Is Financial Contracting Costly? An Empirical Analysis of Debt Covenants and Corporate Investment

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May 30, 2006

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Abstract

We examine whether financing affects economic activity by estimating the impact of debt covenant violations on corporate investment. We begin by showing that covenant violations occur frequently in private loan agreements: 32% of our sample firms and 26% of our sample loans experience at least one violation. Subsequent to violating a covenant, debt financing becomes more costly in that firms experience an average increase of 60 basis points in the cost of debt capital. Using both a regression discontinuity design and propensity score matching technique, we identify a statistically and economically significant decline in corporate fixed investment of approximately 1% of capital per quarter associated with covenant violations. Additionally, the decline in investment accompanying covenant violations is limited to violations occurring: before the passage of Sarbanes-Oxley, among firms without an established lending relationship with their creditor, and in loans underwritten by a single lender, as opposed to a lending syndicate - all consistent with the economic importance of contracting costs.

Does financing matter for investment? Since Modigliani and Miller first posed their irrelevance theorem in 1958, numerous empirical studies have investigated this question. A key result of these studies, as summarized by Stein (2003), is the finding that firms with less debt tend to invest more. However, it is unclear what the precise mechanism is that drives this relationship. Additionally, evidence on the magnitude with which debt impacts investment is relatively sparse. Therefore, the goal of this study is to identify a specific mechanism behind the debt-investment relationship, while also quantifying the magnitude of debt's effect on investment.

Our approach is to estimate the impact of debt covenant violations, or "technical defaults," on corporate investment. A technical default occurs when a firm violates a debt covenant other than one requiring payment of interest or principal. For example, a covenant might require the borrower to maintain a net worth above a lower bound for the life of the contract. If net worth should fall below this bound, control shifts to the creditor who has discretion over the consequences of the violation. As shown in several studies, the most frequent resolution of such a violation is through renegotiation, as opposed to termination of the lending agreement or bankruptcy.[1] Thus, accompanying covenant violations are renegotiation costs, as well as potential increases in the cost of debt capital. Identifying whether these costs impact corporate investment is the focus of our analysis.

While our strategy is conceptually straightforward, empirically identifying the effect of covenant violations on investment faces several challenges. First, the covenant violation coincides with a change in the financial health of the firm, which, if correlated with investment, can lead to a confounding of our results. Second, the covenant violation may also coincide with a shift in the investment opportunities of the firm, which, if not appropriately controlled for, can result in an omitted variable bias. Finally, as our empirical framework is reduced form - following closely that used in much of the literature - we face existing concerns over measurement error in empirical proxies, particularly with respect to the proxy for Tobin's q. All of these concerns threaten identification of any effect and therefore play a significant role in the design of our analysis.

We begin by showing that covenant violations are a frequent occurrence in private loan agreements. Approximately 32% of the firms and 26% of the corresponding loans

[1]We use the term renegotiation here in a general sense to encompass restructurings of the original terms of the contract, as well as waivers of the violation. We elaborate on the resolution of technical default below, which has been investigated in previous studies by Chen and Wei (1993), Smith (1993), and Gopalakrishnan and Parkash (1995).

in our sample experience a violation. Additionally, firms are often repeat offenders, violating covenants in multiple loans. Though, in light of how "tightly" covenants are set, this result is not surprising. Private lenders often set covenant thresholds less than one standard deviation away from the underlying accounting variable at the outset of the loan. This finding is consistent with the prediction of Garleanu and Zwiebel (2005), who suggest that some borrowers may use a tight covenant restriction as a signal of their intention to avoid wealth expropriation. Thus, even relatively moderate changes in the financial health of the firm can lead to a violation.

We then document the effect of covenant violations on the costs of future debt financing by examining the loan terms in contracts entered into after a previous loan experiences a covenant violation. Consistent with earlier studies (e.g., Beneish and Press (1993), DeFond and Jiambalvo (1994), and Sweeney (1994)), loans undertaken after a previous covenant violation reveal, on average, interest rates that are almost 60 basis points higher. A number of loans also experience shortened maturities, reduced allowable borrowings, and additional covenants. Further, covenants in subsequent loans also tend to carry a larger punishment for future violations in terms of the fraction of the outstanding balance that may be recalled by the lender. Ultimately, covenant violations lead to more costly debt financing and less of it.

Our investigation of the link between covenant violations and investment begins with a regression discontinuity design (Thistlewaite and Campbell (1960), Trochin (1984), and Hahn, Todd, and Van der Klaauw (2001)). The discontinuity in the mapping between the underlying accounting variable (e.g., net worth) and the indicator variable identifying whether or not a covenant violation has occurred enables us to mitigate, if not eliminate, endogeneity concerns since only latent, contemporaneous, discontinuous shifts in the investment opportunity set can contaminate our inferences, similar to the situation faced by Rauh (2005).[2] Our results show that capital expenditures decline by approximately 1% of capital per quarter. This figure corresponds to a relative decrease of approximately 12%, a statistically and economically significant result. Importantly, this result is robust to the inclusion of a large number of controls in the regression specification designed to capture changes in the investment environment (e.g., linear and nonlinear market-to-book and cash flow terms) and in the financial health of the firm (e.g., firm size, leverage, and Altman's Z-Score). This result is also robust to the inclusion of continuous, nonlinear

[2]Rauh (2005) uses the discontinuity in mandatory pension contributions to identify the link between internal funds and investment. For other applications of this approach in economics, see studies by Van der Klaauw (2002), Angrist and Lavy (1999), Angrist and Krueger (2000).

functions of the distance between the accounting variable and the covenant threshold, reinforcing the fact that it is the covenant violation driving the investment response.

We then show that the investment response to covenant violations is closely related to several proxies for information asymmetry, renegotiation costs, and credit constraints. In particular, violations occurring after the passage of the Sarbanes-Oxley Act in the summer of 2002 have no subsequent impact on investment. While the motivation for Sarbanes-Oxley (SOX) was primarily to resolve the conflicts of interest between managers and shareholders, the implications of SOX are more broad. SOX has a significant impact on the flow and quality of information between managers and investors and, in particular, has resulted in more reliable accounting information on which covenants are based (e.g., Cohen, Dey, and Lys (2005) and Jain, Kim, and Rezaee (2004)). In effect, our evidence suggests that SOX eases the information asymmetry between managers and creditors by improving the quality of the signal contained in accounting information and, consequently, lessening the monitoring effort expended by creditors.[3]

We also find that the investment reduction accompanying covenant violations is concentrated in firms with few or no previous lending relationships with their current lender. Firms with multiple previous lending relationships with their current lender show no investment response after violating a covenant, consistent with the importance of lending relationships in mitigating contracting costs and renegotiation costs (e.g., Lehmann and Nueberger (2001) and Dahiya, Saunders and Srinivasan (2003)). Similarly, the investment response to covenant violations is only observed in loans underwritten by a single lender, as opposed to a lending syndicate. While this finding might initially seem counterintuitive - renegotiation costs are likely larger for larger lending syndicates - this result is consistent with the hypothesis of Bolton and Scharfstein (1996), who argue that large lending syndicates will only select those loans for which renegotiation costs are likely to be small. Investment by firms with low asset tangibility are also particular susceptible to covenant violations, consistent with the import of higher liquidation values (Rajan and Zingales (1995)) in mitigating contracting costs through increased bargaining power. Finally, the investment response is greater among firms without a credit rating, again consistent with the import of increased bargaining power for firms with greater access to alternative capital markets.

[3]We refrain from concluding that SOX is therefore "good" from either the firm's or society's perspective. Our results are derived from a reduced form framework that precludes extracting welfare implications. Studies by Cunningham (2003) and Jain, Kim and Rezaee (2004) find direct evidence on the improved accuracy of accounting figures after the passage of SOX.

This sensitivity of the association between investment and covenant violations to various proxies for financing frictions is re-assuring for two reasons. First, this sensitivity shows that the investment response is found precisely in those situations where one might expect information asymmetry, renegotiations costs, and credit constraints to be greatest. Second, it suggests that our estimates of the investment drop accompanying covenant violations are largely free from any endogeneity bias. A potential concern with our initial regression discontinuity results is that the covenant violation indicator variable is simply proxying for discontinuous shifts in the investment opportunities of the firm not captured by the other control variables. While this possibility is unlikely given the set of control variables that we examine, it is not impossible. However, if this conjecture were true then the estimated investment decline would be unrelated to the proxies discussed above. For example, the passage of SOX is unrelated to the firm's investment opportunities, yet the investment response to covenant violations before and after SOX are very different. Similarly, it is unlikely that the number of historical lending relationships, the size of the lending syndicate, or historical credit ratings capture future discontinuous shifts in the investment opportunity set of firms above and beyond contemporaneous factors such as Tobin's q and cash flow. Yet, again, the investment response to covenant violations is very sensitive to the borrower-lender relationship, structure of the lending syndicate, and ability to access public debt markets. Thus, we believe that our results are robust to endogeneity concerns.

To further ensure the robustness of our findings, we undertake several additional analysis beginning with a propensity score matching analysis (Rosenbaum and Rubin (1983) and Heckman, Ichimura, and Todd (1997, 1998)). This approach provides an estimation strategy that is robust to departures from linearity and our results from this analysis reveal findings similar to those found with the regression discontinuity design. We also show that our results are robust to measurement error concerns over the proxy for Tobin's q - the market-to-book ratio. Using alternative empirical proxies and the reverse regression bounds technique of Erickson and Whited (2005), we show that both the sign and magnitude of our estimated investment response are robust to this concern.

Our paper is related to several branches of academic research, the closest of which examines the effect of financial frictions on corporate investment.[4] A number of studies have identified the sensitivity of investment to cash flow (e.g., Fazzari, Hubbard, and Petersen (1988), Gilchrist (1991), Hubbard, Kashyap, and Whited (1995), and Calomiris and Hubbard (1995)). In doing so, these studies highlight the fact that external funds

[4]See surveys by Hubbard (1998) and Stein (2003) for reviews of this literature.

are more costly than internal funds. However, as mentioned at the outset, most of these studies do not identify precisely why this premium exists or how large this premium might be. Additionally, most previous work has focused on the link between internal funds and investment, while relatively few studies have explicitly examined the link between debt and investment.[5] Thus, our contribution on this dimension lies in quantifying the direct effect of a specific financing friction (e.g., recontracting costs) on investment.

Our paper is also related to the literature examining the resolution of technical default and positive accounting theory. Most closely related to our study is that of Beneish and Press (1993), who examine the association between covenant violations and investment in the broader context of the consequences of technical default. However, aside from examining a significantly smaller sample of reported covenant violations, Beneish and Press do not address the endogeneity or measurement concerns that figure so prominently in this setting nor do they find any significant association with investment.[6] Studies by Beneish and Press (1993), Chen and Wei (1993), and Sweeney (1994) show how various firm characteristics are related to renegotiation outcomes (e.g., changes in interest rates), which we link to differential effects on investment activity. Finally, our paper is related to a number of studies investigating positive accounting theory, which suggests that firms approaching covenant violations will make income-increasing accounting choices to loosen their debt constraints.[7] Our evidence suggests that there is minimal, if any, managerial manipulation of accounting figures to avoid covenant violations, consistent with the evidence in Healy and Palepu (1990), DeAngelo, DeAngelo, and Skinner (1994), and that our results are robust to this concern.

The remainder of the paper proceeds as follows. Section I describes debt covenants and the implications of covenant violations. Section II discusses the data and outlines the sample construction. We also discuss in detail the relevant measurement issues for our empirical analysis, as well as providing preliminary evidence on the consequences of covenant violations. Section III presents the primary results from the regression discontinuity design. Section IV presents several robustness tests. Section V concludes.

[5]See, for example, studies by Whited (1992), Bond and Meghir (1994), Lang, Ofek, Stulz (1996), and Hennessy (2004) that examine the link between leverage and investment.

[6]Studies by Kahan and Tuckman (1993) and Beneish and Press (1995) investigate the security price impact of changes in covenant structure and covenant violations, showing that these events lead to abnormal bond and stock returns for the borrowing firm, respectively.

[7]See studies, for example, by Watts and Zimmerman (1986), Healy and Palepu (1990), Sweeney (1994), DeFond and Jiambalvo (1994), DeAngelo, DeAngelo, and Skinner (1994), Beneish, Press, and Vargus (2005), and Dichev and Skinner (2002).

I. Covenant Violations and Technical Default

A covenant is a promise in a written contract. In a debt contract, these promises can take many forms. For example, in most every contract borrowers promise to make interest and principal payments. Additionally, borrowers may promise to not pay excessive dividends to shareholders or, they may promise to not let their leverage ratios get too high. While the motivation for the first example is obvious, the motivation for the latter two finds support from two different strands of literature. The first strand is based on the agency theories of Jensen and Meckling (1976), Myers (1977), and Smith and Warner (1979), which suggest that covenants are a curative mechanism that addresses the information asymmetry and agency problems inherent in most firms. The second strand is based on the optimal contracting literature (e.g., Aghion and Bolton (1992) and Hart and Moore (1995)), which views covenants as a mechanism for the allocation of state contingent control rights designed to address the inherent incompleteness of contractual arrangements. Thus, covenants are contractual features designed to address market imperfections.

A covenant violation is simply a breach of contract that leads to default. However, not all defaults are alike. In particular, the term default is traditionally used in the context of an inability to repay the interest or principal owed on a debt obligation. In this study, we focus on a more common type of default known as a "technical default," which refers to a violation of a covenant other than one requiring the repayment of interest or principal. For example, a covenant may require that the borrower's net worth remain above a minimum level or that its leverage remain below a maximum level for the life of the contract. Additionally, these thresholds may be dynamic, changing over time with fluctuations in operating income or credit ratings, for example. When firms violate such covenants they are usually given a grace period of 30 days to cure the breach. After this grace period, however, the firm is considered to be in technical default.

Technical defaults occur almost exclusively in private debt issues, which contain relatively more and "tighter" covenants when compared to public debt issues (Kahan and Tuckman (1995)).[8] By tighter, we refer to covenants in which the distance between the covenant threshold and the actual accounting measure is smaller. For example, consider two loans to two firms and assume that each loan contains a covenant restricting net worth to remain above $1 million. Suppose both firms are identical and that the first firm has a net worth of $2 million and the second firm has a net worth of $3 million. In this case, the covenant is tighter for the first firm than the second firm. That private

[8] Also see studies by Chava, Kumar, and Warga (2004) and Billett, King and Mauer (2005).

debt issues contain more and tighter covenants is not surprising in light of the relatively lower renegotiation costs associated with private debt (Smith and Warner (1979) and Leftwich (1981)) due to the concentration of investors and active monitoring role played by most private lenders (Diamond (1984, 1991), Fama (1985), James (1987), and Rajan (1992)). Indeed, Sweeney (1994) suggests that the few technical defaults observed in public debt issues are usually a consequence of cross-default provisions in the public debt and, consequently, do not correspond to an incremental default. Thus, our discussion and empirical analysis focuses on violations of covenants in private debt contracts.

In response to the violation, lenders have several options at their disposal. First, they can unconditionally waive the violation and leave the existing contract intact. Second, they can conditionally waive the violation, effectively lengthening the grace period in which firms can rectify the problem. Third, they can renegotiate the loan contract and proceed under the new terms. For example, lenders can change the repayment schedule, the interest rate, the allowable borrowings (in the case of credit lines), or even the covenants. Finally, lenders can take more drastic actions such as terminating the lending agreement or demanding complete repayment of any outstanding balance.

Empirical evidence suggests that this latter outcome is rare. Gopalakrishnan and Parkash (1995) survey private lenders (lending officers from large banks and insurance companies), 90% of which indicate that the likelihood of loan termination or complete repayment is very low, if not zero. Rather, these authors find that most technical violations result either in a waiver or in renegotiation of the original contract. Similarly, Chen and Wei (1993) examine 128 technical violations and find that 33 received unconditional waivers, 24 received waivers for a specific period of time, and 71 experienced material changes to the terms of their lending agreement along one or more of the dimensions mentioned above. Finally, Beneish and Press (1993) show that of the 163 loans in which they observe a covenant violation, 56 subsequently relax covenant restrictions and 107 tighten or do not change existing restrictions. However, as Smith (1993) notes, it is likely that the relaxed covenants are those covenants that were initially breached, while the tightened covenants are either new additions or others already present in the contract.

More often than not, the contracts are renegotiated in favor of the lenders so that renegotiations typically lead to an acceleration of debt payments, an increase in the interest rate, decrease in the available credit, or the imposition of additional or tighter covenant restrictions (e.g., Chen and Wei (1993) and Gopalakrishnan and Parkash (1995)). (We provide further evidence on this issue for our sample of firms below.) Ultimately, the

existing evidence clearly suggests that most technical violations result in waivers or renegotiation that often lead to more costly financing or greater difficulty in obtaining financing.[9]

II. Data

A. Loan Data

Loan information comes from a July 2005 extract of Loan Pricing Corporation's (LPC) Dealscan database. The data consists of dollar denominated private loans made by bank (e.g., commercial and investment) and non-bank (e.g., insurance companies and pension funds) lenders to U.S. corporations during the period 1987-2005. According to Carey and Hrycray (1999), the database contains between 50% and 75% of the value of all commercial loans in the U.S. during the early 1990s. From 1995 onward, Dealscan coverage increases to include an even greater fraction of commercial loans. According to LPC, approximately 60% of the loan data are from SEC filings (13Ds, 14Ds, 13Es, 10Ks, 10Qs, 8Ks, and registration statements). The rest is obtained from contacts within the credit industry and from borrowers and lenders - an increasingly important source over time.

The basic unit of observation in Dealscan is a loan, also referred to as a facility or tranche. Loans are often grouped together into deals or packages. For example, in May of 2001 IBM entered into a $12 billion deal consisting of two loans: a short-term, 364-day facility for $4 billion and a 5-year revolving line of credit for $8 billion. While there is a small fraction of corporate loans in the database which are explicitly subordinated to other debt, all of the loans used in this study are relatively senior claims, many of which are also secured (see Bradley and Roberts (2003)). While the data contain information on many aspects of the loan (e.g., amount, promised yield, maturity, etc.), most pertinent to the analysis here is information on restrictive covenants, which generally apply to all loans in a package and whose coverage begins only in 1993.

For the purposes of this study, we focus our attention on the sample of loans with start dates between 1994 and 2005. We exclude 1993 because of relatively sparse coverage of covenants; however, including data from 1993 in our sample has no effect on our results. Additionally, we require that each loan contain a covenant restricting either the current ratio, net worth, or tangible net worth to lie above a certain threshold. (All variable

[9]For further evidence see Beneish and Press (1993), Defond and Jiambalvo (1994), and Sweeney (1994).

definitions are provided in Appendix A.) Because of obvious similarities, and to ease the discussion and presentation of results, we group loans containing a net worth or tangible net worth covenant together and refer to them simply as net worth loans.

We focus on these covenants for several reasons. First, they appear relatively frequently in our sample of loans. Table I shows that covenants restricting the current ratio or net worth are found in 9,294 loans (6,386 packages) with a combined face value of over a trillion dollars (deflated to December 2000 by the All-Urban CPI). Second, studies by Beneish and Press (1993), Chen and Wei (1993), Smith (1993), and Sweeney (1994) suggest that violations of these two covenants, in particular, are the most common source of technical defaults. Finally, 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.

B. Sample Construction

Our starting point for the sample construction is the quarterly merged CRSP/Compsutat database, excluding financial firms (SIC codes 6000-6999). We use quarterly, as opposed to annual, frequency accounting data because most borrowers are required to file with their creditors quarterly reports detailing their compliance with financial covenants. For brevity, we will refer to this subset as the Compustat sample and all variables constructed from these data are formally defined in Appendix A. Compustat data is merged with loan information from Dealscan by matching company names and loan inception dates from Dealscan to company names and corresponding dates in the CRSP historical header file. This merge results in 27,022 packages (37,764 loans) for 6,716 unique firms.

We then draw our sample containing firm-quarter observations in which firms are bound by either the current ratio or net worth covenant. Though our null hypothesis does not discriminate between these two covenants, we also split our sample into two mutually exclusive samples based on whether the loan contains a current ratio or a net worth covenant. We do this to provide insight on any differences between covenants when they occur. However, because of the similarity of the results for the two subsamples and to

avoid any redundancy, we focus our attention on the combined sample for the regression analysis.

Since covenants generally apply to all loans in a package, we define the time period over which the firm is bound by the covenant as starting with the earliest loan start date in the package and ending with the latest maturity date. In effect, we assume that the firm is bound by the covenant for the longest possible life off all loans in the package. We further require that our investment measure and the covenant's corresponding accounting measure are nonmissing. For the current ratio sample, this process results in 5,527 firm-quarter observations corresponding to 514 firms that entered into 644 deals (960 loans). For the net worth sample, this process results in 12,987 firm-quarter observations corresponding to 1,107 firms that entered into 1,466 deals (2,067 loans). Thus, our unit of observation is a firm-quarter, each of which is either in violation of or not in violation of a particular covenant.

Clearly, the selection procedure is non-random and, thus, raises concerns over the extrapolation of any inferences or conclusions to the general population of firms. Panel A of Table II examines this concern by comparing the characteristics of firms found in the Compustat population to those in the current ratio and net worth samples. While some differences are evident, the three samples are, in fact, quite similar along many dimensions. Specifically, a comparison of the median ROA, capital-to-asset, investment-to-capital, and leverage ratios reveal economically similar, if not, identical characteristics. However, the covenant samples contain relatively larger firms with smaller market-to-book ratios, differences that are more acute for the net worth sample. Also, our samples of borrowers tend to have higher cash flows than the general population.[10]

Panel B of Table II performs a similar exercise by comparing the loan characteristics in the current ratio and net worth samples to those in the matched Dealscan-Compustat population. The goal with this comparison is to determine whether loans containing current ratio or net worth covenants are fundamentally different from our general population of loans in Dealscan. Immediately, we see that the current ratio sample contains smaller loans with higher promised yield spreads (over the prevailing 6-month Libor) relative to the other two samples. However, the median loan size does not vary substantially across the three samples and all three samples share the same median maturity and promised yield. Thus, while there are some differences, the loans in our covenant

[10]The large negative average cash flow is an artifact of a small number of extremely large (in magnitude) observations, as suggested by the median value.

samples are generally quite similar to those in the general population, much like the firm characteristics.

As with any observational study, we are cautious with respect to extrapolating any inferences from our study. However, Table II reveals that in addition to being relatively large, our samples are fairly representative of the general populations from which they are drawn.

C. Measurement Considerations

Since the focus of this study is the impact of covenant violations on investment, we discuss several issues concerning the measurement of covenant thresholds and identification of violations. The first issue concerns firms that enter into multiple packages over our period of investigation. Of the 514 (1,107) firms in the current ratio (net worth) sample, 108 (273) entered into more than one deal. The motivation for this activity can come from several sources including increased capital requirements, changes in optimal capital structure, or from refinancings of previous deals. As long as the deals do not overlap (i.e., the first deal matures prior to the start of the second deal), this situation is of little concern.

However, when deals do overlap, a decision must be made concerning the covenant to which the firm is bound. For example, consider two deals, A and B, and assume that each consists of one loan in order to simplify the discussion. Deal A begins in January of 1990 and matures in January of 1995. Deal B begins in March of 1992 and matures in December of 1998. Assuming that deal B is not a refinancing, we define the covenant by which the firm must adhere to during the period of overlap (March 1992 to January 1995) as the more restrictive of the two. That is, the firm must adhere to the larger of the two minimum current ratios (or net worths) specified in the two deals. When the latter deal represents a refinancing, we assume that the covenant in the latter deal is the provision by which the firm must adhere, regardless of whether this new covenant is more or less restrictive than the previous one. So, if deal A specifies a minimum net worth of five million dollars and deal B specifies a minimum net worth of four million dollars, we assume during the period of overlap that the firm is bound by B's covenant when B corresponds to a refinancing and bound by A's covenant otherwise.

Practically speaking, overlapping loans occur relatively infrequently in our samples. The firms with multiple deals in the current ratio and net worth samples entered into a

total of 240 and 632 deals, respectively. Of these deals, 98 and 264 overlap with another deal, and, of the overlapping deals, 83 and 201 are refinancings, respectively.[11]

A second measurement issue concerns dynamic covenants that change over the life of the loan. For the current ratio sample, 80 of the 644 deals have dynamic covenants, the large majority of which increase from an initial value to a final value.[12] For the net worth sample, 760 of the 1,466 packages have a covenant that increases with a fraction of positive net income. While a changing covenant, in and of itself, is of little concern, we have detailed schedules of covenant changes for a relatively small fraction of bellwether loans with "Tear Sheets" in Dealscan. These addendums to the database contain even more detailed loan information, including schedules of changes in the covenants.

For the net worth sample in which covenant "buildup," as it is referred to in the trade, is directly tied to changes in positive net income, this deficiency poses little problem.[13] We compute the new threshold each quarter by simply applying the fraction specified by the covenant to any positive net income, as recorded by Compustat. However, changes in the current ratio are not necessarily uniform. After examining the loans for which we do have detailed information on the schedule of covenant changes and after talking to commercial loan officers at several banks, we decide to linearly interpolate the covenant thresholds over the life of the loan. For example, in August of 2000, American Ecology Corporation entered into a two year deal that required them to maintain a current ratio above 0.75 at the start of the loan and above 1.2 at the end of the loan. Thus, our interpolation scheme implies quarterly changes in this threshold equal to 0.064.

A final measurement consideration concerns a fraction of loans in both samples that are in violation of their covenants at inception of the loan. Specifically, 15% and 7% of the loans in the current ratio and net worth samples, respectively, reveal a violation of the covenant in the initial quarter, similar to the findings of Dichev and Skinner (2002).

[11]For a small number of overlapping loans, 8 in the current ratio sample and 41 in the net worth sample, we do not have information on whether the loan represents a refinancing of an earlier loan. As such, we take three approaches towards the treatment of these loans. First, we exclude them from the analysis. Second, we treat them all as refinancings. Finally, we treat them all as new capital. Under each treatment, the results of our analysis are similar and our conclusions unaffected by the changes.

[12]Two of the 80 deals report a decreasing trend (i.e., a loosening of the restriction) and nine report a fluctuating (i.e., non-monotonic) threshold.

[13]There are a few exceptions as noted by Dichev and Skinner (2002) in which the buildup also adjusts for stock issuances. In these instances, our measure will contain some error because we cannot precisely identify for which loans this adjustment occurs. However, conversations with LPC staff and a perusal of Tear Sheets suggest that this is a relatively infrequent phenomenon.

This occurrence is likely due to measurement error for two reasons. First, loan covenants are rarely, if ever, set so that the firm is in immediate violation of the contract. Second, it is relatively unlikely that the accounting variable changed during the initial quarter such that when the loan began the firm was not in violation but by the end of the first quarter, the firm had violated the covenant. We address this issue by excluding these loans from the analysis, though their inclusion has no effect on our inferences or conclusions.

D. Covenant Violations: Summary Statistics

Table III presents an analysis of covenant violations in our two samples. Panel A presents average, medians (in brackets), and standard errors (in parentheses) for several descriptive measures of violations. The first three rows reveal that covenant violations are not infrequent. Almost 40% and 30% of the firms in the current ratio and net worth samples experience a covenant violation, respectively. Similarly, 33% and 22% of the deals experience a covenant violation and 15% and 11% of the firm-quarter observations are classified as in violation, respectively. Thus, while violations are far from common, they do occur in a significant number of instances.

The fourth and fifth rows present a relative and absolute measure of the initial covenant tightness by measuring the distance between the actual accounting measure and the covenant threshold at the inception of the loan. The relative distance measure is with respect to the initial covenant threshold. For example, if the initial covenant specifies a minimum net worth of $10 million and the actual net worth of the firm is $11 million, the relative tightness measure is equal to 0.1. An identical approach is used to measure the tightness for the current ratio covenant. In relative terms, we see that current ratio covenants are, on average, set more tightly than net worth covenants, though this difference is only marginally significant. Indeed, a comparison of medians suggest that this relation is reversed and coincides with the skew in the underlying distribution of these variables.

In absolute terms, the average spread between the actual current ratio and corresponding initial threshold is 1.05, while that between the actual net worth and corresponding initial threshold is $200 million. To get a better sense of how tight these initial settings are, we compare these initial distances with the standard deviations of the current ratio and net worth variables. The standard deviation of the current ratio is 1.42, implying that the initial tightness of the current ratio is just under one standard deviation. For the net worth covenant, heterogeneous scales require us to compute an average firm specific standard deviation, which results in an estimate of $87.75 million (a nearly identical

result is obtained for tangible net worth). This result implies that net worth covenants are initially set just over two standard deviations below the actual net worth. However, both covenants are initially set tightly relative to the typical variation in the underlying accounting variable.

The sixth row looks at the time until a covenant is first violated, conditional on a violation occurring. To account for loans of varying lengths, we normalize the time by the maturity of the loan so that our measure is on a unit time scale. Thus, the measure indicates whether most loan covenants are violated early or late in the life of the loan. For both types of covenants, violations tend to occur at the midpoint of the loan, consistent with the tightness of these covenants at the beginning of the loan and the short loan maturity.

We then examine the fraction of quarters in the life of the loan that are in violation of a covenant, conditional on at least one violation occurring. A firm can be in violation of a covenant for more than one period if, for example, a waiver is granted or negotiations are delayed. As Table III indicates, when a firm violates a current ratio (net worth) covenant, approximately 27% (33%) of the quarters in which the loan is active are in violation. Assuming an average loan maturity of three years, these estimates translate into between three and four quarters in which firms are in violation when they violate a covenant. Thus, covenants are not simply one time occurrences in the life of a loan and, as we next discuss, violating a covenant often leads to violations in future loans.

Our final result in Panel A looks at the subset of firms that take out multiple deals and violate a covenant in at least one of them. We then compute the average (and median) number of loans in which these firms experience covenant violations. By doing so, we hope to uncover whether firms that violate a covenant are more or less likely to violate again in a subsequent loan. The former likelihood seems to be the case. The median firm with multiple loans and at least one violation violates a covenant in two distinct loans. This is consistent with lenders taking a more aggressive or preventative stance in future loans.

Panels B and C expand on this last result by examining the consequences of loan violations for future loan agreements. Specifically, each panel compares the characteristics of the first loan where a violation occurred with those of subsequent loans. In doing so, we are implicitly conditioning on firms with multiple loans where at least one of the loans contains a covenant violation and there is a subsequent loan appearing in the data after this loan. As the observation figures at the bottom of each panel indicate, this severely restricts our sample size. Additionally, this is an admittedly noisy comparison as firms

may have violated covenants in other loans not recorded by Dealscan or loans taken out before our period of study begins (1994).

For both current ratio (Panel B) and net worth (Panel C) samples, loans subsequent to a technical default experience dramatic increases in interest rates. The average Loan Yield increases by 69 and 51 basis points in the current ratio and net worth samples, respectively. While the statistical significance of these differences is marginal, owing largely to the small number of observations, the economic magnitude is large. For an average loan size of $55 million and $145 million, these increased interest rates correspond to increases of $379,500 and $739,500 over the average three year maturity in the current ratio and net worth samples, respectively. Subsequent loans also experience a reduction in the amount of borrowable funds relative to firm size - more so for the current ratio sample. Thus, firms are able to borrow somewhat less, while paying significantly more for those funds after experiencing a technical default. For net worth loans, the maturity of future loans also declines, though most bank loans are already short-term (3 year) instruments and, therefore, there is little room to further restrict this aspect of the loan.

For both samples, we see a slight, but statistically insignificant, increase in the average number of financial covenants (i.e., restrictions on various accounting ratios). The last four rows of panels B and C presents the effects of technical defaults on future sweep provisions. Sweep provisions require that a fraction of the outstanding loan balance be repaid immediately if certain actions are undertaken by the borrower. For example, a debt sweep of 50% might require the borrower to repay 50% of the outstanding loan balance if the borrower issues debt. Or, a 75% equity sweep may require firms to pay down 75% of the loan in the event of an equity issuance. For each sweep provision, we see that the fraction of outstanding balance that may be recalled in the event of a violating action increases, and dramatically so, after a technical default. Thus, Panels B and C illustrate that debt financing subsequent to a technical default appears relatively more expensive, notwithstanding the low statistical power.

Table IV presents firm characteristics for our two samples, this time stratified by whether the firm is (Bind) or is not (Slack) in violation of the covenant. Several results are worth noting. First, we see an economically and highly statistically significant decline in investment, both in terms of averages and medians, when firms are in violation of their covenants. Average investment falls by 2.1% in the current ratio sample and 2.7% in the net worth sample. Relative to the average investment rates of 8% and 7%, these declines correspond to a 26% and 39% drop in investment, respectively. Second, there is significant heterogeneity in other firm characteristics across the Bind and Slack samples. For both

current ratio and net worth samples, when a firm is in violation of a covenant investment opportunities (market-to-book) and cash flow are significantly lower. We also see an increase in leverage. These results suggest that in order to uncover the true impact of violations on investment, we must control for variation in these characteristics.

III. Results

This section more formally examines the relation between covenant violations and investment. We begin with a non-parametric analysis to highlight the relation absent any restrictions on functional form or distributional assumptions. We then move to a more formal regression setting to account for other influences on the investment decision. Our primary set of regressions rely on a discontinuity design originally pioneered by Thistlewaite and Campbell (1960), which we discuss in the context of covenant violations. We then link this analysis with various proxies for information asymmetry, renegotiation costs, and credit constraints, showing that the impact of covenant violations on investment is closely tied to these underlying frictions.

A. Non-Parametric Analysis

Table V expands the analysis of Table IV by further stratifying each subsample, *Bind* and *Slack*, into quartiles and examining the average investment (other characteristics) in each quartile. This procedure is akin to a non-parametric regression but avoids the sensitivity of estimated response surfaces to the choice of smoothing parameter. Panel A shows what appears to be a kink in the investment rate around the point at which the constraint becomes binding. Investment falls by 0.8% in transitioning from a slack to a binding constraint, which is significantly larger than the adjacent changes (0.4% and 0.3%). A similar result is found in Panel B for the effect of the net worth covenant. Investment declines by 0.9% when moving from a slack to a binding constraint, a large change relative to that found in the adjacent quartiles.

An alternative perspective of the data takes a more explicitly dynamic view. Figure 1 presents average investment in event time relative to the quarter in which a covenant violation occurs. Also presented are 95% confidence intervals, as indicated by the vertical width of the band surrounding the estimated average. Focusing on results for the current ratio sample in Panel A, we see that the average investment rate in the eight quarters prior to a covenant violation ranges between 8.2% and 9.5%. Upon violation, the average investment rate drops just over 6%. Panel B reveals a similar pattern for the net worth

sample with investment dropping fairly precipitously during the quarter of violation.[14] The task now is to identify what fraction of this investment decline is due solely to the covenant violation, as opposed to a consequence of changes in other aspects affecting corporate investment.

B. Regression Discontinuity Design

Our discussion of the regression discontinuity design is purposely brief and given in the context of our specific setting, namely, covenant violations. For more detailed discussions, see Thistlewaite and Campbell (1960), Trochin (1984), and Hahn, Todd, and Van der Klaauw (2001). For recent applications of this approach in Economics and Finance, see studies by Angrist and Lavy (1999), Black (1999), Van der Klaauw (2002), Rauh (2005), and Black, Jang, and Kim (2006).

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." (P. 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 underlying accounting variable into the treatment effect is discontinuous. Specifically, our treatment variable, $Bind_{it}$, is defined as:

$$Bind_{it} = \begin{cases} 1 & z_{it} - z_{it}^{0} < 0 \\ 0 & \text{otherwise.} \end{cases} \tag{1}$$

where z is the observed current ratio (or net worth) and z^0 is the corresponding threshold specified by the covenant.

Our base empirical model for this section is

$$\begin{aligned} Investment_{it} &= \alpha_0 + \beta_0 Bind_{it-1} + \beta_1 Q_{it-1} + \beta_2 CashFlow_{it} \\ &+ \beta_3 Size_{it-1} + \beta_4 X_{it-1} + \eta_i + \nu_t + \varepsilon_{it}, \end{aligned} \tag{2}$$

[14]The width of the standard error bands decreases uniformly as we move closer to the quarter of violation primarily because the number of observations increases. The width of the band decreases more dramatically for the quarter of violation because of a more dramatic increase in the number of observations. This large change occurs because in many cases, firms will be in violation of a covenant for more than one consecutive quarter and, therefore, there are no corresponding pre-violation (event time -1, -2, etc.) observations.

where $Investment$ is the ratio of capital expenditures to the start of period capital, $CashFlow$ is the ratio of income before extraordinary items plus depreciation and amortization to start of period capital, $Size$ is the natural log of the firm's assets, X is a vector of control variables, η_i is a firm fixed effect, ν_t is a period fixed effect, and ε_{it} is a random error term assumed to be correlated within firm observations and potentially heteroscedastic. The parameter of interest is β_0, which represents the marginal impact of a covenant violation on investment (i.e., the treatment effect). Because of the inclusion of a firm specific effect, identification of β_0 comes only from those firms that experience a covenant violation. Therefore, we restrict our attention to the subsample of firms that experience at least one covenant violation.

The appeal of this approach is that the nonlinear relation in equation (1) 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 investment (ε) exhibits an identical discontinuity as that defined in equation (1) - relating the violation status to the underlying accounting variable. That is, even if ε is correlated with the difference, $z - z^0$, our estimate of β_0 is unbiased as long as ε does not exhibit precisely the same discontinuity as $Bind_{it}$ does. While this outcome is unlikely, it is not impossible and, therefore, we take several additional steps below to ensure that our results are not capturing such a latent discontinuity.

Additionally, since the discontinuity is the source of identifying information, we also estimate equation (2) on the subsample of firm-quarter observations that are close to the point of discontinuity. To remove some of the subjectivity associated with the definition of "close," we turn to the literature on non-parametric density estimation (Silverman (1986)). We begin with a measure of optimal window width that is defined as, $0.79Rn^{-1/5}$, where R is the interquartile range and n is the number of observations. This measure corresponds to an optimal tradeoff between bias and variance and, more importantly, is defined independently of corporate behavior. For the current ratio (net worth) sample, this measure is equal to 0.10 (0.09). To keep the definition simple and bolster statistical power, we formally define the Discontinuity sample as those firm-quarter observations for which the absolute value of the relative distance between the accounting variable (current ratio or net worth) and the corresponding covenant threshold is less than 0.20, or approximately two "windows" within the threshold. This restriction reduces our sample size by over 60%.

Panel A of Table VI presents the estimation results for the entire sample. The first

column presents estimation results from the traditional investment specification relying only on Tobin's q and cash flow. The coefficient estimates are consistent with findings in previous studies, namely, a relatively small but significantly positive coefficient on Q and a positive coefficient on *CashFlow*. A slight departure from previous findings is that the cash flow coefficient is only marginally (i.e., at the 10% level) statistically significant.

The second column reveals that after removing both firm and period fixed effects, covenant violations are associated with a decline in investment on the order of 1.4% of capital per quarter. Relative to an average quarterly investment rate of approximately 7%, this estimate translates into a relative decrease in capital expenditures of 20%. However, as highlighted in Table IV, other investment related factors are changing with the covenant violation. Thus, the remaining specifications attempt to control for these factors in order to isolate the marginal effect of the covenant violation.

Specification (3) in Table VI includes traditional control variable: Tobin's q, cash flow, and firm size. Tobin's q and cash flow are both positive and statistically significant, while firm size is indistinguishable from zero. This modification leads to a slight attenuation in the estimated treatment effect from 1.4% to 1.2%. Introducing lagged cash flow into the specification produces a significantly positive coefficient on this control variable but has little impact on the other estimated coefficients and standard errors. Importantly, the estimated treatment effect is still 1.1% and highly statistically significant.

The next two specifications attempt to capture changes in the financial health of the firm that may accompany the covenant violation. Our first control is the book leverage ratio of the firm defined as the ratio of total debt to total assets. Consistent with the findings of Lang, Ofek, and Stulz (1996) and Hennessy (2004), firms with more leverage tend to invest less, on average. Nonetheless, the impact of covenant violations is still statistically significant at even the 1% confidence level. However, leverage may be too coarse of a measure of a firm's financial health. Thus, we examine another measure of the financial health of the firm: Altman's Z-Score (see Altman (1968)).

By construction, Altman's Z-Score is intended to be a sufficient statistic for the financial health of the firm. We use a slightly modified version of his original measure that is frequently used in the capital structure literature as a proxy for the likelihood of default (e.g., Mackie-Mason (1990), Graham (1996), Leary and Roberts (2005)). Briefly, the Z-Score is a linear function of several financial ratios constructed from discriminant analysis. In our definition, a higher Z-Score corresponds to a lower likelihood of financial distress. See Appendix A for the variable definition. As the sixth column indicates, firms with higher Z-Scores (i.e., financially healthier) tend to invest more. Interestingly, firm

size also becomes significantly negatively associated with investment, perhaps a result of multicollinearity. However, most important, this variable has no effect on the magnitude or statistical significance of the estimated treatment effect relative to the inclusion of leverage. Covenant violations are still associated with an approximate 1% decline in investment.

The final column attempts to further isolate the discontinuity corresponding to the covenant violation by including smooth functions of the distance from the default boundary into the specification. More precisely, $CR{-}DefaultDistance$ and $NW{-}DefaultDistance$ are defined as

$$CR\ Default\ Distance = I_{(Current\ Ratio_{it})} \times (Current\ Ratio_{it} - Current\ Ratio^{0}_{it}) \quad (3)$$

$$NW\ Default\ Distance = I_{(Net\ Worth_{it})} \times (Net\ Worth_{it} - Net\ Worth^{0}_{it}) \quad (4)$$

where $I_{(Current\ Ratio_{it})}$ and $I_{(Net\ Worth_{it})}$ are indicator variables equal to one if the firm i, year t observation is bound by a current ratio or net worth covenant, respectively. The $Current\ Ratio^{0}_{it}$ and $Net\ Worth^{0}_{it}$ variables correspond to the covenant thresholds. We include both a linear and square term to pick up any nonlinearities.[15] The results in the seventh column show that these functions do contain some explanatory power for investment beyond their association with the covenant violation, though, economically, the marginal effect is relatively small and appears to be concentrated among net worth loans. As for the impact of these controls on the marginal effect of covenant violations, we see another attenuation in the estimated effect, from 0.9% to 0.6%. Nonetheless, this estimate is still highly statistically significant and, economically, corresponds to an almost 10% relative decline in investment due to the covenant violation.

Panel B of Table VI presents the estimation results for the Discontinuity sample. The observation row towards the bottom of the panel shows that the number of observations has fallen by approximately 60% for each specification, relative to the entire sample. Despite this decline in degrees of freedom, the estimated investment responses are very similar , both in magnitude and statistical significance, to those found in Panel A for the entire sample. Thus, these results reinforce the fact that the identification is, indeed, coming from the discontinuity.

In sum, covenant violations lead to a significant decline in investment. Our next goal is to move closer to understanding why these violations matter for investment, as well as ensuring the robustness of our results.

[15] Including cubic terms has a negligible effect on the results.

C. *Heterogeneity in Information Asymmetry, Renegotiation Costs, and Credit Constraints*

There are several potential reasons why covenant violations impact investment. For example, the decline in investment may coincide with an adverse selection cost associated with information asymmetry between the borrower and lender (e.g., Rajan and Winton (1995)). Or, the decline may coincide with renegotiation costs or a tightening credit constraint. While our reduced form framework does not permit us to identify a unique source, we can examine whether the investment response is concentrated in situations where one might expect to find it, namely, where information asymmetry is high, renegotiation costs are high, and credit constraints are binding. As such, we interact ex-ante proxies for information asymmetry, renegotiation costs, and credit constraints with all of the covariates in equation (2) to determine whether the investment response is related to the hypothesized frictions.

More precisely, the empirical model is:

$$Investment_{it} = \alpha_0 + \Gamma_0 I_{(\omega)} X_{it-1} + \Gamma_1 (1 - I_{(\omega)}) X_{it-1} + \eta_i + \nu_t + \varepsilon_{it}, \qquad (5)$$

where Γ_0 and Γ_1 are vectors of parameters, X is a vector of covariates consisting of $Bind$, Q, $CashFlow$, $Size$, and $Z - Score$, η_i is a firm fixed effect, ν_t is a period fixed effect, and ε_{it} is a firm and period specific random shock that is correlated within firms and potentially heteroscedastic. The indicator function, $I_{(\omega)}$, is equal to one if the event ω is true and zero otherwise, and corresponds to a particular proxy. While we discuss these proxies in detail below, an example of $I_{(\omega)}$ might be that it equals one if renegotiation costs are expected to be high and zero if they are expected to be low. Thus, each covariate in the X vector has two coefficients associated with it: one coefficient corresponding to the variables' interaction with $I_{(\omega)}$ and another corresponding to the variables' interaction with $(1 - I_{(\omega)})$.

Estimation of equation (5) is similar to estimating equation (2) separately on two groups determined by the indicator function except that in equation (5) we do not interact any of the error components with the indicator function. Estimating the interacted model (equation (5)), as opposed to estimating the same model separately on two different samples, provides for a more transparent statistical analysis that compares the coefficients of different interactions from the same regression.[16]

An important by-product of this analysis is a further test for the endogeneity of the covenant violation indicator variable. Specifically, a possible interpretation of the results

[16]We thank Josh Rauh for recommending this approach.

in Table VI is that the covenant violation indicator variable ($Bind$) is simply measuring latent, discontinuous shifts in the investment opportunity set not captured by the other control variables. However, if this is indeed the case, then both of the estimated coefficients on the $Bind$ variable should be the same. Specifically, if γ_0^{Bind} is the coefficient on the interaction term $I_{(\omega)} \times Bind_{it-1}$ and γ_1^{Bind} is the coefficient on the interaction term $(1 - I_{(\omega)}) \times Bind_{it-1}$, then our null hypothesis is that the estimated coefficients γ_0^{Bind} and γ_1^{Bind} are equal. Thus, in so far as our ex-ante measures of information asymmetry, renegotiation costs, and credit constraints are uninformative about firms' future investment opportunities, our analysis here can provide further reassurance in our earlier results.

The first measure that we examine is an indicator for whether an observation occurred before or after the passage of the Sarbanes-Oxley Act (SOX) in the summer of 2002. While SOX is generally viewed as a reaction to conflicts of interest between managers and shareholders, the implications of SOX are much broader. In particular, recent research (e.g., Cohen, Dey, Lys (2005), Jain, Kim, and Rezaee (2004), Rezaee (2004), Ribstein (2002), and Cunningham (2003)) suggests that SOX has (1) improved corporate governance by reducing information asymmetry between insiders and outsiders of the firm, (2) enhanced the quality and increased the flow and transparency of financial information, and (3) improved the effectiveness and credibility of financial statement auditing. By mitigating the information asymmetry between insiders and outsiders, SOX mitigates one of the financial frictions for which covenants are a remedy (e.g., Rajan and Winton (1995)). As such, we might expect the investment response to a covenant violation to be larger in magnitude in the pre-SOX era, when information asymmetry and monitoring costs are greater.

The second and third measures utilize information about the other party to the loan contract, the lender. Several studies (e.g., Lehmann and Nueberger (2001) and Dahiya, Saunders and Srinivasan (2003)) suggest that relationship lending is valuable because it mitigates information asymmetry and reduces renegotiation costs. As such, we expect that firms having a historical relationship with their current lender will experience a relatively smaller decline in investment relative to firms having few past relationships with their current lender. Another lender measure is the size of the lending syndicate. Bolton and Scharfstein (1996) suggest that the number of creditors should be endogenously chosen to account for potential renegotiation costs. As such, all else equal, larger lending syndicates will select loans for which renegotiation will likely be less expensive, as coordination may be more difficult among many creditors. Thus, we expect that covenant violations in loans underwritten by small lending syndicates will experience a greater

decline in investment because of the loan selection.

We also examine the ratio of tangible assets to total assets, whether or not the firm has a credit rating, and the size of the loan relative to total assets. The first measure is a proxy for the liquidation value of the firm in the event of bankruptcy (Rajan and Zingales (1995)). Firms with greater liquidation values will have a stronger bargaining position in bankruptcy and, therefore, should experience a relatively smaller decline in investment upon violating a covenant. Alternatively, firms with more tangible assets may be more informationally transparent then firms whose assets are largely intangible. This interpretation, however, leads to a similar empirical implication. Firms with credit ratings have greater access to external capital, specifically public debt markets, and therefore have greater bargaining power in renegotiation. Therefore, firms without credit ratings should experience a larger decline in investment. Finally, covenant violations in relatively larger loans likely correspond to more serious problems, both from the borrower's and lender's perspective. Thus, covenant violations in relatively larger loans are likely to experience larger declines in investment.

The results of our analysis are presented in Table VII. Panel A presents the results using the entire sample, Panel B the Discontinuity sample. For presentation purposes, we display only the estimated coefficients and t-statistics corresponding to the two covenant violation variables: $I_{(\omega)} \times Bind_{it-1}$ and $(1 - I_{(\omega)}) \times Bind_{it-1}$. Each of the proxies used to define the indicator function, $I_{(\omega)}$, other than the passage of Sarbanes-Oxley, is measured prior to the start of the loan to ensure that these measures are unaffected by events around the time of the covenant violation. Beginning with Panel A, the first column presents the results for the SOX interaction model. The estimates reveal that violations occurring before the passage of SOX lead to significantly larger declines in investment. Specifically, investment declines by an economically and statistically significant 1.4% prior to the passage of SOX. After SOX's passage, the effect fell by 0.8% to 0.6%, a relative decline in the estimated effect of 57%. The row denoted "T-Dif" presents t-statistics of the difference between the two estimated coefficients and shows that the difference is statistically significant. This finding is consistent with SOX playing a role in mitigating the information asymmetry between borrowers and lenders, and decreasing the monitoring costs expended by lenders.

We measure the number of historical lending relationships as the number of times in the past that a borrower has entered into a deal with one of its current lenders. This is an admittedly noisy proxy, particularly since many of our loans are syndicated and, therefore, some borrowers may only deal directly with a few members of the lending

syndicate. However, lender data available on Dealscan is somewhat limited and this noise should attenuate any differences. We focus on loans in which the borrower has no historical lending relationship with its current creditors (None) and loans in which the borrower has at least three relationships (Many). (Using an alternative breakpoint of two or four yields similar results.) The results show that firms violating a covenant with a relatively new lender experience a significant decline in investment (1.4%). Firms violating a covenant with a long-time lender actually experience an increase in investment, though the magnitude is neither economically nor statistically significant. The difference between these two estimates, 1.8%, is, however, economically and statistically significant. This finding is consistent with the importance of lending relationships in mitigating renegotiation costs.

The size of the lending syndicate is categorized into two groups depending on whether their is only one lender (Small) or more than five lenders (Large). The choice of five lenders is arbitrary but, again, our results are unchanged if we perturb this number by one unit in either direction. The results indicate that covenant violations in loans with an individual underwriter experience a particularly large decline in investment of 2.0% of capital per quarter. Covenant violations in loans underwritten by large lending syndicates, on the other hand, experience a statistically insignificant decline in investment (0.4%). The difference between these two responses is highly statistically significant and is consistent with large lending syndicates selecting loans for which potential renegotiation costs are most likely to be low.[17]

We also find that firms with low liquidation values and lacking a credit rating experience significantly larger investment declines in response to covenant violations. To proxy for liquidation values, we sort the ratio of tangible assets to total assets in the quarter prior to the start of the loan into three quintiles. The lowest quintile corresponds to the Low asset tangibility group; the highest quintile corresponds to the High asset tangibility group. Finally, we classify loans into Small and Large categories by whether they correspond to less than 10% of asset value or greater than 40% of asset value in the quarter prior to the start of the loan. While the larger loans do experience a larger investment decline associated with covenant violations, the difference between large and small loans is statistically and economically small. Nonetheless, these three sets of findings are consistent with the notion that firms whose bargaining power is likely to be high in the event of a covenant violation experience insignificant declines in investment, counter to

[17]A related result found by Sufi (2006) is that lead arrangers in lending syndicates retain a larger portion of the loan when borrowers require more monitoring.

that experienced by firms for which bargaining power is likely to be low.

Turning to Panel B, we see that the results obtained using the Discontinuity sample are quite similar. The economic magnitudes of the coefficient differences line up closely with those found using the entire sample. Additionally, statistical significance is actually stronger for the Discontinuity sample in most comparisons, although the different investment responses between the High and Low asset tangibility groups are no longer statistically distinguishable. Thus, as in Table VI, the similarity of results between the entire sample and the Discontinuity sample reaffirms the source of identification and further ensures the robustness of our findings.

In concert, these results suggest that the investment decline associated with covenant violations corresponds to an underlying financing friction present in the loan contracting process. Importantly, they also offer compelling evidence that our findings are not an artifact of endogeneity bias. The number of historical lending relationships, the size of the lending syndicate, the passage of Sarbanes-Oxley, and the presence of a credit rating are unlikely to be correlated with discontinuous shifts in the investment opportunities of our firms. Yet, the investment response to covenant violations is highly sensitive to these factors, consistent with the estimated effect capturing the impact of the underlying financing frictions. The remainder of the paper reinforces this interpretation by undertaking several additional robustness tests.

IV. Robustness Tests

This purpose of this section is to present additional analysis to ensure the validity of our results. First, we reestimate the investment response to covenant violations using an alternative identification strategy based on a propensity score matching technique. This approach provides a non-parametric identification strategy that is robust to departures from the linearity assumed in the regression discontinuity design. Second, we examine whether managerial manipulation of accounting figures may be driving our results. This examination addresses the possibility that managers may dictate the occurrence of covenant violations to coincide with low/poor investment opportunity states not explicitly captured by the regression controls. Third, we examine the effect of measurement error in our proxy for Tobin's q on the estimated treatment effect. This analysis is performed to ensure that our estimated investment response is not contaminated by measurement error in the control variables.

A. Alternative Estimates of the Investment Response: A Matching Approach

This section presents estimates of the investment response to covenant violations using a propensity score matching strategy developed by Rosenbaum and Rubin (1983) and further refined by Heckman, Ichimura, and Todd (1997, 1998), among others. The matching procedure begins by defining the treatment and control groups. In our case, the former consists of firm-quarter observations in violation of a covenant and the latter consists of firm-quarter observations not in violation of a covenant. We further restrict the definition of the control group to those firms that do not experience a covenant violation to ensure that the treatment and control groups do not overlap in terms of firms. Our data are particularly well suited to using a matching method since the pool of controls is relatively large (almost ten times the size of the treatment group). As we will see, this leads to a significant overlap in the support of firm characteristics across the two groups. Additionally, firms in both the treatment and control groups are subject to similar reporting requirements ensuring that the data is uniform across both groups.

Having defined the treatment and control samples, the next step is to estimate a logit regression of the binary variable indicating whether an observation is in violation of a covenant, *Bind*, on variables that are likely to determine such an outcome.[18] The precise specification coincides with a tradeoff between model parsimony and an accurate description of the data generating process. Too few variables may lead to a poor match between treatment and controls while too many variables may lead to a small number of matches. As such, we include in this specification: the market-to-book ratio, cash flow, firm size, book leverage, Altman's Z-Score, and calendar year fixed effects.

The first column in Table VIII, denoted "Pre-Match," presents the estimation results. We first note that there are 1,628 firm-quarter observations that are both in violation of a covenant and have nonmissing data for all of the right hand side variables. These are the members of the treatment group (Violation). Similarly, there are 10,344 members of the control group (Non-Violation) that are not in violation of a covenant and have nonmissing data for all of the right hand side variables. The coefficients indicate highly statistically significant differences between the treatment and control groups, on average. Consistent with the earlier findings in Table IV, we see that covenant violations are associated with fewer investment opportunities (lower market-to-book), less cash flow, higher leverage, poorer financial health, and tend to occur in smaller firms.

Using the predicted probabilities from this estimated model, i.e., the propensity

[18] Employing a probit specification yields nearly identical results.

scores, we match each observation from the treatment group with a unique observation from the control group based on the smallest absolute difference between the estimated propensity scores. That is, the matched sample consists of those unique (i.e., no duplicate observations) pairs for which the absolute difference in the propensity scores is minimized.[19] To ensure that our matched sample consists of only those pairs of observations that are truly similar, we restrict attention to matches for which the difference in propensity scores is less than 5%, or approximately one half of a standard deviation in the estimated propensity scores. Imposing this restriction results in the loss of less than 10% of the matched observations.

To assess the accuracy of the matching procedure, we reestimate the logit regression using the matched sample. The results are presented in the column entitled "Post-Match" in Table VIII. There are now no significant differences between the matched treatment and control groups across any of the dimensions included in the model. Importantly, the results are not simply an artifact of a decline in model degrees of freedom. Relative to the Pre-Match estimates, each coefficient experiences a dramatic decline in magnitude. Though not reported, inspection of the year fixed effects reveals similar insignificant estimates in the Post-Match regression.

With the matched sample, we can estimate the effect of covenant violations in general via a simple regression of investment on an intercept and the covenant violation indicator, *Bind*. This analysis amounts to a simple comparison of sample means for the treatment and control groups but enables us to estimate robust standard errors in a manner consistent with previous analysis. The results are presented in the first column of Table IX and reveal that covenant violations lead to an 0.8% decline in quarterly investment, similar to our findings in Table VI using the regression discontinuity approach.

The remaining columns in Table IX presents the results from a difference-in-difference analysis where the model is

$$Investment_{it} = \alpha_0 + \beta_0 Bind_{it} + \beta_1 I_{(\omega)} + \beta_2 Bind_{it} \times I_{(\omega)} + \varepsilon_{it}, \quad (6)$$

where are all variables are as defined earlier. The coefficient of interest is β_2 on the interaction term, which enables us to compare the effect of covenant violations for two different groups identified by the indicator variable, $I_{(\omega)}$, for example, pre- versus post-SOX observations, few versus many historical lending relationships, etc. Thus, this analysis acts

[19] We thank Erik Bergstralh and Jon Kosanke for the use of their matching code. The original reference article is Bergstralh and Kosanke (1995).

as a robustness test to the results found in Table VII corresponding to the interaction model (equation (5)).

With the exception of Sarbanes-Oxley, all of the coefficient estimates display magnitudes and signs similar to the difference in coefficient estimates found in Table VII. The results for Sarbanes-Oxley are significantly weaker than that found in Table VII, suggesting that this finding may be somewhat sensitive to the linearity assumption. Another difference between the two sets of results is that the matching analysis presented in Table IX exhibit somewhat less statistical significance. Most all of the differences in Panel A of Table VII are statistically significant at the 5% confidence level, as indicated by the "T-Dif" row. However, some of the coefficients on the interaction terms in Table IX, such as those corresponding to lending relations and syndicate size, are only marginally significant at the 10% level. Of course, this decline in statistical power is to be expected for two reasons. First, the sample size has fallen, on average, by almost 25%. Second, the robustness to departures from linearity that accompanies the matching identification strategy leads to a decrease in statistical power - a fundamental tradeoff. Nonetheless, these findings are generally reassuring.

B. Accounting Manipulation and Violation Avoidance

The issue of whether managers make accounting choices to delay or avoid technical defaults has received a significant amount of attention in the accounting literature but with largely mixed results, as noted by Dichev and Skinner (2002). While a complete investigation of this issue is beyond the scope of this study, we provide several pieces of evidence to suggest that this behavior is either rare or non-existent in our sample of firms and that our results are robust to this concern.

We begin by noting that, intuitively, the notion that managers can consistently fool lenders through accounting manipulation is dubious. First, most financial managers are required to fill quarterly compliance reports to creditors detailing the computation and compliance with each of the financial covenants in the loan agreement. Second, creditors in our sample are primarily comprised of commercial banks and, to a lesser extent, other financial institutions such as investment banks and insurance companies. These institutions contain a significant amount of financial expertise and, by definition, are professional monitors whose main business lines include commercial lending. Additionally, most creditors in our sample are not new to the commercial lending business - having entered into tens of thousands of lending agreements over many decades (e.g., Citigroup,

Chase Manhattan, Bank of America). Therefore, we, and undoubtedly most commercial lenders, believe that accounting manipulation is unlikely a useful tool for consistent or egregious covenant avoidance.[20]

Rather, discussions with both corporate financial officers and commercial lenders suggest that a more common approach that managers take when facing an imminent covenant violation is to contact the lender and renegotiate the terms of the contract. Additionally, we find some direct evidence consistent with this course of action in several loan amendments that we are able to gather from Loan Pricing Corporation. For example, on June 1 of 2000, One Price Clothing Stores Inc. renegotiated its revolving line of credit to "amend certain prohibitive covenants," despite no apparent sign of a covenant violation in either the Dealscan data or the footnotes of 10k filings around that time. Similarly, on November 15th of 2001, Valspar Corp. "adjusted certain financial covenants and definitions," again, in spite of no apparent covenant violation at the time. Of course, these preemptive renegotiations are not identified as covenant violations in our data and, therefore, any investment response accompanying these renegotiations will not be captured by our analysis. However, this situation works against our previous findings, suggesting that we may, in fact, be missing part of the investment response associated with renegotiations occurring outside of an actual technical default.

Nonetheless, we take a closer examination of this issue with several additional analysis, beginning with an examination of the distribution of the distance to the covenant threshold presented in Figure 2. Panel A presents the distribution for the current ratio sample, Panel C for the net worth sample. For the bin width, we follow Silverman (1986) and examine three different choices corresponding to variations in the bias-variance tradeoff implicit in this choice:

$$\begin{aligned} h_1 &= 2Rn^{-1/3}, \\ h_2 &= 0.79Rn^{-1/5}, \\ h_3 &= 0.9An^{-1/5}. \end{aligned}$$

The notation used is that R is the interquartile range, n is the number of observations, and A is defined as $\min(\text{Standard Deviation}, R/1.34)$. Since these three bin widths all result in similar histograms, we present only those results obtained by using h_1. To ease

[20]This is not to say that accounting manipulation does not occur, which, in light of recent corporate scandals is clearly untrue. Rather, the relevant issue is whether the manipulation is directed at avoiding covenant violations and can consistently fool sophisticated institutional investors, which is what we argue and show below is less plausible.

the presentation, we trim the 10% right tail of both distributions - observations that are far from the covenant threshold. Also presented in each figure is the (untrimmed) distribution of the initial distance from the covenant threshold measured at the start of the loan.

Of course, without a frame of reference, i.e., a null hypothesis, these figures are of little aid. Therefore, in Panels B and D, we present the distributions of data simulated under the null hypothesis of no managerial manipulation. The simulations begin by restricting attention to all loans in the intersection of the CRSP/Compustat and Dealscan databases that do not contain either a current ratio or net worth covenant. For each of these loans, we randomly assign a current ratio and net worth covenant threshold by drawing thresholds from the empirical distributions of the initial distance from the covenant threshold. Using these thresholds in conjunction with the observed current ratio and net worths over the life of each loan, we are able to measure the distance to the randomly assigned covenant threshold and plot the resulting distribution.[21] This procedure ensures that the resulting distribution is not generated by any accounting manipulation by managers since the current ratio and net worth covenants are artificial.[22]

The similarity between the actual and simulated distributions provides insight into two hypotheses associated with accounting manipulations. First, if managers are actively manipulating accounting numbers to avoid covenant violations, then violations that just barely cross the covenant threshold should be relatively rare. Rather, covenant violations should only occur when the violation is so egregious that accounting manipulation simply cannot prevent it. Examining Panels A and C, we note that the plurality of covenant violations occur just below the threshold. For the current ratio (net worth) sample, these violations are within 9% (7.5%) of the threshold and correspond to approximately 26% (25%) of all covenant violations.[23] Thus, when firms violate a covenant, the most likely violation is one that is very close to the covenant threshold - counter to the first

[21]To ensure consistency with the data, we randomly assign 12.5% of the current ratio thresholds and 48.7% of the net worth thresholds to increase over the life of the loan in accord with the median buildup in both the current ratio (0.03 per quarter) and net worth (50% of positive net income per quarter) samples.

[22]We note that even if managers manipulate other accounting ratios by which they are bound, the simulated data still remain true to the null hypothesis since the covenant thresholds are randomly assigned to firms and, therefore, completely arbitrary.

[23]In unreported analysis, we examine similar histograms that focus only on observations leading up to and including the first violation in a loan. The results are qualitatively similar, except that the fraction of violations occurring just below the covenant threshold is much larger. For the current ratio (net worth) sample, this fraction is 41% (42%).

hypothesis of manipulation.

A second hypothesis (e.g., Dichev and Skinner (2002)) suggests that accounting manipulations to avoid covenant violations will result in a "drop" in density when moving from the bin just above the threshold to that just below the threshold. Indeed, we, like Dichev and Skinner (2002), observe a drop when moving from the bin just above the threshold to that just below. However, comparing Panel A (C) to B (D), we see an almost identical drop in the simulated data, in which there is no accounting manipulation to avoid breaching the artificial thresholds. Therefore, this test is simply uninformative with respect to whether or not managers are manipulating their accounting ratios to avoid covenant violations. That is, this test has no statistical power to distinguish among alternative hypotheses. Intuitively, this is not surprising; covenants are explicitly set by lenders to capture left tail (i.e., bad) events, so that one would might expect a significant drop in the density to the left of the covenant threshold absent any accounting manipulation.

Nonetheless, if managers are manipulating accounting numbers according to this second hypothesis, then this suggests that observations away from the covenant threshold should be free of manipulation. Therefore, we re-examine our regression results (Table 2) and matched sample results (Table VIII) using only those observations whose relative distance to the covenant threshold falls outside the -0.2 to 0.2 window - approximately two bin widths on each side. These observations, according to the second hypothesis, should be largely free from manipulation activity since the firm is either sufficiently far above the threshold that there is no concern over violation or sufficiently far below that manipulation could not have staved off violation. Estimates of the investment response to covenant violations from equation (2) yield virtually identical results to those presented in Table VI and, consequently, are not presented.

Our final analysis of potential accounting manipulation examines the affect of abnormal accruals on our estimates of the investment response to covenant violations. We examine abnormal accruals as a measure of management manipulation because this measure, despite being somewhat noisy (Dechow, Sloan, and Sweeney (1995)), "has the potential to reveal subtle manipulation strategies related to revenue and expense recognition" (DeFond and Jiambalvo (1994), page 149). We follow closely the procedure outlined by DeFond and Jiambalvo (1994) to estimate two measures of abnormal accruals: abnormal total accruals and abnormal working capital accruals. (The construction of these estimates are detailed in Appendix B.) Including these measures, either separately or together, as additional control variables in our regression specifications (equation (2)) reveal

insignificant coefficients for both abnormal accrual variables, regardless of which other control variables are included in the specification. Importantly, including the abnormal accrual variables has no effect on the estimated investment response.

In sum, our evidence suggests that accounting manipulation to avoid covenant violations is either uncommon or simply has a negligible effect on our results. This conclusion is also consistent with the results from section III.C, which revealed that the investment response to covenant violations is closely related to proxies for information asymmetry and recontracting costs. If accounting manipulation is driving our findings, one would have to argue that this manipulation occurs only in loans with a new lender, a small lending syndicate, firms with low asset tangibility, and firms without a credit rating. Thus, while our evidence may not conclusively reject the accounting literature's debt covenant hypothesis, our evidence does suggest that this behavior is not responsible for the investment response to covenant violations that we identify.

C. Measurement Error in Market-to-Book

An existing concern with investment regressions, such as the one employed in this study (equation (2)), is the effect of measurement error. In particular, if market-to-book is a noisy proxy for Tobin's q (e.g., Erickson and Whited (2000), Gomes (2001), Alti (2003), and Moyen (2004)), then measurement error may contaminate other parameter estimates. While the matching approach partially addresses this concern by relaxing the assumption of a linear association between investment and the market-to-book ratio, we take three additional steps to address this concern.

First, we incorporate higher order polynomials of the market-to-book ratio into the regression specification in equation (2). As Erickson and Whited (2000) note, measurement error in market-to-book, or any other variable for that matter, may result in a non-linear projection of investment on the market-to-book ratio. Alternatively, a nonlinear relation between investment and the market-to-book ratio may simply coincide with a non-quadratic capital adjustment cost function. The results, not presented, indicate that the quadratic term reveals a statistically significant association with investment. However, this association has no effect on the estimated investment response, regardless of which other control variables are included in equation (2).

Second, we examine an alternative empirical proxy for Tobin's q suggested by Salinger and Summers (1983) and Erickson and Whited (2000). This measure, denoted $MacroQ$

for its macroeconomic motivation, is defined as:

$$MacroQ_{it} = \frac{Debt_{it} + Equity_{it} + OtherAssets_{it}}{Capital_{it-1}},$$

where each variable is defined in Appendix A. As indicated by Erickson and Whited (2000), this measure has two advantages over the traditional market-to-book ratio; it lessens heteroscedasticity, and, more importantly, it improves the measurement quality. Our analysis consists of replacing the market-to-book ratio in both equations (2) and (5) with $MacroQ$. The results, not presented, are unchanged from those presented in Tables VI and VII.[24]

Finally, we employ the reverse regression bounds approach of Erickson and Whited (2005) in order to ensure the robustness of the sign of our estimate.[25] We discuss the intuition of their method here, referring the reader to their paper for technical details. The reverse regression bounds method places a lower bound on the correlation between market-to-book (the noisy proxy) and Tobin's q (the true variable) such that for any correlation above that bound, the sign of our estimated effect (covenant violation) is unaffected by the measurement error. Intuitively, a high bound, such as 0.9, suggests that the signal-to-noise ratio between market-to-book and Tobin's q must be very large in order for us to have confidence in the estimated direction of our effect. That is, our proxy must be very good. Alternatively, a low bound, such as 0.1, suggests that even a poorly measured proxy will likely have little impact on the estimated sign of our effect.

The estimated simple squared correlation bound for our experiment ranges from 0.18 to 0.22 depending on the particular assumption set, (b) - (d), from Erickson and Whited (2005) that we employ. Additionally, these estimates are quite accurate, all with standard errors less than 0.05. This result implies that if the simple squared correlation between the market-to-book ratio and Tobin's q exceeds 0.22 then we can be confident that the directional effect of covenant violations on investment is indeed negative.[26] While the true squared correlation between market-to-book and Tobin's q is ultimately unobservable, Erickson and Whited (2005) estimate this statistic to be 0.40, suggesting that the direction of our effect is accurately estimated.

[24] An additional modification to account for measurement error is the debt overhang correction term proposed by Hennessy (2004). Unfortunately, the construction of this measure requires the presence of a credit rating, which the majority of our sample lack.

[25] We thank Toni Whited for the use of her code.

[26] This bound is computed under the assumption that the measurement error is independent of the other regressors in the specification but possibly correlated with the error term. This is the same bound that Erickson and Whited use as a benchmark for their discussion of investment regressions.

V. Conclusion

This paper investigates the importance of financing for investment by estimating the impact of loan covenant violations on investment. We find that covenant violations occur relatively frequently and often carry severe consequences for the future cost of debt capital. Using both a regression discontinuity design and propensity score matching technique, we show that quarterly capital expenditures fall by approximately 1% of capital per quarter in response to covenant violations. This amount corresponds to a relative decline in investment activity of approximately 12% per quarter.

We also show that the investment response accompanying covenant violations appears to be concentrated in situations where information asymmetry is high, renegotiations costs are high, or credit constraints are binding. Specifically, we find significant investment declines accompanying covenant violations only when (1) the violation occurs in the pre-Sarbanes-Oxley era, (2) when there is no historical lending relationship between borrower and creditor, (3) when the loan is underwritten by a single lender, (4) when the firm has a relatively low liquidation value, and (5) when the firm does not have a credit rating. These results not only shed light on the financial frictions behind our findings but also help to ensure that our results are not due to an endogeneity bias.

While shedding light on the real effects of financial contracting, our results raise several other questions. For example, is the decline in investment socially optimal? Our reduced form approach precludes us from addressing this question here but a structural approach may be able to uncover the social welfare implications of covenant violations. Also, what are the macroeconomic implications of covenant violations and do they work to exacerbate business cycles, or, is the decline in investment by violating firms offset by increases in investment by non-violating firms? We leave these questions for future research.

Appendix A: Variable Definitions

All level variables are measured in $ millions and are deflated by the all-urban CPI to December 2000.

Current Ratio is the ratio of current assets to current liabilities.

Net Worth is total assets minus total liabilities.

Tangible Net Worth is current assets plus net physical plant, property, and equipment plus other assets minus total liabilities.

Firm Size is the natural logarithm of total assets deflated by the all-urban CPI.

Market-to-Book is the ratio of the market value of assets to total assets, where the numerator is defined as the sum of market equity, total debt and preferred stock liquidation value less deferred taxes and investment tax credits;

ROA is the ratio of operating income before depreciation to total assets;

Capital / Assets is the ratio of net physical property, plant, and equipment to total assets;

Investment or **Investment / Capital** is the ratio of capital expenditures to the start of period net physical property, plant, and equipment (i.e., Capital);

Cash Flow is ratio of income before extraordinary items plus depreciation and amortization to start of period capital;

Other Assets is other assets from the balance sheet;

Total Accruals is income before extraordinary items less net operating cash flow;

Working Capital Accruals is the sum of the change inventory, the change in accounts receivable, and the change in other current assets less the sum of the change in accounts payable, the change in income tax payable, and the change in other current liabilities;

Debt is total debt from the balance defined as short-term plus long-term debt;

Equity is market capitalization of the firm defined as the number of common shares outstanding times the common share price;

Leverage is the ratio of **Debt** to total assets;

Default Distance is the accounting variable (current ratio or net worth) minus the appropriate covenant threshold.

Covenant Violation is an indicator variable equal to one if the firm is in violation of its covenant and zero otherwise.

Loan Size is the ratio of the loan principal to the total assets of the borrower.

Loan Yield is the promised yield of the loan quoted as a spread above the 6-month Libor.

Loan Maturity is the maturity of the loan measured in months.

# of Financial Cov.s is the number of financial covenants restricting different accounting variables.

Asset (Debt, Equity, Insurance) Sweep is the fraction of the outstanding balance of the loan that must be pre-paid in the event the borrower liquidates specific assets (issues more debt, issues equity, underinsures certain assets). Rather than broad provision, as defined here, the contracts specify specific instances in which the sweep provisions are in violation.

Altman's Z-Score is the sum of 3.3 times pre-tax income, sales, 1.4 times retained earnings, and 1.2 times net working capital all divided by total assets

Appendix B: Construction of Abnormal Accruals

The definition of total and working capital accruals follows that found in Collins and Hribar (2000). Our construction of abnormal accruals is motivated largely by DeFond and Jiambalvo (1994) and is defined as the residual from the following firm-specific regression:

$$Accrual_{t-1} = \alpha + \beta_0 \frac{1}{Assets_{t-1}} + \beta_1 \frac{\Delta OperatingIncome_t}{Assets_{t-1}} + \beta_2 \frac{TangibleAssets_t}{Assets_{t-1}} + \varepsilon_t \quad (7)$$

where Accrual corresponds to either total or working capital accruals. That is, we estimate this regression equation twice for each firm in the sample: once where the dependent variable is total accruals and once where the dependent variable is working capital accruals. To mitigate the effect of outliers, we trim the upper and lower one percentile of each variable's distribution.

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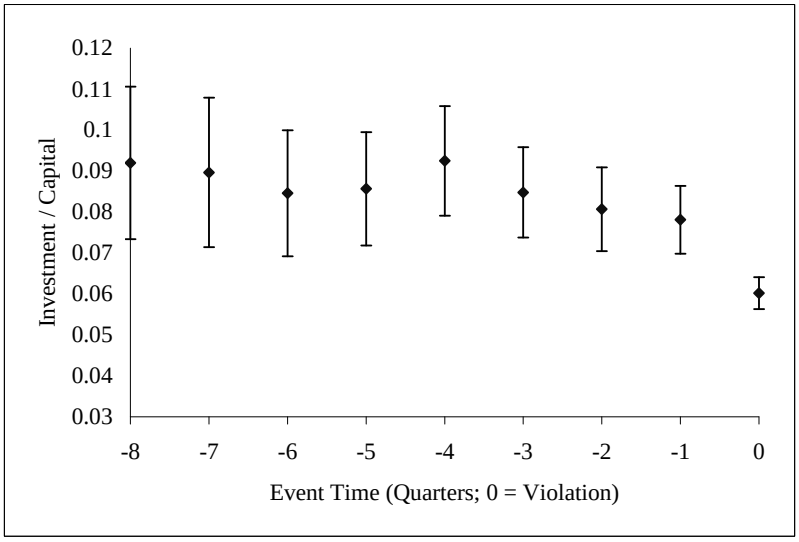
Figure 1
Investment in Event Time Leading Up to Technical Default

The sample consists of all firm-quarters from the merged CRSP/Compustat database in which a covenant restricting the current ratio or net worth of the firm is imposed by a private loan in the Dealscan database during 1994-2005. Financial firms are excluded. The figures present average investment (quarterly capital expenditures divided by the start of period net physical, plant, property and equipment) in event time relative to a covenant violation (event time = 0). The average investment rates are denoted by the diamond in the center of each vertical band, which corresponds to a two standard error interval around the mean.

Panel A: Current Ratio Sample



Panel B: Net Worth Sample

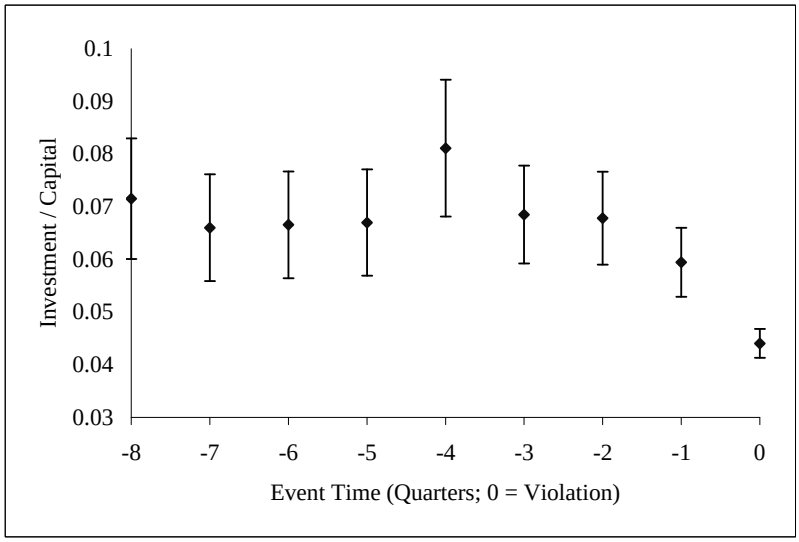
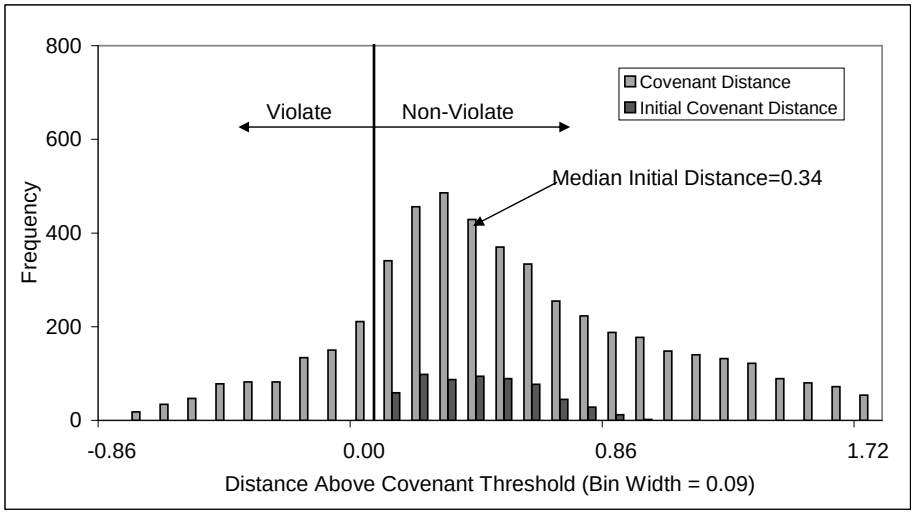


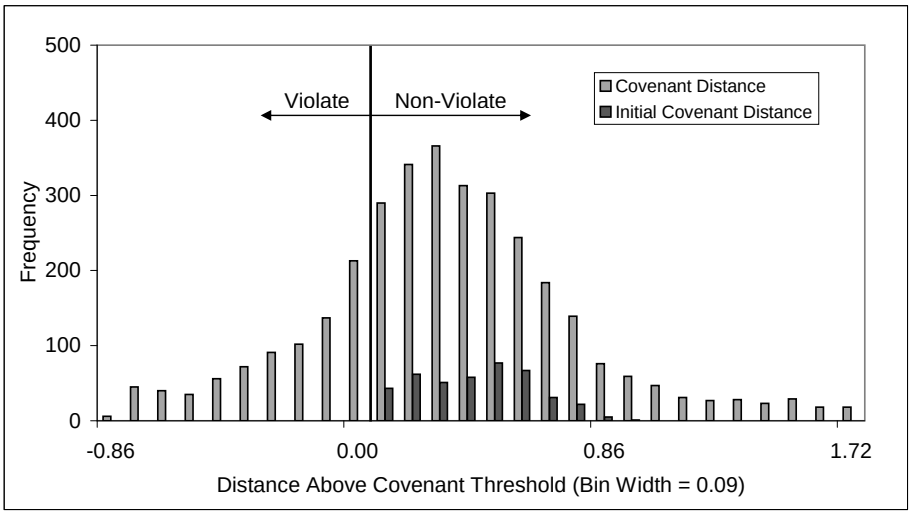
Figure 2
Distribution of the Distance to Covenant Threshold

The sample consists of all firm-quarters from the merged CRSP/Compustat database in which a covenant restricting the current ratio or net worth of the firm is imposed by a private loan in the Dealscan database during 1994-2005. Financial firms are excluded. The figures presents the frequency distribution of the distance to the covenant threshold, defined as $Z_{it} - Z_{it}^0$, where i indexes firms, t indexes time, Z is either the current ratio or net worth accounting variable, and Z^0 is the covenant threshold specified in the loan contract below which the firms is deemed in violation of its covenant. Thus, negative values correspond to observations that are in violation of the covenant. Also presented is the frequency distribution of the initial distance to the covenant threshold at the start of the loan. Panel A (C) presents the distribution for the actual data from the Current Ratio (Net Worth) Sample. Panel B (D) presents the distribution for the simulated data corresponding to the Current Ratio (Net Worth) Sample. To ease the presentation, the 10% right tail of each distribution has been truncated.

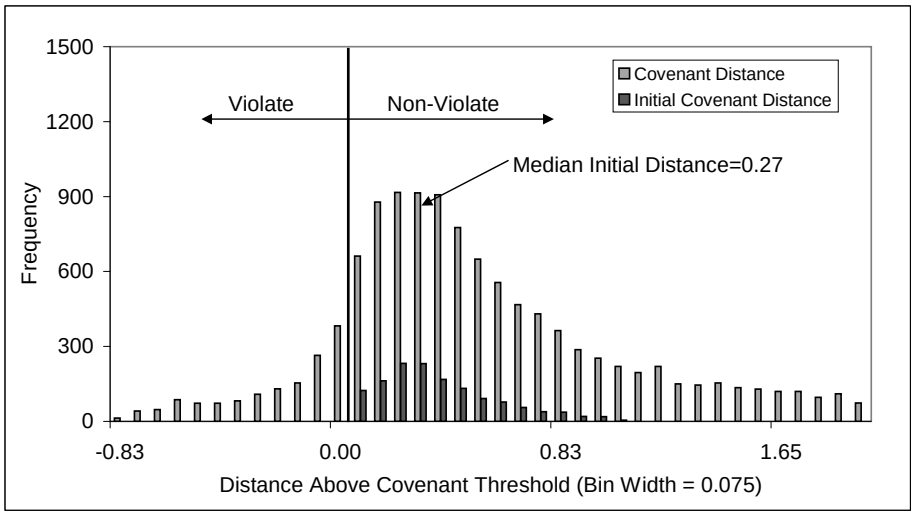
Panel A: Current Ratio Sample (Actual Data)



Panel B: Current Ratio Sample (Simulated Data)



Panel C: Net Worth Sample (Actual Data)



Panel D: Net Worth Sample (Simulated Data)

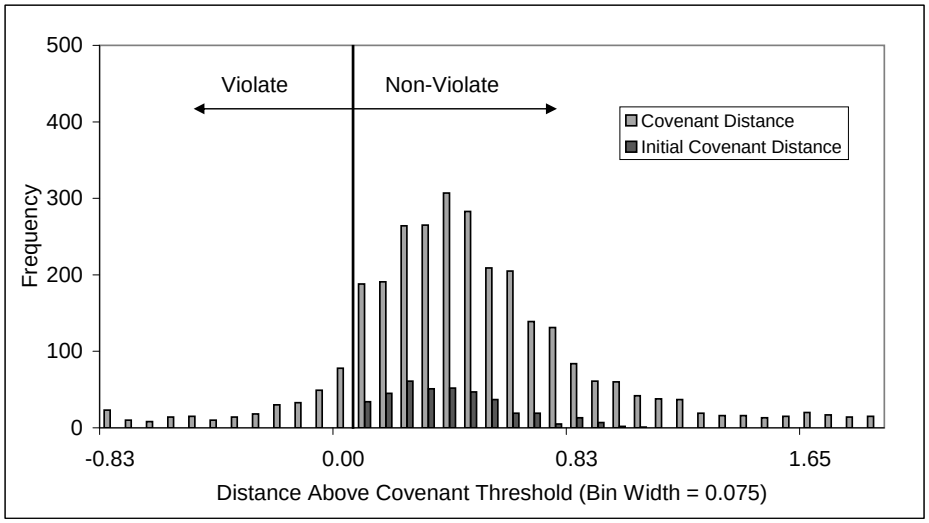


Table I
Covenant Restrictions

The table presents a list of covenant restrictions found in loans to non-financial firms lying in the intersection of Dealscan and the merged CRSP/Compustat files during the period 1994-2005. *Packages* are collections of loans (i.e., tranches) entered into under one collective agreement. *Loan Amount* corresponds to the face value of the loans and are deflated by the all-urban CPI (year 2000).

	Number of	Number of	Loan Amount ($Bil)		
Covenant	Loans	Packages	Average	Median	Total
Max. Debt to EBITDA	7,544	4,417	0.20	0.09	1,480.53
Min. (Tangible) Net Worth	7,196	4,931	0.13	0.03	926.67
Min. Fixed Charge Coverage	6,064	3,514	0.14	0.06	842.95
Min. Interest Coverage	5,856	3,486	0.22	0.10	1,299.70
Max. Leverage ratio	2,401	1,748	0.32	0.15	758.06
Max. Debt to Tangible Net Worth	2,331	1,677	0.06	0.01	137.90
Min. Current Ratio	2,098	1,455	0.06	0.02	126.45
Min. Debt Service Coverage	1,906	1,186	0.07	0.01	126.08
Max. Senior Debt to EBITDA	1,654	857	0.14	0.09	231.34
Min. EBITDA	1,556	886	0.09	0.04	135.32
Min. Quick Ratio	779	555	0.02	0.01	19.16
Min. Cash Interest Coverage	321	165	0.19	0.10	60.70
Max. Debt to Equity	169	119	0.17	0.04	27.95
Max. Senior Leverage	20	10	0.23	0.12	4.54
Max. Loan to Value	19	8	0.05	0.02	0.97

Table II

Firm and Loan Sample Selection Comparison

Panel A presents summary statistics - averages, [medians], and (standard errors) - for three samples of firm-quarter observations. The CRSP/Compustat sample consists of all firm-quarter observations from nonfinancial firms in the intersection of CRSP and quarterly Compustat. The Current Ratio sample consists of all firm-quarter observations from nonfinancial firms in the merged CRSP/Compustat database in which a covenant restricting the current ratio of the firm is imposed by a private loan found in Dealscan during 1994-2005. The Net Worth sample consists of all firm-quarter observations from nonfinancial firms in the merged CRSP/Compustat database in which a covenant restricting the net worth or tangible net worth of the firm is imposed by a private loan found in Dealscan during 1994-2005. Panel B presents summary statistics - averages, [medians], and (standard errors) - for three samples of loan observations. The Dealscan samples corresponds to all Dealscan loans that can be matched to nonfinancial firms in the merged CRSP/Compustat database. The Current Ratio and Net Worth samples are as described above. (Tangible) Net Worth is measured in $ million deflated by the all-urban CPI to December 2000. Variable definitions appear in Appendix A.

Panel A: Firm Characteristics

	CRSP/Compustat		Current Ratio		Net Worth	
Variable	Mean [Median]	(SE)	Mean [Median]	(SE)	Mean [Median]	(SE)
Current Ratio	2.61	(0.00)	2.28	(0.02)	2.35	(0.02)
	[1.77]		[1.94]		[1.84]	
Net Worth ($ Mil)	321.22	(1.28)	156.20	(4.80)	610.13	(15.45)
	[32.35]		[68.62]		[142.72]	
Tangible Net Worth ($ Mil)	117.32	(0.62)	156.16	(4.80)	606.37	(15.47)
	[13.46]		[68.61]		[142.40]	
Firm Size	4.38	(0.00)	5.04	(0.02)	5.67	(0.02)
	[4.35]		[5.06]		[5.65]	
Market-to-Book	2.10	(0.00)	1.44	(0.02)	1.38	(0.01)
	[1.19]		[1.09]		[1.03]	
ROA	0.00	(0.00)	0.03	(0.00)	0.03	(0.00)
	[0.03]		[0.03]		[0.03]	
Capital / Assets	0.31	(0.00)	0.31	(0.00)	0.32	(0.00)
	[0.24]		[0.24]		[0.25]	
Investment / Capital	0.08	(0.00)	0.08	(0.00)	0.07	(0.00)
	[0.05]		[0.05]		[0.05]	
Cash Flow	-0.21	(0.00)	0.09	(0.01)	0.06	(0.00)
	[0.05]		[0.08]		[0.07]	
Leverage	0.28	(0.00)	0.30	(0.00)	0.26	(0.00)
	[0.24]		[0.28]		[0.25]	
Obs	1,122,948		5,527		12,987	

Panel B: Loan Characteristics

	Dealscan		Current Ratio		Net Worth	
Variable	Mean [Median]	(SE)	Mean [Median]	(SE)	Mean [Median]	(SE)
Loan Amount ($Mil)	188.52	(8.95)	55.43	(2.94)	144.92	(8.95)
	[25.00]		[20.00]		[25.00]	
Promised Yield (basis points over LIBOR)	219.81	(3.08)	224.57	(3.87)	208.13	(3.08)
	[200.00]		[200.00]		[200.00]	
Loan Maturity (Months)	46.87	(0.59)	41.68	(0.83)	40.91	(0.59)
	[36.00]		[36.00]		[36.00]	
Number of Loans	37,764		960		2,067	

Table III
Covenant Violations

The table presents summary statistics - averages, [medians], and (standard errors). Panel A presents summary statistics for two samples. The Current Ratio sample consists of all firm-quarter observations from nonfinancial firms in the merged CRSP/Compustat database in which a covenant restricting the current ratio of the firm is imposed by a private loan found in Dealscan during 1994-2005. The Net Worth sample consists of all firm-quarter observations from nonfinancial firms in the merged CRSP/Compustat database in which a covenant restricting the net worth or tangible net worth of the firm is imposed by a private loan found in Dealscan during 1994-2005. *Fraction of Firms (Deals) [Obs] in Violation* is the fraction of firms (Deals) [firm-quarter observations] in each sample which experience a covenant violation. *Relative Initial Covenant Tightness* is the difference between the actual accounting variable (current ratio or net worth) and the initial covenant threshold divided by the initial threshold. *Absolute Initial Covenant Tightness* is the difference between the actual accounting variable (current ratio or net worth) and the initial covenant threshold. *Time to First Violation* is the time under the loan contract until a covenant violation occurs, where time has been normalized to the unit interval to account for variation in loan maturities. *Fraction of Periods in Violation* is the fraction of loan quarters in which the firm is in violation of a covenant, conditional on a violation occurring. *# of Loans Violated* is the number of loans violated by a given firm, conditional on at least one violation. Panels B and C present summary statistics of loan characteristics for firms that enter into multiple deals where at least one deal experiences a covenant violation and there is a subsequent deal. *# of Financial Cov.s* is the number of financial covenants restricting different accounting variables. *Asset (Debt, Equity, Insurance) Sweep* is the fraction of the outstanding balance of the loan that must be pre-paid in the event the borrower liquidates specific assets (issues more debt, repurchases equity, underinsures certain assets). Other variable definitions are provided in Appendix A.

Panel A: Covenant Violation Summary Statistics

	Current Ratio Sample		Net Worth Sample	
Measure	Mean [Median]	(SE)	Mean [Median]	(SE)
Fraction of Firms in Violation	0.38	(0.02)	0.28	(0.01)
	[0.00]		[0.00]	
Fraction of Deals in Violation	0.33	(0.02)	0.22	(0.01)
	[0.00]		[0.00]	
Fraction of Obs in Violation	0.15	(0.00)	0.11	(0.00)
	[0.00]		[0.00]	
Relative Initial Covenant Tightness	0.81	(0.04)	1.04	(0.10)
	[0.50]		[0.39]	
Absolute Initial Covenant Tightness	1.05	(0.06)	200.39	(20.26)
	[0.64]		[24.36]	
Time to First Violation	0.50	(0.02)	0.50	(0.01)
	[0.48]		[0.48]	
Fraction of Periods in Violation	0.27	(0.01)	0.33	(0.01)
	[0.23]		[0.25]	
# of Loans Violated	1.83	(0.08)	1.70	(0.06)
	[2.00]		[2.00]	

Panel B: Implications of Current Ratio Covenant Violations for Future Loan Contracts

	First Loan		Subsequent Loans	
Measure	Mean [Median]	(SE)	Mean [Median]	(SE)
Loan Amount / Assets	0.39	(0.06)	0.29	(0.04)
	[0.33]		[0.24]	
Loan Yield	2.43	(0.20)	3.12	(0.37)
	[2.25]		[2.50]	
Loan Maturity	43.74	(3.13)	44.72	(4.25)
	[37.00]		[36.00]	
# Financial Cov.s	2.77	(0.19)	3.03	(0.24)
	[3.00]		[3.00]	
Asset Sweep	44.44	(12.05)	76.92	(12.16)
	[0.00]		[100.00]	
Debt Sweep	23.53	(10.60)	36.36	(15.21)
	[0.00]		[0.00]	
Equity Sweep	26.47	(10.60)	50.00	(13.76)
	[0.00]		[50.00]	
Insurance Sweep	15.38	(10.42)	22.22	(14.70)
	[0.00]		[0.00]	
Obs (Loans)	30		30	

Panel C: Implications of Net Worth Covenant Violations for Future Loan Contracts

	First Loan		Subsequent Loans	
Measure	Mean [Median]	(SE)	Mean [Median]	(SE)
Loan Amount / Assets	0.23	(0.02)	0.21	(0.02)
	[0.19]		[0.18]	
Loan Yield	2.06	(0.18)	2.57	(0.16)
	[2.25]		[2.55]	
Loan Maturity	46.11	(4.25)	41.10	(3.26)
	[46.00]		[36.00]	
# Financial Cov.s	2.30	(0.17)	2.36	(0.12)
	[2.00]		[2.00]	
Asset Sweep	38.10	(10.86)	90.00	(6.88)
	[0.00]		[100.00]	
Debt Sweep	25.00	(9.93)	52.94	(12.48)
	[0.00]		[100.00]	
Equity Sweep	30.26	(9.86)	56.25	(10.08)
	[0.00]		[50.00]	
Insurance Sweep	17.65	(9.53)	68.42	(10.96)
	[0.00]		[100.00]	
Obs (Loans)	45		49	

Table IV
Firm Characteristics by Covenant Violation

The table presents summary statistics - averages, [medians], and (standard errors) for two samples. The Current Ratio sample consists of all firm-quarter observations from nonfinancial firms in the merged CRSP/Compustat database in which a covenant restricting the current ratio of the firm is imposed by a private loan found in Dealscan during 1994-2005. The Net Worth sample consists of all firm-quarter observations from nonfinancial firms in the merged CRSP/Compustat database in which a covenant restricting the net worth or tangible net worth of the firm is imposed by a private loan found in Dealscan during 1994-2005. These two samples are stratified by whether or not a firm-quarter observation is identified as being in violation (Bind) or not in violation (Slack) of the corresponding covenant. The *Constraint* variables is computed as the difference between the actual accounting variable and the covenant threshold, divided by the threshold. Other variable definitions appear in Appendix A.

	Current Ratio				Net Worth			
	Bind		Slack		Bind		Slack	
Variable	Mean [Median]	(SE)	Mean [Median]	(SE)	Mean [Median]	(SE)	Mean [Median]	(SE)
Constraint	-0.280	(0.007)	0.841	(0.013)	-0.262	(0.006)	1.207	(0.022)
	[-0.231]		[0.547]		[-0.181]		[0.530]	
Current Ratio	1.044	(0.017)	2.503	(0.020)	1.680	(0.032)	2.430	(0.021)
	[0.946]		[2.154]		[1.467]		[1.902]	
Net Worth	140.158	(18.628)	156.449	(4.580)	185.976	(13.350)	654.869	(17.169)
	[27.202]		[73.828]		[44.194]		[159.550]	
Tangible Net Worth	139.870	(18.648)	156.449	(4.580)	185.802	(13.359)	651.708	(17.207)
	[27.147]		[73.828]		[44.132]		[158.923]	
Firm Size	4.865	(0.052)	5.065	(0.020)	4.938	(0.045)	5.757	(0.016)
	[4.884]		[5.088]		[4.811]		[5.724]	
Market-to-Book	1.113	(0.026)	1.498	(0.018)	1.046	(0.026)	1.420	(0.012)
	[0.929]		[1.129]		[0.800]		[1.056]	
ROA	0.016	(0.002)	0.034	(0.001)	0.009	(0.001)	0.030	(0.000)
	[0.026]		[0.035]		[0.018]		[0.033]	
Capital / Assets	0.386	(0.010)	0.299	(0.003)	0.325	(0.006)	0.316	(0.002)
	[0.336]		[0.234]		[0.271]		[0.247]	
Investment / Capital	0.060	(0.003)	0.081	(0.001)	0.044	(0.002)	0.071	(0.001)
	[0.033]		[0.056]		[0.026]		[0.048]	
Cash Flow	-0.074	(0.017)	0.113	(0.005)	-0.100	(0.013)	0.079	(0.003)
	[0.028]		[0.090]		[0.018]		[0.074]	
Leverage	0.420	(0.008)	0.276	(0.003)	0.340	(0.005)	0.246	(0.002)
	[0.404]		[0.257]		[0.346]		[0.243]	
Obs	826		4,634		1,426		11,321	

Table V

Averages by Constraint Tightness

The table presents averages and 95% confidence intervals for two samples. The Current Ratio sample (Panel A) consists of all firm-quarter observations from nonfinancial firms in the merged CRSP/Compustat database in which a covenant restricting the current ratio of the firm is imposed by a private loan found in Dealscan during 1994-2005. The Net Worth sample (Panel B) consists of all firm-quarter observations from nonfinancial firms in the merged CRSP/Compustat database in which a covenant restricting the net worth or tangible net worth of the firm is imposed by a private loan found in Dealscan during 1994-2005. Each sample is split into two groups conditional on whether a firm-quarter observation is in violation of a covenant. Within these two groups, firms are sorted into four portfolios constructed from quartiles of the distance each firm is from its covenant threshold (*Constraint*). The *Constraint* variable is computed as the difference between the actual accounting variable and the covenant threshold, divided by the threshold. Other variable definitions appear in Appendix A.

Panel A: Portfolios Formed on the Distance from the Minimum Current Ratio Covenant

	Violation				Non-Violation			
Variable	Low	2	3	4	5	6	7	High
Constraint	-0.590	-0.324	-0.159	-0.046	0.142	0.397	0.798	2.027
Investment +2 SE	0.052	0.069	0.077	0.083	0.084	0.080	0.086	0.094
Investment	0.044	0.058	0.067	0.071	0.079	0.076	0.081	0.088
Investment -2 SE	0.036	0.047	0.057	0.059	0.075	0.071	0.077	0.083
Obs	206	207	207	206	1,158	1,159	1,159	1,158

Panel B: Portfolios Formed on the Distance from the Minimum Net Worth Covenant

	Violation				Non-Violation			
Variable	Low	2	3	4	5	6	7	High
Constraint	-0.608	-0.290	-0.118	-0.033	0.139	0.385	0.793	3.513
Investment +2 SE	0.042	0.044	0.057	0.057	0.061	0.066	0.074	0.093
Investment	0.036	0.039	0.050	0.050	0.059	0.064	0.071	0.089
Investment -2 SE	0.030	0.034	0.043	0.044	0.056	0.062	0.069	0.086
Obs	356	357	357	356	2,830	2,830	2,831	2,830

Table VI
Investment Regressions

The table presents regression results for a sample of all firm-quarter observations from nonfinancial firms in the merged CRSP/Compustat database 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. The dependent variable in each regression is investment (the ratio of quarterly capital expenditures to capital at the start of the period). All dependent variables are lagged one quarter, except for *Cash Flow* which is contemporaneous with investment. Panel A presents the results for the entire sample. Panel B presents the results for the discontinuity 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 t-statistics (presented in parentheses) are robust to within firm correlation and heteroscedasticity. Variable definitions appear in Appendix A.

Panel A: Entire Sample

	Specification						
Coefficient	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Intercept	0.019	0.058	0.022	0.027	0.046	0.084	0.088
	(3.13)	(11.05)	(0.81)	(0.96)	(1.66)	(2.61)	(2.63)
Covenant Violation	.	-0.014	-0.012	-0.011	-0.009	-0.009	-0.006
	(.)	(-6.03)	(-5.21)	(-4.36)	(-3.79)	(-3.91)	(-2.37)
Market-to-Book	0.020	.	0.019	0.018	0.018	0.017	0.017
	(7.65)	(.)	(7.13)	(6.45)	(6.85)	(6.88)	(6.48)
Cash Flow	0.009	.	0.010	0.008	0.009	0.009	0.009
	(1.70)	(.)	(1.91)	(1.74)	(1.74)	(1.78)	(1.78)
Firm Size	.	.	0.001	0.000	0.001	-0.009	-0.009
	(.)	(.)	(0.29)	(0.12)	(0.33)	(-1.92)	(-1.99)
Lag Cash Flow	.	.	.	0.012	.	.	.
	(.)	(.)	(.)	(2.19)	(.)	(.)	(.)
Leverage	.	.	.	.	-0.060	.	.
	(.)	(.)	(.)	(.)	(-3.89)	(.)	(.)
Altman's Z-Score	.	.	.	.	.	0.012	0.011
	(.)	(.)	(.)	(.)	(.)	(5.88)	(5.23)
CR Default Distance	.	.	.	.	.	.	0.008
	(.)	(.)	(.)	(.)	(.)	(.)	(1.96)
CR (Default Distance)^2	.	.	.	.	.	.	-0.001
	(.)	(.)	(.)	(.)	(.)	(.)	(-1.06)
NW Default Distance	.	.	.	.	.	.	0.000
	(.)	(.)	(.)	(.)	(.)	(.)	(3.05)
NW (Default Distance)^2	.	.	.	.	.	.	0.000
	(.)	(.)	(.)	(.)	(.)	(.)	(3.65)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs	4,839	5,833	4,553	4,403	4,523	4,378	4,375
R^2	0.451	0.440	0.474	0.478	0.480	0.480	0.483

Panel B: Discontinuity Sample

	Specification						
Coefficient	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Intercept	0.027	0.058	0.030	0.037	0.028	0.061	0.042
	(4.50)	(13.37)	(0.89)	(1.07)	(0.98)	(1.59)	(1.03)
Covenant Violation	.	-0.009	-0.012	-0.011	-0.011	-0.012	-0.006
	(.)	(-2.61)	(-3.81)	(-3.22)	(-3.38)	(-3.69)	(-2.02)
Market-to-Book	0.023	.	0.020	0.018	0.019	0.018	0.017
	(5.88)	(.)	(5.54)	(4.97)	(5.45)	(4.95)	(4.83)
Cash Flow	0.014	.	0.018	0.019	0.019	0.021	0.020
	(1.46)	(.)	(1.71)	(1.78)	(1.84)	(1.93)	(1.82)
Firm Size	.	.	0.001	-0.001	0.008	-0.012	-0.007
	(.)	(.)	(0.17)	(-0.06)	(1.21)	(-1.21)	(-0.66)
Lag Cash Flow	.	.	.	0.021	.	.	.
	(.)	(.)	(.)	(2.14)	(.)	(.)	(.)
Leverage	.	.	.	.	-0.142	.	.
	(.)	(.)	(.)	(.)	(-4.15)	(.)	(.)
Altman's Z-Score	.	.	.	.	.	0.017	0.015
	(.)	(.)	(.)	(.)	(.)	(4.01)	(3.52)
CR Default Distance	.	.	.	.	.	.	0.046
	(.)	(.)	(.)	(.)	(.)	(.)	(2.87)
CR (Default Distance)^2	.	.	.	.	.	.	-0.017
	(.)	(.)	(.)	(.)	(.)	(.)	(-2.09)
NW Default Distance	.	.	.	.	.	.	0.000
	(.)	(.)	(.)	(.)	(.)	(.)	(1.28)
NW (Default Distance)^2	.	.	.	.	.	.	0.000
	(.)	(.)	(.)	(.)	(.)	(.)	(1.03)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs	1,946	2,320	1,837	1,790	1,829	1,754	1,751
R^2	0.566	0.537	0.588	0.594	0.605	0.586	0.592

Table VII

Investment Regressions by Ex-Ante Measures of Contracting Costs

The table presents firm fixed-effect investment regression results using a sample of all firm-quarter observations from nonfinancial firms in the merged CRSP/Compustat database 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. The dependent variable in each regression is investment (the ratio of quarterly capital expenditures to capital at the start of the period) and the general specification is:

$$Investment_{it} = \alpha_0 + \Gamma_0 I_{(\omega)} X_{it-1} + \Gamma_1 (1 - I_{(\omega)}) X_{it-1} + \eta_i + \nu_t + \varepsilon_{it},$$

where $I_{(\omega)}$ is an indicator function equal to one if ω is true and zero otherwise, X is a vector of lagged covariates (except for cash flow) including: an indicator variable equal to one if the observation is in violation of a covenant, cash flow, market-to-book, the natural logarithm of total assets, and Altman's Z-Score. Thus, the regression specification interacts an indicator variable corresponding to different proxies for information asymmetry and renegotiations costs with every right hand-side variable, X. Our indicator proxies include: whether an observation occurred before (Pre-SOX) or after (Post-SOX) the passage of the Sarbanes-Oxley Act in the summer of 2002, whether the borrower has had no (None) or more than two (Many) previous lending relationships with a lender in the current lending syndicate, whether the lending syndicate consists of one (Small) or more than five (Large) lenders, whether the ratio of fixed assets to total assets prior to the start of the loan is in the lower (Low) or upper (High) third of the distribution of asset tangibility, and whether size of the loan relative to the total assets of the firm is less than 10% (Small) or greater than 40% (Large). For presentation purposes, we present only the coefficient estimates corresponding to the covenant violation indicator variable. Also reported is a t-statistic of the difference between the two reported coefficients. All t-statistics (presented in parentheses) are robust to within firm correlation and heteroscedasticity. Panel A presents the results for the entire sample. Panel B presents the results for the discontinuity 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. Variable definitions appear in Appendix A. The stratifying variables, except for the Sarbanes-Oxley indicator, are measured prior to the start of the loan.

Panel A: Entire Sample

Sarbanes-Oxley		Lending Relations		Syndicate Size		Asset Tangibility		Credit Rating		Loan Size	
Pre-SOX	-0.014	None	-0.014	Small	-0.020	Low	-0.023	No	-0.013	Small	-0.008
	(-5.336)		(-4.238)		(-4.941)		(-4.203)		(-5.221)		(-1.679)
Post-SOX	-0.006	Many	0.004	Large	-0.004	High	-0.009	Yes	-0.004	Large	-0.012
	(-1.931)		(0.559)		(-1.523)		(-2.732)		(-1.006)		(-3.084)
T-Dif	2.388	T-Dif	2.372	T-Dif	3.052	T-Dif	2.237	T-Dif	2.299	T-Dif	-0.544
Obs	5,198	Obs	3,498	Obs	3,782	Obs	3,424	Obs	5,198	Obs	2,321

Panel B: Discontinuity Sample

Sarbanes-Oxley		Lending Relations		Syndicate Size		Asset Tangibility		Credit Rating		Loan Size	
Pre-SOX	-0.016	None	-0.017	Small	-0.024	Low	-0.019	No	-0.016	Small	-0.011
	(-4.341)		(-4.173)		(-4.375)		(-2.452)		(-4.336)		(-1.899)
Post-SOX	-0.002	Many	0.008	Large	0.002	High	-0.008	Yes	0.001	Large	-0.013
	(-0.465)		(1.139)		(0.558)		(-1.868)		(0.204)		(-2.399)
T-Dif	2.400	T-Dif	2.981	T-Dif	4.111	T-Dif	1.314	T-Dif	3.122	T-Dif	-0.265
Obs	2,154	Obs	1,465	Obs	1,528	Obs	1,455	Obs	2,154	Obs	982

Table VIII
Matching Estimation Results

The table presents two sets of logit estimation results from a matching procedure using the sample of all firm-quarter observations from nonfinancial firms in the merged CRSP/Compustat database 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. The Pre-Match column presents the results of a logit regression of an indicator variable equal to one if the observation is in violation of a covenant and zero otherwise. The Non-Violation observations are drawn from firms that do not experience a covenant violation. The Post-Match column presents the results of a similar logit regression on the subsample of observations for which the difference in the estimated propensity score (i.e., the predicted probability from the Pre-Match logit) is less than 0.10 to ensure a close match. Because of this restriction, 9% of the violating observations are not matched. Included in all logit regressions in both matching procedures but not presented are year fixed effects. Variable definitions are provided in Appendix A.

Parameter	Pre-Match	Post-Match
Intercept	-2.169	0.220
	(-0.158)	(0.440)
Market-to-Book	-0.588	-0.080
	(-11.518)	(-0.698)
Cash Flow	-0.764	-0.060
	(-8.922)	(-0.379)
Firm Size	-0.185	0.057
	(-9.306)	(0.920)
Debt / Assets	3.688	-0.202
	(21.885)	(-0.434)
Altman's Z-Score	-0.482	-0.036
	(-16.662)	(-0.545)
Violation	1,628	1,475
Non-Violation	10,344	1,475
Obs	11,972	2,950

Table IX

Matched Sample Difference-In-Difference Investment Regressions

The table presents estimated investment regressions using a propensity score-matched sample of firm-quarter observations drawn from nonfinancial firms in the merged CRSP/Compustat database 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. The matching is performed on cash flow, market-to-book, the natural logarithm of total assets, book leverage, Altman's Z-Score, and year indicator variables. (Variable definitions appear in Appendix A.) The first column presents the results of regressing investment on an indicator variable equal to one if the observation is in violation of a covenant. The remaining columns present the results from a difference-in-difference regression of investment on the covenant violation indicator, an indicator, $I_{(\omega)}$, equal to one when ω is true and an interaction term between the covenant violation indicator and $I_{(\omega)}$, where $I_{(\omega)}$ corresponds to various binary proxies for information asymmetry and renegotiations costs: whether an observation occurred before (Pre-SOX) or after (Post-SOX) the passage of the Sarbanes-Oxley Act in the summer of 2002, whether the borrower has had no (None) or more than two (Many) previous lending relationships with a lender in the current lending syndicate, whether the lending syndicate consists of one (Small) or more than five (Large) lenders, whether the ratio of fixed assets to total assets prior to the start of the loan is in the lower (Low) or upper (High) third of the distribution of asset tangibility, and whether size of the loan relative to the total assets of the firm is less than 10% (Small) or greater than 40% (Large). For example, firms with no previous lending relationships that also violate a covenant invest 1.9% less on average than firms that violate a covenant but have few lending relationships. Or, firms with loans from small lending syndicates that also violate a covenant invest 1.2% less on average than firms that violate a covenant in a loan from a large lending syndicate. All t-statistics (presented in parentheses) are robust to within firm correlation and heteroscedasticity. The stratifying variables, except for the Sarbanes-Oxley indicator, are measured prior to the start of the loan.

Parameter	Treatment Effect	Sarbanes Oxley (ω =Pre)	Lending Relations (ω =None)	Syndicate Size (ω =Small)	Asset Tangibility (ω =Low)	Credit Rating (ω =No)	Loan Size (ω =Small)
Intercept	0.060	0.064	0.064	0.066	0.076	0.062	0.062
	(30.945)	(27.575)	(24.585)	(21.448)	(21.462)	(28.949)	(18.903)
Covenant Violation	-0.008	-0.011	0.008	0.002	0.003	0.007	-0.026
	(-2.379)	(-2.332)	(0.791)	(0.301)	(0.557)	(0.904)	(-2.890)
$I_{(\omega)}$	.	-0.014	-0.023	-0.021	-0.029	-0.019	0.001
	(.)	(-4.116)	(-3.893)	(-5.127)	(-6.353)	(-4.978)	(0.173)
Covenant Violation $\times$ $I_{(\omega)}$	.	-0.005	-0.019	-0.012	-0.017	-0.017	0.019
	(.)	(0.807)	(-1.686)	(-1.640)	(-1.988)	(-2.090)	(1.818)
Obs	3,941	3,939	2,693	2,952	2,568	3,939	1,722