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The Resolution of Technical Default

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ABSTRACT: Although costs of default underpin the debt covenant hypothesis, prior research provides limited evidence of their nature, magnitude, and impact on shareholder wealth. We show that announcements of technical default are associated with significant stock price declines. Combining post-default changes in terms of debt contracts with stock returns, we examine whether the consequences arising from renegotiation of lending agreements are priced in the market, and estimate that higher costs of borrowing and new restrictions on firms' opportunities impose wealth losses of 1.4% on shareholders. Leverage measures, frequently used in accounting research as proxies for economic effects of debt contracts, are found to be poor surrogates for default or renegotiation costs.

Key Words: *Technical default, Debt covenant violation, Renegotiation, Leverage, Financial distress.*

Data Availability: *A list of sample firms is available from either author.*

I. INTRODUCTION

ACCOUNTING researchers have long maintained that technical default on covenants in debt agreements is costly and negatively impacts shareholder wealth. However, the findings on wealth effects are mixed, and prior research provides little evidence of default costs. For example, Frost and Bernard (1989, 789) write that "evidence of economic consequences operating through debt covenants has usually been weak." Furthermore, though previous research alludes to technical default costs, Watts and Zimmerman (1990, 151) note

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"researchers have been unable to document the magnitude of the costs imposed by technical violation of a debt covenant or the magnitude of renegotiation costs." In this paper, we investigate whether technical default impacts shareholder wealth, and estimate default and renegotiation costs by combining the outcomes of debt contract renegotiation with an analysis of stock prices.

Recent research by Beneish and Press (1993), DeFond and Jiambalvo (1994), and Sweeney (1994) documents effects from resolving technical default, but yields only limited evidence on costs to shareholders. What these authors observe is that renegotiation following technical default entails three principal changes in debt agreement terms: (i) additional covenants, (ii) increased interest rates, and (iii) reduced allowable borrowings. Evidence of incremental covenants—such as new borrowing restrictions and limitations on capital expenditures—is interpreted by Defond and Jiambalvo and by Sweeney as default costs. Beneish and Press (1993) treat these outcomes as consequences of technical default because it is unclear that new covenants *per se* impose costs.

On one hand, for added covenants to be costly, they must be binding. On the other hand, if added covenants give lenders increased control, potential value-preserving benefits can arise (Wruck 1990). That is, from a shareholder perspective it may be optimal to reduce managerial discretion within technical default firms. Thus, it is an open question whether the potential benefits from increased lender oversight exceed the costs of a shrunken opportunity set. Similarly, while post-default interest rate changes and reductions in maximum allowable borrowing are identified by Beneish and Press, DeFond and Jiambalvo, and Sweeney, none provides evidence of whether these affect stock prices, and only one type of cost (refinancing) is estimated by Beneish and Press. Difficulty in obtaining and analyzing debt agreements has been an obstacle in assessing default costs directly. Therefore, we focus on assessments of post-default changes in debt agreements and stock prices.

We establish that defaults on debt covenants are associated with significant shareholder wealth losses. In the three-day period surrounding announcements, the average abnormal return in our sample of 87 first-time disclosures of technical default is –3.52%. We find that some of the consequences of technical default are impounded in stock price. New financing and investing constraints imposed by lenders—a subset of which is binding—are costly, suggesting that the costs of shrinking opportunities exceed the benefits of increased lender monitoring. Higher financing costs resulting from renegotiation also have an adverse impact on shareholder wealth. We find no relation between stock prices and reductions in allowable borrowing, a result attributed to loan reductions in our sample eliminating borrowing slack without requiring large repayments.

We also investigate leverage as an explanatory variable of the abnormal returns around technical default announcements, since it has been widely used in previous research. Because capital structure varies across firms given different investment opportunities (Smith 1993), we also test a change in leverage variable. We find that neither leverage levels nor changes in leverage proxy for default costs. However, leverage changes are associated with the valuation effects of technical default, suggesting they may be a signal about cash flow realizations subsequent to default. We conclude that tests of debt covenant effects are better specified using debt contract data.

The next section develops hypotheses about the stock market effects of technical default. We review the procedures used to obtain our sample in section three. In the fourth and fifth sections, we describe how we measure abnormal performance and present evidence of shareholder wealth losses around technical default announcements. The losses are related to effects of the resolution of technical default. We also assess whether using data drawn from debt contracts enhances the specification of tests of the debt covenant hypothesis. In the last section, we provide a summary and conclusions.

II. HYPOTHESIS DEVELOPMENT

Analyzing how the stock market responds to an event is a widely-used method of measuring the impact of economic events that has not been applied to estimating the magnitude of financial distress costs. One difficulty in its application is an identification problem: stock price responses to announcements of events of financial distress reflect both the costs of distress and a signal about firms' expected future cash flows. Further, the direction of the signal about firms' future cash flows is ambiguous. We use data drawn from debt contracts and control for contemporaneous information releases to address these complications.

The first hypothesis concerns the wealth impact of technical default announcements.[1] On one hand, negative effects of unanticipated technical default announcements stem from at least three sources: the shrinkage in the investment opportunity set associated with the imposition of tighter or supplementary constraints, or through forced sales of collateral assets; other explicit contracting costs, such as refinancing at higher rates or accelerating loan maturities, which entails repayments; and the announcement itself as a signal about lower future cash flows from continuing operations. On the other hand, if technical default is a consequence of bad management rather than exogenous factors, lenders' increased control can thwart or reverse managers' dissipation of firm assets. The possibility of improved efficiency arising from greater lender control might, over time, shift firm cash flows upward. Thus, our first hypothesis test is two-tailed because of potentially opposing effects of technical default. The hypothesis, stated in alternative form, is:

H_1: Announcements of technical default impact shareholders' wealth.

Similar to previous researchers, we assume that investors form expectations about default costs and impound them in stock prices at the time of technical default announcement. But rather than use leverage as a proxy for default costs, we make predictions about cross-sectional variation in the wealth effects of technical default announcements as a function of consequences arising from the resolution of the event of default.[2] Using changes in terms of debt agreements, we identify three consequences of technical default: incremental financing costs, reductions in allowable borrowing, and additional covenants. In the second hypothesis, we evaluate whether increased borrowing costs following renegotiation are negatively associated with stock price reactions. In hypothesis 3, we test whether repayment demands resulting from reductions in allowable borrowing (that can limit firms' investment opportunities) reduce firm value.

H_2: The stock price reaction to announcements of technical default is negatively related to the incremental financing costs arising from renegotiating terms of debt agreements.

H_3: The stock price reaction to announcements of technical default is negatively related to reductions in allowable borrowing.

In hypothesis four, we evaluate the impact of added covenants on firm value. The hypothesis is two-tailed because new covenants restrict opportunities, but also increase lender control and potentially help preserve value.

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[1] We define technical default as the violation of accounting-based covenants in lending agreements. As such, we do not regard firms that default on debt service as technical defaulters.

[2] See, for example, Bowen et al. (1981), Collins et al. (1981), Holthausen (1981), Lilien and Pastena (1982), Daley and Vigeland (1983), Johnson and Ramanan (1988), Trombley (1989), and Chen and Wei (1993).

H_4: The stock price reaction to announcements of technical default is affected by the imposition of additional covenants.

Our fifth hypothesis evaluates the relation between leverage and stock prices. Given the high cost of obtaining and analyzing debt agreements, prior research has used leverage to proxy for expected costs of default. The argument for assuming leverage is correlated with default costs is well-established: as the relative level of debt rises, a firm is more likely to have tighter constraints in its debt agreements to protect creditors, which increases the likelihood of bearing costs of covenant non-compliance. Thus, the hypothesis is:

H_5: The stock price response at the time of technical default announcements is negatively related to leverage.

III. SAMPLE SELECTION

Identification

There is no systematic public record of technical default. However, financial statements disclose defaults because SEC Regulation S-X (§210.4–08) requires that "any breach of covenant of a[n]...indenture or agreement, which... exist[s] at the date of the most recent balance sheet being filed and which has not been subsequently cured, shall be stated in the notes to the financial statements" (SEC 1988).[3] Our sample is based on the firms in technical default studied in Beneish and Press (1993). They used *Compact Disclosure*, the *National Automated Accounting Research System*, and the *Dow Jones News Service* to search financial statements for keywords identifying debt covenant violations, and identified 202 cases of potential default during fiscal years ending between 1983 and 1987. Their sample was pared to eliminate:

a. firms that were not in compliance with environmental regulations or financial institutions not in compliance with reserve requirements,
b. firms that miss an interest or principal payment, or seek protection from creditors under the Federal Bankruptcy Act,
c. firms that appeared in the sample more than once,
d. firms that violated outside the period 1983 to 1987,
e. firms for which Forms 10-K were unavailable in their university library collections.

Beneish and Press (1993) derived a sample of 91 firms. We eliminate four additional firms by requiring that security returns be available on the CRSP Daily Returns tape on the day of, and for 161 days after, the announcement of technical default.

For the 87 firms included in the sample (42 New York and 45 American Stock Exchange firms), the initial year of violation is labelled as Year 0. Over the period 1983–1987, technical default occurs among 4.4% of all firms in the 32 different two-digit SIC industries in our sample.

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[3] There are also three accounting pronouncements that indirectly require default disclosure. Financial Accounting Standard No. 78 (1983) and Emerging Issues Task Force Release 86-30 (1986) dictate disclosure of the circumstances of a default when long-term debt is reclassified as a current liability. SAS No. 59 mandates that lack of compliance with covenants is a basis for auditors disclosing going concern problems. Because of these rules, financial statements reflect the occurrence of material, uncured debt covenant violations.

SIC codes 10–19—extraction and construction companies (7.1%)—and SIC codes 30–39—durable product manufacturers (6.7%)—have higher than average rates of technical default, but no single industry group predominates in the sample.

Using Wilcoxon rank-sum tests, Beneish and Press (1993) compared the distribution of size, leverage, liquidity and profitability measures of firms in technical default to all non-violator firms available on the Compustat Primary, Supplementary, and Tertiary annual file and the Full Coverage file in the two-digit SIC industries to which violators belong. Statistically significant differences obtain; firms which experience technical default are smaller, more levered, less liquid and less profitable than non-violators.

What Leads to Technical Default?

Beneish and Press (1993) present evidence on the causes of technical default. After examining whether voluntary accounting changes or compliance with mandatory policies force firms into default, they conclude "that technical violations are induced by financial distress rather than accounting changes" (p. 243). Papers that employ debt covenant effects in explaining stock price responses to accounting changes have hypothesized and, in few instances, documented reductions of covenant slack. Yet with the exception of Frost and Bernard's (1989) study of 18 firms forced to comply with an SEC rule change on reserve recognition, no paper reports technical default caused by accounting changes. While Frost and Bernard determine that two of 18 firms are in technical default, one was already in technical default in the year prior to the mandated change (Beneish and Press 1993). For the second firm, it is not possible to determine whether the SEC ruling or existing poor financial condition caused the default.

It is also unlikely that accrual usage causes technical default. DeFond and Jiambalvo (1994) present evidence that firms attempt to avoid default by increasing accruals in the year prior to, and to a lesser extent, in the year of technical default. When they examine a sub-sample for which identifiable debt covenant constraints are available, they find that the accrual amounts for these firms were insufficient either to avoid or cause technical default. They suggest this "indicates that the violating firms would find it difficult to manipulate to an extent that would avoid violation" (p. 173).

Thus, neither accounting policy changes nor accrual behavior causes technical default. The balance sheet and profitability characteristics of defaulters noted above suggest that financial distress is a more likely cause. Nonetheless, technical default is of interest to accounting researchers because it is a setting where covenant default costs can be identified and their impact on shareholder wealth assessed.[4]

Disclosure of Technical Default

Disclosures of technical default occur in news media stories, filings made to the Securities and Exchange Commission (Forms 10-K or NT10-Ks), annual reports to shareholders and *Moody's* bond manuals.[5] For each firm in the sample, we collected the following data pertaining to Year 0 (year of initial violation):

(a) The fiscal year-end, taken from a microfiche copy of the Form 10-K.
(b) The date the SEC received the Form 10-K, from the WORKLOAD computer listing of public filings available in the SEC Public Reference Room in Washington, D.C.

[4] Technical default is also important because it is an event associated with increased likelihood of more serious distress, such as debt service default and bankruptcy (Beneish and Press 1995).
[5] A Form NT10-K (non-timely 10-K) filing explains why a firm cannot meet its Form 10-K filing date.

(c) The date the SEC received an NT10-K, if a firm filed one.
(d) The date the SEC received the annual report, taken from WORKLOAD.
(e) All stories in *The Wall Street Journal* from the beginning of the year prior to Year 0 to nine months after Year 0 for which reading *The Wall Street Journal Index* suggested the story may disclose technical default.
(f) All stories in the *Dow Jones News Service* (DJNS) from June 1979 to July 1989 obtained from a keyword search on "violation," "waiver," "covenant," "compliance" and "default." The DJNS is a computerized data base that offers text coverage of the *Dow Jones Broad Tape*, *The Wall Street Journal*, and *Barron's*.
(g) Dates of technical default appearing in *Moody's* manuals' descriptions of firms' outstanding debt.
(h) The four reports preceding technical default for each of the 40 sample firms covered in *Value Line*.

Using these data, we determine the day technical default was first disclosed (Day 0). For those cases where a Form 10-K or annual report reveal violations, we collate the news media stories' dates with the Form 10-K, annual report, NT10-K and *Moody's* dates in Year 0 to obtain Day 0. News media supply Day 0 in 33 instances, Forms 10-K in 52 cases, NT10-Ks provide two, and *Moody's, Value Line* and annual reports supply none.[6]

Technical default is seldom disclosed alone.[7] Over two-thirds of the violation announcements are released concurrently with information about earnings (33 firms), waivers from lenders (44 firms), and auditor decisions (13 audit qualifications, 15 loan reclassifications). We also find five instances of staff reductions and three cases of asset sales. These phenomena are likely to be part of the same economic event, but it is unlikely that they are informationally perfect substitutes. If disclosures other than technical default are partially complementary signals, excluding them from a cross-sectional model explaining security returns could create an omitted variable problem. Therefore, we control for contemporaneous confounding events in assessing whether consequences of technical default are impounded in stock prices.

IV. MEASUREMENT OF ABNORMAL RETURNS

We estimate daily prediction errors PE_{it} for each sample firm i on each event day t using the market model. Market model parameters are estimated over 300 trading days from day +61 to day +360, using the equally-weighted New York and American Stock Exchange index on event day t.

[6] Forms 10-Q could reveal technical default either in Part II, Defaults upon Senior Securities, or in debt footnotes, preempting the annual Form 10-K event date. To ascertain whether a 10-Q precedes the 10-K in revealing the violation, we purchased all three 10-Qs filed with the SEC during the year of violation from Disclosure, Inc. Since it is costly to purchase all these reports, we randomly chose 10 out of the 52 sample firms with 10-K event dates.

Under the assumption that the Form 10-K event date is not the first revelation in the year of violation and that the Form 10-Q is where the incident is disclosed, there is a one in three chance of observing a violation in a 10-Q. We read the 30 10-Qs (10 random firms × 3 quarters) and found no report of technical violation in 10-Qs. If indeed there is a .33 chance of observing violation disclosure in any 10-Q, the probability of observing none in 30 is less than .00001. Even assuming that there is only one chance in ten that a 10-Q is where the default is first reported, the likelihood of observing none is still less than .05. We also purchased every Form 8-K filed during year 0 for the ten randomly-picked firms. A Form 8-K might contain a technical default as an unscheduled material event. None of the 8-Ks reveal violation. We do not find these results surprising because covenant compliance is typically evaluated using annual audited financial statements.

[7] We identify contemporaneous information releases by reference to two sources. We regard as concurrent a story appearing in *The Wall Street Journal Index* or *Dow Jones News Service* during the five trading days −2 to +2 relative to day 0. Second, in the case of Forms 10-K or NT10-K disclosure, a default announcement is contaminated if earnings, audit qualifications, or loan reclassifications have not been previously announced.

A post-event estimation period is appropriate if firms experience abnormal returns or risk shifts immediately preceding the default announcement (see, for example, Dopuch et al. 1986 on audit qualifications, or Holthausen and Leftwich 1986 on bond rating changes).[8]

The prediction errors are averaged across N sample firms on each day t to form an average prediction error, APE_t; these are cumulated over intervals of k days from t through t+k to obtain cumulative average prediction errors, $CAPE_{t,\,t+k}$. The t-statistic used to test whether CAPE differ significantly from zero is based on the time-series variance of portfolio average prediction errors s^2_{APE}, for the 100 days from day –61 to day –160, and incorporates any cross-sectional dependence in the daily prediction errors.

The estimate of the portfolio time-series variance for the test statistic could be sensitive to the estimation period chosen. The variance estimated over days +61 to +161 is 20.3% higher than the corresponding estimate for days –161 to –61. The t-statistics are approximately nine percent lower than those that obtain using the pre-announcement estimation period, but this reduction does little to alter the reported significance of estimates of abnormal performance. Consistent with Holthausen and Leftwich (1986), we choose the post-announcement estimate as it produces more conservative t-statistics.[9]

V. STOCK MARKET EVIDENCE

Abnormal Returns Around Default Announcements

One measure of the economic consequences of technical default is the stock market reaction to default announcements. Table 1 presents cumulative average prediction errors over various intervals beginning 300 trading days before the technical default announcement and ending 60 trading days after the event. Two features of the table are noteworthy. First, poor stock market performance precedes technical default. Technical defaulters' CAPE from –300 to –61 is –14.43%, significantly different from zero at the 10 percent level. In comparison, other events of financial distress are preceded by greater losses in shareholder wealth. Returns of firms which file for bankruptcy under Chapter XI decrease 43 percent in Warner's (1977) sample, and 74 percent in Clark and Weinstein's (1983) in the 12 and three-month periods, respectively, prior to filing. In Gilson et al. (1990), firms in which cash flow deteriorates sufficiently to preclude debt service lose about 45 percent of market value of equity (on a market-adjusted basis) in the year prior to payment default.

Second, the stock price impact of technical default announcements is negative. As table 1, panels A and B show, the mean CAPE from days –1 to +1 is –3.52%, a wealth loss significant at the 5 percent level. The median CAPE is –1.49%, and the range is –48.83% to 27.84%. The

[8] Announcement period results are insensitive to the choice of equal- or value-weighted indices. However, the announcement period results could be sensitive to the choice of estimation period. Thus, we also use a pre-event estimation period from days –300 to –60 and obtain similar results. This is because systematic risk and return variance are not affected by the technical default announcements. The evidence indicates there are no significant changes in systematic risk (ß) pre- and post-default. Mean and median ßs estimated on days +61 to +300 are 1.22 and 1.17. While the post-default estimates are higher than pre-default ß—in the period –300 to –61, mean and median ßs are 1.10 and 1.00—Wilcoxon rank-sum tests cannot reject the hypothesis that the ßs are drawn from the same distribution (H_0: $ß_{pre} = ß_{post}$). Similar results obtain for a test comparing return variances pre-and post-default.

[9] Because the estimation period for some 1986 and 1987 sample firms includes the October 1987 market crash, we modify the variance estimation by excluding the three trading days from October 16 to October 20, 1987. The exclusion alters neither the estimated average abnormal performance nor its statistical significance. It results, on average, in a three percent decrease in variance. We report results including the three trading days. We also assess the significance of the observed abnormal performance using a standardized test statistic described in Dopuch et al. (1986) and obtain similar results.

TABLE 1
Percentage Cumulative Average Prediction Errors (CAPE) and t-statistics for the Sample of 87 Announcements of Technical Violation from 1983 to 1987.

Panel A: Percentage Cumulative Average Prediction Errors

Days relative to event day[a]	*# of days in cumulation*	*% $CAPE_k$*	*t-statistic*[b]
−300, −61	240	−14.43	−1.79
−60, −2	59	−4.93	−1.23
−60, −31	30	−1.09	−0.38
−30, −11	20	−2.05	−0.88
−10	1	−0.75	−1.44
−9	1	0.23	0.44
−8	1	0.83	1.58
−7	1	−0.33	−0.63
−6	1	−0.20	−0.39
−5	1	−0.26	−0.49
−4	1	−0.26	−0.49
−3	1	−0.03	−0.06
−2	1	−1.09	−2.08
−1	1	−1.25	−2.40
0	1	−2.08	−3.99
+1	1	−0.18	−0.35
−1, +1	3	−3.52	−3.89
+2	1	0.37	0.71
+3	1	0.96	1.83
+4	1	−0.87	−1.67
+5	1	0.08	0.15
+6	1	−0.82	−1.57
+7	1	−0.41	−0.79
+8	1	−0.71	−1.36
+9	1	−0.07	−0.12
+10	1	−0.19	−0.36
+11, +30	20	−0.06	−0.03
+31, +60	30	−2.74	−0.97
+2, +60	59	−4.21	−1.05

Panel B: CAPE (−1, +1) Descriptive Statistics

Mean	−3.52%
Std dev	10.61%
Minimum	−48.83%
First Quartile	−6.34%
Median	−1.49%
Third Quartile	.81%
Maximum	27.84%
Percentage negative	64.40%[c]

(Continued)

TABLE 1 *(Continued)*

[a] The event day (Day 0) is the date of the first announcement of a firm's violation of its debt agreements. For 33 out of the 87 firms, the source of announcement is either *The Wall Street Journal* or the *Dow Jones News Service*. The remaining 54 firms disclose violations in Forms 10-K or NT-10Ks.

[b] Market model prediction errors PE_{it} are calculated as $PE_{it} = R_{it} - (\hat{\alpha}_i + \hat{\beta}_i R_{mt})$, where R_{it} and R_{mt} are the continuously compounded rates of return on the common stock of firm i and the equally-weighted NYSE and ASE index on event day t. Market model parameters are estimated over 300 trading days from day +61 to +361 relative to the day of announcement of violation; $CAPE_k = \sum_{t=1}^{k} APE_t$ where $APE_t = 1/N \sum_{i=1}^{N} PE_{it}$. T-statistics are calculated as follows: $t(CAPE_k) = CAPE_k/(ks^2(APE_t))^{1/2}$, where $s^2(APE_t)$ is the estimated variance of average prediction errors over days +61 to +160 relative to Day 0 and k is the number of days in the cumulation period. The statistics are distributed approximately t with 99 degrees of freedom. The critical values at the .10 and .05 levels of significance are 1.66 and 1.99, respectively, for a two-tailed test.

[c] The hypothesis that the proportion of firms with negative CAPEs is equal to .5 is rejected at the five percent level.

distribution of prediction errors in the announcement period is slightly negatively skewed, with the mean located at the 37th percentile. Fifty-six (64 percent) of the 87 CAPEs are negative, and we reject the hypothesis that the proportions of positive and negative abnormal returns are equal at the five percent level. Furthermore, trimming five percent (ten percent) extrema from the sample yields a CAPE in days −1 to +1 of −3.33% (−3.06%), suggesting that the mean CAPE obtained in days −1 to +1 is not driven by a few observations. The subsequent behavior of CAPE suggests that the impact of the violation announcement is permanent. The CAPE for days +2 to +60 is not distinguishable from zero at conventional levels.[10]

Our evidence indicates that technical default is associated with significant shareholder wealth losses.[11] However, it is premature to conclude that technical default is costly because potentially confounding events occur simultaneously and create an identification problem. To address the problem, a cross-sectional model is presented that controls for contemporaneous release of information.

Regression Model

We next investigate whether consequences of technical default originating from debt covenant changes explain part of the stock price response to announcements of technical default. Because default announcements are frequently contaminated by announcements of earnings, audit qualifications, and long-term debt reclassifications, we control for confounding releases. We specify the model as:

$$\begin{aligned} CPE_i = \beta_0 + \beta_1\, FINCOST_i + \beta_2\, LOANRED_i + \beta_3\, ADDCON_i + \beta_4\, LEVG_i \\ + \beta_5\, WAIVER_i + \beta_6\, UX_i + \beta_7\, AUDQUAL_i + \beta_8\, RECLASS_i + e_i \end{aligned} \qquad (1)$$

where

[10] We also assess the statistical significance of the observed abnormal performance using a standardized test statistic. We obtain similar results: the statistic equals −1.90 for CAPE (−300, −61), −1.47 for CAPE (−60, −2), −5.18 for CAPE (−1, +1), and −.83 for CAPE (+2, +60).

[11] This finding differs from those in Frost and Bernard (1989), who are unable to observe negative abnormal returns for their full cost firms affected by a May 1986 SEC ruling that reduced loan covenant slack. The difference in our results probably arises from our examination of cases of default, whereas Frost and Bernard study covenant slack reductions.

CPE_i = firm i's cumulative prediction error for the three days –1 to +1 surrounding technical default announcement.

$FINCOST_i$ = Present value of incremental interest costs arising from increased borrowing rates on violated agreements, warrants costs, and cost of servicing refinanced debt, deflated by the market value of equity two days prior to default announcement.

$LOANRED_i$ = Percentage change in amount of available loan credit, computed as the percentage change in maximum allowable borrowing pre- and post-violation.

$ADDCON_i$ = Percentage change in the number of accounting, investing and financing constraints following default and renegotiation, measured as: (Constraints post-violation – constraints pre-violation)/Constraints pre-violation.

$LEVG_i$ = Ratio of total debt to total assets at the end of year 0 (or in six cases where year 0 data are not available at day 0, at end of year –1).

$WAIVER_i$ = 1 if, at date of default announcement, firm i has received a waiver of violation from its lender, otherwise 0. At the date of technical default announcement, 44 of 87 firms (51 percent) had waivers.

UX_i = Unexpected quarterly earnings deflated by the price of a firm's common stock in day –2. Unexpected quarterly earnings are computed using the Value Line *Investment Survey* and a seasonal random walk if the firm is not in Value Line for 33 firms which announce technical default jointly with earnings.

$AUDQUAL_i$ = A control variable coded 1 for contaminated default announcements joint with audit qualification for firm i, otherwise 0.

$RECLASS_i$ = A control variable coded 1 for contaminated default announcements joint with reclassification of firm i long-term debt as current liability, otherwise 0.

Testing the cross-sectional model requires proxies for consequences of technical default. Beneish and Press (1993) report that 61 firms (of 91) renegotiate their debt agreements; they identify changes in covenants from debt contracts for 43 firms and use financial statement disclosures to identify changes for the remaining 18. We require that data be available at day 0, so our measures differ from those in Beneish and Press (1993). Specifically, we find that 21 of the 43 firms with renegotiated contracts have not finished renegotiating at day 0.[12] We estimate equation (1) either by eliminating these 21 firms, or by coding as "0" any variable based on contract term changes for the 21 firms.[13] We discuss below how we measure the three debt contract-based variables.

The first variable, denoted FINCOST, is based on the increase in debt service cost imposed by lenders post-violation, the cost of issuing warrants to lenders as an inducement for rate concessions and the incremental cost of servicing refinanced or exchanged debt. Our estimates are similar to those in Beneish and Press (1993), and originate from analyses of renegotiated debt

[12] For the 43 firms with available renegotiated agreements, the mean time from technical default announcement to renegotiation is 1.6 months. The range is from 2 months *prior* to the announcement to eight months after. The fact that renegotiation is sometimes completed before the technical default announcement is consistent with Lummer and McConnell's (1989) evidence that lenders have private information about borrowers' financial affairs. The renegotiation process for technical default is relatively fast compared to the time to renegotiate for firms in default of debt service (15 months in Gilson et al. 1990, table 5).

[13] We compare the 21 firms to the 66 firms along dimensions of size, leverage, liquidity, and probability of bankruptcy. Using Wilcoxon rank-sum tests, we find that the 21 firms differ in that they have significantly larger bankruptcy probabilities. This finding is consistent with renegotiation requiring more time to complete for riskier firms.

agreements or Forms 10-K data, except for two differences. First, changes in interest rates must be observable by day 0; second, we use loan amounts outstanding at year 0 to calculate the impact of increased post-default borrowing costs (Beneish and Press 1993 use year +1 amounts). Since FINCOST is measured as discounted incremental cash flows deflated by market value of equity, accurate measurement should yield a coefficient estimate equal to -1. We estimate that the average increase in the cost of credit represents .96% of the market value of equity prior to default.[14]

The second variable, LOANRED, is the percentage reduction in the amount of allowable borrowing. Subsequent to default, lenders often reduce the amount of allowable borrowing, and LOANRED proxies for the shrinkage in firms' investment opportunity sets resulting from restrictions on credit. Using data available at day 0, we find an average loan reduction of 8.7%.

The third variable, ADDCON, is the percentage change in the number of covenants following renegotiation, based on data available at day 0. Beneish and Press (1993, table 6) report numerous increases in the number of debt covenants post-violation and show a mean increase of 9.4% in the number of covenants for their sample. Using constraint data observable at day 0, we find a mean increase in the number of covenants of 5.4%. Most of the new proscriptions impose limits on managerial discretion to make investing and financing decisions. Although we cannot assess directly whether all the new covenants are binding, we believe that to be the case for a subset. Twelve firms are prohibited from issuing additional debt without creditor permission, and 11 firms need creditor approval to refinance debt. Eight firms are required to sell assets to repay their loans, and six firms must obtain approval prior to divesting assets.[15]

Estimation Results

Table 2 presents ordinary least squares estimates of equation (1). The sample sizes in specifications 1 and 2 differ because debt contracts are renegotiated after technical default is announced for 21 of the 87 defaulters. In specification 1, the variables FINCOST, LOANRED, and ADDCON are set to zero for these 21 firms, since changes in contract terms are not observable at day 0. In specification 2, the model is estimated after deleting the 21 firms. Both regressions are significant, with F-statistics (p-values) of 3.38 (.00) and 3.26 (.00). Since the results are qualitatively similar, we discuss specification 1 below and refer to specification 2 when the differences are of interest.[16]

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[14] Our estimates do not include a fee lenders can charge to renegotiate, which could be imposed by the lead bank in a multiple lender agreement. They range between 1/16% to 1/8% (6.25 to 12.5 basis points) of the credit granted, with the fee varying depending on financial condition of the borrower and competition for bank loans. Nine firms in the sample negotiated multi-bank agreements, but we were unable to discern whether they paid renegotiation fees. The inability to measure this fee is not likely to affect our inference since the fee is so small.

[15] We considered four other variables in lieu of and in addition to the contract-based variables to examine whether the model could be improved. First, given evidence in Chen and Wei (1993), we used the probability of bankruptcy in the year of default to capture variation in costs. Second, we tested a variable measuring the change in bankruptcy probability between years −1 and 0 as a proxy for changes in the risk of default. Third, we used a variable indicating the number of constraints pre-default to capture variation in the shrinkage of firms' opportunities via added constraints. The rationale was that the number of new constraints might depend on the original number of constraints. Fourth, we considered the market value of common stock prior to default. This proxy for size was included given evidence that smaller firms earn greater positive abnormal returns. None of these variables enhanced the specification of equation (1), and they are not reported.

[16] We perform specification tests to assess the presence of heteroskedasticity in the residuals and multicollinearity in the regressors for specification 1. A White (1980) test on the model (excluding the dummy variables) does not reject the hypothesis of homoskedasticity. The Chi-square value is 43.1, with 41 degrees of freedom. The probability of a greater value is 0.38. Further, only three of the 36 pairwise Pearson correlations between regressors are significant at the five percent level. The diagnostic of Belsley et al. (1980) for multicollinearity indicates that the independent variables are not collinear. The condition number (the square root of the ratio of the highest to lowest eigenvalue of the instrument matrix) of 8.04, is much lower than the benchmark of 30 suggested by Belsley et al.

TABLE 2

Determinants of Stock Price Reaction for First-Time Announcements of Technical Default on Covenants. Coefficient Estimates (t-statistics) from Cross-Sectional Regressions of Firms' Cumulative Prediction Errors (CPE) for the Three Day Period (days –1 to +1) Surrounding Announcements.

$$CPE_i = B_0 + B_1\,FINCOST_i + B_2\,LOANRED_i + B_3\,ADDCON_i + B_4\,LEVG_i + B_5\,WAIVER_i + B_6\,UX_i + B_7\,AUDQUAL_i + B_8\,RECLASS_i + e_i$$

Predicted Sign:		(–)	(–)	(–)	(–)		*Control Variables*				
	B_0	*FINCOST*	*LOANRED*	*ADDCON*	*LEVG*	*WAIVER*	*UX*	*AUDQUAL*	*RECLASS*	*REGRESSION F*	*ADJUSTED* R^2
Specification 1[a]	–.034	–.650**	.004	–.135**	.012	.054**	.189**	–.046	–.055**	3.38**	.181
(N = 87)	(–.99)	(–1.69)	(.17)	(–1.79)	(.30)	(2.23)	(2.24)	(–1.54)	(–1.97)		
Specification 2[a]	–.047	–.767**	–.031	–.174**	.067	.041	.153	–.071*	–.079**	3.26**	.215
(N = 66)	(–1.19)	(–1.91)	(–1.07)	(–2.12)	(1.37)	(1.42)	(1.55)	(–1.86)	(–2.29)		

* Significant at the .10 level (one-tailed test; two-tailed test for control variables).

** Significant at the .05 level (one-tailed test; two-tailed test for control variables).

[a] The sample size in specifications 1 and 2 differs because contracts are renegotiated after the technical default is announced (day 0) for 21 of the 87 defaulters. In specification 1, the variables FINCOST, LOANRED and ADDCON are set to zero for these 21 firms since no effect is observed at day 0. In specification 2, the model is estimated after deleting the 21 firms.

Variable Definitions:

FINCOST: Present value of incremental interest costs due to increased borrowing rates on the violated debt agreements, warrant costs, and costs of servicing refinanced debt, deflated by market value of equity on day –2.

LOANRED: Percentage change in amount of credit, that is (Allowable borrowing post-violation-allowable borrowing pre-violation)/Allowable borrowing previolation.

ADDCON: Percentage change in number of accounting constraints after violation and renegotiation, that is (Constraints post-violation – constraints previolation)/Constraints pre-violation.

LEVG: Ratio of total debt to total assets at the end of the fiscal year of violation (year 0) (in six cases we use year – 1 data since year 0 data are unavailable at the time of technical default announcements).

WAIVER: 1 if, at date of violation announcement, violation is waived, otherwise 0.

UX: Price-deflated unexpected quarterly earnings. Unexpected earnings are calculated as the difference between actual quarterly earnings per share and a forecast, which is the most recent Value Line forecast for eight firms and a seasonal random walk for the remaining 25. The share price of a firm's common stock at the end of day –2 relative to event day 0 is used as a deflator.

AUDQUAL: 1 for violation announcement joint with audit qualification, otherwise 0.

RECLASS: 1 for violation announcement joint with auditor reclassification of long-term debt to current liability, otherwise 0.

The variable FINCOST tests whether the stock price impact varies according to the incremental financing costs imposed by lenders following technical default. The coefficient estimate on FINCOST is –.650, significantly different from zero at the five percent level, with a t-statistic of –1.69 (one-tailed test, as specified in H_2). This result is consistent with investors expecting higher costs of financing post-default. It corroborates the incremental financing cost estimates based on changes in terms of debt contracts from Beneish and Press (1993), since we cannot reject the null that the FINCOST coefficient equals –1.

The LOANRED variable tests the price impact associated with reductions in allowable borrowing. If reductions significantly restrict lines of credit, we expect a negative association with prediction errors (as indicated in H_3). The coefficient estimate on LOANRED of –.006 is not distinguishable from zero. Although sample firms have their credit lines reduced an average of 8.7%, we attribute the lack of significance to the fact that loan reduction eliminates some borrowing slack (firms had borrowed an average of 85 percent of the maximum allowable credit) without requiring violators to make large repayments.

ADDCON proxies for the extent to which lenders add restraints on managers, and allows the stock price effect to vary according to the relative increase in number of constraints (as specified in H_4). The coefficient estimate for ADDCON is –.135, significant at the five percent level with a t-statistic of –1.79, suggesting that restrictions on firms' opportunity sets are costly. Since the average increase in number of covenants (reported above) is 5.4%, the mean shareholder wealth loss from added restrictions is –.73% (.054 times –.135). If ADDCON also proxies for value-preserving benefits from increased lender monitoring and control, the benefits seem to be outweighed by the restricting of firms' opportunities from new constraints. Note that, by construction, ADDCON treats added constraints as if they are equally restrictive.

The LEVG variable is included to test the assumption in prior research that the relative level of debt is related to the expected costs of default (hypothesis 5). The coefficient estimate on LEVG of .012 is not distinguishable from zero. The lack of significance on the variable is subject to two possible interpretations.[17] First, LEVG has no explanatory power over that of the contract-based variables. Second, it is possible that, following Smith (1993), regressions using leverage levels are misspecified because optimal debt levels vary across firms with differing investment opportunity sets. Both explanations are investigated below.

Other information is released concurrently with technical default announcements. The WAIVER variable allows the stock price effect to differ according to whether firms report that lenders suspend their contractual rights for a period of time. The coefficient estimate on WAIVER is positive and significant at the five percent level (.054, t-statistic = 2.23). This finding is consistent with waivers reflecting lenders' private information that a borrower is worthy of credit continuation. It is also consistent with Chen and Wei's (1993) finding that lenders grant waivers to technical defaulters with better future prospects. In specification 2, we drop 21 firms without data observable at day 0 and the WAIVER variable does not attain significance. A possible explanation is that waivers matter more for the 21 firms. They require more time to renegotiate since, as previously noted, these firms have higher bankruptcy probabilities.

We control for the two most common contaminants we observe: earnings releases and auditor decisions.[18] Earnings are concurrent with technical default announcements for 33 of the 87 firms.

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[17] Similar results obtain with three alternative measures of leverage, the ratios of long-term debt to total assets, total debt to equity, and total debt to market-value of equity.

[18] We also considered controlling for other, less frequent events. We included dummy variables indicating firms that had staff reductions and asset sales; neither had a significant impact nor altered our findings.

Twenty-six (79 percent) of the 33 forecast errors are negative; seven are positive. The coefficient on UX is positive and significant at the five percent level (.189; t-statistic = 2.24). While consistent with the empirical regularity that negative earnings surprises are bad news, the coefficient estimate is low relative to prior research. This likely reflects the facts that some of the earnings surprises are large, and that not all firms have concurrent earnings signals. Thirteen announcements of audit qualification, and 15 announcements of reclassification of long-term liabilities to short-term debt occur at day 0. The coefficient on RECLASS is negative and significant, consistent with debt reclassification signaling adverse cash flow effects from potential acceleration of debt; AUDQUAL does not attain significance in specification 1.

Examination of Leverage Results

In table 3, we investigate potential explanations for the results on the leverage variable. Specifically, table 2 estimation results of equation (1) indicate that the leverage variable had no explanatory power over the contract-based variables. Since prior research uses leverage as a surrogate for the constraints imposed by debt covenants, we compare a specification that includes the FINCOST, ADDCON, and LEVG variables to one that contains LEVG only. Testing this in table 3, we find that the coefficient on LEVG is still not significant when the contract-based variables are deleted, suggesting that leverage is an unsuitable proxy for default costs. The exclusion test between specifications 1 and 2 indicates that the regression is better specified when the contract-based variables are included.[19]

We also reproduce our tests using a leverage change variable ($LEVG_0$ – $LEVG_{-1}$) in specifications 3 and 4. Regressions using leverage levels may be misspecified because optimal debt levels can vary across firms, given different investment opportunities and asset structures. That is, using a leverage level variable treats a firm with leverage of 60 percent as twice as risky as one with leverage of 30 percent, even though the former may be a food distributor and the latter a steel producer.

The estimated coefficients on the leverage change variable (–.038, t-statistic = –1.69) are identical in both specifications 3 and 4. The negative coefficient indicates a more adverse stock price impact for firms with greater leverage increases from year –1 to year 0.[20] Assuming that firm leverage follows a random walk, the result can be interpreted as the stock price reaction to an adverse leverage forecast error. Comparing both specifications, we find that in specification 3 leverage change attains significance in conjunction with the debt contract-based variables. When the contract-based variables are removed in specification 4, we obtain the same coefficient estimate, suggesting that the change in leverage is not likely to proxy for default or renegotiation costs. This finding is corroborated by the low correlation between the contract-based variables and change in leverage (Pearson r = 0.19 with FINCOST; and 0.11 with ADDCON). Since leverage change is associated with the valuation effects of technical default, it may be that the

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[19] This result seems at odds with the evidence in Beneish and Press (1993, 247) that leverage is significantly associated with the incremental financing costs imposed by technical default. However, their evidence is based on a sub-sample of 48 violators for which they can ascertain whether borrowing rates are renegotiated. It is possible that leverage only serves as a good proxy for incremental interest costs when there is indeed renegotiation.

[20] Similar results obtain when we compute leverage as the ratio of total debt to market value of equity. However, the coefficients are not significant when we define leverage as either long-term debt/total assets or total debt/equity. The first ratio, which measures only long term debt, potentially understates leverage since many firms in technical default are required by auditors to reclassify their long term debt to current liabilities. Further, because technical defaulters are in financial distress, the second measure may suffer from a small or negative denominator problem that biases against finding a significant relation (e.g., 41 percent of defaulters have book equity less than $10 million, and equity is negative for 13 percent of them).

TABLE 3

Debt Contract-Based Variables vs. Leverage as Determinants of Stock Price Reactions to Announcements of Technical Default on Covenants. Coefficient Estimates (t-statistics) from Cross-Sectional Regressions of Firms' Cumulative Prediction Errors (CPE) for the Three Day Period (days –1 to +1) Surrounding Announcements.

$$CPE_i = B_1 + B_2\,FINCOST_i + B_3\,ADDCON_i + B_4\,LEVG + B_5\,WAIVER_i + B_6\,UX_i + B_7\,AUDQUAL_i + B_8\,RECLASS_i + e_i$$

Predicted Sign:		(–)	(–)	(–)		*Control Variables*					
	B_1	*FINCOST*	*ADDCON*	*LEVG*	*WAIVER*	*UX*	*AUDQUAL*	*RECLASS*	*REGRESSION F*	*ADJUSTED* R^2	*EXCLUSION F*[b]
Specification 1[a]	–.035	–.649**	–.134**	.012	.055**	.189**	–.046	–.055**	3.91**	.191	
	(–1.05)	(–1.70)	(–1.80)	(.29)	(2.51)	(2.25)	(–1.56)	(–1.97)			
Specification 2[a]	–.039	–	–	.002	.050**	.205**	–.033	–.052*	4.03**	.150	3.07**
	(–1.16)	–	–	(.05)	(2.23)	(2.40)	(–1.09)	(–1.82)			
Specification 3[a]	–.016	–.645**	–.132**	–.038**	.050**	.162*	–.039	–.053**	4.40**	.219	
	(–.84)	(–1.74)	(–1.80)	(–1.69)	(2.31)	(1.95)	(–1.36)	(–1.98)			
Specification 4[a]	.026	–	–	–.038**	.045**	.182**	–.027	–.051*	4.71**	.177	3.14**
	(–1.42)	–	–	(–1.65)	(2.04)	(2.14)	(– .92)	(–1.87)			

* Significant at the .10 level (one-tailed test; two-tailed test for control variables).
** Significant at the .05 level (one-tailed test; two-tailed test for control variables).
[a] In specifications 1 and 2 the leverage variable is the leverage level variable defined in table 2. In specifications 3 and 4, the leverage variable is the change in the ratio of total debt to total assets from year –1 to year 0, when year 0 financial statement data are available (for six firms it is the change between year –2 and year –1). All other variables are as defined in table 2.
[b] Tests of exclusion for the contract variables. The test statistic distributed F(2,79) has cut-off points 2.72 and 2.15 at the five percent and ten percent levels.

variable proxies for riskier future cash flows from operations or reduced investment opportunities (Press and Weintrop 1990, 90–91; Skinner 1993, 416). These conjectures may be topics for future research.

Overall, we find that technical default is costly to shareholders. In particular, default and renegotiation costs originate from post-default financing costs and from imposition of additional constraints that shrink firms' opportunity sets. Our model estimates that default and renegotiation costs reflected in stock prices represent an average of 1.4% of the market value of equity. Because our evidence derives from an examination of changes in debt contract terms, we are able to demonstrate that tests for debt covenant effects are better specified when contract data, rather than leverage proxies, are used.

VI. SUMMARY AND CONCLUSION

Tests of the debt covenant hypothesis in prior research have focused on accounting measurement changes. However, since financial distress rather than measurement changes causes technical default, prior research provides limited evidence on technical default costs. We study the resolution of technical default, a setting where default and renegotiation outcomes can be observed and their stock price effects assessed. We combine post-default changes in terms of lending agreements with stock returns to examine whether the consequences arising from renegotiation of debt contracts in technical default are priced in the market. Specifically, higher costs of borrowing and new limitations on firms' opportunity sets negatively impact shareholder wealth. We also find that leverage does not capture costs of default in cross-sectional regressions with abnormal returns as the dependent variable. This finding holds even when we apply leverage measures that control for differences in capital structures across firms. We conclude that tests for debt covenant effects are better specified using data drawn from lending agreements.

In interpreting the results in the paper, we note that the sample likely represents the tail of the population of firms in technical default. Current GAAP disclosure rules make it impossible to assess how many covenant defaults occur relative to how many are reported. Given this caveat, our evidence that technical default costs are reflected in stock prices confirms the usefulness of analyzing market reactions to test predictions of the debt covenant hypothesis. In addition, our evidence suggests that leverage may be signalling information about firms' future prospects. It is an avenue for further research to investigate whether adjusted measures of leverage are correlated with cash flow realizations subsequent to default.

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