

## **Large-sample evidence on the debt covenant hypothesis\***

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## Large-sample evidence on the debt covenant hypothesis

### Abstract

We use *Dealscan*, a database of private corporate lending agreements, to provide large-sample tests of the debt covenant hypothesis. *Dealscan* offers several advantages over the data available in previous debt covenant studies, principally through much larger sample sizes, more representative samples, and the availability of extensive actual covenant detail. These data advantages allow us to construct powerful tests, in which we find clear support for the debt covenant hypothesis. Apart from direct tests of the debt covenant hypothesis, we exploit these data to provide broad evidence on the economic role of debt covenants. Specifically, we find that private lenders use debt covenants as “trip wires” for borrowers, that private debt covenants are set tightly, and that technical violations occur relatively often, in about 30% of all loans. We also find that violations are not necessarily associated with financial distress, consistent with the idea that the consequences of violation vary considerably depending on the borrowers’ economic circumstances, and that violations are often waived for healthy firms. Finally, since we measure covenant slack directly, we report evidence that the extensively-used leverage variable is a relatively poor proxy for closeness to covenants.

## 1. Introduction

Much accounting research examines the role of debt covenants in the context of the contracts written between lenders and corporate borrowers. There are two strands to this literature. First, a good deal of empirical research investigates the validity of the ‘debt covenant’ hypothesis – the idea that managers make accounting choices that reduce the likelihood their firms will violate accounting-based debt covenants. Despite the large number of studies in this area, the evidence on this hypothesis remains largely mixed, as discussed further below. Second, a number of studies investigate the causes and consequences of debt covenant violations.<sup>1</sup> The impression one gets from these studies, collectively, is that debt covenant violations are rare events but when they do occur the costs to borrowers are significant. Overall, however, with the exception of early work by Leftwich (1983), we know little about the general process through which accounting-based covenants in private debt agreements are set and enforced.

Our paper uses *Dealscan*, a database of private lending agreements, to shed new light on the contractual role of accounting-based debt covenants in resolving the conflicts of interest that arise between lenders and corporate borrowers.<sup>2</sup> *Dealscan* offers several advantages over the samples and data available in earlier accounting research papers. First, *Dealscan* allows us to construct precise measures of closeness to debt covenants in private debt agreements. Given the lower renegotiation costs of these agreements, debt covenants are optimally set more tightly in private lending agreements than in public debt agreements (Smith and Warner 1979), so covenants in private debt agreements are more likely to affect managers’ financial reporting decisions. However, the terms of private lending agreements are often unobservable. As a

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<sup>1</sup> On the first point see, for example, Healy and Palepu (1990), DeFond and Jiambalvo (1994), DeAngelo et al. (1994), and Sweeney (1994). On the second point, see for example Beneish and Press (1993, 1995) and Chen and Wei (1993).

<sup>2</sup> Dealscan is a database provided commercially by Loan Pricing Corporation described in more detail below.

result, researchers use proxies for closeness to debt covenants, for example, researchers sometimes assume that higher leverage is associated with closeness to debt constraints. Such proxies contain measurement error (e.g., Mohrman, 1993) and are subject to interpretational difficulties (e.g., Ball and Foster 1982, Leftwich, 1990). *Dealscan* provides actual and specific information on the terms of lending agreements, so we use *actual* covenant “slack,” avoiding the need for proxy variables. In addition, we use our data to provide direct evidence on the relation between covenant slack and firm leverage, which is of interest in evaluating the validity of the leverage proxy.

Second, much of the current literature examines the accounting choices of managers of firms that end up in technical default (e.g., DeAngelo et al., 1994; DeFond and Jiambalvo, 1994; Sweeney, 1994). As the authors of these studies recognize, such evidence is by its nature one-sided since it only sheds light on what later prove to be unsuccessful attempts to avoid covenant violations. Our use of *Dealscan* avoids the need to sample *ex-post* based on realized violations. Third, comprehensive samples of private debt agreements with debt covenant detail are difficult to obtain. As a result, extant studies typically use relatively small samples of violations (often less than 100 firms) for firms in distress. *Dealscan* contains data on thousands of commercial loans issued by banks in the U.S., and allows us to generate samples that are much larger and more representative than those available in previous research. (Our current ratio sample contains over 1,300 loans, yielding over 13,000 loan-quarter observations and our net worth sample contains over 280 loans, yielding over 2,300 loan-quarter observations.) Fourth, our large samples allow us to avoid measures of accounting choice such as discretionary accruals models which are generally of low power (Dechow, Sloan, and Sweeney, 1995). Instead, similar to Burgstahler and Dichev (1997), we exploit the large sample size by reporting histograms of the

differences between firms' reported accounting measures and the relevant covenant thresholds (i.e., "covenant slack"), and so can provide direct and clear evidence of management.

Our principal findings relate to the debt covenant hypothesis: we find an unusually small number of loan/quarters with financial measures just below covenant thresholds and an unusually large number of loan/quarters with financial measures at or just above covenant thresholds. These effects are not attributable to how loan covenants are set and are more pronounced before initial violations. Overall, these findings offer strong support for the debt covenant hypothesis.

We also document several new regularities related to contracting in private lending agreements that are consistent with economic intuition. We find that covenants in private lending agreements are set tightly relative to the variation in the underlying variables and that violations are common, occurring in approximately 30% of loans. This is consistent with the view that private lenders use covenants as "tripwires" which provide them with an option to step in and take action when circumstances warrant. In addition, we document that the financial circumstances associated with covenant violations are not nearly as dire as those reported in previous studies, consistent with the idea that violations commonly occur for relatively healthy firms, and are not necessarily associated with financial distress.

Finally, we provide direct evidence on the validity of the leverage variable frequently used in the literature as a proxy for closeness to debt covenants. While we do find a correlation between actual covenant slack and leverage, the correlation is small in economic terms, implying that leverage is a relatively poor proxy for covenant slack.

While *Dealscan* offers several advantages over data available in previous studies, several caveats are in order. First, the "distributional" approach that we employ, while providing relatively unambiguous evidence of *management*, does not provide evidence of the *form* of

management. For example, we do not know whether managers achieve the requisite covenant benchmarks through their accounting choices, through “real” actions (such as issuing equity to increase net worth or selling plant and equipment to strengthen liquidity), or through other means.<sup>3</sup> Second, while *Dealscan* provides detailed and precise information about loans at their inception, it provides limited coverage of subsequent changes in the terms of loans, making it difficult to investigate the consequences of debt covenant violations.

The next section of the paper provides a discussion of the economics of the process through which private debt agreements are negotiated, written, and subsequently enforced. This discussion allows us to develop several testable predictions which we then address in our empirical tests. Section 3 provides details of the sample and research design, while section 4 provides the results of our tests. Section 5 provides a summary and conclusions.

## **2. The debt covenant hypothesis and the economics of private lending**

### *2.1 Research on the debt covenant hypothesis*

The debt covenant hypothesis is one of the principal testable implications of positive accounting theory (Watts and Zimmerman 1986, 1990). According to this hypothesis, managers have incentives to make financial reporting decisions that reduce the likelihood that accounting-based covenants in their firms’ debt agreements will be violated. The strength of these incentives depends on the costs of violating the firm’s debt covenants, that is, on the costs of technical default (Smith and Warner 1979, Holthausen and Leftwich 1983). Accounting researchers typically investigate two research questions related to this hypothesis. First, are

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<sup>3</sup> On the other hand, the broad range of actions managers can take to avoid violating (say) the net worth covenant necessitates a broad approach like ours.

managers' accounting choices driven by incentives to avoid the violation of accounting-based debt covenants? Second, how large are the costs of violating the firm's debt covenants?

With respect to the first question, evidence from existing studies of the debt covenant hypothesis is mixed. Using measures of "discretionary" accruals, DeFond and Jiambalvo (1994) find that managers use abnormal accruals to avoid debt covenant constraints. Sweeney (1994) finds that managers of firms in technical default made income-increasing accounting changes in the periods before the violation, consistent with the debt covenant hypothesis. However, Healy and Palepu (1990) and DeAngelo et al. (1994) do not find support for the debt covenant hypothesis.

With respect to the second question, several studies examine the costs associated with debt covenant violations by examining samples of firms that report covenant violations in their annual financial statements (e.g., Chen and Wei 1993, Beneish and Press 1993). As the authors of these studies recognize, these samples are likely to include only the most serious debt covenant violations because the SEC requires firms only to report those violations that have not been "cured" as of the report date (SEC Regulation S-X Rule 4-08). Firms wishing to avoid reporting violations can do so fairly easily through renegotiation with their lenders unless their circumstances are sufficiently serious as to preclude any form of favorable renegotiation. As a result, these studies generate relatively small samples of violations (usually less than 100 firms meet the selection criteria over a period of several years), report relatively large costs of violation (Beneish and Press (1993) report average incremental interest costs of nearly 1% of their firms' market value of equity), and find that a large proportion of violations result in serious consequences rather than waivers (e.g., Chen and Wei find that of their sample of 128 violations, only 57 (44%) receive waivers). Using *Dealscan* we are able to identify all loan violations rather

than just those included in firms' financial statements. Thus, our sample provides clearer evidence on the overall frequency with which debt covenant violations occur, a number not previously reported in the literature.

## *2.2 The Economics of Private Lending*

The economics of the relation between lenders and corporate borrowers makes it unlikely that covenant violations are always, or even often, associated with financial distress and serious consequences for the borrower. As Chen and Wei (1993) and Smith (1993) discuss, covenant violations give lenders the *option* of requiring the immediate repayment of the loan, but lenders have a range of alternatives available to them, and often choose actions that are less drastic than calling the loan. Further, since renegotiation costs are relatively low in private debt agreements, private lenders are likely to optimally set covenant constraints fairly tightly. Smith argues that one stylized strategy for private lenders to follow is to set debt constraints just below the firm's current value.<sup>4</sup> If the firm's operating performance is in line with normal business conditions or better, covenants are not violated and the debt is serviced as normal. If, on the other hand, the firm's operating performance deteriorates, covenants are quickly violated, giving the lender the ability to reassess the loan. The lender then obtains updated information from the firm, including its managers' forecasts about future performance, and decides on an action from its menu of alternatives. If the lender believes that the firm remains a good risk, it resets the constraint, again to just below the current level and maintains its ability to step in at short notice if operating performance deteriorates further. If the firm's performance improves, there is no further violation and the debt is serviced as normal. However, if performance continues to deteriorate,

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<sup>4</sup> An interesting example of this is Lucent which recently negotiated a syndicated loan from a group of banks in the face of financial difficulty. The *Wall Street Journal* reported (2/28/2001 at B6) that the terms of the agreement require Lucent to maintain net worth of \$23 billion, just below its actual (9/30/2001) net worth of \$26.2 billion.



the lender goes through the renegotiation process again, and may eventually get to the point where more drastic alternatives are necessary. Thus, private lenders use debt covenants as an early warning signal to maintain close scrutiny over the performance of the borrower, implying that the information content and consequences of debt covenant violations are likely to vary depending on the borrowers' economic circumstances

For managers of firms in good financial health, violations are likely to be relatively low cost events involving a review of operations by the lender and an agreement to reset covenant terms to correct the violation. However, managers of healthy firms still have incentives to avoid covenant violations. Any review of the firm's operations by outsiders is likely to be costly – in terms of managerial time, the need to generate updated financial reports, and the need for management to explain and justify its forecasts and strategy – and something managers prefer to avoid.<sup>5</sup> Overall, it is likely that even managers of firms with good performance seek to avoid violations, especially if they can do so at relatively low cost.

From the lenders' standpoint, it pays not to "pull the trigger" and call the loan too early. There is competition among banks for good loans, and imposing costs on borrowers when circumstances do not warrant this results in a loss of business.<sup>6</sup> Thus, lenders, in setting covenant terms, must balance the costs of violation (potential loss of the banking customer) against the benefits (the need to identify a problem loan as early as possible, to maximize recoveries).

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This seems "tight" given that Lucent's recent history of losses and forecast restructuring charges of \$4 billion (of which \$1.6 billion had already been booked).

<sup>5</sup> Gilson and Warner (1998) provide evidence that firms switch from (private) bank debt to (public) junk bond financing, not because of deteriorating operating performance, but to free themselves from the tight debt constraints and lender monitoring that bank loans involve. This is consistent with the idea that this form of close monitoring is costly to borrowers even if violations are routinely waived.

<sup>6</sup>Banks often have other business relationships with borrowers that they also risk losing by alienating the customer.

Survey evidence supports the view that while covenant violations are fairly common, serious borrower consequences are not. Gopalakrishnan and Parkash (1995) provide evidence on the consequences of covenant violations from a relatively large scale survey of borrowers (CFOs of Fortune 500 firms) and private lenders (lending officers from large banks and insurance companies). They identify five possible lender responses to covenant violations: (1) termination of the lending agreement, (2) demand for immediate repayment, (3) increased collateral, (4) increased interest rate, (5) imposition of additional covenant constraints, (6) waiver of the violation. Their survey results indicate that a waiver is the most common response, with both borrowers and lenders indicating a medium to high probability of waiver. The imposition of additional constraints is the second most likely outcome. More serious consequences are rare. An increase in collateral is assigned a zero or low probability by both groups. Perhaps most strikingly, they find that both borrowers and lenders assign the lowest likelihood to serious consequences such as termination of the lending agreements or immediate repayment – more than 76% of borrowers and more than 90% of lenders indicate a zero or low probability for these events. Thus, consistent with our analysis, private lenders call loans only in extremely rare circumstances.

The costs and benefits of avoiding covenant violations are *both* likely to be substantially larger for managers of firms in financial difficulty. In terms of costs, once firms' financial performance becomes poor, managers' ability to manage results to avoid debt covenant constraints is likely to be reduced. For example, if economic performance is declining, managers' must make increasingly aggressive accruals choices to increase reported earnings (especially if their accounting "slack" has already been reduced) while real choices are likely to be constrained. In addition, we know from previous research (Chen and Wei 1993; Beneish and

Press 1993) that serious violations often involve the simultaneous violation of multiple covenants, making it more difficult to avoid violation. The common use of cross-default provisions means that if violations occur and circumstances are serious, covenant problems with one loan have implications for many if not all of the firm's loans.<sup>7</sup> Thus, with serious problems, avoiding covenants is difficult. However, because the consequences of violation are also likely to be substantially more serious when the firm is in economic difficulty, the benefits of avoiding violation increase as well. Thus, it seems likely that managers will attempt to manage reported numbers to avoid covenants as their firms' financial performance deteriorates (since the cost of serious violations are relatively high) but that at some point, if financial performance continues to deteriorate, the costs become too large.

Evidence from the literature supports the view that after some point managers no longer have incentives to make accounting decisions that loosen debt covenant constraints. DeFond and Jiambalvo (1994) look at a sample of firms that violate debt covenants. As the authors note, given their sample selection, these are likely to be firms in financial difficulty so that the violations are serious. The authors find that in years before the violation, managers of these firms make income-increasing accounting choices, presumably in an attempt to avoid covenant violations. However, they also find that in the violation year, there is no evidence of positive manipulation (e.g., because of related events such as going concern violations and management changes), most likely because it no longer pays them to do so. DeAngelo et al. (1994) report that for firms with consistent losses, which include firms that violate covenants in private debt agreements, accounting choices are systematically income-*decreasing*, likely because managers of these firms realize that avoiding the covenants has become too costly. These authors argue

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<sup>7</sup> Cross-default provisions typically stipulate that defaults on other debt of the firm (over some dollar threshold) constitute a default for this particular debt issue as well.

that the managers make income-decreasing choices to *acknowledge* their firms' problems, and to enhance their firms' bargaining position when renegotiating with lenders. Moreover, once such serious covenant violations occur, heightened scrutiny by the sophisticated private lenders make it less likely that managers can "fool" these users.<sup>8</sup> Overall, we expect that managers' incentives to avoid debt covenant violations are diminished once an initial violation occurs.

### *2.3 Empirical Predictions*

To summarize, our principal empirical focus is on the debt covenant hypothesis, and most of our tests investigate whether managers' make choices to reduce the likelihood their firms will violate accounting-based debt covenants. In addition, we expect that private lenders use debt covenant violations as early warning signals that give them the option to review and renegotiate debt agreements. Specifically, we expect that accounting-based debt covenants are set relatively tightly, that violations are common, and that the consequences of covenant violations vary according to the economic circumstances of borrowers. Finally, we expect that while managers have fairly strong incentives to avoid initial violations, their incentives weaken considerably after an initial violation, so that multiple violations are common.

## **3. Sample and research design**

### *3.1 The Dealscan population*

Our sample comprises private lending agreements drawn from the December 1999 release of *Dealscan*, assembled and marketed by the Loan Pricing Corporation (LPC). *Dealscan* is a historical database containing data for about 60,000 loans, high-yield bonds, and private placements worldwide. The majority of the data are for U.S. loans, which are the focus of our

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<sup>8</sup> This is in contrast to the situation for healthy firms, where private lenders can likely "see through" managers' choices but optimally choose not to do so. The evidence in Leftwich (1983) makes it clear that private lenders are

analysis. *Dealscan* data start in 1986, and are continually expanded and updated. According to Carey and Hrycay (1999), by 1992 loan agreements in the database comprised between 50 to 75 percent of the value of outstanding loans in the U.S. and by 1995 *Dealscan* covered the large majority of sizable commercial loans. Most loans on *Dealscan* are syndicated, i.e., they are underwritten and financed by a consortium of banks, insurance companies, and other financing entities. A large number of *Dealscan* loans are complex deals that package together different “facilities”, often with different maturities. A typical package might include a one-year line of credit and a longer-maturity term loan. According to LPC, about 60 percent of the loan data are from SEC filings (13Ds, 14Ds, 13Es, 10Ks, 10Qs, 8Ks, and Registration Statements). The rest of the data are obtained from direct research by LPC, mainly from contacts with borrowers, lenders, and the credit industry at large. Non-SEC filings sources of data have become relatively more important in later years.

The data are arranged by loans, and the amount of loan information varies across loans but almost always includes borrower, lender(s), amount of loan, date of loan inception, projected maturity, purpose, and pricing. In many cases, *Dealscan* also provides additional information like collateralization, specific covenants data, conversion dates, sinking fund requirements, and other background information (e.g., that the new loan replaces a particular existing loan). A premium version of *Dealscan*, available to us, includes a product called TearSheets, which offers extensive detail on a subsample of bellwether loans in the *Dealscan* population.

We use the built-in automated search capabilities of *Dealscan*, supplemented by manual search, to identify borrowers and covenant information at loan inception. Because the automated search feature was introduced in 1993 and only accesses loans entered in the database after that date, we obtain from LPC a supplemental full text file of relevant deals in 1993 and before, and

manually search these data as well to maximize the number of observations in our sample. We then match our deals to *Compustat* and use *Compustat* to track the actual realizations of corresponding accounting variables over the life of the loan.

To provide a benchmark for assessing the generalizability of our results, we compare the sample of borrowers on *Dealscan* to the *Compustat* population and outline sample selection in table 1. A total of 34,786 loans are available on *Dealscan* between January 1989 and December 1999. We use borrower ticker symbols to match to *Compustat*. Requiring a ticker reduces the set of available deals considerably, to 14,848 loans, which reflects the large number of borrowers that are government entities, privately-held, or subsidiaries of publicly-held firms.<sup>9</sup> 8,004 of these loans are successfully matched to *Compustat*, with the losses reflecting differences between exchange tickers and *Compustat* coding, foreign borrowers, and the fact that *Compustat* data are sometimes not available during the term of the loan.<sup>10</sup> This sample represents 2,810 unique *Compustat* borrowers and is of similar size to that obtained by Dennis et al. (2000). For this sample of 8,804 loans, 114,330 loan/quarters of data are available from *Compustat*.

Table 2 compares the set of firms in the *Dealscan-Compustat* intersection to the *Compustat* population. The *Dealscan* firms are larger and more highly levered than the *Compustat* population. The median firm in the *Dealscan-Compustat* intersection has total assets of \$370 million and a debt-to-assets ratio of .33, compared to \$83 million and .27 for the median *Compustat* firm. These differences are expected because *Compustat* includes a large number of smaller, newly listed, growth firms that are unlikely to be able to obtain bank debt financing (Smith and Watts 1992, Fama and French 2001).

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<sup>9</sup> That is, we are able to match 42% of the loans on *Dealscan* to *Compustat*, a proportion that is very similar to the 40% that Dennis et al. (2000) report.

<sup>10</sup> For the samples of loans used in our tests, described below, we use other data in the database to manually resolve ticker symbol discrepancies.

### 3.2 The current ratio and net worth covenant samples

Because our tests require details of accounting-based debt covenants, we examine loans with either a (minimum) current ratio covenant or a (minimum) net worth covenant. We choose these covenants for two reasons. First, previous research identifies these covenants as the ones that most commonly lead to technical default: net worth (or tangible net worth) covenants lead to the most frequent violations, followed by current ratio covenants (Beneish and Press 1993; Chen and Wei 1993; Sweeney 1994). Second, for large sample testing, we require covenant measures that are standardized and relatively unambiguous. As shown in the table below, *Dealscan's* search feature includes 12 financial covenants, listed in order of the frequency with which the covenants appear in different loans.

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*Number of loan facilities with certain accounting-based covenants, as identified by Dealscan's automated search feature, ordered by relative frequency:*

Debt to Cash Flow	3,016
Interest Coverage	2,941
Fixed Charge Coverage	2,720
Tangible Net Worth	2,446
Net Worth	1,945
Debt to Tangible Net Worth	1,735
Debt Service Coverage	1,480
Leverage ratio	1,400
Current Ratio	1,374
Senior Debt to Cash Flow	566
Cash Interest Coverage	163
Debt to Equity	144

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The debt to cash flow covenant appears in the most deals. However, a perusal of the details accompanying each loan reveals that this ratio is defined in more than a dozen different ways, precluding large sample tests. For example, "debt" can mean total debt, or funded debt, or

funded debt less cash, while "cash flow" can be cash from operations, EBIT, EBITDA, etc. We encounter similar difficulties with the fixed charge coverage, debt service coverage, and other ratios. This is consistent with Leftwich (1983), who documents that private lenders often make adjustments to GAAP when defining financial statement variables as part of the way that loans are customized to suit the characteristics of different borrowers. For the net worth and current ratio covenants, on the other hand, we see relatively few adjustments or qualifications, facilitating a match to *Compustat* data. In the end, we choose the current ratio and net worth covenants since these ratios are the ones that are both economically important (as evidenced by the frequency of violations) and for which there is least ambiguity in the measures. As discussed below, we require the net worth sample to have *TearSheet* detail because even that covenant has quantitative, loan-specific adjustments.

To construct the current ratio sample (summarized in table 1) we first use the automated search feature in *Dealscan* to identify all U.S. loans with a current ratio covenant, an active date between January 1989 and December 1999, and other necessary details such as name and ticker symbol of borrower, loan inception date, projected maturity, and amount of loan. A total of 1,374 loans meet these criteria. Of this total, 736 loans have tickers on *Dealscan*, 504 of which can be matched directly to *Compustat*. We add deals for which we are able to manually match the firms (*Compustat* sometimes modifies exchange tickers to reflect additional company identifying information<sup>11</sup>), increasing sample size to 805 deals. In addition, because *Dealscan's* automated search feature was only effective after 1993, we obtained from LPC a text file summarizing a special text-string computer-based search of the entire *Dealscan* database for 1993 and earlier years. A manual search of this file yields over five hundred additional loans



that meet our criteria, increasing our final sample size to 1,313 deals representing 971 firms and 13,052 loan/quarter observations.

The net worth sample is constructed in a similar way (again see table 1), subject to the additional constraint that a *TearSheet* is available. The *Dealscan* automated search yields 1,945 loans with net worth covenants (larger than the current ratio sample). However, only 320 of these loans have *TearSheets*, which are necessary to adjust minimum net worth for subsequent "build up," described below. 224 of these loans have tickers on *Dealscan*, 149 of which can be matched directly to *Compustat*. We then: (1) add loans for which we are able to manually match the firms, increasing sample size to 233 loans, and (2) conduct a manual search, further increasing final sample size to 288 loans representing 236 unique borrower firms and 2,339 loan/quarter observations.<sup>12</sup>

We perform two types of adjustments/data checks with respect to covenant detail. First, covenant terms are set at inception to vary over the life of the loan, typically becoming more stringent over time. For example, a current ratio covenant might stipulate a minimum current ratio of 1.5 at loan inception, increasing to 1.8 after the first year, and 2 after the second year. We manually search for such adjustments and adjust the dates, as necessary. In addition, most net worth ratios specify a minimum required net worth at loan inception, but then adjust that minimum as a function of subsequent net income and stock issuances. For example, a covenant may require minimum net worth of \$150 million at inception, with that number subsequently increasing by 50 percent of "cumulative positive net income" and 75 percent of stock issues.

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<sup>11</sup> For example, Compustat adds dots to stock tickers in the Research File, adds dots and letters to tickers of companies with more than one class of stock, etc. For more detail, see the definition of the variable "Stock Ticker Symbol" in the Compustat manual.

<sup>12</sup> 12 loans appear in both the current ratio and net worth samples.

These subsequent adjustments are known collectively as "build up".<sup>13</sup> Since these build-up parameters are usually only available in loan "TearSheets," the net worth sample is restricted to loans with that detail. Our data are manually adjusted to reflect all such adjustments and modifications.

Second, *Dealscan* only provides information about projected loan life at inception. Because loans may be refinanced, rolled over, or otherwise amended or replaced before projected maturity,<sup>14</sup> effective loan life frequently differs from projected loan life. This difference is potentially important because our tests require information about effective loan life, the period during which the covenant is actually binding and affecting managerial behavior. However, our checks reveal that the difference between projected and effective loan life has little effect on results. Specifically, we are able to infer the effective life of about half of our loans because *Dealscan* maintains an Active/Inactive designation for most loans. A loan is flagged as "active" if it is active as of the date that particular version of *Dealscan* was released. Thus, all loans designated as "active" in our version of the database have covenants that are in effect from deal inception through December 1999. We perform all tests both for the full samples and for the subsamples of Active deals only. Since results are similar, we only report the full samples results.<sup>15</sup>

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<sup>13</sup>As in the example, the most common definition of "build up" includes a specified proportion of cumulative positive net income plus a (usually different) specified proportion of stock issuances. Sometimes the "build up" also includes adjustments for repurchases and/or cash dividends, although these are much less common, presumably because they loosen the constraint. The fact that including losses would also loosen the constraint presumably explains why the adjustments only reflect "positive" net income.

<sup>14</sup>Most loans are typically refinanced or amended to increase loan amount, change loan characteristics (e.g., convert a revolver into a term loan), or change loan constraints. Loan amendments that only change the interest rate are fairly rare because most loans today have floating interest rates, typically tied to the prime rate or LIBOR.

<sup>15</sup>We also performed further checks for the Inactive loans of the current ratio sample. For Inactive loans, it is unclear whether the loan has simply expired (projected life equals effective life) or has been replaced or amended (projected life does not equal effective life). We address this difficulty by searching for subsequent loans for the same company. When we are able to identify loans that replace previous loans we use the difference between the date of the inception of the new loan and the inception of the old loan to calculate the effective life of the old loan. In this way, we are able to infer effective loan life for about half of the Inactive loans. Again, sensitivity checks

Another important research design consideration relates to whether our *Dealscan* covenant measure matches with the *Compustat* measure that we use. We choose the current ratio and net worth covenants because our investigation of specific loans from *Dealscan* reveals that these are the ratios about which there is the least ambiguity in terms of variable definitions. From *Compustat*, we define the current ratio as current assets (Quarterly data item, QDI, #40) divided by current liabilities (QDI #49), and net worth as stockholders' equity (QDI #60) adjusted (where appropriate) for cumulative positive net income (cumulative positive QDI #69) and stock issuances (computed for the first fiscal quarter as QDI #84, and for fiscal quarters 2-4 as the change in QDI #84<sup>16</sup>). We then check, for each observation, to ensure that the match between *Dealscan* and *Compustat* is reasonable. For example, cases where there was an immediate violation and/or where the numbers were very different were investigated and reconciled, if necessary by going to the underlying financial statements.

We provide descriptive evidence on our two samples of loans, as well as a comparison to broader populations, in table 2. Most sample loan facilities are short to medium-term obligations, with a median maturity of just under three years (this is very similar to what Dennis et al. 2000 report). Facility size varies depending on the sample -- the median is \$85 million for all *Dealscan* loans matched to *Compustat*, \$23 million for the current ratio sample, and \$250 million for the net worth sample.<sup>17</sup> We expect the net worth sample to include bigger loans (and bigger borrowers) because it is constrained to loans with *Tearsheet* detail, which are the larger

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demonstrate that the tenor of the results remains the same across loans with projected and verified effective maturity.

<sup>16</sup> *Compustat* cumulates these amounts within fiscal years.

<sup>17</sup> Since these are loan facilities, they do not have to be drawn down immediately. Presumably, if the facilities are not drawn down, the covenants which are the focus of our analysis are not legally binding and so not relevant to managers' decisions. However, we have done some analysis which shows that there is a noticeable increase in the level of these firms' funded debt on *Compustat* that coincides with the initial loan quarter, supporting the fact that most facilities are immediately drawn down. Moreover, since lenders usually charge a commitment fee based on unused facilities, it seems unlikely that loan agreements would be negotiated but not utilized.

"bellwether" loans. The loan facilities are economically important for the borrowing firms -- at the median, the facilities represent 20% to 30% of total assets, and around 80% of total funded debt. The borrowing firms are similar to, although larger than, the median *Compustat* firm, with median market-to-book ratios of about 2 and leverage ratios of about 30%. Apart from the differences in size, firms in the current ratio and net worth samples are similar to other borrowers on *Dealscan*.

#### 4. Results

In this section we report preliminary evidence on covenant slack, a variable not previously available to accounting researchers on a large scale (section 4.1), tests of the debt covenant hypothesis (section 4.2), and evidence on the frequency and nature of debt covenant violations (section 4.3).

##### *4.1 Evidence on the relation between loan terms and firm characteristics*

An ongoing and unresolved issue in the empirical literature on the debt covenant hypothesis is the construct validity of firm leverage as a measure of closeness to debt covenants.<sup>18</sup> Our data allow us to address this question directly and so we calculate correlations between leverage and actual covenant slack for loans in the current ratio and net worth samples. The data indicate that leverage, while correlated with closeness to covenants, is a fairly noisy proxy. For the current ratio sample the (Spearman) correlation is -.30 and for the net worth sample the correlation is -.42 (the Pearson correlations are -.22 and -.11 respectively). While statistically significant, neither correlation is very large economically – for example, even the larger correlation of -.42 for the net worth sample implies that leverage accounts for only about 18% of the variation in

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<sup>18</sup> Because so many researchers use leverage to measure closeness to debt covenant constraints, Watts and Zimmerman (1986) refer to this hypothesis as the "debt/equity" hypothesis. For papers that address the construct validity of this measure, see Press and Weintrop (1990), Duke and Hunt (1990), and Mohrman (1993).

actual covenant slack. This suggests that simple variables such as leverage are relatively poor proxies for actual closeness to covenants.<sup>19</sup>

Our sample also allows us to provide evidence on how tightly covenants are set in private loan agreements. One of the predictions from previous papers and section 2 is that private lenders, given relatively low recontracting costs, set covenants tightly to give themselves the option to renegotiate and in this way closely monitor borrowers. There is no direct evidence on this prediction of which we are aware but it is strongly borne out in our data.<sup>20</sup> For the current ratio sample, we find average (median) slack at loan inception of .80 (.52) while for the net worth sample covenant slack represents 3.8% (6.0%) of total assets. These amounts are small relative to actual variation in these variables – the average (median) standard deviation of these measures, based on *quarterly* data, is .53 (.32) for the current ratio and 4.8% (3.2%) for net worth. As table 2 reveals, the average maturity of our loans is about 11 quarters, so it is clear that private lenders set covenants tightly with respect to the possible variation of the relevant ratios over the life of the loan.

Table 3 also documents correlations between firm characteristics, covenant features, and the probability of subsequent covenant violation. For example, efficient contracting implies that lenders consider the expected future variability of the corresponding financial measure in setting covenant “slack.” Other things equal, we expect a positive relation between the standard deviation of covenant variables and covenant “slack” at loan inception.

Panel A of table 3 reports Spearman rank correlations among the variables *Covenant Violation*, *Covenant Slack*, *Loan Maturity*, *Standard Deviation of Current Ratio Realizations*,

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<sup>19</sup>This is consistent with what Mohrman (1983) finds for a small sample of oil and gas firms.

<sup>20</sup>Several papers report that the large majority of technical defaults result from violations of private lending agreements, consistent with the idea that covenants in private lending agreements are set more tightly (e.g., Beneish and Press 1993; DeAngelo et al. 1994; Sweeney 1994).

and *Total Assets* for the current ratio sample.<sup>21</sup> *Covenant Violation* is an indicator variable set equal to 1 if a loan has at least one covenant violation and 0 otherwise.<sup>22</sup> *Standard Deviation of Current Ratio Realizations* is the standard deviation of the current ratio computed using quarterly data over the life of the loan. Other variables are as defined previously. Consistent with economic intuition that larger firms are financially stronger and exhibit less volatility, we find that larger firms have lower standard deviations of their current ratios (correlation  $\rho = -0.30$ ), longer loan maturities ( $\rho = 0.10$ ), and fewer covenant violations ( $\rho = -0.08$ ). There is also a strong negative relation between the probability of covenant violation and covenant slack at loan inception ( $\rho = -0.49$ ), consistent with the basic idea that, on average, tighter covenants lead to more violations.

Consistent with efficient contracting, we find an economically significant positive relation between covenant slack at loan inception and the standard deviation of current ratio realizations ( $\rho = 0.35$ ). This impression is reinforced by two other results. The simple correlation between covenant violation and standard deviation is insignificant and low in magnitude ( $\rho = 0.05$ ), suggesting that firms with high current ratio variability have only somewhat higher chance of default. However, holding covenant slack constant, the *partial* correlation between covenant violation and standard deviation is large ( $\rho = 0.27$ ). In other words, lenders not only rationally build in more slack for firms with more variable current ratios, they seem to build in just enough slack to nearly offset the *ceteris paribus* mechanical relation between the volatility of the measure and the likelihood of violation.

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<sup>21</sup> We report Spearman correlations because of outliers and non-normalities in some of our variables. Results for Pearson correlations are qualitatively similar.

<sup>22</sup> The results are almost identical if we use Number of Violations instead of the dichotomous Covenant Violation.

Panel B offers corresponding results for the net worth sample. Many of the relations discussed in panel A are confirmed here as well. For example, larger firms have lower standard deviations of scaled net worth ( $\rho = -0.32$ ), and covenant violations are negatively related to covenant slack ( $\rho = -0.54$ ). However, we do not find reliable evidence that larger firms have fewer covenant violations, perhaps because there is less size variation in this sample. The evidence about the endogenous choice of covenant slack is consistent with Panel A but somewhat weaker. There is a positive and significant simple correlation between covenant slack and standard deviation in scaled net worth ( $\rho = 0.09$ ), which increases to 0.31 for the partial correlation controlling for endogenously determined covenant slack.

#### *4.2 Tests of the debt covenant hypothesis*

We present our main results in a series of histograms that document the empirical distributions of covenant slack over loan life. Covenant slack is measured as the difference between the actual realization of the appropriate variable for that quarter and the corresponding covenant threshold for that variable and quarter. Negative values signify violation of covenants while nonnegative values signify compliance. If managers are trying to avoid debt covenant violations, we expect to observe unusually few observations just below zero and unusually many observations just above zero.

A question that arises in depicting such empirical distributions is how to choose histogram “bin width.” Choice of bin width needs to balance the conflicting demands of fineness and precision. Fineness demands that bin widths are sufficiently narrow to trace even subtle properties of the distribution, while precision of estimation demands that bin widths are sufficiently wide that idiosyncratic noise is filtered out. This means that bin width should be positively related to the variability in the data and negatively related to sample size. There is no

theory that dictates the correct bin width; all text discussions indicate these choices are based on “rules of thumb.” For example, some suggest using  $BW = 2(IQR)n^{-1/3}$ , where BW signifies bin width, IQR is the sample interquartile range, and n is the number of observations (Silverman 1986, Degeorge, Patel, and Zeckhauser 1999).

We use this general rule adjusted for considerations relevant to our sample. It is easier to detect unusual properties in the distribution (both visually and statistically) when abnormal behavior is confined to the intervals immediately to the left and to the right of zero. Thus, bin widths need to be sufficiently wide that managerial discretion with respect to covenant variables is confined mostly to moving from the bin immediately to the left of zero to the one to the right.<sup>23</sup> To achieve this, we use bin widths of 0.2 for the current ratio sample and .045 for the net worth sample, both of which are approximately twice the widths from the  $2(IQR)n^{-1/3}$  rule. Sensitivity tests using alternative bin widths indicate that our results are not sensitive to these choices.<sup>24</sup>

Figure 1 presents the histogram of the current ratio covenant slack for the full sample of 13,052 loan-quarter observations.<sup>25</sup> The X-axis presents the bins, where bin width is 0.2. Bin 0 contains all observations with current ratio covenant slack in the interval [0, 0.2), bin 1 contains all observations in [0.2, 0.4), bin -1 contains all observations in [-0.2, 0), etc. An examination of figure 1 reveals that the distribution is single-peaked, somewhat skewed to the right, and fairly

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<sup>23</sup> Note that managerial discretion might begin before the company is actually faced with violation. Various forms of managerial discretion might be employed as soon as the firm comes “close” to the covenant threshold, to prevent technical default owing to the variability of normal operations.

<sup>24</sup> The possibility of measurement errors in our covenant slack variables also argues for using wider bin widths. Detailed results using alternative bin-widths are available from the authors upon request.

<sup>25</sup> The empirical distributions have a number of extreme observations resulting in prolonged and “bumpy” tails. In all figures, we truncate tail extremities to concentrate on the main properties of the sample. These truncations are fairly minor. For example, in figure 1 we truncate about 3 percent of all observations. The text and tables report the number of observations *before* these truncations are made.



smooth. There is only one discernable discontinuity in the overall smoothness of the distribution, which occurs in the vicinity of zero.

We use the method of Burgstahler and Dichev (1997) to judge the statistical significance of this discontinuity. Intuitively, this method tests for deviations from smoothness, where under the null hypothesis of no abnormal behavior smoothness means that the expected number of observations in any given bin is equal to the average of the number of observations in the two immediately adjacent bins. The test statistic is defined as the difference between the actual number of observations in any given bin and the expected number of observations, divided by the estimated standard error of the difference.<sup>26</sup> Under the null hypothesis of smoothness, these standardized differences are distributed approximately Normal with a mean of 0 and a standard deviation of 1. We expect that managerial behavior to avoid covenant violation would tend to move observations from bin -1 to bin 0, so we expect to observe standardized differences that are unusually negative for bin -1, and unusually positive for bin 0.<sup>27</sup>

The standardized differences in figure 1 are 8.03 for bin 0, -4.85 for bin -2, and -3.50 for bin -1, suggesting that there are significantly more (less) observations than expected under smoothness in bin 0 (bins -1 and -2). This suggests that managers have successfully moved their firms from the bins just below covenant thresholds to that just above the threshold. The other regularity of interest in figure 1 is the relatively large number of violations. Of the 13,052 loan-quarter observations, 2,204 (17%) are negative, indicating violations. This is consistent with our prediction that violations occur relatively often and are not as unusual as one might expect based

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<sup>26</sup> See Burgstahler and Dichev (1997) for complete estimation details.

<sup>27</sup> Note that managerial actions to move observations from bin -1 to bin 0 affect simultaneously the standardized differences in these two bins. Thus, the standardized differences for these two bins are not independent. Therefore, the standardized differences for bin -1 and bin 0 should not be interpreted as independent tests of the same hypothesis.

on extant research. We defer more detailed discussion of violations to the section below to focus here on the debt covenant hypothesis.

To investigate whether managerial incentives to avoid covenant violations are stronger when there are no previous violations, we split the current ratio sample into a subsample that comprises all observations up to and including a first violation, and a complementary subsample that comprises only observations following an initial violation. The histogram for the first sample is presented in figure 2 and comprises most of the observations in the full sample ( $N = 9,546$ ). There is a clear difference between figures 1 and 2. While the right tail of the histogram in figure 2 looks similar to that in figure 1, the left tail in figure 2 is markedly thinner than that in figure 1. More specifically, covenant violations comprise 17 percent of the observations in figure 1, but only 5 percent of the observations in figure 2. The net result is that the apparent discontinuity around zero is even more pronounced in figure 2. Standardized differences again confirm the statistical significance of the discontinuity. The standardized difference for bin -2 is -6.7, for bin -1 is -12.5, and for bin 0 is 7.4. This indicates that managerial incentives to avoid debt covenant violations are stronger for initial violations.

Figure 3 presents the histogram for the subsample of observations that follow an initial covenant violation. The histogram in figure 3 is starkly different from the distributions in figures 1 and 2. To start with, the whole distribution is more uneven, as expected with fewer observations ( $N = 3,506$ ). However, the most pronounced difference is in the relative number of violations; here violations comprise 49 percent of the observations. In other words, the likelihood of violation is substantially higher after an initial violation.

There is no apparent discontinuity around zero in figure 3, suggesting that managers have little incentive or ability to avoid debt covenant violations after an initial violation. However,

communications with lenders as well as the discussion in section 2 suggests that following an initial violation, covenants may be reset to a new level, generally becoming looser. These covenant changes are supposed to be included as amendments to the original debt agreement. However, we find few explicit covenant amendments on *Dealscan*, so it is possible that some of the observations we classify in bin -1 are actually loan-quarters where borrowers are in compliance with new, lower covenant thresholds.

Overall the evidence in figures 1, 2, and 3 is consistent with the debt covenant hypothesis. However, a potential alternative explanation for the results is that they are attributable to the way that loan covenants are set by lenders *ex ante*, as opposed to reflecting managers' *ex-post* choices. Specifically, if at loan inception private lenders systematically set covenant thresholds just below actual levels, as we argue in section 2, we would *expect* to see a disproportionate number of observations just above covenant thresholds. However, while this explanation may apply for quarters at or just after loan inception, it is unlikely to explain discontinuities in subsequent quarters.<sup>28</sup> Moreover, this argument cannot explain why we observe unusually few observations just below covenant thresholds. Nevertheless, to explore this explanation more fully, we start with the set of observations up to and including a first violation (to abstract from the multiple violation effect of figure 3) and split this sample into those loan-quarters at or near loan inception (the first four loan-quarters for a loan) and the other loan-quarter observations. These results are reported in figures 4 and 5 respectively. It is clear from figure 4 that there are a relatively large number of observations in bin 0 relative to bin -1, supporting the idea that lenders set covenants just below actual values (the standardized

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<sup>28</sup> Since we have approximately ten times as many loan-quarters as loans, most of our observations are for periods other than at loan inception, so this explanation can, at best, only partially explain our results. From table 1, we see that for the current ratio (net worth) sample there are 13,052 (2,339) loan-quarters for 1,313 (288) loans, so that inception quarters account for only 10% (12%) of the total number of observations in our figures.

differences are -7.6 in bin -1 and 7.7 in bin 0). However, it is not the case that the observations are all in bin 0, and there are many observations in bins to the right of bin 0. More important, when we remove those observations corresponding to the first four quarters (see figure 5), we still find strong evidence of a disproportionately large jump between bins -1 and 0, although here bin 1 also seems important. For figure 5, the standardized differences are -10.3 for bin -1, 2.9 for bin 0, and 5.2 for bin 1.

We turn next to the net worth covenant sample. We follow the same order, so that figures 6-10 correspond to their current ratio counterparts in figures 1-5. Figure 6 presents a histogram of net worth covenant slack for all available observations in the net worth sample. As with figure 1, figure 6 shows a single-peaked distribution which is right-skewed and smooth except for a pronounced discontinuity around zero slack. This discontinuity is more pronounced in figure 6 than in figure 1, with the 114 observations in bin -1 more than tripling to 387 in bin 0. Statistical tests confirm this impression: the standardized differences are -7.9 for bin -1 and 5.6 for bin 0.

Figure 7 presents the histogram for net worth observations up to and including an initial covenant violation. The immediate impression from figure 7 is the stark discontinuity between bin -1 and bin 0, perhaps the most pronounced of all of the figures. Bin -1 contains only 46 observations, nearly eight times fewer than the 357 observations in bin 0 (the standardized differences for these bins are large at -12.7 for bin -1 and 6.4 for bin 0). Thus, figure 7 provides strong evidence in support of the debt covenant hypothesis.

Figure 8 contains net worth covenant observations following an initial covenant violation. Due to the small number of observations ( $N = 439$ ), the distribution is uneven. However, the main message of figure 8 is clear: following an initial covenant violation, there is no evidence

that managers attempt to avoid debt covenant violations; here 87 percent of loan-quarters are violations.<sup>29</sup>

The persistence of net worth violations is likely due to the nature of the net worth covenant. Net worth covenant thresholds are typically expressed in dollar amounts, adjusted upwards to reflect “build up.” For a firm to breach a net worth covenant, it has likely suffered substantial losses. Since losses tend to be persistent (e.g., DeAngelo et al. 1994), firms that violate net worth covenants face an uphill battle to avoid subsequent violations (losses shrink net worth, while the covenant threshold is not adjusted downward). If the firm returns to profitability, net worth increases, but the covenant threshold becomes a moving target, typically being adjusted upward by at least 50 percent of net income. Thus, it is not surprising that subsequent violations are more common in the net worth sample than the current ratio sample.

Finally, we again report results for the sample of all loan-quarters up to and including an initial violation, partitioned into loan-quarters within a year of loan inception (figure 9) and loan-quarters after the first year (figure 10). Inconsistent with the idea that covenants are set just below actual values, the bin with the most observations in figure 9 is bin 1 rather than bin 0. More importantly, when we remove the potentially confounding effect of loan inception (see figure 10), the results are clearly consistent with the debt covenant hypothesis, with ten times as many observations in bin 0 than in bin -1, and little difference between bins 0, 1, and 2.

Summarizing, figures 1 through 10 and the related statistical tests comprise clear evidence that: (1) managers systematically make choices which allow their firms to avoid debt covenant violations, (2) managers’ incentives and/or ability to avoid covenant violations are reduced once an initial violation occurs. It is also clear from these figures that violations are

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<sup>29</sup> Given the small sample, we do not calculate standardized differences for figure 8. As with figure 3, in interpreting figure 8 one should keep in mind that, following an initial violation, covenant thresholds are sometimes

relatively frequent, much more so than expected based on previous research. We next provide evidence on the frequency and circumstances of loan violations.

### *4.3 Loan violations*

We argue in section 2 that violations of private lending agreements are not homogeneous, and include both the relatively infrequent, serious violations documented in previous papers (such as Chen and Wei 1993, Beneish and Press 1993, DeFond and Jiambalvo 1994) and the more frequent, “routine” violations that result in less serious borrower consequences. Because our sample includes all violations, we expect to observe that: (1) violations are relatively common, and (2) borrowing firms' financial position and performance are, on average, below normal levels but are not at a level indicative of serious financial distress.

Table 4 reports on the relative frequency of violations. Violations occur in 37% of all loans for the current ratio sample and 30% of all loans for the net worth sample. In addition, most loans with violations have multiple violations; 78% of loans with violations in the current ratio sample and 82% of loans with violations in the net worth sample have multiple violations. Thus, violations are common and often result in multiple violations.<sup>30</sup>

We next turn to the circumstances surrounding violations. In table 5 we compare the financial circumstances of borrowing firms in loan-quarters with and without violations to provide evidence on the financial circumstances associated with violations.

To gauge these firms' financial position and profitability, table 5 reports sample medians for firm size (total assets), leverage, return-on-assets (ROA), loss incidence, change in ROA, interest coverage, and current ratio. Panel A reports results for the current ratio sample and panel

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reset, and our measure of slack does not reflect these adjustments

<sup>30</sup> We again caution that our data do not reflect instances where covenants are renegotiated after violations. If, as we suspect, covenants are sometimes reset to more achievable levels after violations, the numbers in table 4 overstate the frequency of multiple violations.

B reports results for the net worth sample. First, consistent with the view that adverse financial circumstances lead to violations (e.g., Beneish and Press 1993), it is clear that these firms are in worse financial circumstances in violation quarters than in non-violation quarters. Median assets are lower, leverage is higher, and ROA, interest coverage, and the current ratio are all lower for these firms in violation quarters than in non-violation quarters. For example, for the current ratio (net worth) sample, median ROA is 2.4% (2.3%) in non-violation quarters compared to 1.4% (-.6%) in initial violation quarters, and 1.3% (1.5%) in all violation quarters (these numbers are not annualized). For the current ratio (net worth) sample, losses are reported 15% (8%) of the time in all non-violation quarters, 35% (51%) of the time in initial violation quarters, and 33% (26%) of the time in all violation quarters.

The firms in our samples are not in the dire financial straits evident in samples of more serious violations. Beneish and Press (1993) report median ROA of -2.0% for their firms in the year of violation and that 85% of their firms report annual losses. DeFond and Jiambalvo (1994) report a median annual earnings decline for their violation firms of 6% of assets and a loss frequency of 80%. In contrast, as documented above, our firms report median ROA of between -.6% and 1.4% in the initial violation quarter, *positive* ROA of between 1.3% and 1.5% across all violation quarters, and loss proportions of between 35% and 51% in initial violation quarters and between 33% and 26% in all violation quarters, implying frequencies of *annual* losses well below those reported by Beneish and Press (1993) and DeFond and Jiambalvo (1994). Overall, these results indicate that debt covenant violations are associated with declines in financial performance but not necessarily with serious financial distress.

Overall, our results are consistent with the argument from section 2 that technical violations of private lending agreements are fairly common and that often violations do not reflect serious financial difficulty on the part of the borrowing firms.

## **5. Conclusion and suggestions for future research**

We use a large database of private lending agreements to provide direct evidence on the debt covenant hypothesis. We report strong evidence that managers take actions to avoid debt covenant violations: we find an unusually small number of firm/quarters with financial measures just below covenant thresholds and an unusually large number of firm/quarters that just meet or beat covenant thresholds. This effect is especially pronounced before an initial covenant violation, consistent with the notion that initial violations are substantially more costly.

We also find that debt covenant violations are common – about 30 percent of loans have at least one violation. However, for most firms these violations are not associated with financial distress. In addition, we find that debt covenants are set relatively tightly in private lending agreements. Together, these facts suggest that private lenders use debt covenant violations as a screening device, and frequently waive violations or reset covenants without imposing serious consequences on borrowing firms.

Finally, since we can measure covenant slack directly, we also assess the extent to which firm leverage, frequently used in the literature as a proxy for closeness to covenants, actually correlates with this construct. We find that while covenant slack and leverage are correlated in the “right” direction, the magnitude of the correlation is fairly small in economic terms, implying that leverage is a relatively poor proxy for closeness to covenants.



The evidence in this study opens a number of possibilities for future research. For example, since Leftwich (1983), we have known that private lending agreements are customized. However, we know little about the specifics of how lenders pick individual covenants, and package them together in structuring lending agreements. Improving data availability could provide interesting insights into how debt covenant use has evolved over time or is adjusted for differing firm characteristics. Another natural line of inquiry is to extend the analysis offered here to other covenants. It might be also interesting to compare and learn more about settings with different levels of incentives for avoidance of covenant violation.

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**Table 1**  
**Details of Sample Formation Procedures for Two Samples of *Dealscan* Loans with Accounting-Based Debt Covenants: (1) Loans with a Current Ratio Covenant, and (2) Loans with A Net Worth Covenant**

	All <i>Dealscan</i> Loans	Current ratio sample	Net worth sample
Full set of loans on <i>Dealscan</i> with active dates between January 1989 and December 1999 and available data on loan size and maturity	34,786	n.a.	n.a.
Loans obtained using <i>Dealscan's</i> automated search procedure	n.a.	1,374	1,945
Of loans above, those with TearSheet available <sup>1</sup>	n.a.	n.a.	320
Of loans above, those with available ticker symbol for matching to <i>Compustat</i>	14,848	736	224
Of loans above, those successfully matched with <i>Compustat</i> and having <i>Compustat</i> data available	8,004	504	149
Including loans for which tickers are manually matched <sup>2</sup>	n.a.	805	233
Final sample of loans, including loans obtained through manual search of <i>Dealscan</i>	n.a.	1,313	288
Number of borrowing firms represented	2,810	971	236
Number of loan-quarters of available data	114,330	13,052	2,339

1. For a subset of bellwether loans, *Dealscan* provides much greater loan detail in a supplementary product known as a "TearSheet." This detail is needed to compute covenant slack for the net worth sample.
2. *Dealscan's* automated search feature was implemented in 1993, and so does not search loans included in the database prior to this date, so we search manually through the database for those loans not covered by the automated search feature.

**Table 2**  
**Comparative Descriptive Information for All Dealscan Loans Matched to Compustat, the Compustat Population, the Current Ratio Sample, and the Net Worth Sample. Table Reports Sample Medians.**

	<i>Dealscan-Compustat intersection</i>	<i>Compustat</i>	Current ratio sample	Net worth sample
Total Assets (\$m)	370	83	107	786
Leverage, defined as the ratio of total funded debt to total assets	.33	.27	.30	.33
Market-to-book ratio	2.02	1.86	2.01	2.19
Loan maturity, in months	44	n.a.	34	33
Loan amount <sup>1</sup> (\$m)	85	n.a.	23	250
Loan amount divided by total funded debt <sup>1</sup>	.77	n.a.	.78	.83

3. These loans are often bank revolving credit lines which are not necessarily drawn down on these dates. Thus, this ratio can exceed one.

The numbers reported for the Compustat population are based on all available firm-quarters for the period corresponding to the Dealscan samples; i.e., 1989-1999. Funded debt is defined as long-term debt (Compustat QDI #51) plus the debt included in current liabilities (Compustat QDI #45). Market-to-book ratio is calculated as the market value of common equity at quarter end divided by stockholders' equity. Loan maturity is from Dealscan and represents either loan maturity at inception or effective maturity when the latter is available. Loan amount is from Dealscan and represents the size of the loan facility; loans may not be fully "drawn down."

**Table 3**  
**Table Reports Statistics on Covenant Slack at Loan Inception Along with Spearman Rank Correlation Coefficients (p-values in parentheses) Among Selected Loan and Borrower Characteristics for Both the Current Ratio and Net Worth Samples**

**Panel A: Current ratio sample (obs. = 1,313)**

Mean (Median) Covenant Slack at Loan Inception: .80 (.52)

Mean (Median) of Loan Level Standard Deviations of Current Ratio: .53 (.32)

	Covenant Violation	Loan Maturity	Standard Deviation of Current Ratio Realizations	Total Assets
Covenant Slack	-.49 ( <i>&lt;.01</i> )	-.02 (.56)	.35 ( <i>&lt;.01</i> )	-.04 (.11)
Covenant Violation		0.14 ( <i>&lt;.01</i> )	0.05 (.06)	-0.08 ( <i>&lt;0.01</i> )
Loan Maturity			0.11 ( <i>&lt;0.01</i> )	0.10 ( <i>&lt;0.01</i> )
Standard Deviation of Current Ratio Realizations				-0.30 ( <i>&lt;0.01</i> )

Partial correlation, holding Covenant Slack constant.

	Standard Deviation of Current Ratio Realizations
Covenant Violation	0.27 (.001)



**Panel B: Net worth covenant sample of loans (obs. = 288)**

Mean (Median) Covenant Slack at Loan Inception (deflated by assets): .038 (.060)

Mean (Median) of Loan Level Standard Deviations of Net Worth (deflated by assets): .048 (.032)

Spearman rank correlation matrix

	Covenant Violation	Loan Maturity	Standard Deviation of Net Worth to Total Assets	Total Assets
Covenant Slack	-.54 ( <i>&lt;.01</i> )	-.08 (.16)	.09 (.12)	.03 (.57)
Covenant Violation		0.12 (.05)	0.21 ( <i>&lt;.01</i> )	-0.06 (.27)
Loan Maturity			0.19 ( <i>&lt;0.01</i> )	0.06 (.32)
Standard Deviation of Net Worth to Total Assets				-0.32 ( <i>&lt;0.01</i> )

Partial correlation, holding Covenant Slack constant.

	Standard Deviation of Net Worth/Total Assets
Covenant Violation	0.31 ( <i>0.001</i> )

Notes.

Leverage is defined as the ratio of funded debt to total assets, where funded debt is long-term debt (Compustat QDI #51) plus the debt included in current liabilities (Compustat QDI #45). Covenant Violation is an indicator variable set to 1 if the loan has at least one covenant violation and 0 otherwise. Covenant Slack is defined as the actual value of the covenant variable at the end of the loan inception quarter minus the corresponding covenant threshold. Loan Maturity is projected maturity in months, except we use actual maturity, when available. Standard Deviation of Current Ratio Realizations is calculated as the standard deviation of the actual current ratio realizations over the available life of the loan. Standard Deviation of Net Worth/Total Assets is defined analogously. Available life of the loan is since inception until 12/1999 for active loans, actual verified life for inactive loans for which we have additional information, and projected life for all other inactive loans. Total Assets is total assets at loan inception.

**Table 4**  
**Relative Frequency of Loan Violations for the Current Ratio and Net Worth Samples**

**Panel A: Current ratio sample (obs. = 1,313)**

Number of loans with at least one violation (percentage of total number of loans)	489 (37 percent)
Number of loans with repeated violations (percentage of total)	383 (29 percent)
Number of loan-quarters violating covenant (percentage of total)	2,204 (17 percent)

**Panel B: Net worth covenant sample (obs. = 288)**

Number of loans with at least one violation (percentage of total number of loans)	87 (30 percent)
Number of loans with repeated violations (percentage of total)	71 (25 percent)
Number of loan-quarters violating covenant (percentage of total)	472 (20 percent)

**Table 5**  
**Median Values of Selected Measures of Borrowers' Financial Circumstances**  
**for Loan-Quarters in Which Loans Were and Were Not in Technical**  
**Violation of Certain Accounting-Based Debt Covenants**

**Panel A: Current Ratio Sample**

	All non-violation quarters	Initial violation quarters	All violation quarters
Obs.	9,072	443	1,998
Total assets	163	111	122
Leverage (funded debt divided by total assets)	.273	.369	.388
ROA	2.37%	1.42%	1.32%
Percentage with losses (negative ROA)	14.5%	35.4%	32.6%
$\Delta$ ROA	.008%	-.004%	.041%
Current ratio	2.14	1.16	1.07
Interest coverage	3.19	1.53	1.47

See next page for variable definitions.

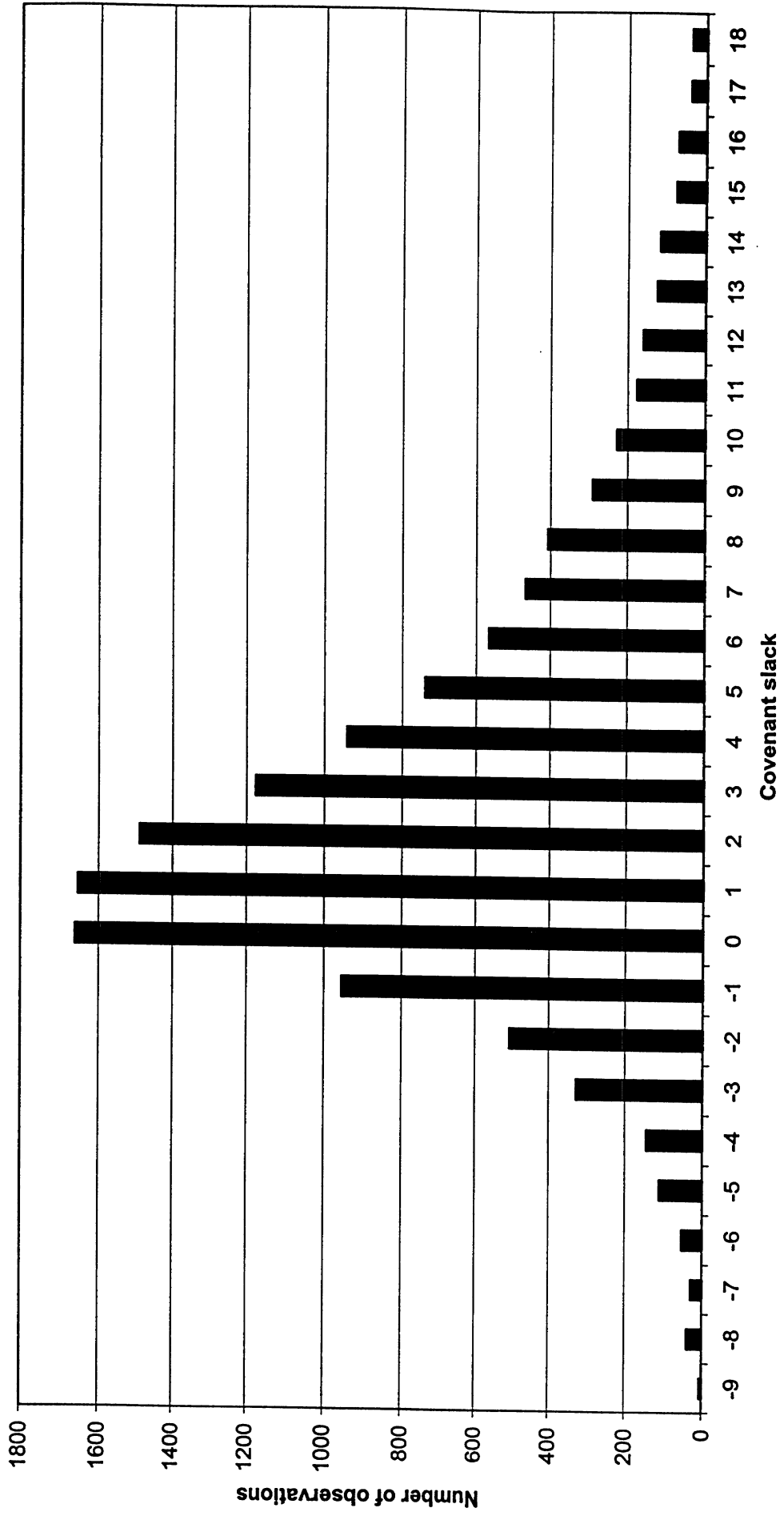
**Table 5**  
**Median Values of Selected Measures of Borrowers' Financial Circumstances**  
**for Loan-Quarters in Which Loans Were and Were Not in Technical**  
**Violation of Certain Accounting-Based Debt Covenants**

**Panel B: Net Worth Sample**

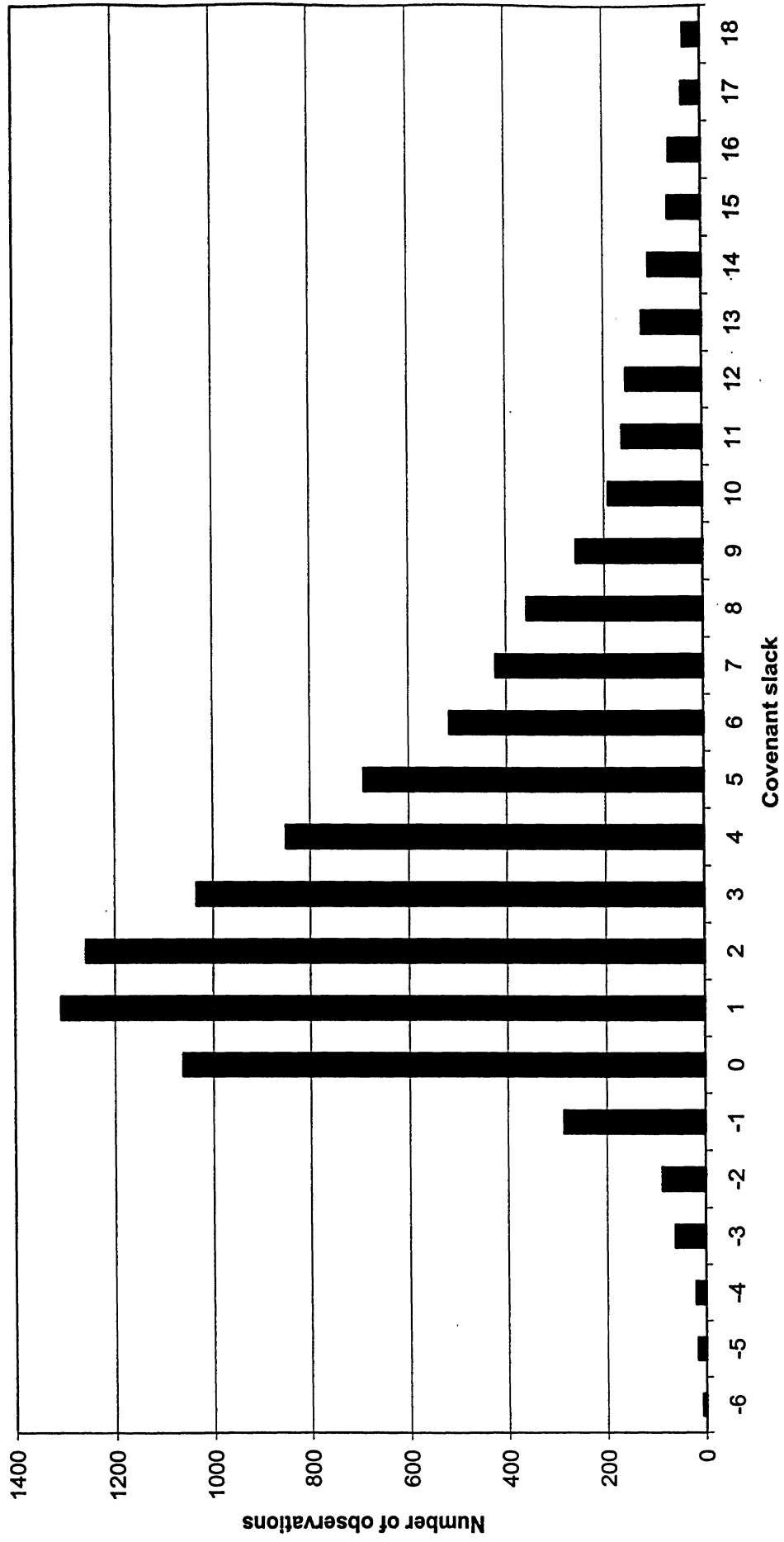
	All non-violation quarters	Initial violation quarters	All violation quarters
Obs.	1,867	83	472
Total assets	1,185	902	931
Leverage (funded debt divided by total assets)	.337	.466	.465
ROA	2.31%	-.63%	1.51%
Percentage with losses (negative ROA)	8.4%	51.3%	25.8%
$\Delta$ ROA	.1%	-1.0%	.1%
Current ratio	1.64	1.39	1.42
Interest coverage	3.8	-.9	1.6

Total assets is Compustat QDI #44. Leverage is defined as the ratio of funded debt to total assets, where funded debt is long-term debt (Compustat QDI #51) plus the debt included in current liabilities (Compustat QDI #45). ROA is return on assets defined as EBIT (Compustat QDI #22 + Compustat QDI #23) divided by total assets.  $\Delta$ ROA is the change in ROA from the previous quarter. The current ratio is current assets (Compustat QDI #40) divided by current liabilities (Compustat QDI #49). Interest coverage is EBIT divided by interest expense (Compustat QDI #22). Non-violation quarters include all loan-quarters, for loans with and without violations, in which covenant slack is nonnegative. Initial violation quarters comprise quarters in which covenant slack is first negative for a given loan. All violation quarters comprise all quarters for which covenant slack is negative.

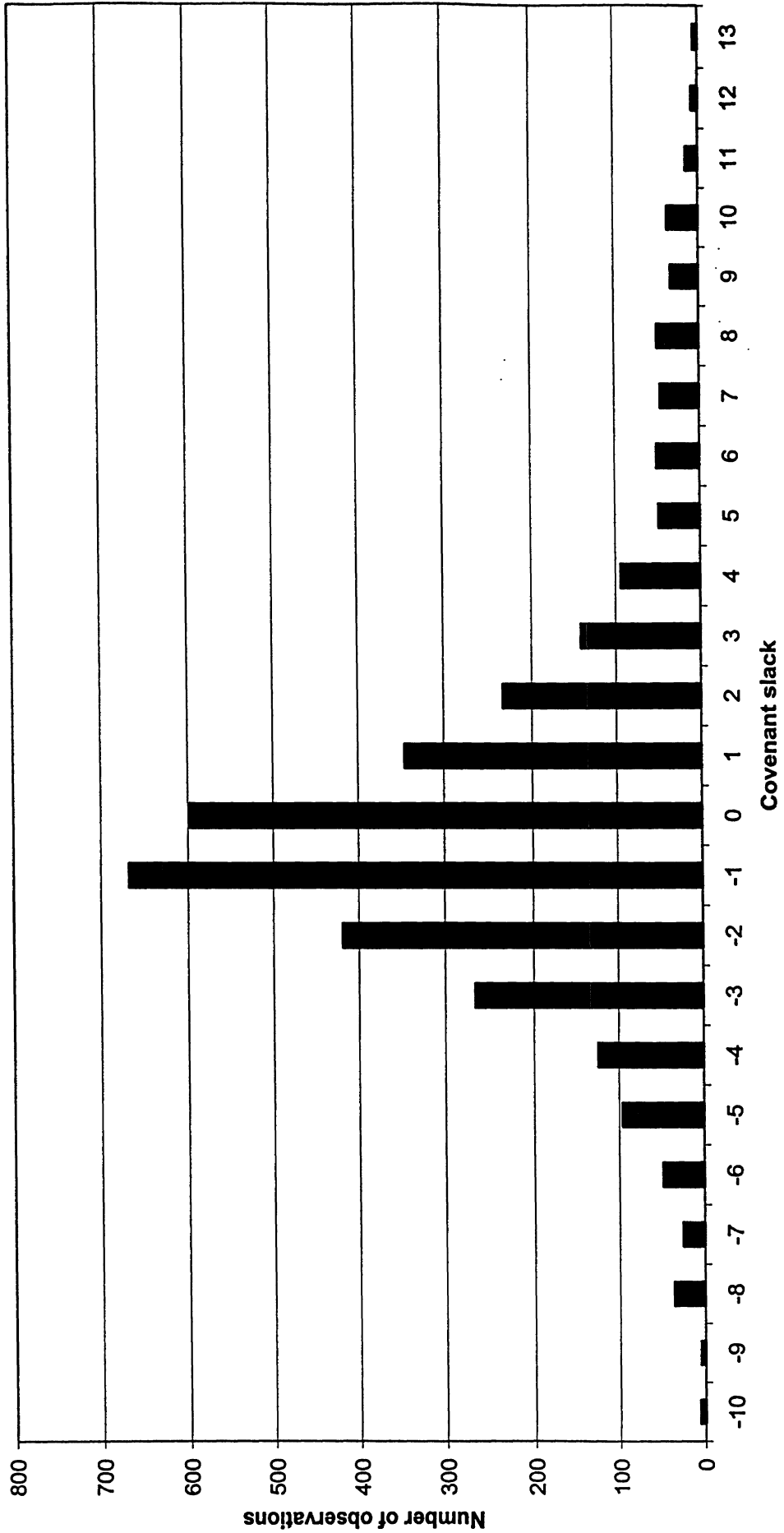
**Figure 1**  
**Histogram of current ratio covenant slack (bin width of 0.2)**  
**All available observations (N = 13,052)**



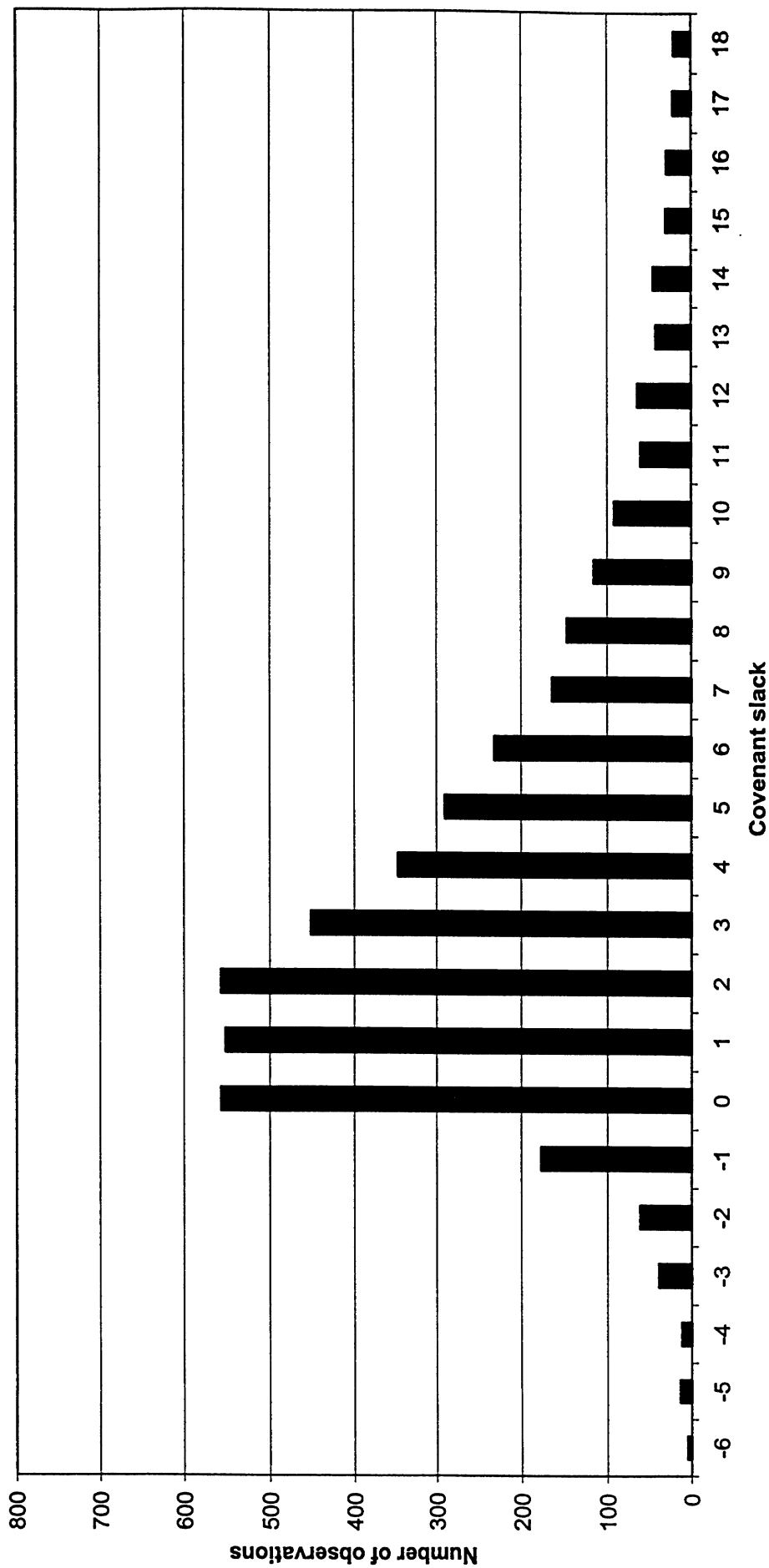
**Figure 2**  
**Histogram of current ratio covenant slack (bin width of 0.2)**  
**Only observations up to and including a first violation (N = 9,546)**



**Figure 3**  
**Histogram of current ratio covenant slack (bin width of 0.2)**  
**Only observations following first violation (N = 3,506)**

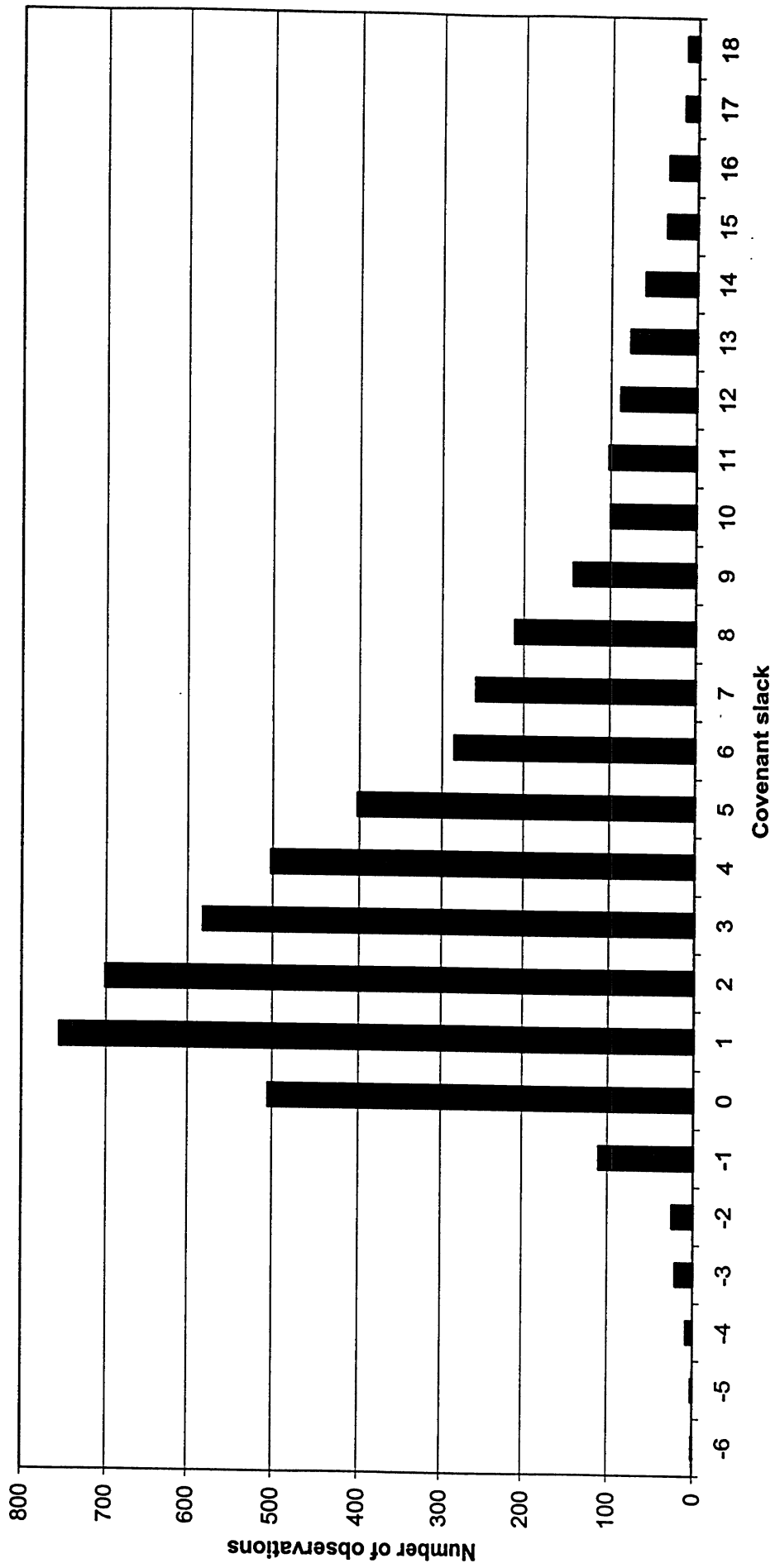


**Figure 4**  
**Histogram of current ratio covenant slack (bin width of 0.2)**  
**Only observations up to and including a first violation**  
**occurring during the first year of the debt contract (N = 4,298)**

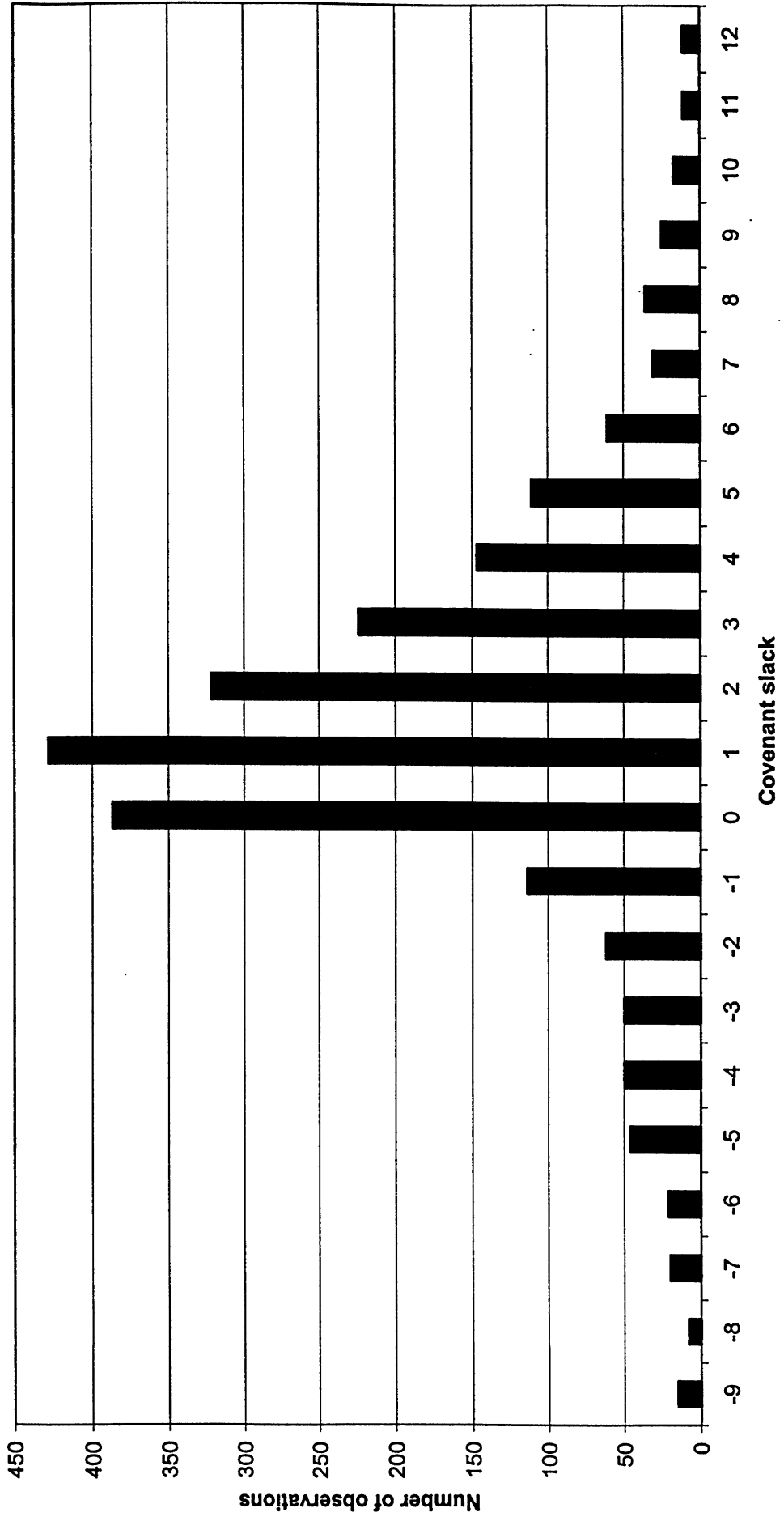




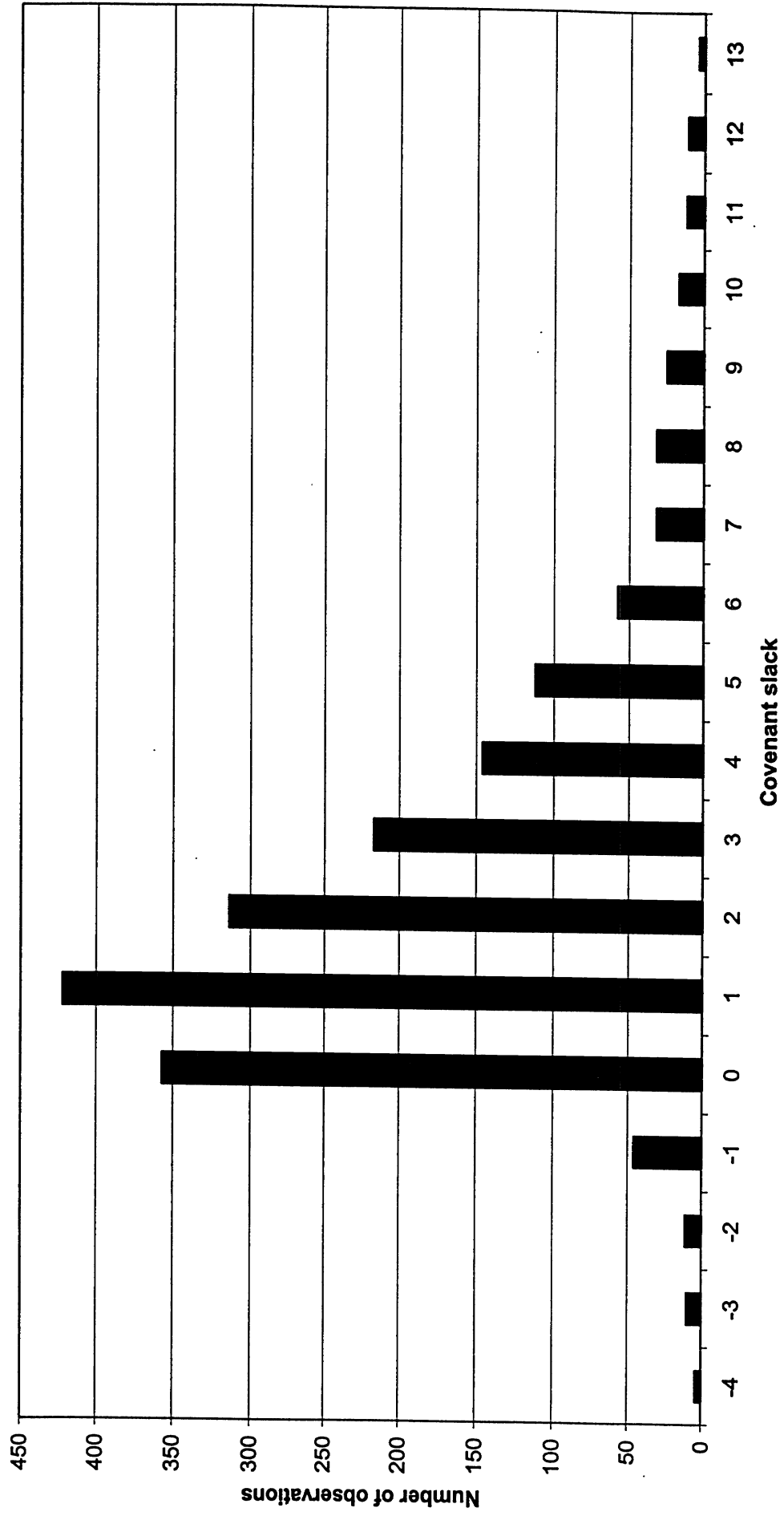
**Figure 5**  
**Histogram of current ratio covenant slack (bin width of 0.2)**  
**Only observations up to and including a first violation**  
**occurring after the first year of the debt contract (N = 5,248)**



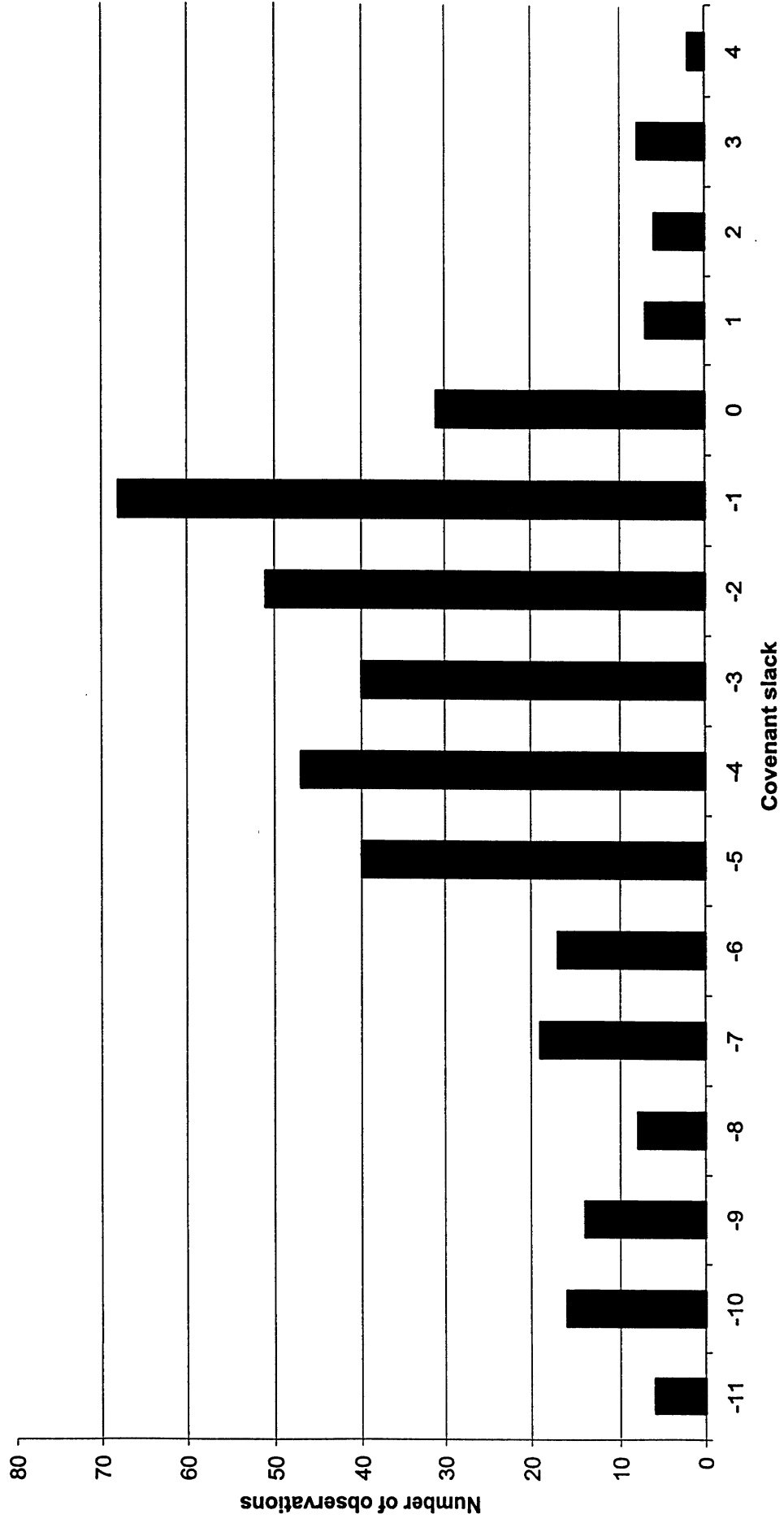
**Figure 6**  
**Histogram of net worth covenant slack (bin width of 0.045)**  
**All available observations (N = 2,339)**



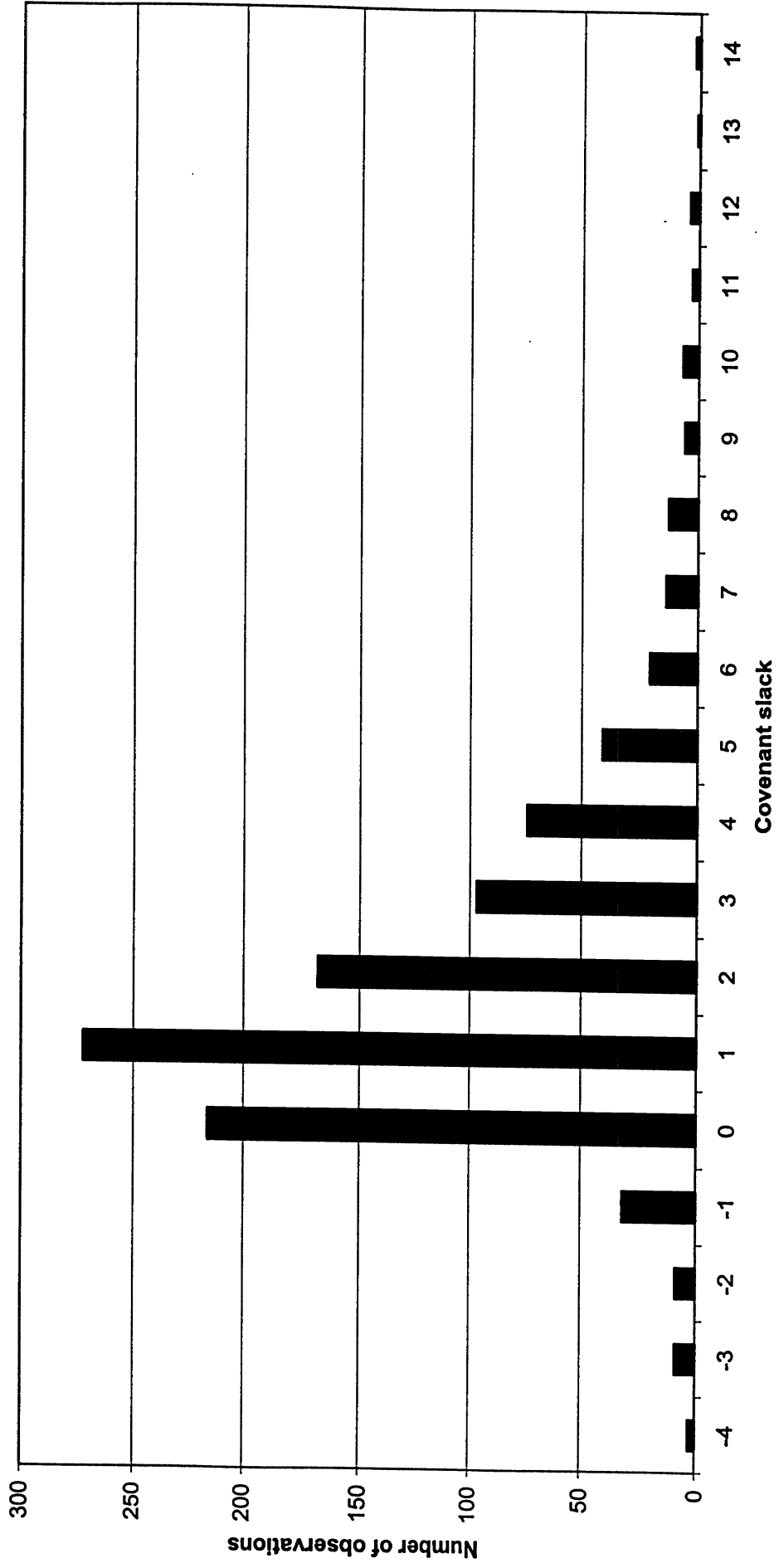
**Figure 7**  
**Histogram of net worth covenant slack (bin width of 0.045)**  
**Only observations up to and including a first violation (N = 1,900)**



**Figure 8**  
**Histogram of net worth covenant slack (bin width of 0.045)**  
**Only observations after a first violation (N = 439)**



**Figure 9**  
**Histogram of net worth covenant slack (bin width of 0.045)**  
**Only observations up to and including a first violation**  
**occurring during the first year of the debt contract (N = 1,016)**



**Figure 10**  
**Histogram of net worth covenant slack (bin width of 0.045)**  
**Only observations up to and including a first violation**  
**occurring after the first year of the debt contract (N = 884)**

