

Large-sample evidence on the debt covenant hypothesis*

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Abstract

We provide two major innovations with respect to existing research on the debt covenant hypothesis. First, we use the DealScan database to identify a large sample of private debt agreements, which allows us to provide direct evidence about covenant design and the frequency of covenant violations. Second, we test for summary evidence of management by examining distributional properties of accounting variables around debt covenant thresholds. We find strong evidence that managers take actions to avoid covenant violations, especially before an initial violation. However, we also find that covenant violations are common, occurring for about 30 percent of the observations in our sample. A reconciliation of these two findings with other evidence suggests that lenders view a covenant violation mainly as an early warning, triggering a review of the debt situation. Following the review, violations for healthy firms are typically resolved with low-cost waivers, while troubled firms face more serious consequences in the form of increased interest rates and tighter restrictions. Thus, it seems that the relative benefits and costs of management vary considerably across firms, which explains why both violations and avoidance of violations are common.

1. Introduction

This study investigates whether and how managers manage reported financial statement numbers to avoid violation of accounting-based debt covenants. This proposition is well-known and influential in accounting research, and there is a large literature on its empirical validity (e.g., Watts and Zimmerman 1986, 1990). However, existing evidence is somewhat indirect, limited, and circumstantial. There are two inherent difficulties in testing the debt covenant hypothesis:

- Debt covenants are set more tightly in private debt agreements (Smith and Warner 1979), so that these covenants are more likely to affect managers' financial reporting decisions. However, private debt agreements are often unobservable. As a result, researchers use proxies for closeness to debt covenants; for example, researchers sometimes look at covenants in public debt agreements or assume that higher debt/equity ratios are associated with closeness to debt covenant constraints. Such proxies contain measurement error (e.g., Mohrman 1993) and are subject to interpretational difficulties (e.g., Leftwich 1990).
- Managerial actions to avoid violation of debt covenants are largely unobservable. Thus, managerial manipulation is typically inferred from indirect evidence. For example, some researchers examine whether managers of firms that end up in technical default make opportunistic accounting choices in periods before default occurs (e.g., DeAngelo et al. 1994; Sweeney 1994). Such evidence is by nature one-sided because it only sheds light on ex post unsuccessful efforts to avoid debt covenant violation. It seems at least as important to learn more about the accounting choices of managers whose firms successfully avoid violations. Ex ante evidence on

debt covenants is also important because we know little about covenant design and the relative frequency and costs of technical default (Smith 1993).

In addition, covenant violation data are typically hand-collected. As a result, existing studies use fairly small samples, typically on the order of several dozen to one or two hundred observations.¹ Ultimately, the consequence of all of these shortcomings is low test power, which makes it difficult to interpret and generalize existing evidence on the debt covenant hypothesis. These limitations may also explain why existing evidence on the debt covenant hypothesis is mixed (e.g., DeAngelo et al. 1994, DeFond and Jiambalvo 1994, Healy and Palepu 1990).

We introduce several methodological innovations that directly address these limitations. First, we use a new data source (DealScan) that allows us to derive large test samples.² The samples in this study comprise nearly 7,000 loan/quarter observations based on about 1,000 loan agreements, up to ten times larger than samples in previous studies. Second, we avoid the dependence on proxies because DealScan provides actual and specific debt covenant data. Third, our tests are simple, robust, and intuitive, and do not rely on discretionary accrual models or other such more indirect forms of evidence. Akin to Burgstahler and Dichev (1997), to test for abnormal behavior we investigate histograms of the differences between firms' reported accounting measures and relevant covenant thresholds (i.e., covenant "slack"). For example, if a firm's current ratio covenant specifies that the current ratio should not fall below two, we take the actual realizations of the firm's current ratio over the life of the loan and subtract two. If

¹ For example, Beneish and Press (1993), DeAngelo et al. (1994), and DeFond and Jiambalvo (1994) all use samples of less than 100 firms.

² DealScan is part of a product called LPC Market Access, from Loan Pricing Corporation, New York, NY. We provide more details about our data below.

managers' accounting choices are motivated by a desire to avoid debt covenant violations, we expect to see a preponderance of slightly positive observations and a dearth of slightly negative observations in the distribution of these differences.

We investigate two of the most common covenants, the current ratio and the net worth covenant, for two reasons. First, the consensus in previous research is that violations occur most often for various net worth and working capital covenants (e.g., Beneish and Press 1993, Sweeney 1994), so we expect these covenants to offer a powerful setting for tests of the debt covenant hypothesis. Second, we try to avoid ambiguity and measurement error in our variables. Private debt agreements are typically customized to fit the needs of borrowers and lenders. Thus, even identically-sounding debt covenants are often defined differently across firms. For example, a "debt/cash flow" covenant could encompass literally dozens of different definitions across firms (e.g., "debt" can be total debt or funded debt only or funded debt minus cash, while "cash flow" can be earnings before interest and taxes or earnings before interest and taxes, depreciation and amortization or cash flow from operations). Such differences introduce definitional ambiguities that make the standardization needed for testing extremely difficult. In contrast, the current ratio and net worth have relatively well-accepted and uniform definitions, ensuring comparability across firms.

Our first major finding is strong distributional evidence that managers take actions to avoid debt covenant violations. Both visual inspections and statistical tests indicate that the number of observations just below the violation cutoff is unusually small compared to the number of observations at and just above the cutoff. This tendency is especially pronounced for observations up to and including the first violation quarter,

while we find no evidence of violation avoidance following an initial violation. Our second major finding is that covenant violations are common, occurring for about 30 percent of the loans in our samples.

Next, we integrate these two findings with other evidence to draw a larger and more coherent picture of the role and use of debt covenants. The impression that emerges from existing research (Gopalakrishnan and Parkash 1995) and an informal survey of current practitioners is that private lenders use covenants mainly as a screening device, where covenant violations trigger a review of the borrowers' debt situation. Typically, healthy firms receive a quick resolution with a waiver, while troubled firms face more serious scrutiny, which could lead to higher borrowing costs such as increased interest rates and tightened restrictions on operating and financing choices. Thus, troubled firms seem to have stronger incentives to avoid violations. However, troubled firms may also have more difficulty avoiding violations because financial problems often trigger the concurrent breach of several covenants. Summarizing, the evidence suggests that the relative costs and benefits of managing vary considerably across firms, which likely explains why both violations and avoidance of violations are common.

The rest of the paper is organized as follows. Section 2 expands on the theory and relation to previous research. Section 3 describes the sample and research design. Section 4 presents the results and a discussion. Section 5 concludes.

2. Theory and relation to previous research

The debt covenant hypothesis predicts that the closer a firm is to an accounting-based debt covenant, the stronger its managers' incentives to intervene in the financial

reporting process in such a way as to avoid costly violation of accounting-based covenants. The roots of this proposition stem from the well-known “positive accounting theory” (Watts and Zimmerman 1986) as well as from an analysis of the way that debt contracts are written to resolve stockholder-bondholder conflicts (Smith and Warner 1979, Leftwich 1983). Positive accounting theory is based on the premise that managers’ accounting choices have economic consequences because contracts designed to alleviate various agency conflicts within firms are often written in terms of accounting numbers and because contracting and monitoring are costly (e.g., see Holthausen and Leftwich 1983). Thus, firms’ accounting choices are material because they affect the promulgation of proper incentives and the resolution of agency conflicts.

Positive accounting theory is usually associated with the debt covenant hypothesis and two other well-known propositions: that managers use their accounting discretion to maximize their bonuses (the “bonus plan hypothesis”) and to minimize the costs of governmental and regulatory intrusion and oversight (“the political cost hypothesis”). We have little systematic evidence on the relative importance of these three propositions.³ However, a recent survey of top management suggests that the debt covenant hypothesis is more important than the bonus plan and political cost hypotheses in affecting managers’ accounting choices (Gopalakrishnan and Parkash 1995). Thus, the debt covenant hypothesis is important in its own right, but even more so as a centerpiece of a larger and influential stream of thought in accounting research.

³ Christie (1990) aggregates the results of numerous studies on positive accounting theory and concludes that observed accounting choices are systematically associated with a number of variables that are believed to proxy for contracting and monitoring costs related to the bonus plan, debt/equity and political costs hypotheses. However, as discussed in Ball and Foster (1982) and Leftwich (1990), the construct validity of these proxy variables is often unclear.

The prominence of the debt covenant hypothesis has elicited a sustained research effort to test its empirical validity. This research has led to a better understanding of the institutional environment and the causes and consequences of covenant-related technical default. For example, we know that technical defaults are usually triggered by deteriorating corporate performance but also by mandated accounting changes (Beneish and Press 1993, El-Gazzar 1993, Lys 1984, Mohrman 1993), that defaults occur almost exclusively for private debt agreements and for affirmative covenants (e.g., DeAngelo et al. 1994, Sweeney 1994), that defaults often involve a simultaneous breach of multiple covenants (Chen and Wei 1993), and that defaults translate into material costs and negative stock returns for offending firms (Beneish and Press 1993, 1995). However, we still know little about the structure of debt covenants and the ex ante probability of technical default (Smith 1993). More importantly, this literature still does not provide a convincing answer to the central question of whether managers systematically take actions to avoid covenant violations.

Primarily because debt covenant data are often not publicly available, extant research has two additional limitations. First, many early studies use the debt/equity ratio as a proxy for closeness to debt covenants (see Christie 1990 for a list of these papers). This choice seems reasonable in light of research by Duke and Hunt (1990), Press and Weintrop (1990) and others which shows that higher debt-equity ratios tend to be associated with closeness to debt covenants. However, more recent research by Mohrman (1993) and Beneish and Press (1995) finds little or no reliable relation between these variables. Second, Beneish and Press (1993, 1995) argue that the debt covenant hypothesis is plausible because the costs of technical default are “large.” Based on a

sample of covenant violations found in annual reports/10K filings, they find that firms in violation face a material increase in their cost of debt and suffer negative stock price reactions. However, a recent survey of Fortune 500 borrowers and large lenders suggests that most debt covenant violations are quickly resolved with waivers, and only relatively extreme and prolonged defaults end up in annual reports/10K filings (Gopalakrishnan and Parkash 1995). Thus, the evidence in Beneish and Press is likely to be descriptive for extreme defaults only, and may not generalize to the full population of violations.

The 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, using somewhat different samples and research designs, Healy and Palepu (1990) and DeAngelo et al. (1994) do not find support for the debt covenant hypothesis. So far, it has been difficult to reconcile these disparate findings. In addition, these studies typically choose firms that violate debt covenants ex-post, and so offer little insight into the accounting choices of firms that successfully avoided covenant violations. Finally, previous studies of necessity tend to use small, hand-collected samples, limiting generalizability.

These shortcomings in existing evidence largely reflect the inherent difficulties facing researchers in this area. Debt covenant data are usually available only for public debt agreements. However, because of the relatively high contracting and renegotiation costs associated with such agreements, covenants in public debt agreements are set

loosely, and so are unlikely to provide firms' managers with real operating constraints.⁴ Thus, covenants used in public debt agreements are of limited value for testing the debt covenant hypothesis. Private debt agreements provide a more promising test setting but are more difficult to obtain. Most existing studies derive their samples from U.S. Securities and Exchange Commission (SEC) filings because the SEC mandates disclosure of all material information regarding lending agreements. However, the sheer cost of culling covenant data from SEC filings restricts the size of samples of private debt agreements, which are typically on the magnitude of several dozen to a hundred or two hundred observations.

As with many tests of positive accounting theory, the second major difficulty that faces tests of the debt covenant hypothesis is that managerial actions are unobservable, and can only be inferred indirectly. Existing research infers opportunistic behavior from investigations of abnormal accruals and accounting method choices. However, small samples, questionable models of abnormal accruals (e.g., Dechow et al. 1995), and other methodological problems tend to produce test results that are not consistent across studies and that are sometimes difficult to interpret. For example, Sweeney (1994) interprets the LIFO liquidations in her sample as income-increasing accounting choices when these events may simply reflect declining demand for these firms' products. In addition, we have little evidence on the relative frequency of management to avoid debt covenant violations or on the extent to which managers' operating ("real") decisions are affected by incentives provided by debt agreement covenants.

⁴ Public debt obligations must meet the requirements of the Trust Indenture Act of 1939 (TIA). TIA requires that changes in debt covenants need to be approved by investors holding at least two-thirds of the debt. Thus, renegotiating public debt is a slow and costly process. As a result, public debt covenants are

We implement two research design innovations relative to the existing literature. First, we use a new computerized database that yields large samples of commercial loans with actual and specific debt covenant information. Second, similar to Burgstahler and Dichev (1997), we investigate the distributions of realized accounting measures around the thresholds specified in debt covenants. If managers make accounting choices to avoid violating debt covenants in their firms' debt agreements, we expect to see an unusually large number of observations just above the debt covenant threshold, and an unusually small number of observations just below this threshold. This method provides several advantages. First, it is simple, direct, and intuitive. Second, it captures the overall effect of all of the economic and/or accounting decisions made by firm managers. Third, in addition to the main results, we are able to document a number of interesting regularities concerning the design and use of debt covenants, and the causes and consequences of their violation.

3. Sample and research design

Our sample comprises private lending agreements drawn from the December 1999 release of the DealScan database, 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 large majority of the data are for U.S. loans, which are the focus of our analysis. DealScan data start in 1986, and are continually expanded and updated. Coverage in the early years is somewhat unsystematic and incomplete. However, by about 1992, loan agreements in the database

set so that they impose few real operating constraints, and are rarely violated. For further detail, see Smith and Warner (1979) and Watts and Zimmerman (1986).

comprise between 50 to 75 percent of the value of outstanding loans in the U.S. (Carey and Hrycay 1999), and since 1995 DealScan covers the great majority of sizable commercial loans. Most of the 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 collected from credit agreements contained in SEC filings. The rest of the data are from other sources, mainly from various relations and contacts with borrowers, lenders, and the credit industry at large. Non-SEC filings sources of data have become relatively more important in later years, as LPC emerged as the leading provider of systematic credit information.

The amount of loan information detail varies considerably across DealScan loans. However, DealScan information almost always includes borrower, lender(s), amount of loan, date of loan inception, projected maturity, purpose, and pricing. In most cases, DealScan also provides considerable 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 also 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 to identify borrowers and covenant information at loan inception, while we use Compustat to track

the actual realizations of corresponding accounting variables over the life of the loan.⁵ For example, we screen DealScan for all U.S. loans with available current ratio covenant data, and search Compustat to obtain realized current ratios for these borrowers over the life of the corresponding loan. In addition to covenant detail, we require the following DealScan information: name and ticker symbol of borrower, loan inception date, projected maturity, and amount of loan. We first match DealScan loans to Compustat using the borrower's ticker symbol. However, Compustat often modifies exchange tickers to reflect additional company identifying information.⁶ In the absence of a straight ticker match, we examine company names and manually match appropriate observations, which leads to a considerable increase in sample size.

We perform extensive additional checks and enhancements with respect to two other features of the covenant data in DealScan. First, covenants often have floating terms over the life of the loan, where the covenant terms typically become more stringent over time. For example, a debt 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. Another typical example is a covenant which requires net worth to be no less than \$150 million at inception, with the initial number increasing over time by 50 percent of cumulative positive net income and 75 percent of stock issues (these subsequent adjustments are known collectively as “build up”). We manually check and, as

⁵ The automated searches on DealScan access only loans made after about 1993. Currently, we are pursuing special arrangements to run such searches on the full population of DealScan loans. We expect that these additional searches will lead to a considerable increase in our sample sizes.

⁶ 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.

necessary, modify all observations to ensure that our sample reflects the effect of floating covenant terms.

Modifications of the current ratio are fairly straightforward because the current ratio stays the same in the majority of the cases. When the current ratio changes, we break the life of the loan into smaller subperiods with constant current ratios. For example, a three-year loan that has a current ratio covenant of 1.5 at inception but increases to 2 after a year is treated as a one-year loan with a covenant of 1.5, followed by a two-year loan with a covenant of 2. The treatment is more complicated for the net worth covenant because the searchable summary information on DealScan does not provide enough detail to accurately compute the “build up.” Thus, we limit our net worth sample to those DealScan loans with both a net worth covenant and TearSheet detail, and use the TearSheet detail in conjunction with Compustat data for periodic (up to quarterly) updates of the specified net worth levels. For example, assume that a loan specifies minimum net worth of \$500 million plus 60 percent of positive net income subsequent to issuance. To reflect this covenant feature, for each quarterly observation we compute a minimum net worth covenant requirement that equals \$500 million plus the sum of 60 percent of the preceding quarters’ positive income.

In addition, we perform checks with respect to another feature of DealScan data. DealScan only provides information about projected loan life at inception. However, loans are often refinanced, rolled over, or otherwise amended or replaced before projected maturity.⁷ As a result, effective loan life frequently differs from projected loan

⁷ 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.

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. More specifically, we are able to infer the effective life of about half of our loans from other information on DealScan.

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 sensitivity checks by running 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.⁸

4. Results

4.1 Preliminary evidence

We begin by providing descriptive evidence on our sample of loans. Panel A of table 1 presents descriptive statistics for the current ratio covenant sample, while panel B presents descriptive statistics for the net worth sample. Panel A reveals that our current ratio sample comprises 805 loans from 648 different firms, which yields 5,436 loan-quarters after merging with Compustat. The summary statistics in panel A indicate that covenant violations are common: 32 percent of all loans have at least one violation over

⁸ 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 more than half of the Inactive loans. Again, sensitivity checks demonstrate that the tenor of the results remains the same across loans with projected and verified effective maturity.

the life of the loan. The relatively high relative frequency of covenant violations is surprising given existing evidence that technical default leads to economically material costs and negative stock price reactions (Beneish and Press 1993, 1995).⁹ In addition, the evidence suggests that most initial violations evolve into a pattern of repeated violations: 23 percent of our sample loans have more than one violation. The evidence in panel B largely confirms the impression from panel A. Covenant violations occur for 27 percent of the net worth sample, while 21 percent have two or more violations. The major difference from panel A is that the net worth sample is considerably smaller, comprising 233 loans for 201 different firms. This occurs because our current ratio sample includes all available loans with current ratio covenants, while our net worth sample is limited to those loans with both net worth covenant and TearSheet data, which tend to be the larger “bellwether” loans.

The bottom half of panel A presents descriptive statistics for the distributions of selected variables. The evidence reveals that most sample loans are short to medium-term obligations, with an average maturity of three years. The data also confirm our expectation that these loans are large, with average (median) size of \$59 million (\$20 million).¹⁰ In comparison, Carey, Prowse, Rea, and Udell (1993) find that 82 percent of all bank loans are under \$1 million, and 96 percent are under \$10 million. Nevertheless, our sample exhibits a considerable range of loan amounts, from as little as \$1 million to more than \$1 billion. Loan amounts are also large relative to the total amount of debt

⁹ Another surprising finding is that many violations occur in the very first quarter after signing the debt agreement. To corroborate this finding, we cross-check our first-quarter current ratio violations with data from Global Access. We find Global Access data for 20 out of the 22 violations examined. Global Access data agrees with the Compustat data in 18 out of these 20 cases. For the other two loans, recalculating the results with Global Access data means one violation is confirmed, and one is overturned. Overall, this check confirms that our measures of financial performance and covenant slack are fairly accurate.

outstanding. The median ratio of loan amount to funded debt is close to one, suggesting that these loans comprise a large part of these firms' total debt financing.¹¹ The results in panel B are similar, although here, as expected given sampling differences, the loans are considerably larger at all percentiles – for example, median loan size is \$250 million in panel B compared to \$20 million in panel A. As is the case in panel A, these loans comprise a large fraction of these firms' total debt outstanding, with a median ratio of 0.84. Thus, loan amounts are large in both absolute and relative terms, which suggests that these samples provide a powerful setting to test the debt covenant hypothesis.

In table 1 we also present summary statistics on sample firms' total assets, leverage, and market-to-book ratios, and compare them to Compustat benchmarks. We expect our sample firms to be larger, more mature, and with less growth options than other firms, since these characteristics are typically associated with higher leverage (e.g., Smith and Watts 1992). These conjectures are partially borne out by our data. The total assets results confirm that both samples contain relatively large firms. Firms in the current ratio sample are of about the same median size as firms in the Compustat population (\$99 million vs. \$94 million), while firms in the net worth sample are larger than typical Compustat firms (median of \$986 million). The current ratio sample has median leverage and market-to-book ratios that are comparable to Compustat benchmarks (median leverage of 0.28 compared to 0.25 for Compustat; median market-to-book of 2.08 compared to 2.03 for Compustat). However, both the median leverage

¹⁰ The size of our loans is roughly the same as that in existing research (e.g., mean of \$37.6 million and median of \$20 million in Beneish and Press 1993).

¹¹ This ratio often exceeds one (sometimes considerably) because many loans in our sample are lines of credit/revolvers that are not fully drawn down.

and market-to-book ratio are moderately higher for firms in the net worth sample (which has median leverage of 0.33 and median market-to-book of 2.40).

Table 2 reports correlations between selected variables for our samples of loans to document some simple and economically plausible relations between firm characteristics, covenant features, and the probability of subsequent covenant violation. For example, efficient contracting arguments suggest that in devising covenants, lenders consider the expected future variability of the corresponding financial measure. Thus, we expect a positive relation between the realized standard deviation of covenant variables (which all else equal increases the likelihood of violations) and covenant “slack” at loan inception.

Panel A of table 2 reports Spearman rank correlations among the variables *Covenant Violation*, *Covenant Slack*, *Loan Maturity*, *Standard Deviation of Current Ratio Realizations*, and *Total Assets* for the current ratio sample.¹² *Covenant Violation* is a dichotomous variable equal to 1 if a loan has at least one covenant violation and 0 otherwise.¹³ Panel A reveals a number of significant correlations among the five variables. 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 $\rho = -0.25$), longer loan maturities ($\rho = 0.14$), and fewer covenant violations ($\rho = -0.11$). There is also a strong negative relation between the probability of covenant violation and covenant slack at loan inception ($\rho = -0.50$), consistent with what we would expect.

¹² We report Spearman correlations because of outliers and non-normalities in some of our variables. Results for Pearson correlations are qualitatively similar.

¹³ The results are almost identical if we use Number of Violations instead of the dichotomous Covenant Violation.

Consistent with efficient contracting, we find a strong positive relation between covenant slack at loan inception and the standard deviation of actual current ratio realizations ($\rho = 0.38$), suggesting that private lenders tailor covenants to suit firms' specific characteristics. This impression is reinforced by the joint consideration of two other results. The simple correlation between covenant violation and standard deviation is statistically significant but fairly low in magnitude ($\rho = 0.10$), 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 considerably stronger ($\rho = 0.33$). 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.

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 ($\rho = -0.33$), and covenant violations are negatively related to covenant slack ($\rho = -0.55$). However, we do not find reliable evidence that larger firms have fewer covenant violations, perhaps because most of these firms are larger than those in panel A (i.e., there is less variation in firm size). In addition, the evidence about the endogenous choice of covenant slack is more ambiguous. Covenant slack is positively but weakly related to the standard deviation in scaled net worth ($\rho = 0.13$). In addition, there is almost no difference between the simple correlation between covenant violation and standard deviation of scaled net worth ($\rho = 0.16$), and the partial correlation controlling for endogenously determined covenant slack

($\rho = 0.18$). The generally weaker results in panel B are probably due to the differing sampling requirements and the relatively small number of observations in this sample.

4.2 Main results

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 at the loan-quarter level, and is defined as the difference between the actual realization of the appropriate variable for that quarter and the corresponding covenant threshold for that variable and quarter. Nonnegative values of covenant slack signify compliance with the covenant, and negative values signify violation. If managers are trying to avoid debt covenant violations, we expect to observe unusually few observations immediately to the left of zero and unusually many observations to the right of zero.

A question that arises in depicting such empirical distributions is how to choose histogram bin width. Generally speaking, 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 fairly subtle properties of the distribution, while precision of estimation demands that bin widths are sufficiently wide that idiosyncratic noise is filtered out at the bin level. In practical terms, this means that bin width should be positively related to the variability in the data and negatively related to sample size. Existing theory and empirical applications suggest one particular rule, $BW = 2(IQR)n^{-1/3}$, 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 adapt this general rule to account for three additional considerations related to bin width choice. First, 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. Moreover, 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. Second, current ratio covenant thresholds are typically stated in increments of 0.1. We want to use comparable scale in our bin width choice for the current ratio sample. Third, to improve comparability we want to maintain a comparable number of bins across our two covenant samples. Since the current ratio sample is considerably larger than the net worth sample, that means that bin widths will be relatively wider for the current ratio sample. After some calibration and sensitivity analysis, for all figures of the current ratio sample we use a bin width of 0.2, which is about triple the bin width of the $2(IQR)n^{-1/3}$ rule. For the net worth sample, we use bin widths that are double the widths from the $2(IQR)n^{-1/3}$ rule.¹⁴

Figure 1 presents the histogram of the current ratio covenant slack for the full sample of 5,436 loan-quarter observations.¹⁵ The X-axis is presented in term of bin ranks

¹⁴ The possibility of measurement errors in our covenant slack variables also argues for using wider bin widths.

¹⁵ 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 deletions are fairly minor. For example, in figure 1 we deleted about 2 percent of all observations. The text and tables report the number of observations *before* these deletions are made.

to the left and right of zero, where bin width is 0.2 of covenant slack. In other words, 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. Observations in bins 0 and above signify covenant compliance, and observations in bins below 0 signify violation. An examination of figure 1 reveals that the distribution is single-peaked, somewhat skewed to the right, and fairly smooth. There is only one discernable discontinuity to the overall smoothness of the distribution, which occurs in the vicinity of zero. Moving from bin -1 to bin 0, the number of observations jumps sharply by 348 observations, from 277 to 625. The next largest jump in the distribution has an absolute magnitude of only 137 observations.

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 deviation of the difference.¹⁶ 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

¹⁶ This statistic seems reasonable for the range of the entire distribution except at the very peak (mode) of the distribution. To account for that, we eliminate the modal bin in calculating the standardized differences.

are unusually large and negative for bin -1 , and unusually large and positive for bin 0 .¹⁷ To further verify the robustness of the results, we report not only the absolute magnitude but also the relative rank of the standardized differences. In other words, even if the standardized differences are not to be literally interpreted as t-statistics, the relative ranks of the differences around zero give a sense for how unusual they are relative to the rest in the distributions.

The standardized differences in figure 1 are -3.42 for bin -1 and 4.01 for bin 0 , suggesting that there are significantly more (less) observations than expected under smoothness in bin 0 (bin -1). In addition, these standardized differences are much larger in absolute magnitude than the rest of the standardized differences in figure 1: the next two largest standardized differences have values of -1.06 and -0.98 . Thus, the statistical results confirm that bins -1 and 0 have a pattern of unusually large deviations from smoothness that are consistent with managerial actions to avoid violations of the current ratio covenant.

Managerial incentives to avoid covenant violations are likely to be stronger when there are no previous violations. To investigate this conjecture, 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. Not surprisingly, the subsample in figure 2 comprises most of the observations in the full sample ($N = 4,468$). However, there is a striking difference between figures 1 and 2.

¹⁷Note 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.

While the right tail of the histogram in figure 2 looks similar to that in figure 1 (these are the covenant compliance observations in bins 0 and above), the left tail in figure 2 is markedly thinner than that in figure 1 (the violation observations in bins -1 and below). More specifically, covenant violations comprise about 15 percent of the observations in figure 1, but only about 6 percent of the observations in figure 2. The net result is that the discontinuity around zero is even more pronounced in figure 2. More specifically, the ratio of number of observations in bin 0 to bin -1 is 2.26 for figure 1 and 3.66 for figure 2. Standardized differences confirm the statistical significance of this discontinuity. The standardized difference for bin -1 is -3.98 and for bin 0 is 3.21. The absolute magnitude of these differences is much larger than those for the rest of the distribution, with the next two largest differences being 1.10 and -0.91. This result is consistent with the prediction 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 quite different from the distributions in figures 1 and 2. To start with, the whole distribution is a lot more jagged and uneven, as expected with fewer observations ($N = 968$). However, the most pronounced difference is the sheer relative number of negative observations in figure 3. In fact, violation loan-quarters actually outnumber compliance loan-quarters, accounting for 55 percent of the observations. In other words, the conditional probability of future violations given an initial violation is 55 percent, in sharp contrast to the 6 percent probability of a violation conditional on no prior violations.

In addition, 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. The statistical evidence confirms this impression. The standardized differences are 1.30 for bin -1 and 2.11 for bin 0, which are not unusual relative to the next two largest standardized differences of 2.07 and 2.25. However, as discussed in more detail later, communications with lenders reveal that following an initial violation, covenants are frequently 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 only few explicit covenant amendments on DealScan, so the slack in figure 3 may be measured with error.

We next turn to the net worth covenant sample. We follow the same order, so that figures 4, 5, and 6 are analogous to their current ratio counterparts in figures 1, 2, and 3, respectively. Figure 4 presents a histogram of net worth covenant slack for all available observations. Recall that the net worth sample is considerably smaller because it comprises the set of loans with both net worth covenants and TearSheets ($N = 1,516$). An inspection of figure 4 reveals that the histogram of net worth covenant slack is similar to the current ratio histogram in figure 1. Specifically, figure 4 shows a single-peaked distribution which is right-skewed and fairly smooth except for a pronounced discontinuity around zero slack. This discontinuity is more pronounced in figure 4 than in figure 1, with the 67 observations in bin -1 quadrupling to 266 in bin 0. Statistical tests confirm this impression. The standardized differences are 2.53 for bin -1 and 2.30 for bin 0, exceeding by a wide margin the next two largest values of -0.71 and 0.60 .

Figure 5 presents the histogram for net worth observations up to and including an initial covenant violation. The immediate impression from figure 5 is the stark discontinuity to the left and right of zero slack, perhaps the most pronounced of all of the figures. Bin -1 contains only 33 observations, which is nearly eight times fewer than the 252 observations in bin 0. The standardized differences for these bins are large at -2.75 for bin -1 and 1.90 for bin 0 when compared to the two next largest values of -0.61 and -0.60. Taken as a whole, figure 5 provides strong evidence that management takes actions to avoid debt covenant violations.

Finally, the histogram in figure 6 contains only net worth covenant observations following an initial covenant violation. Due to the small number of observations (N = 206), the distribution is rather coarse and jagged. However, the main message of figure 6 is clear: following an initial covenant violation, there is no evidence that managers attempt to avoid debt covenant violations.¹⁸ Once again, in sharp contrast to figure 5, it is the observations that signify compliance that are relatively rare in figure 6. Taken literally, the data in figure 6 suggest that following an initial net worth covenant violation, 85 percent of subsequent loan-quarters are also violations.

This evidence is likely due to the nature of the net worth covenant. Net worth covenant thresholds are typically expressed in terms of dollar amounts, adjusted upwards to reflect the “build up” (a percentage of positive net income and stock issues subsequent to debt issuance). For a firm to breach a net worth covenant, it has likely suffered substantial losses. Since loss firms tend to experience a prolonged period of negative profitability (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 earned. Thus, it is not entirely surprising that subsequent violations are more common in the net worth sample as compared to the current ratio sample. As with figure 3, in interpreting figure 6 one should keep in mind that, following an initial violation, covenant thresholds are sometimes reset, and our measure of slack might not reflect these adjustments.

4.3 Discussion

Figures 1 through 6 comprise strong evidence that managers systematically take actions to avoid debt covenant violations. This evidence is broadly consistent with some prior research (e.g., DeFond and Jiambalvo 1994, Sweeney 1994). However, the DealScan evidence also suggests that covenant violations are common.¹⁹ To corroborate our results, and more generally to piece together a larger and more coherent picture of the role and use of financial covenants, we sought a better grounding in practice by conducting phone interviews with bankers in our geographic region. To match the DealScan loan profile, we interviewed lenders with considerable current experience in large syndicated commercial loans.²⁰

The bankers' responses were mostly in agreement with each other. In addition, they often refer to "industry practice", "industry standard" or "experience in the industry"

¹⁸ Given the small sample, we do not calculate standardized differences for figure 6.

¹⁹ We find that virtually all debt agreements on DealScan have a provision called "Default rate", which specifies an interest rate applicable in the event of default. The standard inclusion of this provision in debt contracts also suggests that covenant violations are common.

²⁰ Our main insights come from bankers from Bank One, National City, and Cigna. In addition, we obtained more limited information from other bankers (from Sumitomo Bank, Bank One, and Republic Bank) that work in related fields like mortgage lending and project finance.

to corroborate their arguments or claims. We also find agreement between their comments and the conclusions of a recent large-scale survey of borrower and lender use of accounting information in corporate lending agreements (Gopalakrishnan and Parkash 1995). Thus, we are fairly confident that these responses are representative of current lending practices. The results of these interviews are summarized as follows:

1. Bankers confirm that covenant violations are relatively common. Typical estimates are that about 20 to 30 percent of loans experience at least one violation, with investment-grade debt having fewer violations and non-investment grade loans having as much as 40 to 50 percent violations.
2. Debt covenant violations are typically viewed as early warning signals, and are rarely followed by immediate drastic actions. A covenant breach triggers a review of the borrower's situation which in most cases results in a covenant waiver, and sometimes amends the debt agreement to reset the covenant at a new, looser level. More costly consequences, such as imposing additional constraints or increasing interest rates are less common, are only used when the company's operations and financial position have deteriorated. High-cost measures such as converting the loan to on-demand or recalling the loan are rare because such extreme measures are only warranted when there is a pronounced decline in financial health, and it is exactly in those circumstances that recalling the loan becomes impossible or impractical.
3. Based on these bankers' opinions, it is plausible that corporate managers manage reported financial statement numbers to avoid covenant violations. However, since the cost of most covenant violations is relatively small for healthy firms, their view is

that managers of most firms face only moderate incentives to manage the reported numbers to meet debt covenants.

Overall, the combined evidence suggests that the main function of loan covenants is to provide an early warning system, where covenant violations trigger a re-assessment of the loan situation. In most cases, if the borrowers' financial position is sound, covenant violations are followed by waivers and even by loosening the covenant. More drastic consequences such as imposing interest rate increases, additional restrictions on operations, and recalling the loan are fairly rare, and are warranted only when there is substantial erosion in the borrowers' business and financial position.

Thus, incentives to avoid covenant violations are likely to vary considerably across firms. For most healthy firms, these incentives are probably of only minor to moderate importance. However, other things equal, managers are likely to prefer to avoid the scrutiny and transaction costs involved in obtaining a waiver.²¹ It would be interesting to seek further evidence on whether managers of troubled firms take more aggressive actions to avoid covenant violation. It is also possible that this tendency is tempered by the fact that troubled firms usually experience a broad deterioration of financial condition, so that managers may not be able to prevent the simultaneous breach of several covenants. The dynamic interplay between the varying levels of incentives and costs of management probably explains why we observe relatively many violations along with systematic evidence that managers take actions to avoid violations.

The fact that debt covenant violations are typically resolved with waivers also leads to the re-thinking of some existing results. Existing research on the causes and

²¹ Gilson and Warner (1998) find that healthy growing firms switch from private debt to public debt so as to loosen debt covenant constraints, consistent with these constraints being costly even for healthy firms.

consequences of debt covenant violations uses SEC filings to identify violators, and finds that violations are most often triggered by deteriorating financial strength, and lead to material adverse outcomes to borrowing firms (Beneish and Press 1993, 1995).

However, firms need to disclose violations in their SEC filings only if the violations are not waived or otherwise resolved before the filing. As mentioned above, it is usually troubled firms that cannot obtain waivers. Thus, covenant violation samples derived from SEC filings suffer from an ex post “bad outcome” bias. Accordingly, existing findings about the causes and consequences of covenant violations need to be qualified for this bias.

5. Conclusion and suggestions for future research

We use a large database of private lending agreements to provide direct and comprehensive 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. This regularity and evidence from practice suggest that private lenders use debt covenant violation mainly as a screening device. Most covenant violations have fairly mild consequences, often resulting in waivers or even loosening of covenants for healthy firms. Only extreme violations, accompanied by a deterioration in

the borrower's financial position, warrant costlier measures such as raising the interest rate, imposing additional constraints, or recalling the loan. Thus, managers of healthy firms likely face weaker incentives to avoid debt covenant violations than managers of troubled firms, although, other things equal, they still likely prefer to avoid the scrutiny of private lenders. The considerable relative variation in costs and benefits of managing likely explains why we observe relatively high frequencies of covenant violations along with a relatively large number of situations in which managers apparently take actions to avoid violations.

The evidence in this study opens a number of possibilities for future research. For example, we know that lending agreements are typically 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
Descriptive Statistics

Panel A: Current ratio sample of loans

Summary statistics for the sample

| | |
|--|------------------|
| Number of loans | 805 |
| | |
| Number of firms | 648 |
| | |
| Number of loans with at least one violation (percentage of total number of loans) | 259 (32 percent) |
| Number of loans with repeated violations (percentage of total) | 182 (23 percent) |
| | |
| Number of loan-quarters | 5,436 |
| Number of loan-quarters violating covenant (percentage of total) | 789 (15 percent) |

Empirical distributions of key variables

| Variables | Mean | StD | 5% | 25% | 50% | 75% | 95% | | Compustat Medians |
|-------------------------------------|------|------|------|------|-------------|------|------|--|-------------------|
| Loan Maturity (months) | 36 | 30 | 6 | 14 | 27 | 55 | 84 | | -- |
| Loan Amount | 59 | 107 | 2 | 9 | 20 | 60 | 225 | | -- |
| Loan Amount/ Funded Debt | 9 | 305 | 0.11 | 0.46 | 0.99 | 2.06 | 21.4 | | -- |
| Total Assets | 245 | 450 | 11 | 40 | 99 | 252 | 850 | | 94 |
| Leverage | 0.30 | 0.22 | 0.01 | 0.13 | 0.28 | 0.43 | 0.67 | | 0.25 |
| Market/Book | 2.64 | 5.07 | 0.51 | 1.31 | 2.08 | 3.38 | 7.44 | | 2.03 |

This table presents descriptive statistics for our samples of loans with current ratio covenants (Panel A) and net worth covenants (Panel B). Loan maturity is either maturity at inception or effective maturity when available. Loan amount refers to the amount of the loan facility with the longest maturity. Loan amounts often refer to revolver/credit lines; thus, amount actually borrowed could be substantially different. Funded debt is defined as long-term debt (Compustat quarterly item 51) plus long-term debt in current liabilities (item 45) as of the end of the loan inception quarter. Leverage is equal to (Funded Debt)/(Total Assets.) Market/Book is equal to market value at quarter end divided by total equity. Compustat benchmarks are calculated for all available Compustat firms for the years which comprise the current ratio sample (1994 to 1999). Loan Amount and Total Assets are in \$ millions.

Panel B: Net worth covenant sample of loans

Summary statistics for the sample

| | |
|--|------------------|
| Number of loans | 233 |
| | |
| Number of firms | 201 |
| | |
| Number of loans with at least one violation (percentage of total number of loans) | 63 (27 percent) |
| Number of loans with repeated violations (percentage of total) | 48 (21 percent) |
| | |
| Number of loan-quarters | 1,516 |
| Number of loan-quarters violating covenant (percentage of total) | 244 (16 percent) |

Empirical distributions of key variables

| Variables | Mean | StD | 5% | 25% | 50% | 75% | 95% | Compustat Medians |
|-------------------------------------|------|------|------|------|-------------|------|-------|--------------------------|
| Loan Maturity (months) | 39 | 26 | 5 | 13 | 36 | 60 | 83 | -- |
| Loan Amount | 428 | 506 | 80 | 175 | 250 | 500 | 1200 | -- |
| Loan Amount/ Funded Debt | 9.97 | 67 | 0.16 | 0.50 | 0.84 | 1.98 | 9.43 | -- |
| Total Assets | 2339 | 4025 | 169 | 496 | 986 | 2114 | 10095 | 94 |
| Leverage | 0.33 | 0.18 | 0.02 | 0.22 | 0.33 | 0.44 | 0.62 | 0.25 |
| Market/Book | 3.27 | 6.69 | 0.93 | 1.63 | 2.40 | 3.49 | 6.53 | 2.03 |

Table 2
Correlations between key sample variables

Panel A: Current ratio sample of loans (obs. = 805)

Spearman rank correlation matrix

| | Covenant Slack | Loan Maturity | Standard Deviation of Current Ratio Realizations | Total Assets |
|--|-------------------------|-------------------------|--|-------------------------|
| Covenant Violation | -0.50 <i>(0.001)</i> | 0.06 <i>(0.076)</i> | 0.10 <i>(0.004)</i> | -0.11 <i>(0.001)</i> |
| Covenant Slack | | 0.012 <i>(0.688)</i> | 0.38 <i>(0.001)</i> | -0.01 <i>(0.802)</i> |
| Loan Maturity | | | 0.08 <i>(0.027)</i> | 0.14 <i>(0.001)</i> |
| Standard Deviation of Current Ratio Realizations | | | | -0.25 <i>(0.001)</i> |

Partial correlation, holding Covenant Slack constant.

| | Standard Deviation of Current Ratio Realizations |
|--------------------|--|
| Covenant Violation | 0.33 <i>(0.001)</i> |

Panel B: Net worth covenant sample of loans (obs. = 233)

Spearman rank correlation matrix

| | Covenant Slack | Loan Maturity | Standard Deviation of Net Worth/Total Assets | Total Assets |
|--|------------------|------------------|--|------------------|
| Covenant Violation | -0.55 (0.001) | 0.11 (0.105) | 0.16 (0.018) | -0.06 (0.372) |
| Covenant Slack | | -0.08 (0.225) | 0.13 (0.052) | 0.06 (0.392) |
| Loan Maturity | | | 0.13 (0.067) | -0.04 (0.558) |
| Standard Deviation of Net Worth/Total Assets | | | | -0.33 (0.001) |

Partial correlation, holding Covenant Slack constant.

| | Standard Deviation of Net Worth/Total Assets |
|--------------------|--|
| Covenant Violation | 0.18 (0.007) |

Table 2 presents simple and partial correlations between key variables of interest, p-values follow in parentheses. Panel A presents the results for the current ratio sample and Panel B presents the results for the net worth sample. Covenant Violation is a dichotomous variable, equal 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.

Figure 1
Current ratio covenant
Histogram of covenant slack (bin width of 0.2)
All available observations (N = 5,436)

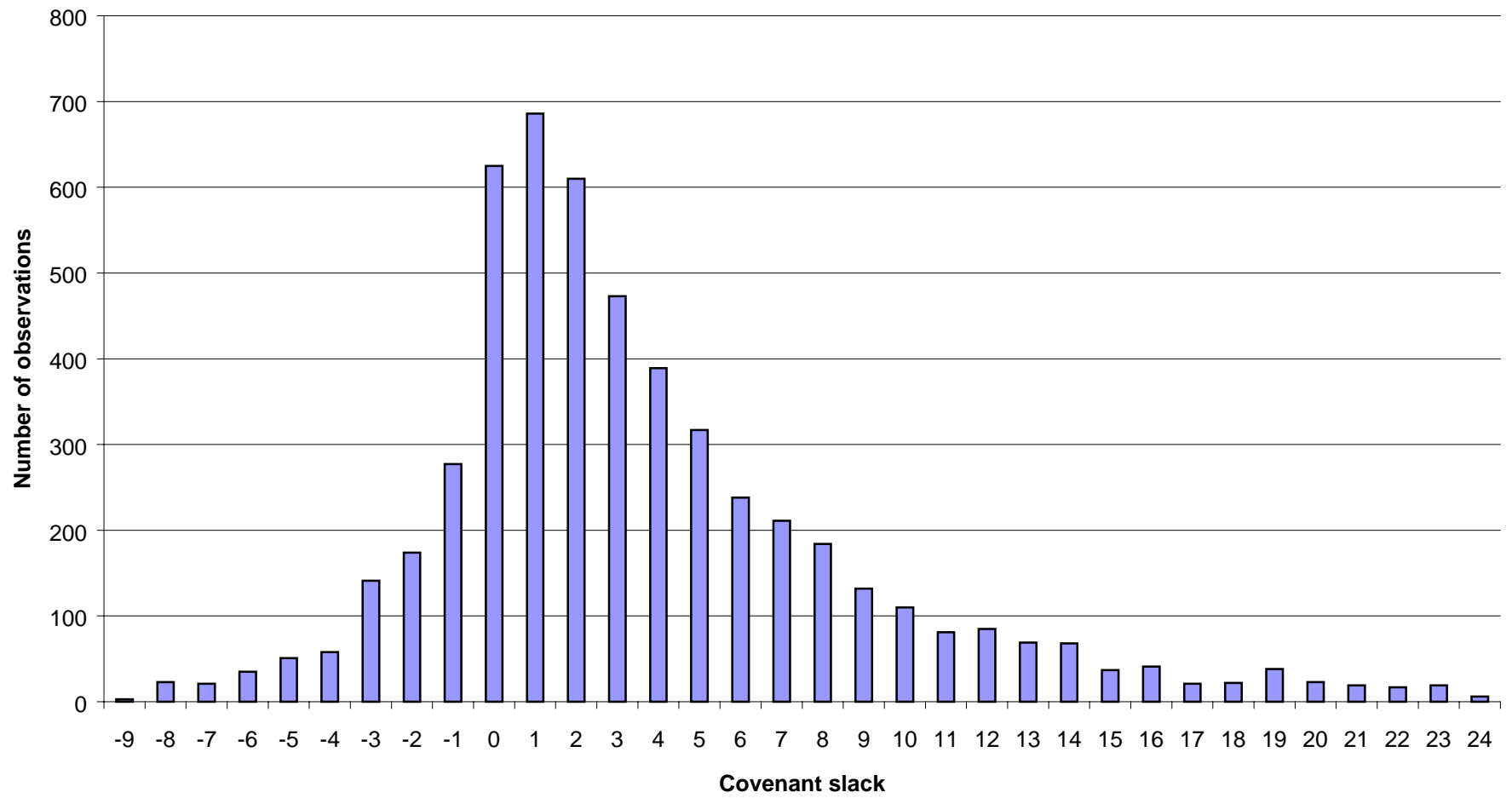


Figure 2
Current ratio covenant
Histogram of covenant slack (bin width of 0.2)
Only observations up to and including a first covenant violation (N = 4,468)

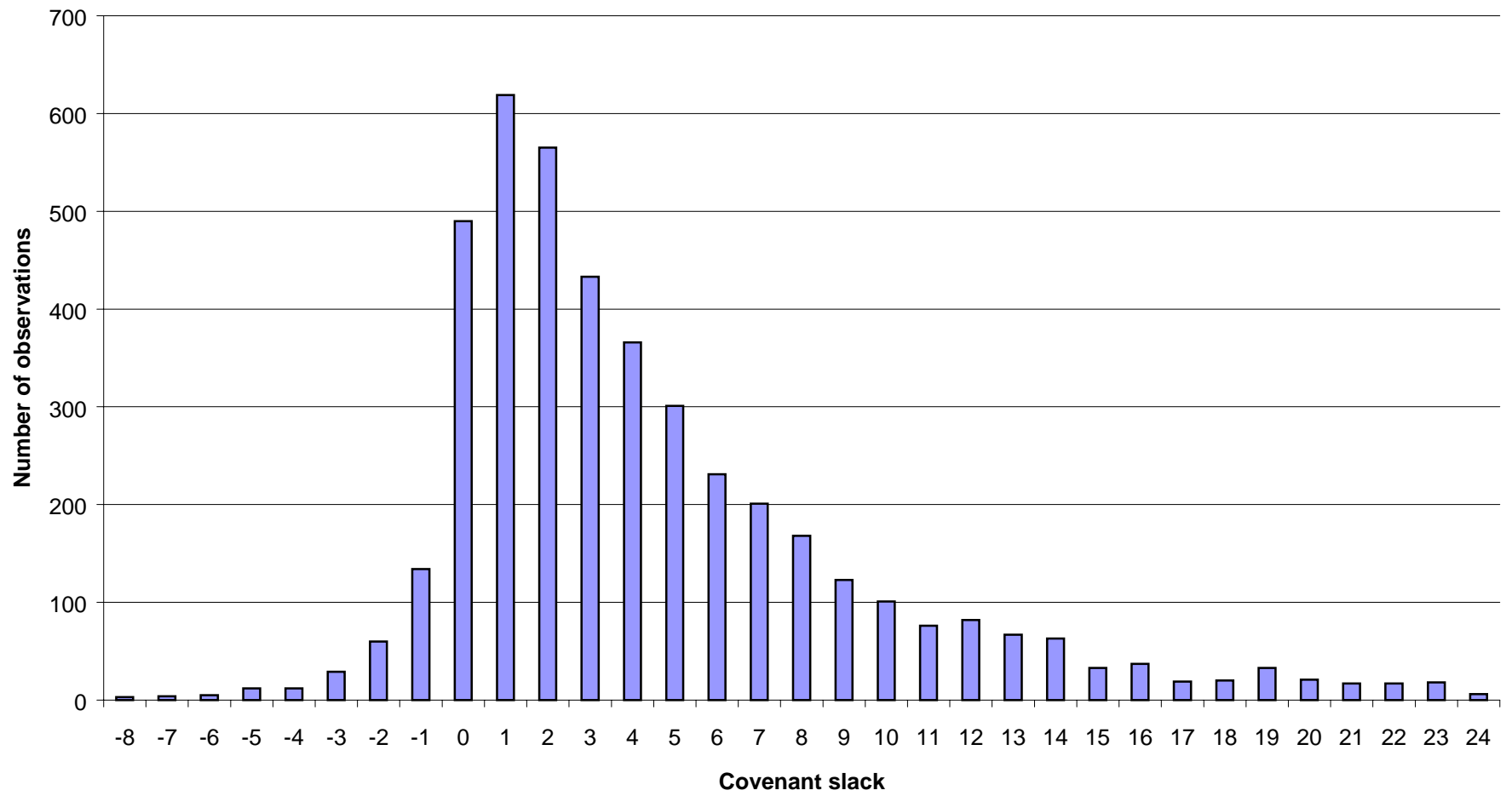


Figure 3
Current ratio covenant
Histogram of covenant slack (bin width of 0.2)
Only observations following an initial covenant violation (N = 968)

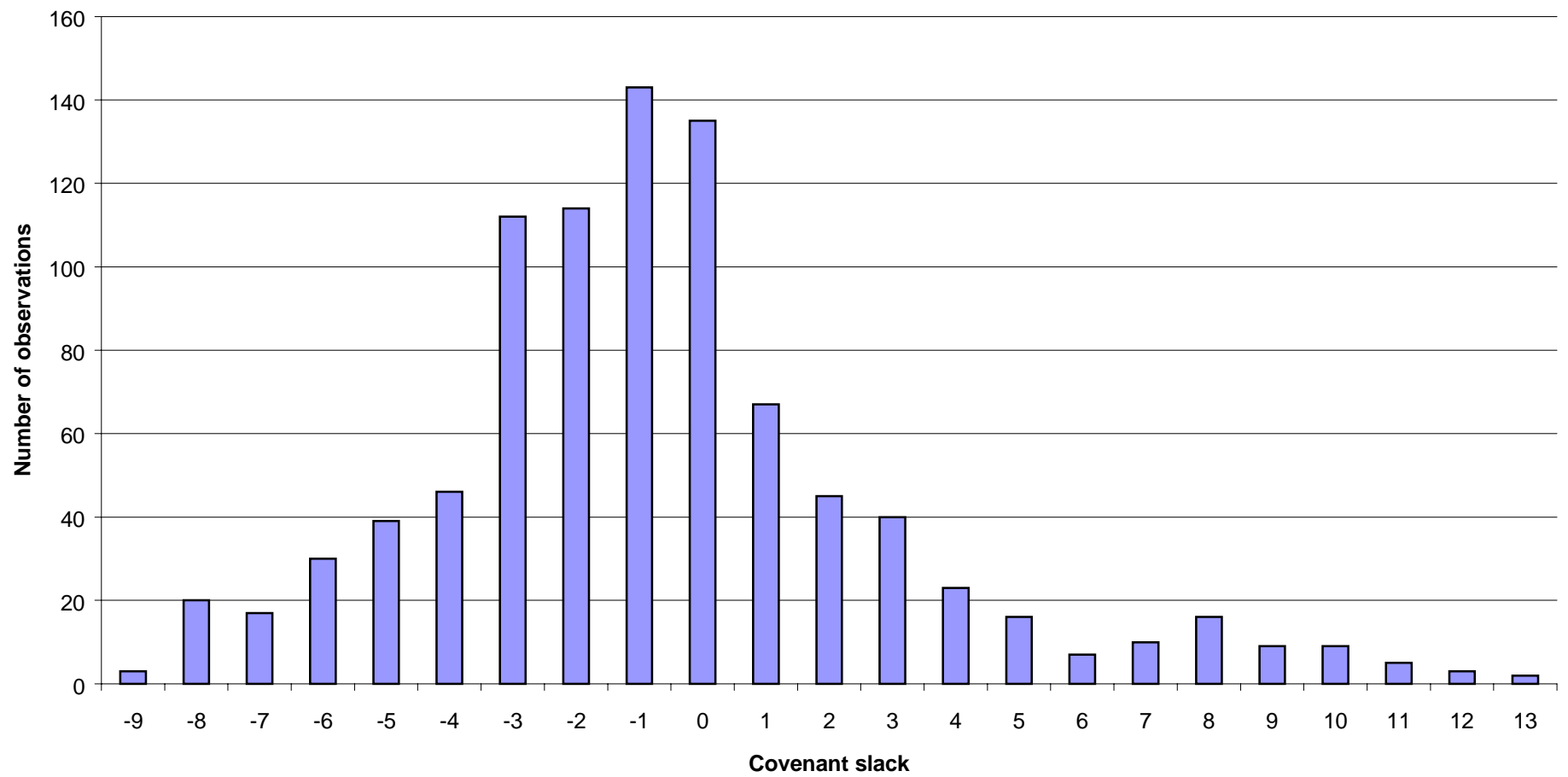


Figure 4
Net worth covenant
Histogram of scaled covenant slack (bin width of 0.044)
All available observations (N = 1,516)

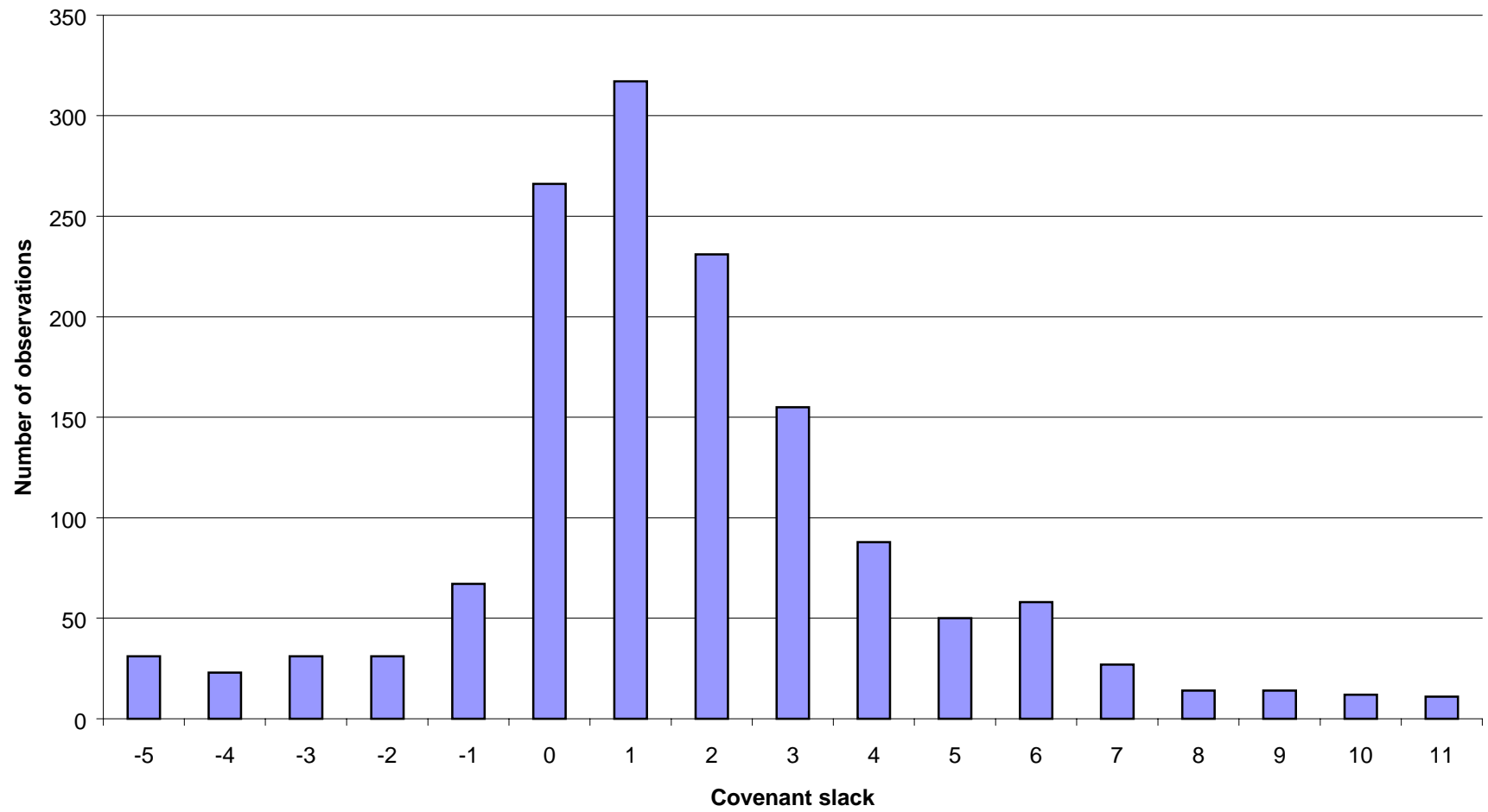


Figure 5
Net worth covenant
Histogram of scaled covenant slack (bin width of 0.044)
Only observations up to and including a first covenant violation (N = 1,310)

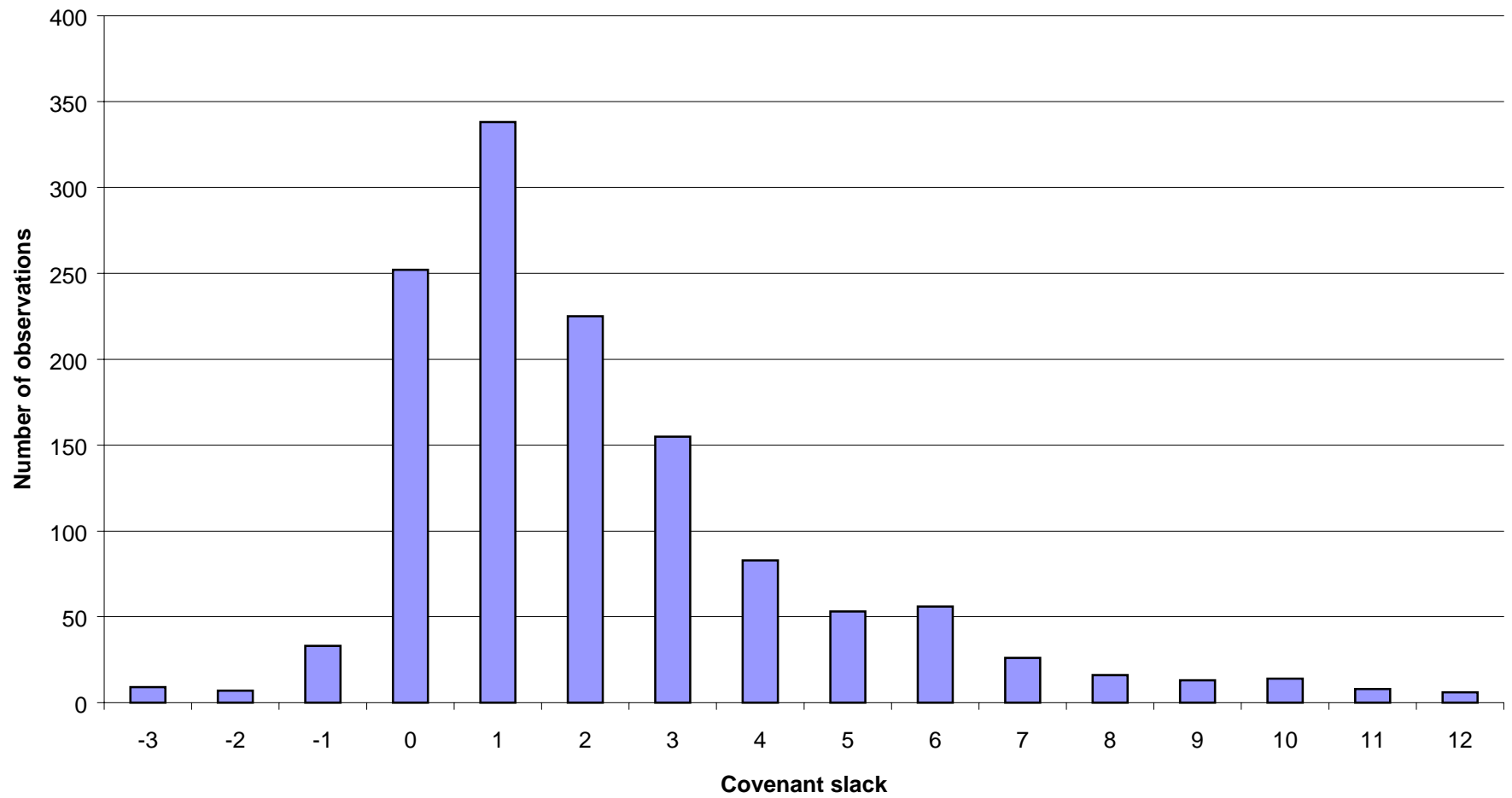


Figure 6
Net worth covenant
Histogram of covenant slack (bin width of 0.128)
Only observations following an initial covenant violation (N = 206)

