerial actions threaten the value of a debt claim, ex ante debt contracts will enable creditors to intervene in firms' financing decisions prior to payment default. Thus, a natural question to ask is: To what extent do such interventions impact corporate financial policy and capital structure?

The goal of this paper is to answer this question. Our results suggest that creditors frequently exert direct influence over firms' financing decisions outside of payment default states. Further, creditors force changes in security issuance decisions that stand in contrast to the preferences of managers. Overall, our findings quantify the impact of divergent creditor and managerial preferences on capital structure, and they identify a precise mechanism, the transfer of control rights, through which agency problems affect security issuance decisions.

[1] Studies by Frank and Goyal (2003), Fama and French (2005), and Leary and Roberts (2006) all provide evidence suggesting that the pecking order fails to provide an accurate description of observed financing behavior. Studies by Alti (2006), Hovakimian (2006), Kayhan and Titman (2007), Leary and Roberts (2005), and Liu (2005) all provide evidence refuting the implications of market timing. Finally, survey evidence from Graham and Harvey (2001) show that tax and bankruptcy cost considerations rank fourth and seventh, respectively, in terms of their importance in the decision to use debt financing.

Our empirical analysis focuses on violations of financial covenants in private credit agreements. When a firm violates a financial covenant, the origination contract gives creditors a number of rights, including the right to immediately accelerate the loan ("the acceleration right") and the right to terminate any unused portion of revolving credit facilities ("the termination right"). In our setting, creditor "control" is defined as the ability of creditors to use these rights to force changes in security issuance decisions that would otherwise not occur. Given that covenant violations rarely lead to payment default or bankruptcy (Gopalakrishnan and Parkash (1995)), our analysis focuses on the extent to which creditors exert influence over security issuance decisions outside of payment default.

We employ a novel data set that includes the universe of financial covenant violations reported on firm's annual and quarterly SEC filings between 1996 and 2005. We begin by documenting that more than one quarter of all publicly listed firms in the US violate a financial covenant at some point during our sample horizon. Among firms with an average leverage ratio of at least 5%, this fraction approaches one third. The high incidence of covenant violations in our unconditional sample of public firms complements extant research that documents that violations are common among firms that utilize private credit agreements (Dichev and Skinner (2002)). The high incidence also implies that covenant violations are relevant for a large fraction of public firms.

We then show that net debt issuing activity declines sharply and permanently following a covenant violation. More specifically, net debt issuance, as a fraction of total assets, declines by over 0.7% in the quarter immediately after the violation. Further, this decline is persistent, lasting for over two years after the violation. In terms of magnitudes, the impact of a covenant violation on net debt issuing activity is significantly larger than the marginal effects of traditional control variables. 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 a 0.52% quarterly decline in net debt issuances – approximately three fourths of the impact of a covenant violation. The consequence of this persistent contraction in net debt issuance following a violation is a decline in leverage of over 3%. Thus, covenant violations lead to statistically and economically significant changes in financial policy.

We undertake a variety of tests to ensure that the estimated response of financial policy to covenant violations 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, all of our regression specifications employ both firm and period fixed effects. Additionally, our results are robust to a number of controls, including parametric and semi-parametric controls for the variables on which financial covenants are often written. That is, we incorporate smooth and discontinuous functions of measures on which covenants are written, such as the debt to EBITDA ratio, to account for the possibility that these measures contain information about managers' preferences for issuing debt.

We also show that leverage rebalancing, or mean reversion in leverage ratios (e.g., Leary and Roberts (2005), Flannery and Rangan (2006), Kayhan and Titman (2007)), is not behind our findings. We examine the difference between the net debt issuances of violators and non-violators across the leverage distribution, and find that violators with relatively low leverage ratios reduce net debt issuance by more than non-violators with high leverage ratios. Further, we find that covenant violators decrease net debt issuance in response to higher leverage ratios by 27% more than that predicted by mean reversion alone.

To reinforce our identification strategy and support a causal interpretation of our results (i.e., creditors force changes in financial policy that would not have otherwise occurred), we also undertake a regression discontinuity design in order to control for the possible endogeneity of the covenant threshold and, consequently, the violation itself (Chava and Roberts (2006)). Using a sample of loans from Reuters LPC's *Dealscan* database, we are able to identify the covenant threshold and measure the distance from this threshold for a subsample of publicly traded firms. This information enables us to address any remaining endogeneity concerns by (1) incorporating into the regression specification smooth functions of the distance to the covenant threshold, and (2) focusing on the subsample of observations close to the covenant threshold, effectively homogenizing the violation and non-violation states. Our results reveal a nearly identical decline in net debt issuing activity following a covenant violation (0.6%), thereby supporting our interpretation of a causal effect of the violation.

Finally, to gain even further insight into the precise mechanism by which creditors influence security issuance decisions after a covenant violation, we examine a random sample of the SEC filings of violators to identify the specific actions that creditors take. Over 30% of the violators explicitly state that creditors reduce the credit facility amount in response to the covenant violation. More specifically, creditors cut off access to unused lines of credit (Sufi

(2007a)), reduce the size of the existing credit facility, and force the borrower to refinance with another bank, consistent with earlier small sample studies examining the resolution of technical default (e.g., Beneish and Press (1993) and Chen and Wei (1993)). In addition, when a borrower violates a covenant, the lenders on the violated agreement rarely force the firm to reduce *other* debt, suggesting that creditors are primarily concerned with the value of their own claim. Thus, creditors appear to influence security issuance decisions through self-interest rather than a consideration of all costs and benefits of debt financing.

Our main contribution to the capital structure literature is to document that a significant fraction of the observed variation in corporate financial policies is due to the direct influence of creditors. As such, our results suggest that theories in which a firm's capital structure is determined uniquely by managers are incomplete, and that a consideration of creditor incentives may offer insight into the determinants of corporate capital structures. This approach stands in contrast to much of the extant literature, in which creditors are assumed to be passive outside of bankruptcy and where managers make security issuance decisions based on taxes, bankruptcy costs, mispricing, or information asymmetry.[2] Our results also stand in contrast to those of Dichev and Skinner (2002), who interpret the high frequency with which violations occur as evidence that creditors do not impose serious consequences on borrowing firms.

In addition, our results document a precise channel through which the misalignment of incentives between managers and investors impacts financial policy (e.g., Berger, Ofek, and Yermak (1997)).[3] That is, we show *how* agency problems manifest themselves in corporate capital structures by identifying financial covenants, and the corresponding transfer of control rights, as a precise mechanism that creditors use to address incentive conflicts. Further, our results suggest that, in response to violations, creditors force firms to reduce leverage by more than they otherwise would have. This finding suggests that creditors are particularly concerned with managerial agency problems that are exacerbated when leverage ratios are high (Jensen and Meckling (1976)).

[2] To the extent that creditor's decisions following control rights transfers are influenced by their own access to financial capital, our study is also related to recent works by Faulkender and Petersen (2006), Leary (2006), Sufi (2007b), and Lemmon and Roberts (2007) that identify a role for fluctuations in the supply of capital in shaping financial policy.

[3] A number of studies document a negative association between leverage and growth opportunities (e.g., Bradley, Jarrell, and Kim (1984) and Frank and Goyal (2003)), often interpreted as evidence of debt overhang's impact on financing (Myers (1977)). For reviews of the capital structure literature, see Harris and Raviv (1991), Myers (2003), and Frank and Goyal (2005).

Finally, while there is a significant body of literature examining the importance of covenants in debt contracts (e.g., Smith and Warner (1979), Beneish and Press (1993, 1995), Chen and Wei (1993), Sweeney (1994)), we are the first, to our knowledge, to examine how the use of financial covenants fits into the broader capital structure debate. Most closely related to our study are recent works by Chava and Roberts (2006) and Nini, Smith, and Sufi (2006), who show that one implication of financial covenant violations is a reduction in investment activity. Our analysis here shows that financial policy is yet another margin on which creditors intervene in the operation of the firm.

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 through V present the results. Section VI concludes.

I. Data

A. Sample Construction

We begin with all non-financial *Compustat* firm-quarter observations from 1996 through 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 in the full sample. 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.[4] (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 5th and 95th percentiles.[5] 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.[6]

[4] More precisely, we require for each firm-quarter observation nonmissing data for both the contemporaneous and lagged value for 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.

[5] Our technique of Winsorizing *Compustat* control variables at the 5th and 95th percentile, as opposed to the 1st and 99th percentile, is meant to be conservative. Our goal is to reduce the impact of outliers on the predictive power of *control variables* in our analysis. Consistent with this conservatism, the effect of covenant violations on outcomes is stronger in magnitude and statistical significance when we Winsorize control variables at the 1st and 99th percentile.

[6] The largest drop in sample size is due to the fact that data on either current or lagged EBITDA (*item21*) are missing for over 20,000 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)). As Sufi (2007a) notes, the SEC has reinforced this requirement in recent interpretations: "companies that are, 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 their 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 manually 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 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. Overall, the violations tracked in our data represent, on average, more serious violations than violations that could be cured before the end of the reporting period. We explicitly investigate the implications of this non-random selection process later in our analysis.

B. Summary Statistics

Although the SEC requires firms to report unresolved financial covenant violations, they do 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 present summary statistics in Table I for financial covenants contained in a sample of 3,603 private credit

agreements entered into by 1,894 of the firms in our sample.[7] As Table I demonstrates, 97% of the 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 items (29%), coverage ratios (74%), net worth (45%), liquidity (15%), and cash flow (13%).

Table I also hints at the importance of financial covenants in the borrower's capital structure determination. Almost 80% of the credit agreements contain a financial covenant that restricts a ratio with debt in the numerator. In addition, most minimum coverage ratios contain interest payments in the denominator; these coverage ratio covenants therefore place an implicit limit on debt. Overall, almost 90% of the credit agreements contain either an explicit or implicit restriction on the borrower's total debt.

Panel A of Table II documents that 26% of firms in our sample experience a financial covenant violation at some point between 1996 and 2005. Among firms with an average leverage ratio of 0.05 or higher, the percentage of covenant violators increases to 30%. Further, it is important to remember that these are lower bounds on the actual number of covenant violations because our sample conditions on reported violations. It also is important to emphasize that our sample consists of the universe of public firms, with only a few screens based on data availability. Thus, technical defaults occur for a substantial fraction of publicly-listed firms.

Panel A of Table II also presents the fraction of violators by industry, size, and whether the firm has an S&P corporate credit rating. 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. 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 of Table II presents the one 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 one year probability of violating a covenant of 1%, while firms rated BB have a 7% probability. Relative to the one year **payment** default probabilities reported by S&P, the probabilities of a covenant

[7] For more details on these private credit agreements and how they were obtained, see Nini, Smith, and Sufi (2006). There are slightly fewer observations in Table I than in Nini, Smith, and Sufi (2006) given that some agreements detail financial covenants in an attached exhibit that is not included in the SEC filing.

violation are significantly larger in every rating category except firms rated "CCC" or worse, which contains 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 non-trivial probability of violating a financial covenant.

Table III 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 similarly and, consequently, are not reported.[8] (See the Appendix for variable definitions.)

The covenant control variables include many of the accounting ratios on which financial covenants are written (see Table I). 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)).

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 precisely what financial covenants require and what happens when they are violated. To do so, we use the revolving credit agreement between

[8] As Chen and Wei (1993) note: "*Financial Accounting Standard No. 78* (FASB 1983) requires that debt with covenant violations be classified as a current liability unless a waiver has been granted for more than one year." (page 220) Thus, a potential concern with any results examining changes in long term debt from the balance sheet is that they reflect a reclassification, as opposed to an actual change in net debt issuing activity. However, all of our analysis relies on changes in total debt (long- plus short-term) from the balance sheet. Additionally, the similarity of our findings for net debt issuances defined using the statement of cash flows further alleviates this concern.

Digitas Inc. and Fleet National Bank, originated on July 25, 2000, as an illustrative tool. Section 11 of the agreement details the financial covenants, a small excerpt of which is presented below.

> 11. FINANCIAL COVENANTS OF THE BORROWER.
> The Borrower covenants and agrees that …:
> 11.1. Leverage Ratio. The Borrower will not, as of the last day of any fiscal quarter, permit the Leverage Ratio for such fiscal quarter to exceed 2.50:1.00.
> 11.2. Minimum EBITDA. The Borrower will not, as of the end of any Reference Period, permit the consolidated EBITDA of the Borrower and its Subsidiaries for such Reference Period to be less than $20,000,000.

If a borrower fails to comply with any of the financial covenants, then the borrower is in "technical default" of the agreement. Provisions in the credit agreement grant creditors the right to immediately accelerate outstanding amounts in response to technical defaults. In addition, technical defaults give creditors the right to terminate any unused portion of lines of credit or revolving credit facilities. In the Digitas credit agreement, these rights are outlined in Section 14.1 of the agreement and, more generally, are fairly common across most credit agreements.

> 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.

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)).[9]

[9] Thus, extant research suggests that private credit agreements give creditors the ability to force borrowers into ex post renegotiation after covenant violations, where the contract provides the creditor with significant bargaining power. This feature of private credit agreements is broadly consistent with hypotheses developed in the incomplete

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 (2007a), Chava and Roberts (2006), and Nini, Smith, and Sufi (2006)). 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

The majority of the existing empirical research on capital structure is motivated by trade-off, pecking order, and market timing theories. An implicit assumption in these theories is that managers are the sole decision-maker for capital structure decisions, as long as the firm meets its payment obligations. While creditors can indirectly affect managerial decisions through prices, they do not directly influence the security issuance decisions of firms outside of payment default states. The assumption of creditor passivity can be motivated by optimal contracting models in which cash flows are assumed to be either unobservable or non-verifiable. Under this assumption, the resulting optimal contracts specify that creditors remain passive as long as interest payments are met (Townsend (1979), Gale and Hellwig (1985), Hart (1995) and Hart and Moore (1998), see also Diamond (1984)).

There is an alternative class of models in which imperfect performance signals are assumed to be contractible. With this assumption, creditors may exert direct influence over firm financial and investment policy, even if the borrower meets its payment obligations. Jensen and Meckling (1976) 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 even before firms have defaulted on payment obligations.

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

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contracts literature (e.g., Hart and Moore (1988)). Rajan and Winton (1995) suggest that forced renegotiation after covenant violations improves the monitoring incentives of banks.

are likely to distort the manager into inefficient decisions, the decision right is transferred to creditors, as in Zender (1991). Importantly, the contractible state signal is not limited to payment default. Indeed, as they emphasize in their conclusion, the manager continues to receive monetary payoffs even after creditors obtain the decision right.[10]

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. As in Aghion and Bolton (1992), a negative realization of the signal does not necessarily entail payment default; therefore, creditors may obtain influence over firm policy outside of states of bankruptcy.

Although the allocation of control is an important aspect of these models, creditor "control" does not entail creditors literally replacing managers as decision-makers in order to maximize the value of the firm. To the contrary, control rights in the Dewatripont and Tirole (1994) and Aghion and Bolton (1992) frameworks refer to *limited* rights given to creditors to influence firm policy 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 influence firm policy going forward. However, the rights do not give creditors the ability to run all aspects of the firm.[11]

The null hypothesis that we take to our empirical analysis is that creditors play a passive role in firms' capital structure decisions before payment default. The alternative hypothesis is

[10] Hart (1995) has criticized this aspect of the Aghion and Bolton (1992) model: "One of the most basic features of a debt contract is the idea that what triggers a shift in control is the non-payment of a debt … the Aghion-Bolton contract does not have this property (p 101)."

[11] Kaplan and Stromberg (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.

that creditors use the rights they obtain following a covenant violation to force changes in security issuance decisions that would otherwise not occur.

III. The Effect of Covenant Violations on Capital Structure

A. Identification and Empirical Strategy

Our empirical analysis seeks to document whether creditors use their acceleration and termination rights following covenant violations to force changes in financial policy. Identification concerns are first-order given that managers may alter financial policy in response to the variables on which covenants are written. The key counter-factual question is: would managers have reduced net debt issuance by the same amount even in the absence of the covenant violation?

To illustrate both the identification issue and our empirical strategy, we use a simple hypothetical example. Panel A of Figure 1 presents a hypothetical relation between managerial preferences for net debt issuance (scaled by assets) as a function of the debt to EBITDA ratio for a firm. In this example, the hypothetical relation is highly nonlinear and nonmonotonic. The vertical dashed line at 3.0 corresponds to the covenant threshold, implying that a debt to EBITDA ratio in excess of 3.0 is in violation of the covenant. As evident from the figure, the correlation between the event of a covenant violation (debt to EBITDA > 3.0) and net debt issuing activity is negative, which is consistent with the alternative hypothesis that covenant violations cause a reduction in net debt issuance.

However, increases in the debt to EBITDA ratio are also likely correlated with changes in managerial preferences over debt policy. For example, an increase in the debt to EBITDA ratio may also be associated with an increase in the probability of bankruptcy. According to a tax-bankruptcy cost tradeoff theory, the increase in the probability of bankruptcy may lead managers to reduce net debt issuance. Therefore, without controlling for variation in the debt to EBITDA ratio, our estimate of the impact of covenant violations ignores the fact that managers would have reduced net debt issuance even in the absence of the violation.

To address this concern, we estimate the impact of the covenant violation by focusing only on the discontinuous change in financial policy occurring precisely at the covenant threshold. We isolate the discontinuity in the regression specifications by including as right hand side variables a covenant violation indicator variable along with linear, non-linear, and non-

parametric functions of the underlying variables on which covenants are written. 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 exact same ratios and levels of the ratios as creditors to determine financial policy.

Both anecdotal and statistical evidence suggest that this assumption is valid. First, discussions with commercial lenders suggest 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.[12] Second, extant research suggests 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 foregone interest payments, it is unlikely that creditors place covenants at thresholds that managers would have used themselves in the absence of the covenants.

Panel B of Figure 1 illustrates the intuition behind our identification strategy. Our identifying assumption enables us to approximate managerial preferences for net debt issuance with a wide range of smooth and discontinuous functions of the debt to EBITDA ratio. With the inclusion of the functions of debt to EBITDA, as well as other controls, the coefficient estimate on the violation indicator variable, represented by the "jump" in Figure 2, represents the impact of the violation on net debt issuance.

B. Graphical Analysis

We begin our investigation with a firm fixed effects analysis to identify the effect of the covenant violation on financial policy and corporate leverage in event time relative to the violation. Specifically, we estimate the following specification for the outcome variable *y*:

$$y_{it} = \alpha_i + \sum_{f=1}^{4} \theta_f + \sum_{t=1996q3}^{2005q2} \delta_t + \sum_{j=-2}^{8} \beta_j I\left(Violation_{it+j}\right) + \eta_{it} \qquad (1)$$

[12] 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.

where i indexes firms, t indexes quarters, α_i corresponds to a firm fixed effect, θ_f corresponds to a fiscal quarter fixed effect, δ_t corresponds to calendar year-quarter fixed effect, $I(Violation_{it+j})$ is a set of indicator variables surrounding the quarter in which a covenant violation occurred (j=0), and η_{it} is a random disturbance assumed to be possibly heteroskedastic and correlated within firms (Petersen (2006)). The β_j correspond to the deviation of y from the firm-specific average for the quarters around the time of the covenant violation. To ensure that are results are not an artifact of a changing sample composition (i.e., firm exit), we restrict attention to firms that are in the sample for at least eight quarters after the covenant violation; however, relaxing this restriction has little effect on our parameter estimates.

Figures 2 through 4 present graphical representations of β_{-2} through β_8, along with corresponding 90% confidence intervals, for y equal to net debt issuance, net equity issuance, and book leverage, respectively. Figure 2 shows that for the three quarters up to and including the quarter of the violation, firms experience no significant change in net debt issuance, and there is no discernable trend.[13] Immediately following the violation, firms experience a sharp decrease in net debt issuance. By the second quarter after the covenant violation, net debt issuance activity has fallen by 0.7% of assets relative to the issuance activity in the quarter of the violation. This decline is not only 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 3%. Additionally, this change in net debt issuance policy shows persistence. Even two years later, net debt issuance is significantly lower than it was in the three quarters up to and including the quarter of the covenant violation.

Figure 3 presents the results for net equity issuance. Unlike net debt issuance results, there is no sharp change in net equity issuance right after the covenant violation. There is some evidence of an increasing trend following the violation; however, it is statistically weak and economically small.

Figure 4 shows that the sharp and persistent reduction in net debt issuance revealed by Figure 2 has a significant effect on leverage ratios. By the fourth quarter after the covenant violation, firm leverage is statistically significantly lower than that in the quarter *before* the covenant violation. By the sixth quarter after the violation, firm leverage is not statistically

[13] Unreported results examining earlier periods (-8 through -2) also reveal no discernible trend in net debt issuing activity prior to the violation.

distinct from the average leverage of the firm outside the covenant violation window. In other words, in six quarters, the firm reduces its leverage from almost 300 basis points above the firm mean back to the firm mean. The mean leverage ratio of firms that violate a covenant at some point during our sample horizon is 0.27, which implies a relative reduction in leverage of over 10% following the covenant violation.

Before we more formally address identification concerns in the next section, we highlight several features of Figures 2 and 4 that suggest our results thus far are a consequence of the covenant violation, as opposed to shifts in managerial preferences. First, Figure 4 shows that leverage ratios are well above the firm mean even before the covenant violation, but Figure 2 shows that the decrease in net debt issuance begins only after the firm violates a financial covenant. Second, Figure 4 suggests that firms push their leverage ratios after the violation well below the leverage ratio measured before the covenant violation. These two facts suggest that firms are already above their long run average leverage ratio before the covenant violation, but they only reduce net debt issuance after violating a covenant. Finally, Figure 2 shows that the major change in net debt issuance policy is concentrated in the quarter immediately after the covenant violation. In fact, the quarter immediately after the violation is the only quarter in which the firm experiences a statistically significant change in net debt issuance policy.

C. Isolating the Impact of the Covenant Violation

C.1. Covenant Controls

In this sub-section, we implement the identification strategy discussed in Section III.A in order to isolate the impact of the covenant violation on net debt issuance. For the empirical analysis of the full sample, we construct a matrix of right-hand-side variables, X, consisting of 16 variables on which covenants are written. The matrix includes 12 non-interaction (i.e., level) covenant controls: 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 asset ratio, and the lagged and current interest expense to lagged assets ratio. We also allow for four interaction terms: 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. 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 (Table I).

Following the extant empirical capital structure literature (e.g., Rajan and Zingales (1995)), the matrix *X* also includes the lagged natural logarithm of assets, the lagged tangible to total assets ratio, and the lagged market to book ratio.[14] Given this matrix *X*, we estimate the following firm fixed effects specification,

$$\frac{D_{i,t} - D_{i,t-1}}{A_{i,t-1}} = \alpha_i + \sum_{f=1}^{4} \theta_f + \sum_{t=1996q3}^{2005q2} \delta_t + \beta_0 * Violation_{i,t} + \beta_1 * Violation_{i,t-1} + \Gamma * f(X_{i,t-1}, X_{i,t}) + \eta_{it}, \quad (2)$$

where *f(X)* corresponds to a vector of functions of the variables on which covenants are written, and all other variables discussed above. In unreported analysis, we restrict attention to only lagged right hand variables, measured at time *t-1*, in order to allay any concerns over spurious contemporaneous correlations. The results are qualitatively and quantitatively similar.

Column (1) of Table IV 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 Γ=0). The results show that net debt issuance falls from 8 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 (2006)). The specification reported in column (2) adds linear controls for the 12 non-interaction covenant control variables mentioned above. The adjusted R-square increases almost threefold to over 14% relative to the baseline fixed effects specification. However, the magnitude of the covenant violation coefficient declines only moderately and is still economically and statistically large. The specification reported in column (3) includes the four interaction terms mentioned above, which have little impact on the adjusted R-square 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

[14] Unreported analysis incorporating the median industry leverage ratio (Frank and Goyal (2003)), cash flow volatility, and the marginal tax rate (Graham (1996)) produce qualitatively similar findings.

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 x 16) indicator variables, where each indicator variable equals one if the year-quarter observation for a firm falls in the relevant quintile of the covenant control distribution. The Adjusted R^2 of the regression increases by more than 3 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 unaffected, remaining at -51 basis points with a *t*-statistic of almost 7.0.

The results in Table IV suggest that the covenant violation is uniquely associated with a drop in net debt issuance, even after controlling for the variables on which covenants are written. That is, the decline in net debt issuing activity does not appear to be driven by changes in any of the variables on which covenants are written or changes in previously identified determinants of capital structure. These results suggest that net debt issuance decreases by 51 basis points more than it would have in the absence of the violation.

To gauge the economic significance of these results, it is useful to compare them to the marginal effect of traditional determinants of financial policy. Table V presents the parameter estimates and within firm standard deviations of the control variables used in our specifications and traditionally found in previous empirical capital structure studies (e.g., Rajan and Zingales (1995), Baker and Wurgler (2002), and Frank and Goyal (2005)). 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 (two standard deviation) changes in the underlying variable. Thus, covenant violations coincide with both statistically and economically significant changes in financial policy.

C.2. Managerial Rebalancing of Leverage Ratios

In this sub-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 (e.g., Leary and Roberts (2005), Flannery and Rangan (2006), and Kayhan and Titman (2007)) 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 covenant violation itself.[15]

To address this concern, we first examine the change in net debt issuance for covenant violators versus non-violators 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, i.e., both violators and non-violators. The first column shows a rebalancing effect among non-violators, albeit a non-monotonic 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) documents that the net debt issuance of covenant violators is lower in every quartile of the 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 non-violators in the highest leverage quartile, a difference that is statistically distinct from zero at the five percent 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 non-violators 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 non-violators 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 non-violators with high debt to EBITDA ratios. For example, violators in the second quartile have net debt issuance of -30 basis points while non-violators in the fourth quartile have net debt issuance of 32, a difference that is statistically distinct from zero at the one percent level. If one interprets the debt to EBITDA ratio as a measure of financial health, the results in Panel B suggest that financially healthy violators reduce net debt issuance by more than financially unhealthy non-violators. This supports the interpretation that the covenant violation, and not simply financial distress, is causing the reduction in net debt issuance.

[15] The results in Table IV already mitigate this concern by showing that the magnitude of the effect of covenant violations on net debt issuance is robust to both parametric and non-parametric controls for the lagged leverage ratio.

Table VII examines the rebalancing alternative in a regression context. The specification in column (1) is identical to the specification reported in column (1) of Table IV, except for the inclusion of the lagged leverage ratio and the interaction of the lagged leverage ratio with the lagged covenant violation indicator variable. (The inclusion of additional control variables leads to qualitatively similar findings.) As the coefficient estimate on the lagged leverage ratio indicates, firms reduce net debt issuance when leverage ratios increase. This finding coincides with the mean reversion 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 non-violators in response to increases in leverage ratios. In fact, net debt issuance decreases by an additional 1.4% for covenant violators, an additional decline of more than 27% relative to the base line mean reversion effect.

Column (2) reports a similar specification with the debt to EBITDA ratio. The results are very similar. 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 non-violators.

The coefficient estimates in Table VI also provide a useful interpretation of magnitudes. Relative to managerial rebalancing, the estimates suggest that creditors force a reduction in net debt issuance by 20% to 30% more in response to higher debt levels. The estimates highlight the conflict between creditors and managers, and suggest that creditors utilize their control rights to lower net debt issuance by significantly more than we would otherwise observe.

C.3. Avoiding Covenant Violations

One possible concern with our results above is that they reflect the ex ante actions of managers who attempt to avoid violating a covenant through accounting manipulation or cutting investment. Before formally addressing this concern, we note that the fact that managers may take actions to avoid covenant violations strengthens our main conclusion: covenants have an important effect on firm policy that is counter to the preferences of the manager.

Nonetheless, to address this concern, we begin 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)). 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 (2006)), all of which are formally defined in the Appendix. 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 to be used in the computation of the covenants' accounting ratios (Taylor and Sansone (2007)).

Second, the survey results of Graham, Harvey, and Rajgopal (2005) suggest that managers do not manipulate accounting figures to meet financial reporting benchmarks, such as covenants. Rather, managers explicitly state that they take real actions, such as cutting investment, to meet these goals. In our context, the fact that some managers may cut investment to avoid a violation should bias us *against* finding an effect of covenant violations on net debt issuing activity because 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 would be *less* likely to force a reduction in net debt issuance among our observed sample of violators.

D. Short-Run vs. Long-Run Impact

In Tables IV through VII, we examine the impact of covenant violations in the quarter immediately after the covenant violation in order to isolate the causal effect of creditor control rights on financing decisions. In Table VIII, we examine the long run impact of the covenant violation on net debt issuance and leverage ratios. The regression specifications in columns (1) and (2) of Table VII are identical to the specifications reported in columns (1) and (4) of Table

IV, respectively, but for the inclusion of covenant violations indicators for eight quarters after the covenant violation. The sample for the specification is smaller given the necessity of having violation data for all quarters.

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. As illustrated in Figure 1, net debt issuance for firms 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 IV; 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) demonstrate the long run effect of the sharp and persistent 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. 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 column (4) are from a specification which includes standard controls used in the capital structure literature (lagged natural logarithm of assets, lagged asset tangibility, lagged market to book, and the current and lagged EBITDA, cash flow, and net income scaled by lagged assets). The results are similar.[16]

IV. Regression Discontinuity Design

In this section, we isolate the analysis to a sample of loans for which we know the covenant thresholds. Such an analysis alleviates two concerns with the results above. First, the exact covenant threshold is unknown in our analysis based on violations reported in SEC filings. This could introduce bias if the covenant threshold or the distance to that threshold contains information about managers' preferences for debt financing. Second, the analysis above focuses

[16] We implicitly account for the dynamic properties of leverage by allowing for serial correlation in the within firm error structure (Lemmon, Roberts, and Zender (2006)).

on covenant violations reported in SEC filings, which tend to be more severe on average than all violations. This may bias our results in favor of finding an effect, given that the more severe violations may proxy for changes in unobservable firm quality.

A. Data

Both the data and empirical strategy of this section are similar to that found in Chava and Roberts (2006). To avoid any redundancy and manage the length of our study, we purposely keep the discussion of the data and methodology brief in order to focus our attention on the results, referring the reader to their study for further details.

The dataset used in this section of the paper begins with a sample of loans from the *Dealscan* database that we are able to successfully merge with the quarterly *Compustat* database by linking company names and loan inception dates.[17] This merge generates a sample of 37,764 loans, or tranches, grouped together into 27,022 deals and corresponding to 6,716 firms. Because covenants generally apply to all loans in a deal, we focus our attention on the deal level. Further, we restrict the sample horizon to loans with start dates between 1994 and 2005, and containing a covenant restricting either the current ratio or net worth/tangible net worth to lie above a certain threshold.

The motivations for this sample selection are as follows. First, significant covenant coverage in the *Dealscan* database begins only in 1994. Second, current ratio and net worth covenants appear relatively frequently in the *Dealscan* database, contained in 6,386 deals with a combined face value of over one trillion dollars (Dichev and Skinner (2002) and Chava and Roberts (2006)). Second, as Dichev and Skinner (2002) note, the accounting measures used for these two covenants are standardized and unambiguous. This is in contrast to other covenants that restrict, for example, the ratio of debt to EBITDA. Depending on the specific loan, "debt" may refer to long term debt, short term debt, total debt, funded debt, secured debt, etc. Covenants relying on measures of leverage or interest payments face similar difficulties, which is consistent with the evidence provided by Leftwich (1983) who suggests that one way in which private lenders customize their contracts is through adjustments to GAAP when defining financial statement variables.

[17] We are grateful to a number of research assistants, as well as Michael Boldin and the Wharton Research Data Services (WRDS) staff for aid with this matching process.

Our 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 to the covenant threshold implied by the terms of the contract. As Chava and Roberts (2006) describe, the measurement of the threshold is non-trivial since covenants often change over time, firms enter into overlapping loan agreements, and firms can amend their loans after inception. All of these issues are explicitly addressed in Appendix B of their study, and we follow their construction.

The most important advantage of this dataset is that the distance to the covenant threshold is observable. For each firm quarter in this sample of firms, we know how far each firm is from its covenant threshold and whether the firm is in violation of the covenant. This fact helps mitigate the non-random reporting of covenant violations since this dataset will capture both reported and unreported violations. This fact also mitigates the concern that the covenant threshold contains information about managers' preferences for financing since we can now incorporate this variable directly into the regression specification.

B. Empirical Strategy

Our empirical strategy in this section can be viewed as a refinement of that discussed above in Section III.A. More specifically, this sample 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:

$$Violation_{it} = \begin{cases} 1 \text{ if } z_{it} - z_{it}^{0} < 0 \\ 0 \text{ otherwise} \end{cases} \tag{3}$$

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:

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

where all variables are as defined before. 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.

C. Results

The estimation results using the entire *Dealscan* sample are presented in Panel A of Table IX. The first specification presents the response of net debt issuance to the covenant violation conditional only on firm and year-quarter fixed effects. The estimated magnitude of the effect is close to that found in the previous section: a 0.5% reduction in net debt issuance following the violation. This correspondence mitigates concerns that the results in Section III are biased due to mismeasurement of the underlying variables on which covenants are written.

Column (2) adds a set of control variables often found in empirical studies of capital structure (e.g., Rajan and Zingales (1995) and Frank and Goyal (2005)). The estimated response of net debt issuance increases slightly but is basically unchanged in terms of magnitude and statistical significance. Finally, column (3) takes full advantage of the discontinuity design by including smooth functions of the underlying distance to the covenant threshold. We include both a linear and quadratic term for the current ratio and net worth distances interacted with an indicator variable identifying whether or not the loan contains a current ratio or net worth covenant, respectively. The estimated treatment effect increases slightly to 0.6%, while remaining statistically significant. In unreported analysis, we also examine the effect of including higher order polynomial terms of the distance to the covenant threshold. The results are qualitatively similar.

Because the discontinuity is the source of identifying information, we also estimate equation (4) on the subsample of firm-quarter observations that are close to the point of discontinuity - the "Discontinuity" sample. To remove some of the subjectivity associated with the definition of "close," Chava and Roberts (2006) choose a window width around the covenant threshold equal to 0.20, which is based on the optimal window width for a nonparametric density estimation of a unimodal distribution. The key point is that the choice of window width, while subjective, is at least removed from any financing demands that the firm may have. This restriction aids in homogenizing the sample and sharpening the identification. Intuitively, if a

borrower has a covenant restricting net worth to be greater than $1 billion, then there should be little difference in the borrower when its net worth is $1.05 billion versus $0.95 billion except for the effect of the covenant violation.

The results for the Discontinuity sample are presented in Panel B of Table IX. Following Angrist and Lavy (1999), we do not include the distance to the covenant violation in this specification because the range of the distance in the discontinuity sample is narrow enough that the indicator function is a valid instrument without these controls. Practically speaking, the collinearity between the indicator variable and smooth functions of the distance to default is large within a small interval because step functions are a basis for all smooth functions. Thus, disentangling the effects of the covenant violation captured by the indicator variable from those captured by the functions of the distance to the covenant threshold becomes infeasible.

While the coefficient estimates for both specifications are only marginally significant (at the 10% level), the point estimates are virtually identical to those found in the entire sample. This suggests that the problem is not one of identification but of statistical power – the Discontinuity sample is only 40% of the size of the entire sample.

Overall, the results presented in this section are almost identical to Section III. This suggests that biases associated with self-reported violations and mismeasurement of the underlying variables on which covenants are written are not polluting our estimates. The results further support the interpretation that the reduction in net debt issuance is a direct result of the covenant violation.

V. Additional Evidence from SEC filings

The previous section presents large-sample evidence of the impact of covenant violations on security issuances by firms. In this section, we provide additional evidence from a random sample of covenant violators for which we directly examine the 10-Q and 10-K filings in the quarters around a covenant violation. An examination of the filings is useful given that many 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 note that a creditor took some action does not imply that the creditor in fact took no action.

The explanation of the resolution of a covenant violation by Insteel Industries, Inc. on their 2001 second quarter 10-Q filing provides direct evidence of how creditors use their rights to force a reduction in net debt issuance:

> At September 30, 2000, the Company was not in compliance with certain financial covenants of its senior secured credit facility, which constituted an event of default … On January 12, 2001, the Company and its senior lenders agreed to an amendment to the credit agreement that modified these financial covenants, curing the event of default. Under the terms of this amendment, the maturity date of the credit facility was accelerated from January 31, 2005 to January 15, 2002 …The Company also agreed to permanent reductions in the revolving credit facility from $60.0 million to $50.0 million at January 12, 2001; to $45.0 million at October 1, 2001, and to $40.0 million at December 31, 2001 …
>
> These amendments have significantly increased the Company's interest expense as a result of: (1) scheduled increases in the applicable interest rate margins; (2) additional fees, a portion of which are calculated based upon the Company's stock price, payable to the lenders on certain dates and in increasing amounts based upon the timing of the completion of a refinancing of the credit facility, and (3) higher amortization expense related to capitalized financing.

For Insteel Industries, Inc., creditors reduced the credit facility, shortened the maturity of the loan, and raised the interest rate as a direct response to the covenant violation. There is no indication anywhere in the filing that managers planned on altering the facility or reducing their debt financing had the covenant violation not occurred.

Another example is Environmental Tectonics Corp, who violated financial covenants in their agreement with PNC Bank in the fiscal quarter ending May 28th, 2004. As they report on in their August 2004 10-Q filing:

> During the first quarter of fiscal 2005 [which ended on May 28th, 2004], as a result of the Company's recent operating losses and its violation of certain financial covenants contained in the Agreement, PNC advised the Company that it was instituting certain changes to the revolving credit facility. The changes included reducing the facility to $6,000,000 and requiring the Company to cash collateralize the full facility. These changes became effective on June 2, 2004.
>
> On August 24, 2004, the Agreement was amended to substantially reduce the operating facility … The revolving facility was reduced from $14,800,000 to $5,000,000 … Under the amended Agreement, the Company could no longer borrow cash loans. The Company's long-term bonds were left intact.

In this example, PNC Bank used the right to accelerate the loan to reduce the available credit facility for the company. Indeed, the firm explains that it was PNC Bank, and not management, that "instituted" the changes. Interestingly, while PNC Bank reduced the revolving facility, the bank did not require the company to reduce their long-term bonds.[18]

To uncover systematic trends in creditor actions, we examine the SEC filings of a random sample of 100 covenant violators. As Table X shows, in 31% of the cases, the creditors reduce the size of the credit facility in response to the covenant violation. Creditors reduce the size of the facility by cutting off access to the line of credit (5%), terminating the credit agreement entirely (8%), or reducing the size of the overall existing credit facility (18%). Firms report that creditors increase the interest rate for 13% of the violations, and also collateralize the credit facility for 7% of violations. Finally, in 7% of the violations, the creditors force the borrower to issue convertible securities or equity. As mentioned above, given that firms are not required to report the outcome of the violation, these percentages are lower bounds on the true actions taken by creditors.

While creditors on violated agreements often reduce the size of the credit facility, we do not find one single example of the creditors forcing the borrower to repay *other* debt obligations. In other words, creditors use their termination and acceleration rights to protect the value of their own claim, and do not appear interested in implementing their own notion of an optimal capital structure. Creditors appear to affect security issuance decisions by taking actions designed to protect the value of their own claim, as opposed to active management of the firm's finances.

The examination of SEC filings provides complementary evidence to the large sample evidence presented in Sections III and IV. Specifically, the evidence here illustrates that the hypothesized mechanisms behind our large sample evidence do indeed occur in practice, suggesting that creditors directly influence net security issuance decisions by forcing a reduction in the size of the outstanding credit facility or increasing the cost of debt capital.

VI. Conclusion

This paper documents that the transfer of control rights accompanying covenant violations has significant consequences for corporate debt policy over and above any changes in

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

managers' preferences for debt. Specifically, net debt issuances decline, on average, by 70 basis points in the quarter following a covenant violation. This sharp reduction in net debt issuance is persistent for two years following the violation, and leads to a reduction in leverage ratios by 3%. These findings are robust to controls for the accounting variables on which covenants are written, as well as leverage rebalancing by firms. In fact, covenant violations significantly amplify reductions in net debt issuing activity accompanying leverage rebalancing. Creditors use the acceleration and termination rights following a covenant violation to reduce the size of the credit facility, cut off access to unused lines of credit, and increase interest spreads. Given that 1 in 3 firms that use debt financing experience a financial covenant violation at some point in our sample, our findings show that creditors are able to exert control over the security issuance decisions of a large fraction of public firms. These findings suggest that a consideration of control rights is an important part of the capital structure debate.

In addition to identifying a role for control rights in determining financial policy, our results highlight an alternative perspective of capital structure that may shed light on several unresolved issues. 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, Opler, and Titman (2001), 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 main reason for limiting debt financing. We believe that a consideration of creditor control rights over financial policy outside of bankruptcy may help explain debt conservatism, and may provide an explanation that is more in line with survey evidence. Our findings show that firms appear ex post conservative because creditors use their acceleration rights to force reductions in debt against the will of managers. Our findings also suggest that firms may appear ex ante conservative given the expected consequences associated with a loss of control over firm policy going forward. We look forward to future research that pursues these considerations.

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Appendix

Variable Definitions:

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 def]

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

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

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, Wong, and Welch (1998), and whose derivation follows closely that found in Bharath, Sunder, and Sunder (2006). 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.

Covenant violation search terms:

"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."

Table I
Financial Covenants

This table presents the percentage of private credit agreements with various financial covenants. The sample includes 3,603 private credit agreements made to 1,894 firms.

Type of Covenant	Fraction:	Type of Covenant	Fraction:
Financial covenant	96.5%	Net worth/Tangible net worth	45.2%
		Net worth	25.6%
Debt to cash flow	57.5%	Tangible net worth	19.0%
Total debt to cash flow	56.1%	Stockholders' equity	0.8%
Senior debt to cash flow	8.6%		
		Liquidity-based	14.7%
Debt to balance sheet item	29.2%	Current ratio	7.9%
Debt to total capitalization	19.8%	Quick ratio	2.4%
Debt to net worth	6.9%	Working capital	1.5%
Debt to other balance sheet item	3.4%	Other liquidity-based	3.6%
Debt in numerator covenants	**79.1%**	Cash flow-based	12.7%
Coverage ratio	74.3%		
Fixed charge coverage ratio	38.1%		
Interest coverage ratio	38.0%		
Debt service coverage ratio	4.5%		
Other coverage ratio	3.9%		
Debt or coverage ratio covenants	**89.2%**		

Table II
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 of payment default according to S&P. S&P 1-year cumulative default probabilities are equal-weighted averaged 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	28.5%
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%	

Table III
Summary Statistics

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

	Mean	Median	St. Dev.
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 $debt_t$ /$assets_t$	0.228	0.182	0.221
Covenant control variables			
Net $worth_t$ /$assets_t$	0.495	0.518	0.287
Net working $capital_t$ /$assets_t$	0.254	0.235	0.271
$Cash_t$ /$assets_t$	0.199	0.092	0.231
$EBITDA_t$/$assets_{t-1}$	0.006	0.026	0.068
Cash $flow_t$/$assets_{t-1}$	-0.007	0.017	0.074
Net $income_t$/$assets_{t-1}$	-0.022	0.006	0.077
Interest $expense_t$/$assets_{t-1}$	0.005	0.003	0.006
Other control variables			
Market to book $ratio_t$	2.338	1.572	1.947
Tangible $assets_t$/$assets_t$	0.270	0.194	0.230
Ln($assets_t$)	4.900	4.910	2.384

Table IV
Covenant Violations and Net Debt Issuance

This table presents coefficient estimates of firm fixed effects regressions of net debt issuance on covenant violations and controls. The specifications reported in columns 2, 3, and 4 include lagged natural logarithm of total assets, the lagged tangible assets to total assets ratio, and the lagged market to book ratio as control variables. In addition, the specification in column 2 includes the 12 *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 asset ratio, and the lagged and current interest expense to lagged assets ratio. Specification 3 includes the covenant control variables in addition to 4 *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. Specification 4 includes all covenant control variables and covenant control interaction variables, these variables squared and to the third power, and 5 quantile indicator variables for each of the controls. All specifications include quarter indicator variables and indicator variables for the fiscal quarter. Standard errors are reported in parentheses and are clustered by firm.

Dependent variable: Net debt issuance$_t$/assets$_{t-1}$ (basis points)

	(1)	(2)	(3)	(4)
Covenant violation$_t$	8.4	3.6	2.5	3.5
	(8.1)	(7.8)	(7.9)	(78)
Covenant violation$_{t-1}$	-62.2**	-50.0**	-54.1**	-50.5**
	(7.8)	(7.4)	(7.4)	(7.4)
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	0.141	0.146	0.163

*,** statistically distinct from 0 at the 5 and 1 percent, respectively

Table V
Economic Magnitude of Violation on Net Debt Issuance

This table presents a comparison of the magnitude effects 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 quarter indicator variables and indicator variables for the fiscal quarter. 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 absolute value of a 2 standard deviation change in the right hand side variable on net debt issuance.

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 $_{t-1}$	-531.3**	0.023	-24.4
	(59.4)		
Number of firm-quarters	135,736		
Number of firms	6,381		
R^2	0.107		

*,** statistically distinct from 0 at the 5 and 1 percent, respectively

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 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	107	99
Leverage Quartile 2	56	15**
Leverage Quartile 3	40	-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	14	-23**
Debt to EBITDA Quartile 4	32	-20**

*,** statistically distinct from “no covenant violation” at the 5 and 1 percent, respectively

Table VII
Covenant Violations versus Leverage Rebalancing
A Regression Approach

This table presents coefficient estimates from firm fixed effects regressions of net debt issuance on covenant violations and controls. The sample includes firms that have an average book leverage ratio of 0.05 or greater for the sample. In column (1), the specification includes an interaction between the lagged covenant violation indicator variable and the lagged leverage ratio. In column (2), the specification includes an interaction between the lagged covenant violation indicator variable and the lagged debt to EBITDA ratio, and observations with negative EBITDA are excluded. Standard errors are reported in parentheses and are clustered by firm.

Dependent variable: Net debt issuance$_t$/assets$_{t-1}$ (basis points)

	(1) Leverage Ratio	(2) Debt to EBITDA ratio
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 0 at the 5 and 1 percent, respectively

Table VIII
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 IV. 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, and the current and lagged net income to lagged assets ratio. All specifications include quarter indicator variables and indicator variables for the fiscal quarter. Standard errors are reported in parentheses and are clustered by firm.

Dependent variable:	Net debt issuance$_t$/assets$_{t-1}$ (basis points)		Leverage ratio (basis points)	
	(1)	(2)	(3)	(4)
Covenant violation $_t$	19.6*	11.6	290.7**	214.4**
	(8.7)	(8.3)	(28.8)	(28.4)
Covenant violation $_{t-1}$	-32.8**	-28.7**	222.9**	157.7**
	(8.8)	(8.3)	(24.9)	(24.6)
Covenant violation $_{t-2}$	-51.6**	-43.2**	128.4**	107.0**
	(8.5)	(8.0)	(22.8)	(22.4)
Covenant violation $_{t-3}$	-27.6**	-21.9**	125.3**	107.0**
	(8.8)	(8.2)	(22.7)	(22.4)
Covenant violation $_{t-4}$	-26.6**	-22.7**	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**	-25.4**	40.7	27.1
	(9.1)	(8.6)	(21.6)	(21.0)
Covenant violation $_{t-7}$	-17.9*	-17.3*	9.4	2.1
	(8.7)	(8.2)	(23.0)	(22.4)
Covenant violation $_{t-8}$	-30.6**	-33.7**	-22.9	-21.8
	(9.1)	(8.6)	(27.8)	(27.0)
Control variables:	none	All covenant control variables from Table IV, 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 0 at the 5 and 1 percent, respectively

Table IX
Covenant Violations and Net Debt Issuance
Regression Discontinuity Sample

This table presents coefficient estimates of firm fixed effects regressions of net debt issuance on covenant violations and controls. The sample consists of all firm-quarter observations in which a covenant restricting the current ratio or net worth of the firm is imposed by a private loan found in *Dealscan* during 1994-2005. Panel A presents the results for the entire *Dealscan* sample. Panel B presents the results for the discontinuity *Dealscan* sample, defined as those firm-quarter observations in which the absolute value of the relative distance to the covenant threshold is less than 0.20. All specifications include quarter indicator variables. Standard errors are reported in parentheses and are clustered by firm.

PANEL A: ENTIRE *DEALSCAN* SAMPLE

Dependent variable: Net debt issuance$_t$/assets$_{t-1}$ (basis points)

	(1)	(2)	(3)
Covenant violation$_{t-1}$	-47.1*	-53.7*	-59.8*
	(22.0)	(23.4)	(25.2)
Covenant control variables	None	Market to book, Profitability, Firm size, Z-score, Tangibility, Industry median leverage	Market to book, Profitability, Firm size, Z-score, Tangibility, Industry median leverage, linear and squared distance to default
Number of firm-quarters	4,609	4,609	4,609
R^2	0.125	0.137	0.139

*,** statistically distinct from 0 at the 5 and 1 percent, respectively

Table IX
Covenant Violations and Net Debt Issuance
Regression Discontinuity Sample

PANEL B: DISCONTINUITY *DEALSCAN* SAMPLE

Dependent variable: Net debt issuance$_t$/assets$_{t-1}$ (basis points)

	(1)	(2)
Covenant violation$_{t-1}$	-62.0	-58.3
	(35.6)	(35.4)
Covenant control variables	None	Market to book, Profitability, Firm size, Z-score, Tangibility, Industry median leverage
Number of firm-quarters	1,752	1,752
R^2	0.283	0.307

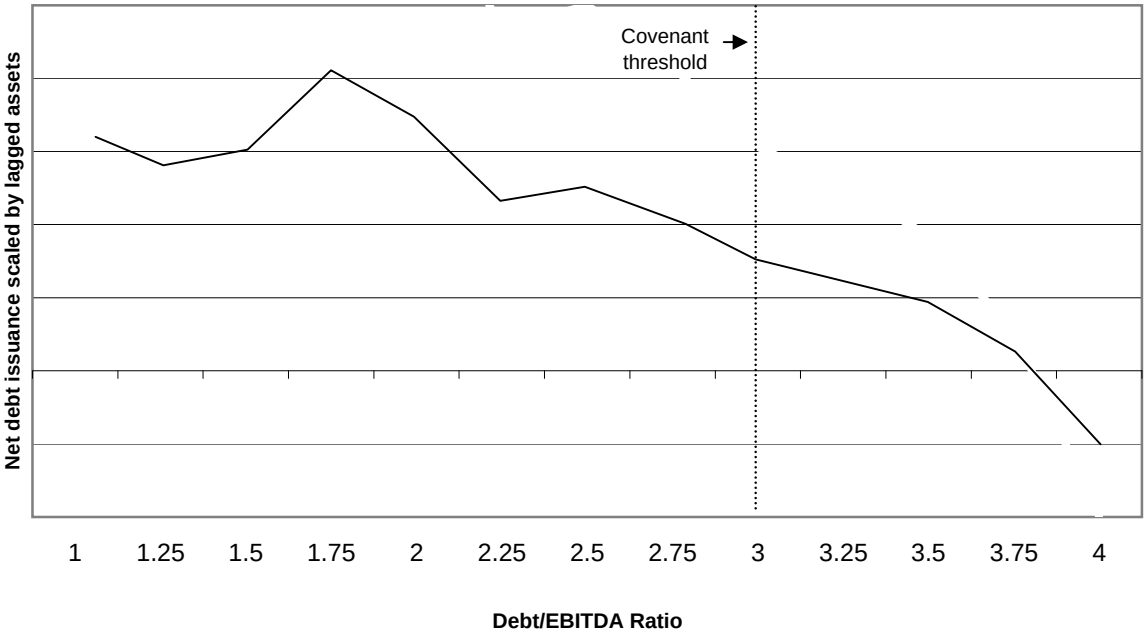
*,** statistically distinct from 0 at the 5 and 1 percent, respectively

Table X
The Response of Creditors to Covenant Violations

This table presents evidence from SEC 10-K and 10-Q filings on how creditors respond to financial covenant violations. The data reported in this table are for a random sample of 100 covenant violators for whom we examine the filings in the quarter of and after the violation.

	Fraction
As a direct result of violation, fraction of borrowers that report:	
Reduction in size of credit facility	0.31
Borrower loses access to revolver/line of credit	0.05
Existing credit agreement terminated	0.08
Existing credit agreement reduced in size	0.18
Interest rate increased	0.13
Borrower forced to issue warrants/equity	0.07
Additional collateral required	0.07

Figure 1
Regression Discontinuity
Panel A



Panel B

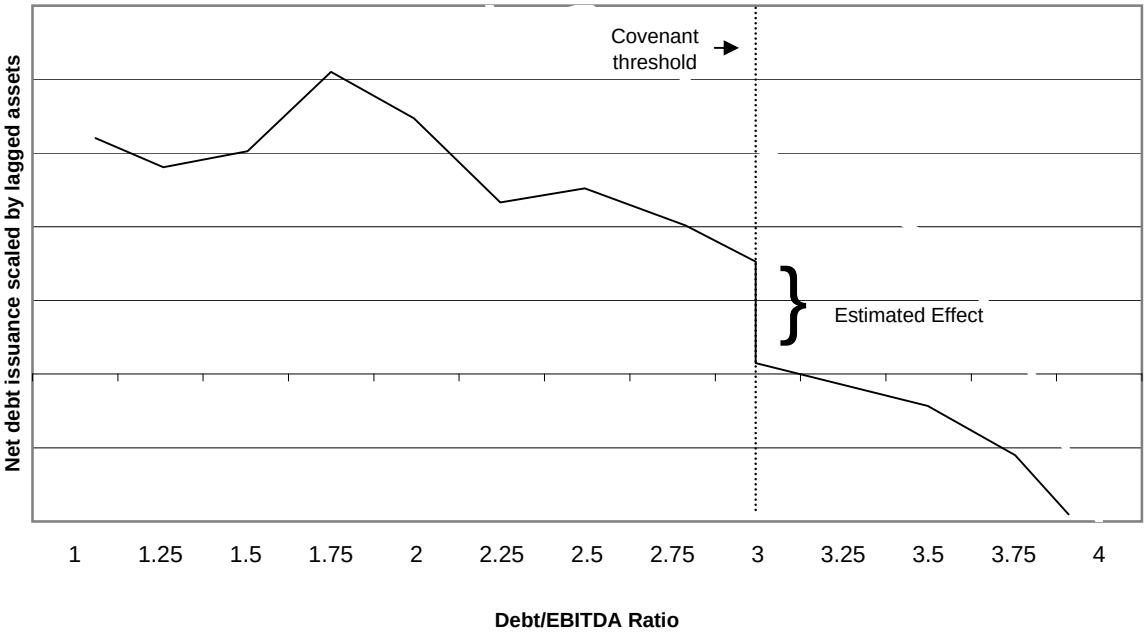


Figure 2
Net Debt Issuance Before and After a Covenant Violation, Full Sample

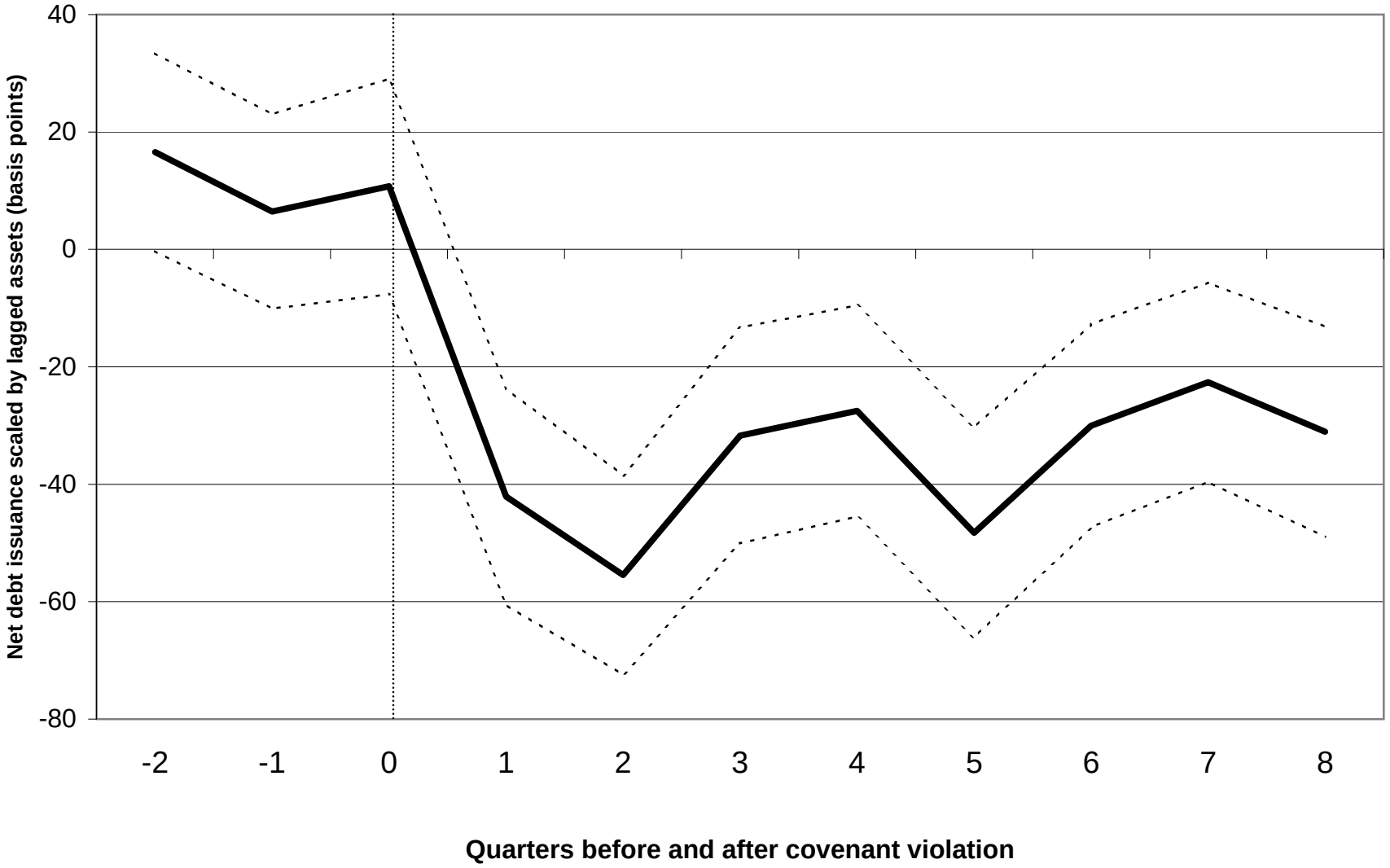


Figure 3
Net Equity Issuance Before and After a Covenant Violation, Full Sample

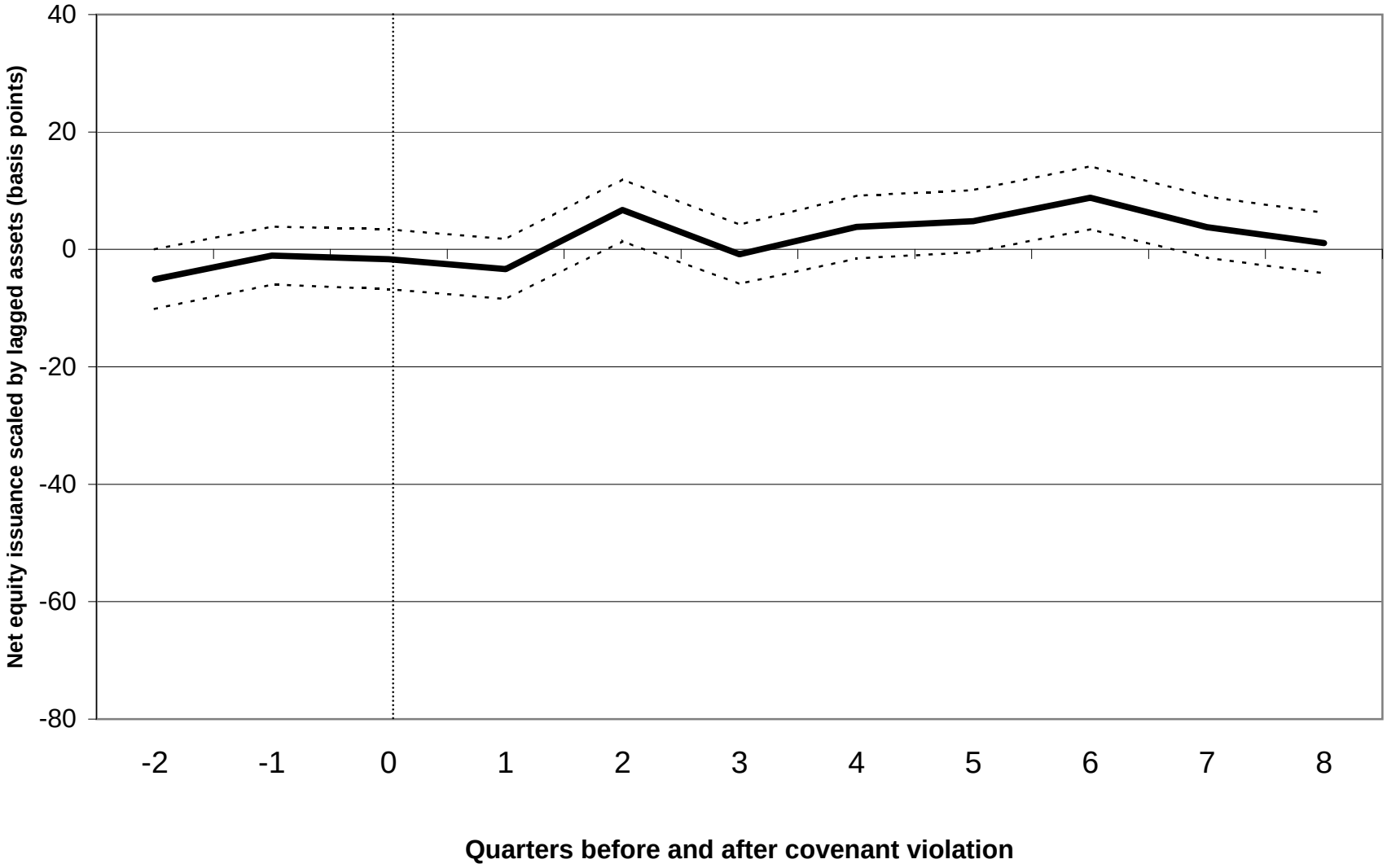


Figure 4
Book Leverage Ratio Before and After a Covenant Volation, Full Sample