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Control Rights and Capital Structure: An Empirical Investigation*

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Abstract:

We show that a large number of financing decisions of solvent firms are dictated by creditors, who use the transfer of control rights accompanying financial covenant violations to address incentive conflicts between managers and investors. 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. 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 gradual decline in leverage of almost 3%. These findings represent the first, of which we are aware, piece of empirical evidence highlighting the role of control rights in shaping corporate financial policies outside of bankruptcy.

A fundamental question in financial economics concerns how firms make financing decisions (e.g., Myers (1984)). 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 tradeoff (Scott (1976)), the pecking order (Myers and Majluf (1984)), and the market timing (Baker and Wurgler (2002)) theories all assume that *managers* are the only decision maker behind financial policy. These theories effectively treat the capital structure decision as a portfolio problem facing managers, where 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 this view has had some success in explaining corporate financial policy, it has also encountered a number of critics suggesting that the capital structure puzzle posed by Myers (1984) is far from solved (Lemmon, Roberts, and Zender (2006)).[1]

In contrast to the creditor passivity assumed by traditional capital structure theories, there is a strand of the incomplete contracting literature which hypothesizes that creditors may exert control 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 managers misbehave or are prone to misbehave, creditors may intervene in firms' financing decisions *prior* to payment default. By allocating control rights in this manner, these theories imply that, at times, investors, as opposed to managers, determine financial policy. In so far as these two parties have incongruent incentives, which party makes the decision may have very different implications for corporate capital structures.

This paper examines the empirical relevance of this view of capital structure. Our evidence shows that creditors frequently exert direct control over firms' financing decisions outside of states of payment default, and that the decisions made by creditors

[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 (2006) 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.

stand in stark contrast to what managers prefer. More precisely, 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. Our results show that control right considerations are an important - and previously missing - element of the capital structure debate.

Our analysis centers on a unique dataset containing information on the universe of credit agreements and financial covenant violations reported on firm's annual and quarterly SEC filings between 1996 and 2005. Using these data, we begin by documenting several interesting facts. First, 97% of credit agreements contain at least one financial covenant and almost 80% of these agreements explicitly restrict the amount of debt that a firm may have in their capital structure. Second, 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. Thus, financial covenants are not only a prominent feature of debt contracts (Smith and Warner (1979) and Bradley and Roberts (2003)) but they are also frequently violated (Dichev and Skinner (2002)) and, importantly, rarely lead to default or acceleration of the loan (Gopalakrishnan and Parkash (1995)).

We then show that after firms violate a financial covenant, their net debt issuing activity declines sharply and permanently. Figure 1 reveals that net debt issuing activity, as a fraction of total assets, declines by 0.7% in the two quarters immediately after the violation. Further, this decline is persistent, lasting for over two years after the violation. The ultimate consequence of this decline in issuing activity can be seen in Figure 3, which shows a persistent decline in leverage ratios following the violation. Two years after the violation, leverage has declined by almost 3%, a relative decline of 12% when compared to the average leverage ratio.

Because identification is a primary concern, 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. Using a firm fixed effects specification, we find that net debt issuing activity declines by over 0.7% of assets

in the quarter immediately following the covenant violation - a statistically and economically significant amount. Importantly, this result is 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) and Flannery and Rangan (2006)), is not behind our findings. Firms with relatively high leverage ratios decrease their net debt issuing activity by an *additional* 2% of assets per quarter after violating a covenant - a 25% additional decrease in net debt issuances relative to that predicted by mean reversion alone. In sum, these results highlight the divergence between managerial and creditor objectives in financial policy, and identify a specific mechanism, the transfer of control rights, through which agency problems impact financial policy.

To reinforce our identification strategy and support a causal interpretation of our results (i.e., creditors induce 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 the *Dealscan* database, we are able to measure the precise distance from the covenant boundary 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 mitigating endogeneity concerns. Additionally, these findings alleviate sample selection concerns over self-reported covenant violations in SEC filings because the *Dealscan* sample contains *all* covenant violations – reported and unreported.

Finally, to gain even further insight into the precise mechanism by which creditors influence debt policy 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 (Sufi (2007a)), and 13% report an increase in the interest spread as a result of the violation. These findings provide additional evidence supporting our main result: a large number of significant financing decisions are dictated by creditors who use the transfer of control rights accompanying covenant violations to address the incongruence between managerial and creditor preferences.

Overall, our study departs from the existing literature by addressing the capital structure problem from the perspective of control rights, as opposed to traditional perspectives such as taxes, bankruptcy costs, and information asymmetry. We are the first, to our knowledge, to document that creditors dictate the capital structure decisions of a large number of public firms, even outside of bankruptcy. This result suggests that theories in which a firm's capital structure is determined uniquely by managers are incomplete. Given that creditors determine security issuance decisions with high frequency, our findings suggest that a consideration of creditor incentives may offer insight into the determinants of leverage ratios.[2]

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] We find a significant divergence between managerial and creditor preferences for debt financing, and we find that creditor-dictated leverage ratios are significantly lower than managerial-chosen leverage ratios. The latter finding suggests that creditors are particularly concerned with managerial agency conflicts 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).

While there is a significant body of literature examining the importance of covenants in debt contracts (e.g., 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 following the transfer of control rights. While our focus is on the ex post consequences of creditor control, our findings suggest that managers may keep leverage ratios lower than would otherwise appear optimal (Graham (2000)) given a fear of losing control over firm policy.

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. Section III begins with a non-parametric analysis of the impact of covenants violations on financing, followed by a discussion of the identification problem, empirical strategy, and results. Section IV presents the results of our regression discontinuity design. Section V presents evidence from discussions in 10-Q and 10-K SEC filings. Section VI presents further discussion of our results and their implications for ex ante financing behavior. Section VII 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 initially require to measure covenant violations. 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

[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.

variables used in this study are formally defined in Appendix A.) To mitigate the impact of data errors and outliers on our analysis, we Winsorize all variables at the 5th and 95th percentiles, though our results are unaffected by Winsorizing at the 1st and 99th percentiles. Finally, because our primary analysis relies on within firm variation, we include only firms for which there are at least four consecutive quarters of available data. In concert, these criteria reduce the sample from 176,993 firm-quarter observations to 135,736 firm-quarter observations.[5]

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. 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

[5] 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.

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 self-selection 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 (a shortcoming that we resolve later for a subsample of our data). 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.[6] 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,

[6] 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.

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 Trade-Wholesale consisting of relatively smaller firms, on average. We also observe that 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 violation are significantly larger in every rating category except firms rated CCC or worse, which contains 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 issuances and book leverage), our "covenant control variables," and "other control variables." For presentation purposes, we focus our attention on net debt issuances computed from the change in balance sheet debt and net equity issuances computed from the statement of cash flows. However, we also examine net debt issuances computed from the statement of cash flows and net equity issuances computed from the split-adjusted change in shares outstanding (Fama and French (2005)). The results are qualitatively similarly and, consequently, are not reported.[7] (See Appendix A for variable definitions.)

[7] 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

The covenant control variables group contains 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 event of a violation and the propensity to issue debt. The third group, other control variables, contains 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, it is useful to first clarify precisely what financial covenants require and what happens when they are violated. To do so, we use the revolving credit agreement between 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

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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, the similar of our findings for net debt issuances defined using the statement of cash flows alleviates this concern.

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 (Gopalakrishnan and Parkash (1995), Chen and Wei (1993), Beneish and Press (1993)).[8] 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), 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 default.

[8] 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 contracts literature (e.g., Hart and Moore (1988)).

B. Theory and Hypothesis Development

Theoretical research in security design and optimal financial contracting makes assumptions about the structure of the information and the contracting environment, and shows that debt securities may be optimal in the presence of agency conflicts. Townsend (1979) and Gale and Hellwig (1985) are among the earliest contributions. In their models, borrowers witness a state variable correlated with profitability that is unobservable to creditors. The optimal contract in this environment specifies a fixed payment in the unmonitored states; if a fixed payment is missed, creditors choose to "observe" the state and borrowers repay as much as possible. The authors interpret this contract as a standard debt contract in which creditors receive residual firm value when an interest payment is missed. Hart (1995) and Hart and Moore (1998) use an incomplete contracts framework in which cash flows are non-verifiable, managers can divert project returns, and creditors have the ability to seize physical assets. Under some additional assumptions, they too show that standard debt contracts that allow seizure of assets conditional on non-payment are optimal.

These models share the hypothesis that debt contracts allocate creditors residual control rights of physical assets in response to a payment default. As long as the firm meets its payment obligations, creditors play a passive role in firm financial and investment policy. This hypothesis is reflected in the three theoretical frameworks that have most strongly influenced empirical capital structure research: the trade-off, pecking order, and market timing frameworks. While the hypotheses of these three frameworks are distinct, they all treat the capital structure decision as a portfolio problem facing managers, as alluded to at the outset of the paper. Thus, in these theories, creditors do not have any direct control over the capital structure decisions of firms, unless the firm defaults on a payment obligation.

There is an alternative class of models in which creditors play a more active role in firm financial and investment policy, even if the borrower meets its payment obligations. Jensen and Meckling (1976) assume the existence of debt and equity securities and 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 to creditors in future states where managerial private benefits are most likely to distort the manager into inefficient decisions. Importantly, the decision right may shift to creditors even in the absence of a payment default. Indeed, as they emphasize in their conclusion, the manager continues to receive monetary payoffs even after creditors obtain the decision right.[9]

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 acquire control rights after signs of poor performance. Loss of control serves as a managerial disciplining device, and therefore helps mitigate moral hazard. In their model, a noisy signal correlated with firm performance is contractible, and control shifts to creditors conditional on negative realizations of the signal. Importantly, a negative realization of the signal does not necessarily entail payment default; therefore, creditors may obtain a degree of control over firm policy outside of states of bankruptcy.

While these three models assume different types of incentive conflicts, they reach a similar conclusion: in the presence of agency conflicts, optimal financial contracts may allocate a certain degree of control over firm policy to creditors even before a payment default. Extant research shows that creditors exert control over firm capital expenditure policy after negative performance but before payment default (Chava and Roberts (2006) and Nini, Smith, and Sufi (2006)). However, to our knowledge, there is no existing empirical research in the capital structure literature that documents creditors' direct influence on the security issuance decisions of solvent firms.

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. This is

[9] 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)."

consistent with the theories of Townsend (1979), Gale and Hellwig (1985), Hart (1995), and Hart and Moore (1998). It is also consistent with current empirical capital structure research based on trade-off, pecking order, and market timing frameworks. The alternative hypothesis is that creditors play a more direct role in the security issuance decisions of firms before payment default. In the context of financial covenant violations, the alternative hypothesis is that creditors use their acceleration right to directly influence the capital structure decisions of borrowers subsequent to technical defaults. Evidence in favor of the alternative hypothesis would suggest that a consideration of creditor control rights outside of payment default states is an important - and previously missing - part of the capital structure debate.[10]

III. The Effect of Covenant Violations on Capital Structure

A. Security Issuances and Covenant Violations

In this subsection, we present a first look at the impact of covenant violations on financial policy. Our goal is to provide descriptive evidence illustrating the response of firms' financial policies to technical default. In the following sections, we provide a detailed discussion of the techniques we employ to address endogeneity concerns.

We begin with a pure 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:

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

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

[10] One could interpret the ex post loss of control before bankruptcy as an ex ante agency cost of debt financing, which forces managers to keep leverage ratios lower than they would in the absence of agency conflicts. Such an interpretation would be consistent with an extended trade-off theory, something we explore further in the conclusion. However, to our knowledge, there is no empirical research that documents how an ex post loss of control before bankruptcy affects ex ante capital structure decisions.

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 1 through 3 present graphical representations of β_{-2} through β_8, along with corresponding 90% confidence intervals, for y equal to net debt issuances, net equity issuances, and book leverage, respectively. Beginning with Figure 1, we observe 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. Indeed, these estimates are only slightly above the firm-specific mean but insignificantly so.

Immediately following the violation, firms experiences 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 issuances are significantly lower than they were in the three quarters up to and including the quarter of the covenant violation.

Figure 2 presents the results for net equity issuances. Unlike net debt issuances results, there is no sharp change in net equity issuances right after the covenant violation. There is some evidence of an increasing trend following the violation; however, it is statistically weak and economically small.

Figure 3 shows that the sharp and persistent reduction in net debt issuances revealed by Figure 1 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 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.

B. Identification

As discussed in Section III, the alternative hypothesis for our empirical analysis is that creditors use their acceleration rights to directly influence capital structure subsequent to a covenant violation. The evidence in Figures 1 and 3 is consistent with this hypothesis; however, the evidence does not necessarily reject the null hypothesis that management makes capital structure decisions independent of direct creditor influence. The primary concern is that the decline in net debt issuing activity and leverage would have occurred even if the covenant violation had not occurred.

Consider the implications of a negative cash flow shock, for example. Such a shock may increase the debt-to-EBITDA ratio above the covenant threshold leading to a covenant violation. However, a decline in cash flow may also lead to an increase in the probability of bankruptcy and, according to a tax-bankruptcy cost tradeoff theory, lead to a decrease in net debt issuing activity and leverage in order to reduce the expected costs of bankruptcy. Alternatively, lower cash flow may lead to lower expected taxes and, again according to the traditional tradeoff theory, a decrease in net debt issuing activity and leverage as firms require less debt to shield them from lower anticipated tax payments. Thus, without controlling for cash flow, the responses observed in Figures 1 and 3 may be an artifact of omitted variables and, consequently, have nothing to do with the violation.

Additionally, financial covenants are often written on variables that may themselves be correlated with management's optimal capital structure policy in the absence of the covenants themselves. For example, the classic tax-bankruptcy cost trade-off theory suggests that when leverage rises, expected costs of bankruptcy rise, and, consequently, firms should decrease their leverage. In this case, the basic concern is that the decline in net debt issuances and leverage would have occurred even if there had been no covenant violation because managers would have rebalanced their capital structures (e.g., Leary and Roberts (2005)), for example.

Several features of Figures 1 and 3 mitigate these concerns. Figure 3 shows that leverage ratios are well above the firm mean even before the covenant violation, but Figure 1 shows that the decrease in net debt issuance begins only after the firm violates a financial covenant. Figure 3 also 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 optimal leverage ratio before the covenant violation, but they only reduce net debt issuance after violating a covenant. Finally, Figure 1 shows that the major change in net debt issuance policy is concentrated in the quarter immediately after the covenant violation. The evidence in Figures 1 through 3, together with evidence from credit agreements describing creditors' acceleration rights, suggest that management would not have decreased net debt issuance as sharply had they not violated the covenant.

Nonetheless, the next two subsections take additional steps toward ensuring a causal interpretation of our results.

C. Covenant control variables

The first identification concern that we address is that the variables on which covenants are written (e.g., debt-to-EBITDA) contain information about managerial preferences for net security issuing activity. If so, then any change in security issuance policy following a covenant violation may simply reflect a corresponding change in managerial preferences. To address this concern, we incorporate smooth and discontinuous functions of variables on which covenants are typically written. The identifying assumption is that by conditioning on various functions of covenant control variables, we are able to isolate variation in covenant violations that is independent of the underlying variables (or functions of these variables) that management may use to adjust capital structure in the absence of the covenant violation. 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 net debt issuance policy. Discussions with commercial lenders suggest that this is not the case—covenant

restrictions are often one of the most highly contested components of the credit agreement during the pre-origination negotiations.[11]

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), as well as collinearity considerations. Many of the accounting ratios behind financial covenants are highly correlated (e.g., debt to cash flow versus senior debt to cash flow, fixed charge coverage ratio versus interest coverage ratio, etc.). Thus, near-singularity concerns preclude an all-inclusive set of covenant controls.

Following the extant empirical capital structure literature (e.g., Rajan and Zingales (1995)), the matrix *X* also includes the natural logarithm of assets, the lagged tangible to total assets ratio, and the lagged market to book ratio.[12] Given this matrix *X*, we estimate the following firm fixed effects specification,

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

[11] 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 of JP Morgan Chase & Co.

[12] 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.

where *f(X)* corresponds to a vector of functions of controls for the variables on which covenants are written, and all other variables discussed above.

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 $\Gamma=0$). 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 point estimate is statistically distinct from zero at less than the one percent level, with 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 magnitude of the coefficient declines slightly but remains statistically distinct from zero at the one percent level. In column (3), we add the four interaction terms mentioned above, which have little impact on the estimated covenant violation coefficient.

Finally, column (4) presents the results for a kitchen sink specification including the following controls: the 16 covenant control variables (level and interaction terms), higher order polynomial terms (squared and cubic terms) for each of the 16 covenant controls, and quintile indicator variables for each of the 16 covenant controls. To be clear, the last set of controls consists of 80 (5 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 7.4.

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. Given our identification

assumption, these results suggest that net debt issuance decreases by 51 basis points more than they would have in the absence of the covenant violation. In terms of magnitudes, a quarterly reduction in net debt issuance of 51 basis points takes a firm from the median of the net debt issuance distribution to the 28th percentile.

D. Managerial Rebalancing of Leverage Ratios

The next identification concern is managerial rebalancing. Previous research suggests that managers dynamically rebalance their leverage ratios (e.g., Leary and Roberts (2005) and Flannery and Rangan (2006)) and many managers explicitly report having a target range for the debt to equity ratio (Graham and Harvey (2001)). This behavior is a threat to our identification strategy if the covenant violations simply coincide with leverage ratios that are, in some sense, too high. That is, absent the covenant violation, a reduction in net debt issuing activity would have occurred in the process of firms' rebalancing their leverage ratios. In fact, the results in Table IV already address 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 book leverage ratio.

Nonetheless, in this subsection, we take a closer look at the managerial rebalancing hypothesis. Specifically, we examine the change in net debt issuances for covenant violators versus non-violators across the leverage distribution. In Table V, the sample is split into quartiles based on the level of 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 evidence in previous studies mentioned above. Column (2) documents that the net debt issuances of covenant violators are 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. The evidence shows that all firms tend to rebalance when leverage ratios increase; however, the covenant violators reduce net debt issuance by more and at lower lagged leverage ratios.

Table VI examines the rebalancing alternative in a regression context. The specifications are identical to those reported in column (3) of Table IV, except for the inclusion of interactions of the lagged leverage ratio with the lagged covenant violation indicator variable. In column (1), we report a specification which includes the linear interaction term. 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 issuances decrease by an additional 2% for covenant violators, an additional decline of almost 25% relative to the base line mean reversion effect.

In the specification reported in column (2), we explore the distributional difference in responses to higher leverage ratios for violators and non-violators and relax the linearity assumption in the column (1). We include three indicator variables for the lagged leverage ratio quartiles plus four interaction terms of the quartile indicator variables and the lagged covenant violation. Consistent with managerial rebalancing, the estimates in column (2) imply that firms that enter into the 3rd and 4th quartile of the leverage distribution reduce net debt issuance by 35 basis points and 62 basis points, respectively. When a firm violates a covenant, net debt issuance decreases by an additional 52 and 110 basis points for these two quartiles, respectively. While firms that enter into the second quartile of the leverage distribution do not reduce net debt issuance by a statistically significant amount, covenant violators reduce net debt issuance by a statistically significant 37 basis points. Thus, covenant violators reduce net debt issuance by significantly more than non-violators across most of the leverage distribution, above and beyond any reduction coinciding with normal rebalancing motives.

These findings have a useful interpretation in that they highlight the wedge between the managers' optimal capital structure and the creditors' optimal capital

structure. For the second, third, and fourth quartiles of lagged leverage, the estimates in Table VI imply that covenant violators reduce net debt issuance by twice as much as would be predicted from managerial rebalancing alone. This is consistent with creditors taking a more conservative approach to debt usage than managers.

E. Short-Run vs. Long-Run Impact

In Tables IV through VI, 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 VII, we examine the long run impact of the covenant violation on net debt issuances 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. Net debt issuance for the firm 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. 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 issuances on leverage ratios. Column (3) presents estimates from a specification including only fiscal quarter and quarter indicator variables as controls, and shows that leverage ratios gradually decline in response to the covenant violation. By 6 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.[13]

IV. Regression Discontinuity Design

While we have attempted to control for all confounding effects in the preceding analysis, a remaining concern is that the covenant threshold or the distance to that threshold contains information about managers' preferences for debt financing. An additional concern is that the self-reporting of covenant violations in SEC filings may bias our results in favor of finding a (large) effect because only "severe" violations are reported. To address these concerns, we turn our attention to a different dataset that does not rely on self-reporting of covenant violations and contains sufficient information to construct measures of the covenant threshold and the corresponding distance to that threshold. Doing so enables us to employ a regression discontinuity design (e.g., Hahn, Todd, and Van der Klaauw (2001)) aimed at reinforcing a causal interpretation of our results and ensuring that our findings are not driven by sample selection.

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.

A. Data

The data 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.[14] 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

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

[14] 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.

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. 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.

Our final analysis 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, as such, we simply follow their construction. Most importantly, the determination of whether or not a firm is in violation of its covenant is very precise for this sample. For each firm quarter in this sample of firms, we know precisely how far each firm is from its covenant threshold.

Before outlining our empirical framework, it is useful to briefly describe the distinction between this dataset constructed from *Dealscan* and the previous one constructed from the SEC filings. The clearest distinction stems from the fact that not all violations must be reported. Technically, only violations that remain unresolved at the time of reporting must be documented in SEC filings. Extant research documents that a number of violations are waived (Chen and Wei (1993)) or lead to renegotiations in which the terms of the contract are modified to alleviate the breached covenant(s).

Another distinction comes from the fact that only credit agreements in excess of 10% of assets are required to be filed with the SEC. According to Loan Pricing Corporation, the distributors of *Dealscan*, approximately 60% of their loans come from SEC filings, while the rest are obtained from contacts in the credit industry – an increasingly important source over time. Thus, an important by-product of this analysis is that it offers a test of any sample selection bias associated with the filings data. Because only reported violations are captured by the SEC filings, one concern with our previous results is that they reflect only the most egregious violations, suggesting an overstatement of the actual impact of control changes. Alternatively, the sample selection associated with the filings data may be representative of an underlying economic distress associated with the covenant violation. Again, the selection bias leads to an overstatement of the estimated impact of covenant violations.

B. Empirical Strategy

Hahn, Todd, and Van der Klaauw (2001) note that, "the regression discontinuity data design is a quasi-experimental data design with the defining characteristic that the probability of receiving treatment changes discontinuously as a function of one or more underlying variables" (page 1). In the current context, covenant violations correspond to the treatment and non-violations the control. What enables our research design to fit into the regression discontinuity paradigm is that the function mapping the distance between the underlying accounting variable and the covenant threshold into the treatment effect is discontinuous. Specifically, our treatment variable, *Bind*, is defined as:

$$Bind_{it} = \begin{cases} 1 \text{ if } z_{it} - z_{it}^{0} < 0 \\ 0 \text{ otherwise} \end{cases} \qquad (3)$$

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 base empirical model for this section is similar to that in the previous section:

$$\frac{D_{it} - D_{it-1}}{A_{it-1}} = \alpha_i + \sum_{f=11}^{4} \theta_f + \sum_{1994q1}^{2004q1} \delta_t + \beta_0 Bind_{it-1} + \beta_1' X_{it-1} + \eta_{it}, \qquad (4)$$

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. The appeal of this

approach is that the nonlinear relation in equation (4) provides for identification of the treatment effect under very mild conditions (Hahn, Todd, and Van der Klaauw (2001)). Indeed, in order for the treatment effect, β_0, to *not* be identified, it must be the case that the unobserved component of net debt issuances, ε, exhibits an identical discontinuity as that defined in equation (3) - relating the violation status to the underlying accounting variable. Because we can now measure the distance from the threshold, we can include smooth functions of this distance into the set of control variables X. Doing so alleviates the endogeneity concern because the identification of the treatment effect is driven entirely by the discontinuity at the threshold and not from the distance to the threshold, which is now a control variable.

C. Results

The estimation results using the entire sample are presented in Panel A of Table VIII. The first specification presents the response of net debt issuances to the covenant violation conditional only on firm and year-quarter fixed effects. Interestingly, the estimated magnitude of the effect is close to that found in the previous section, a 0.5% reduction in net debt issuances following the violation. This correspondence is comforting because it suggests that the sample selection concerns outlined earlier may be misplaced. That is, regardless of whether one looks at violations reported to the SEC or violations - reported or not - occurring in the *Dealscan* sample, the average financing response is quite similar.

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 issuing activity increases slight 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 results illustrate that the distance to the threshold contain relatively little information about security issuances, beyond that contained in the other control variables. None of the

coefficients on these four measures are statistically distinguishable from zero. Additionally, 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, for example, then there should be little difference in the borrower when its net worth is $1.05 billion versus $0.95 billion but for the covenant violation.

The results for the Discontinuity sample are presented in Panel B of Table VIII. 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. The statistical significance of these estimates is weaker than that found in the full sample primarily because of a decline in degrees of freedom, as opposed to economic significance. The number of observations decreases by over 60% when we

focus only on firms close to the covenant threshold. Thus, these results further emphasize that the event of the covenant violation is driving the financing response, as opposed to managerial preferences over capital structure.

V. Additional Evidence from SEC filings

The previous section presents large-sample evidence on the causal effect of the violation 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 quarter of and after the covenant violation. Many firms provide detailed explanations of the outcome of the covenant violation, which provides unique insight into how creditors use their acceleration rights. The drawback of these data is that firms voluntarily choose the level of detail for their explanation to shareholders. 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. In other words, our analysis of firm explanations provides a lower bound for the actual actions taken by creditors.

To give a sense of the data collection process, here is an example of an explanation from Insteel Industries, Inc. on their 2001 second quarter 10-Q filing:

> 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 this particular company, as a direct result of the financial covenant violation, creditors reduced the credit facility, shortened the maturity of the loan, and raised the interest rate.

Table IX presents the results for a random sample of 100 covenant violators. 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.

While we caution against viewing these results in isolation, they provide 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 indirectly influence net security issuance decisions by increasing interest rates, for example. Importantly, none of these actions would have been possible had the transfer of control rights not occurred.

VI. Further Discussion

While our analysis focuses on the ex post consequences of a transfer of control, an interesting and important corollary question is whether the ex post loss of control associated with debt financing leads managers to keep leverage ratios lower than they otherwise would. In other words, can the ex post loss of managerial control documented in our study help explain the distribution of leverage in the cross-section and perhaps the under-leverage puzzle (Graham (2000))?

Unfortunately, a complete investigation into this question is beyond the scope of this study. However, our findings here lay the foundation for future investigation into the effects of the loss of control on ex ante capital structure decisions. More specifically, we have presented three findings that inform future research. First, we find that creditors

dictate capital structure decisions for a large fraction of public firms, and actions taken by creditors conflict with managerial desires. This result suggests that ex post agency conflicts are likely a first-order concern for ex ante capital structure decisions. It also suggests that that agency conflicts cannot be resolved completely through ex ante contracting, and thus may affect managerial preferences for debt financing. Second, the fact that creditor-determined leverage ratios are lower than manager-determined leverage ratios suggests that agency conflicts are exacerbated when leverage ratios are high (Jensen and Meckling (1976)). From a theoretical standpoint, this latter finding is consistent with models in which hypothesized leverage ratios in the presence of agency conflicts are lower than leverage ratios in the absence of such conflicts.

Finally, our evidence on ex post control transfers complements survey and anecdotal evidence suggesting that managers keep leverage ratios low to preserve financial flexibility. For example, in their survey of CFOs, Graham and Harvey (2001) report that the single most important consideration in setting debt policy is "financial flexibility," or the ability to finance projects out of internal funds. One possible interpretation of financial flexibility is that managers also avoid debt financing given the potential loss of control it entails. This interpretation is consistent with the following quotes from the SEC filings of firms in our sample, a few of which are presented below.

> The Credit Facility also requires the company to meet certain financial ratios and tests. These covenants…significantly limit the operating and financial flexibility of the Company and limit its ability to respond to changes in its business…"
>
> -Conmed Corp, 10-K filing, December 1998

> Our debt instruments may limit our financial flexibility and increase our financing costs. The instruments governing our debt contain restrictive covenants that may prevent us from engaging in certain transactions that we deem beneficial and that may be beneficial to us.
>
> -Kinder Morgan, 10-K filing, December 2002

> Any additional equity financing could result in substantial dilution to stockholders, and debt financing, if available, will most likely involve restrictive covenants that preclude us from making distributions to stockholders and taking other actions beneficial to stockholders.
>
> -Advetntrx Pharmaceuticals Inc, 10-Q filing, September 2005

As always, we are cautious with respect to interpreting and extrapolating from this anecdotal evidence. However, this evidence does suggest that the importance of financial flexibility in the Graham and Harvey (2001) survey responses may reflect a desire by managers to avoid the possible loss of control associated with debt financing.

VII. Conclusion

This paper documents that the transfer of control accompanying covenant violations has significant consequences for corporate debt policy over and above any changes in 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 greatly amplify reductions in net debt issuing activity accompanying leverage rebalancing. Thus, creditors take a significantly more conservative stance on optimal or target leverage, relative to that taken by managers, and this stance is reflected in observed financial policies via the state contingent allocation of control rights embedded in most every debt contract.

In addition to identifying a role for control rights in determining financial policy, our results highlight an alternative perspective on 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 A: 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

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 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
Covenant Violations versus Leverage Rebalancing

This table presents evidence on covenant violations and managerial leverage rebalancing. Firm-quarter observations at time *t* are separated into quartiles based on the leverage ratio at *t-1*. For each quartile, the mean net debt issuance scaled by lagged assets at time *t* are reported for firms that violate and do not violate a financial covenant at time *t-1*. The sample includes firms that have an average book leverage ratio of 0.05 or greater for the sample.

	Mean net debt issuance scaled by lagged assets (basis points)$_t$	
	No covenant violation$_{t-1}$	Covenant violation$_{t-1}$
Leverage Quartile 1	106.5	99.4
Leverage Quartile 2	55.7	14.5**
Leverage Quartile 3	39.5	-15.9**
Leverage Quartile 4	69.3	-27.2**

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

Table VI
Covenant Violations and Leverage Rebalancing
A Regression Approach

This table presents coefficient estimates from firm fixed effects regressions of net debt issuance on covenant violations and controls. In column (1), the specification includes an interaction between the lagged covenant violation indicator variable and the lagged leverage ratio. In column (2), the sample is split into 4 quartiles based on the lagged leverage ratio. The sample includes firms that have an average book leverage ratio of 0.05 or greater for the sample. All specifications include quarter indicator variables and indicator variables for the fiscal quarter, in addition to lagged natural logarithm of total assets, the lagged tangible assets to assets ratio, and all covenant control variables in specification (3) reported in Table IV. Standard errors are reported in parentheses and are clustered by firm.

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

	(1) Interaction	(2) Interaction across distribution
Leverage ratio $_{t-1}$	-819**	-726**
	(34)	(48)
Leverage ratio $_{t-1}$ *Violation$_{t-1}$	-213**	
	(39)	
Leverage quartile 2$_{t-1}$		-14.9
		(8.3)
Leverage quartile 3$_{t-1}$		-34.7**
		(12.5)
Leverage quartile 4$_{t-1}$		-61.7**
		(16.5)
Leverage quartile 1$_{t-1}$ * Violation $_{t-1}$		5.5
		(18.4)
Leverage quartile 2$_{t-1}$ * Violation $_{t-1}$		-36.6**
		(14.2)
Leverage quartile 3$_{t-1}$ * Violation $_{t-1}$		-51.5**
		(14.2)
Leverage quartile 4$_{t-1}$ * Violation $_{t-1}$		-109.8**
		(14.3)
Number of firm-quarters	104,383	104,383
Number of firms	4,765	4,765
R^2	0.189	0.189

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

Table VII
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 VIII
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)
Market to book$_{t-1}$		56.4**	59.3**
		(14.3)	(14.1)
Profitability$_{t-1}$		-835.2	-846.8
		(433.9)	(438.2)
Firm size$_{t-1}$		-130.6**	-150.7**
		(-47.7)	(47.8)
Altman's Z-Score$_{t-1}$		65.3**	72.6**
		(17.9)	(18.4)
Tangibility$_{t-1}$		675.4**	634.7**
		(270.4)	(270.0)
Industry Median Leverage$_{t-1}$		-69.4	-0.7
		(174.2)	(174.3)
Default Distance (CR)$_{t-1}$			-37.2
			(34.4)
(Default Distance (CR)$_{t-1}$)^2			-7.2
			(7.6)
Default Distance (CR)$_{t-1}$			0.1
			(0.0)
(Default Distance (CR)$_{t-1}$)^2			0.0
			(0.0)
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 VIII
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)
Market to book$_{t-1}$		20.3
		(48.4)
Profitability$_{t-1}$		255.9
		(989.4)
Firm size $_{t-1}$		-385.0**
		(91.6)
Altman's Z-Score $_{t-1}$		181.0
		(46.0)
Tangibility $_{t-1}$		777.1
		(546.3)
Industry Median Leverage $_{t-1}$		-363.0
		(283.1)
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 IX
The Response of Creditors to Covenant Violations

This table presents evidence from SEC 10-K and 10-Q filings on how creditors respond to 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
Net Debt Issuance Before and After a Covenant Violation, Full Sample

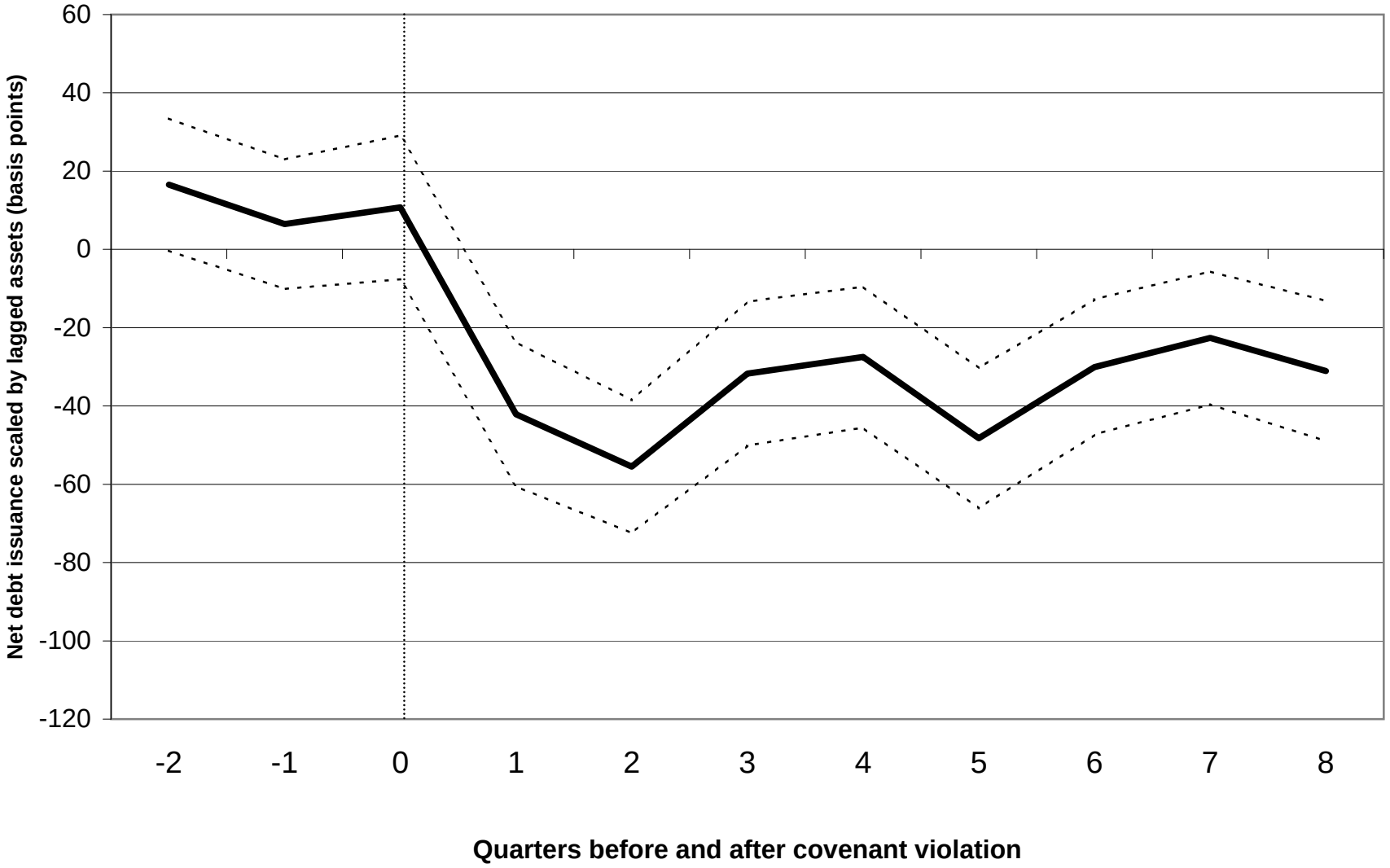

Figure 2
Net Equity Issuance Before and After a Covenant Violation, Full Sample

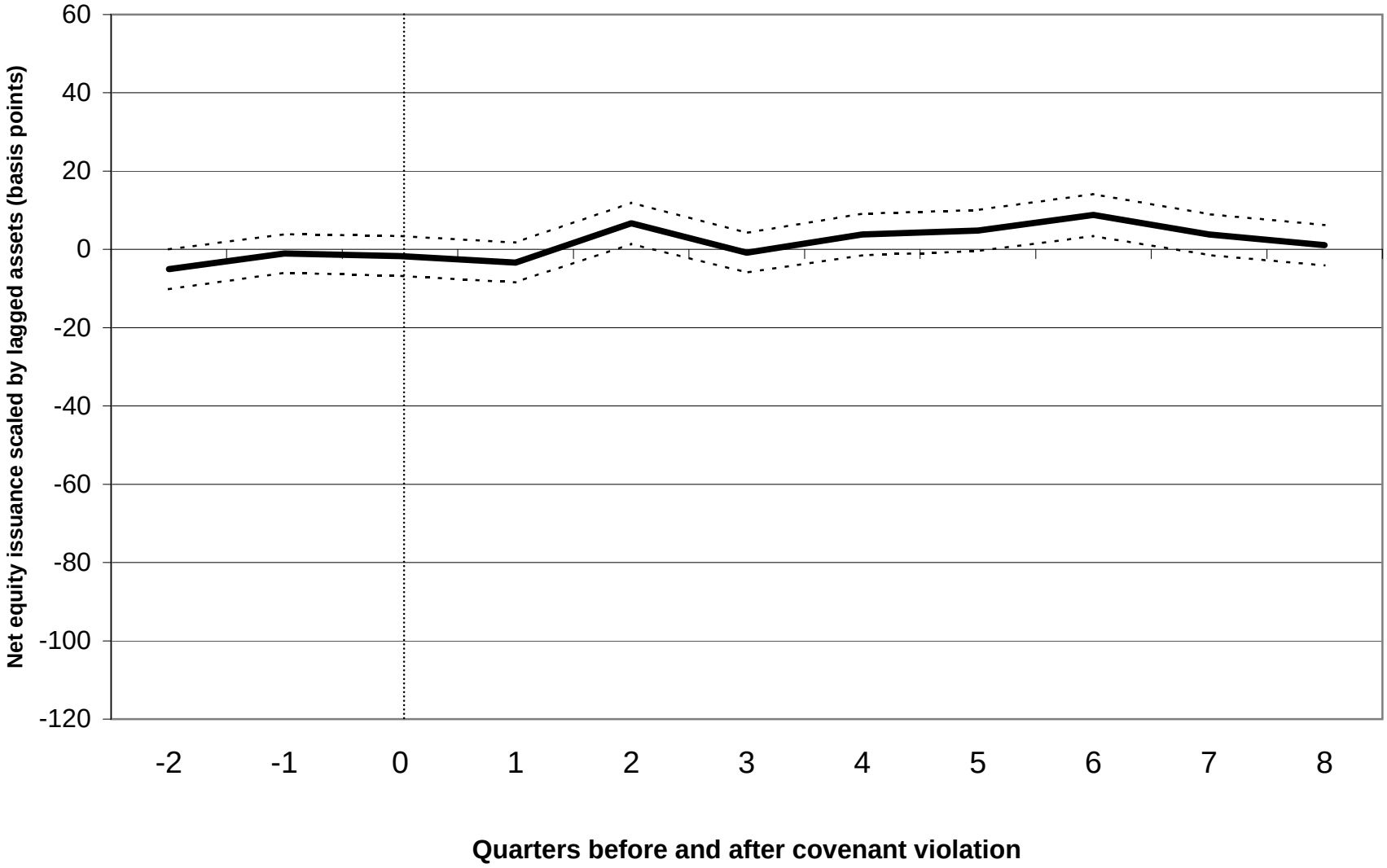

Figure 3
Book Leverage Ratio Before and After a Covenant Volation, Full Sample

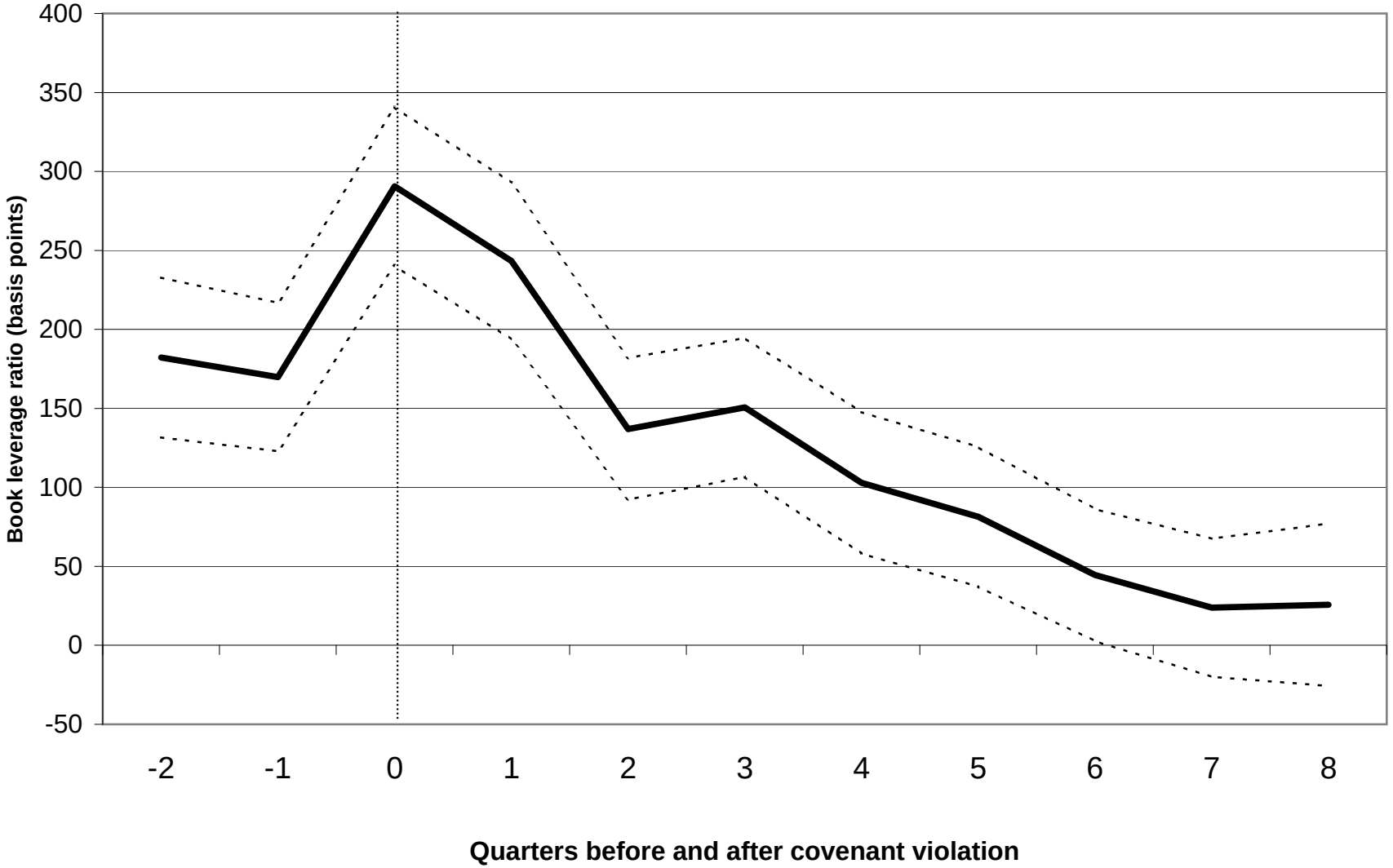

Figure 4
Net Debt Issuance before and after Covenant Violation, Regression Discontinuity Sample