Journal of Accounting Research

DOI: 10.1111/1475-679X.12456
Journal of Accounting Research
Vol. 60 No. 5 December 2022
Printed in U.S.A.

CHICAGO BOOTH

Do Borrowers Intentionally Avoid Covenant Violations? A Reexamination of the Debt Covenant Hypothesis

ADAM BORDEMAN* AND PETER DEMERJIAN†

Received 18 May 2021; accepted 21 June 2022

ABSTRACT

In this study, we replicate and extend the Dichev and Skinner [DS: 2002] study on the debt covenant hypothesis (DCH). We start by replicating DS and find results consistent with theirs. We then extend their work by changing three aspects of the research design: histogram bin width, calculation of slack, and statistical test of discontinuity. We find that the inference from DS is generally robust to varying these choices, although sensitive to different bin widths, during their sample period. We extend our analysis to the period 2000–2019 and find that support for DCH remains robust. We do, however, find a lack of support for DCH when examining the most common financial covenant, debt-to-EBITDA. These findings suggest a more nuanced

*Cal Poly San Luis Obispo; †Georgia State University

Accepted by Luzi Hail. The authors appreciate feedback from two anonymous reviewers, Herb Hunt and Melissa Martin, and the support of the Orfalea College of Business at Cal Poly San Luis Obispo and the College of Business Administration at the University of Illinois at Chicago. An online appendix to this paper can be downloaded at https://www.chicagobooth.edu/jar-online-supplements.

perspective on DCH, whereby different types of financial covenants provide different incentives and abilities to avoid technical default.

JEL codes: G30, G32, M40, M41

Keywords: debt covenant hypothesis; debt contracting; financial covenants

1. Introduction

The debt covenant hypothesis (DCH) is one of the key testable theories of the positive accounting paradigm described in Watts and Zimmerman [1986]. The hypothesis is based on the idea that financial covenants in debt contracts—provisions that require the borrower to maintain a threshold level of an accounting-based metric, such as interest coverage or net worth—are costly to violate. Watts and Zimmerman, in their "debt/equity" hypothesis, predict that the cost of covenant violation affects borrower behavior. Specifically, they predict that borrowers with high leverage (a proxy for closeness to covenant violation) will make income-increasing accounting policy choices. This theory has been adapted into the broader DCH, which predicts that firms close to covenant violations take action to avoid technical default through accounting policy changes, accruals, or real activities. DCH has received considerable empirical support. Sweeney [1994] and DeFond and Jiambalvo [1994] test hand-collected samples of covenant violations and find that firms make income-increasing accounting decisions and manage earnings upward prior to covenant violation. More recently, Kim, Lei, and Pevzner [2010] find that firms use real activities management to avoid violation, and Franz, HassabElnaby, and Lobo [2014] show that firms use both accrual earnings management and real activities management to avoid technical default.

Dichev and Skinner [DS: 2002] provide some of the most convincing support for DCH. Unlike the papers noted above, which use models of discretionary accruals or real activities to examine DCH, DS use a histogram-based analysis in the style of Burgstahler and Dichev [1997] to examine two financial covenants, minimum current ratio and minimum net worth, and measure the difference between the covenant threshold and the actual value of the accounting-based covenant metric, or *slack*. The authors organize covenant slack into histogram bins and measure the smoothness of the distribution of bin density around the threshold of technical default. The findings show a pronounced discontinuity, with a disproportionately large number of slack observations just above the technical default threshold (peak) and a disproportionately small number of slack observations just below (trough). These significant discontinuities are interpreted as supporting DCH. The findings in DS are robust, showing statistically significant discontinuities across a variety of subsamples of covenant slack.

In this paper, we propose a reexamination of the DCH. Although DS provide clear, strong support for the hypothesis and their paper remains influential, we believe that a reexamination is justified for several reasons. First, as is the case in all empirical studies, DS make a variety of measurement and specification choices that potentially influence the inferences from their study. A reexamination allows us to test the robustness of their findings to different research design choices. Second, research methodologies evolve over time, allowing for new methods and tests that can change inferences from past research. In our reexamination, we use an alternative statistical test that had not been developed when DS conducted their research.

Third, 20 years have passed since their paper was published. These two decades have seen significant developments in financial reporting and the broader economy. Some of these changes, including the passage of the Sarbanes–Oxley Act, the global financial crisis of 2007 and 2008, and the rise of securitization in the private loan market, have likely influenced the ability and incentives of borrowers to avoid covenant violations. Although past research suggests that borrowers acted in accordance with DCH in the period that DS study, whether this is still the case is an empirical question. Finally, current ratio and net worth covenants are not commonly used financial covenants and have fallen even more out of favor in recent years; thus, inferences of DCH based on these covenants may not generalize.

We start our reexamination of DCH by reproducing DS. Although we attempt to precisely replicate the sample, measurement choices, and empirical tests in their study, changes in the Loan Pricing Corporation (LPC)/Dealscan data since DS's analysis make a precise sample replication impossible. We also note that some covenants in our sample have either dynamic thresholds (current ratio) or build-up provisions (net worth) that affect the measurement of slack. We hand-collect data from Securities and Exchange Commission (SEC) filings to address these measurement issues.[1] Considering changes in data availability, we reproduce their sample as best as we can; our descriptive statistics suggest that our reproduced samples for current ratio and net worth covenants are close to those of DS, and we do not believe that sample differences introduce systematic bias into our results. Using these samples and following the DS research design, our findings are consistent with DCH. In fact, our distributional results closely mirror the findings of DS for each of the five specifications tested for the current ratio and net worth covenant samples. We conclude that, despite possible sample differences, we faithfully reproduce DS's analysis.

In the next phase of our reexamination, we run three research design-based extensions of DS, focusing on the subsample of observations up to and including the first documented covenant violation. In our first extension, we examine alternate histogram bin widths. DS use ad hoc bin widths that approximate a doubling of the interquartile range-based formula of Degeorge, Patel, and Zeckhauser [1999]. We explore narrowing and widening the DS bin widths. For our second extension, we vary the measurement

[1] DS discuss both dynamic thresholds and build-up provisions. We discuss their approach to these data issues, along with more detail on our hand-collection procedure, in subsection 3.3.

of slack in the histograms. DS measure slack using covenant-specific formulas. For our alternative measure, we rely on a general formula from the literature: scaling the distance to technical default by the threshold (El-Gazzar and Pastena [1991]). For this slack measure, we use the bin width calculation of Degeorge, Patel, and Zeckhauser [1999] and double the bin width for symmetry with DS's bin widths. Our third extension replaces the standardized difference test statistic (as developed in Burgstahler and Dichev [1997] and used in DS) with the regression-based discontinuity test from Byzalov and Basu [2019]. We find evidence of discontinuities for each of these research design variants.

The research design extensions all focus on the DS sample period, 1989–1999. In our next set of tests, we run analyses using the out-of-sample period 2000–2019. We start by examining histograms using the DS research design, including their bin widths and slack formulas. Under these assumptions, our findings continue to support DCH. We then examine histograms from the out-of-sample period using the alternative measurement rules previously described, including alternate bin widths and our threshold-scaled slack measurement. Although we continue to see evidence of discontinuities in the current ratio sample, the net worth results are attenuated at narrow bin widths. Overall, although there is some sensitivity around bin width choice, our findings suggest that DS's results are largely robust to alternative research design choices and the out-of-sample period.

When considering the debt contracting landscape, we see that the use of current ratio and net worth covenants has been declining over time (Demerjian [2011]), and we confirm that this trend has continued through the current period. Support for DCH based on these infrequently used covenants may not generalize, potentially limiting inference from DS for current debt contracting practice. Accordingly, in our final analysis, we prepare and test discontinuities in slack histograms for a more frequently used covenant, maximum debt-to-EBITDA. Measuring slack using the standard contractual definition of debt-to-EBITDA (Demerjian and Owens [2016]), our findings are inconclusive regarding DCH. Specifically, the results for different bin widths and slack measurements are mixed and, in some cases, contradict the predictions of DCH. We posit that differences in the roles of capital and performance covenants may explain these inconsistent results. Christensen and Nikolaev [2012] show that capital covenants (including current ratio and net worth) limit agency conflicts ex ante, while performance covenants (including debt-to-EBITDA) allocate control rights ex post. We attribute the inconclusive results for debt-to-EBITDA covenants to borrowers having less ability and incentive to avoid violating performance covenants. We also discuss how broader changes in debt contracting may affect borrowers' ability and incentive to avoid covenant violation in the out-of-sample period.

Our study builds on the findings in DS, providing evidence of the sensitivity of the results to alternative research designs, time periods, and covenants. Our results indicate that the findings of DS are robust to a variety of research design extensions and to a more recent period and that, even with some sensitivity to bin width, their inferences on current ratio and net worth are supported. It is notable, however, that our results do not provide unconditional support for DCH; our analysis of debt-to-EBITDA slack provides inconclusive findings, indicating a more nuanced perspective on DCH than seen in prior literature, and presents some paths for future research. Our analysis also illustrates some key issues with data in debt covenant studies, particularly the role of dynamic thresholds and build-up provisions in the calculation of slack.

Finally, our study addresses a key question in accounting research and scientific research in general: Can the results be reproduced? This issue is explored in Hail, Lang, and Leuz [2020], who document concern among accounting researchers as to the extent of irreproducibility in the literature. By reproducing DS and finding results largely robust to variation in research design choices and period, we, as a field, can have increased confidence in the inferences from their study. Our results also suggest that evolution in the institutional environment and in our instrumentation as researchers (e.g., measurement of covenant slack for a wider range of covenants) allows for a broader view of the original question presented in DS. Our hope is that this study will lead to further research on the interaction between accounting information and debt contract outcomes and, ultimately, facilitate progress in this line of research.

2. *Background and Motivation*

2.1 FINANCIAL COVENANTS IN POSITIVE THEORY

One of the key paradigms in accounting research is positive accounting theory. This theory, proposed by Watts and Zimmerman [1978, 1986], provides testable propositions on accounting choices and, more generally, on financial reporting behavior. The theory proposes that financial reporting choices are driven, at least in part, by incentives related to financial reporting. Watts and Zimmerman [1986] describe how debt contracts, and covenants in particular, can motivate financial reporting choices in their "debt/equity" hypothesis. Although there are several classes of debt covenants in practice, the debt/equity hypothesis focuses on financial covenants.[2] Financial covenants, or as they are sometimes termed, maintenance covenants, are debt contract provisions that require borrowers to maintain a threshold level of an accounting-based financial metric, such as current ratio or net worth. Failure to maintain the threshold results in

[2] Other types of covenants include positive and negative covenants. Positive covenants require an action by the borrower. Common examples include providing timely financial reports to the lender or insuring key assets. Negative covenants restrict or limit actions by the borrowers. Examples include restrictions on mergers and acquisitions or limits on new debt issuance by the borrower. Although some are linked to accounting information, many positive and negative covenants are not related to accounting information.

technical default. In technical default, control rights revert to lenders. These control rights allow lenders to act to protect their debt investment, for example, by tightening loan terms or accelerating contract maturity. Financial covenants are pervasive in private debt contracts; Drucker and Puri [2008] report that 95% of BBB-rated loans and 80% of A-rated loans include these provisions.

Technical default, due to the transfer of control to the lender, is presumed to be costly (Watts and Zimmerman [1986]). Positive accounting theory thus suggests that the presence of financial covenants and the threat of costly technical default provide an incentive for the borrower to make accounting choices to avoid such an outcome. Furthermore, borrowers close to their covenant thresholds have the strongest incentive to manage their financial reporting to avoid technical default.

At the time Watts and Zimmerman [1986] developed their theory, there was virtually no publicly available source for financial covenant data, so the authors proposed leverage as a proxy to identify borrowers close to covenant thresholds. Early studies, such as Duke and Hunt [1990] and Press and Weintrop [1990], use leverage to measure closeness to covenant thresholds. Both studies yield mixed results for the debt/equity hypothesis. Begley [1990], writing concurrently, notes that the tests in these studies are joint tests of the debt/equity hypothesis and the authors' proxy for closeness to covenant thresholds. Begley recommends a more careful explication of the underlying theory of the hypothesis and further development of proxies to measure covenant strictness. Later, empirical research that examines the debt/equity hypothesis uses new data that have become available to measure covenant threshold closeness more directly, and in the process, the debt/equity hypothesis evolved into the "debt covenant" hypothesis.

2.2 THE DEBT COVENANT HYPOTHESIS

Subsequent studies test DCH using covenant technical defaults as a proxy for closeness to covenant thresholds. Sweeney [1994] uses a sample of 130 technical defaults and examines whether firms that default made income-increasing accounting choices in the period leading up to their technical default. DeFond and Jiambalvo [1994] use a sample of 94 technical defaults and examine whether firms report positive abnormal accruals leading up to technical default. Both studies yield findings that support DCH, with either income-increasing accounting choices or positive abnormal accruals in the period that precedes and includes the violation.[3] The inferences from these studies are limited, however, by the use of realized technical defaults as a proxy for closeness to covenant thresholds, a research design choice necessitated by a lack of available financial covenant data.

[3] DeAngelo, DeAngelo, and Skinner [1994] examine a sample of 76 companies with persistent losses and find no evidence of earnings management to loosen covenants that constrain dividends.

The lack of available covenant data to test DCH was alleviated with the introduction of the LPC/Dealscan database (Dealscan) in the late 1990s. Dealscan provides detailed information on private bank loans in a machine-readable format. The database includes loan amounts, maturities, interest spreads, and data on covenants. For financial covenants, the database provides the type of covenant, the threshold, and some limited data on covenant threshold changes and build-up over time.[4] Despite the possibility of a direct test of DCH using Dealscan data, there were deficiencies in the data that limited its use by researchers. As DS discuss, Dealscan provides a general category for each financial covenant but does not provide detailed measurement information. For example, Dealscan may classify a financial covenant as "minimum interest coverage." Interest coverage is typically defined as earnings divided by interest expense, but Dealscan does not provide precise measurement detail; for example, earnings could be net income, EBIT, EBITDA, or some other variant. This potential heterogeneity resulted in researchers not considering many of the covenant categories in Dealscan when examining DCH.

DS acknowledge this issue and identify two covenants with definitions they considered "standardized and relatively unambiguous," the minimum current ratio and minimum net worth. Confident of the definitions used in practice, DS were able to, for the first time in the literature, precisely measure covenant slack (the difference between the actual value of the covenant metric and the contracted threshold). The authors sort covenant slack observations into bins in a similar fashion to Burgstahler and Dichev [1997] and examine the resulting density distributions for evidence of discontinuities around the violation threshold. DS's findings provide support for DCH: The distributions reveal an unusually large number of observations in bin 0 (the bin just above the covenant threshold) and an unusually small number in bin –1 (the bin just below the covenant threshold). DS provide the most robust findings in support of DCH, and their study is considered seminal.

2.3 MOTIVATION: THE NEED TO REEXAMINE EVIDENCE OF THE DCH

Although the pioneering work of DS provides robust evidence of DCH, we believe there are several reasons to reexamine their findings. First, studies that examine discontinuities using histograms require research design choices. Varying these choices can provide insight into the robustness of the findings. Second, there is no consensus measurement of the key variable for DCH, covenant slack. DS use one measurement, but there are others that are potentially valid and supported in the literature. Finding support for DCH under different measurements of slack would strengthen the

[4] Li, Vasvari, and Wittenberg-Moerman [2016] show that nearly half of syndicated loans have covenant thresholds that tighten (i.e., become more restrictive) over time. Beatty, Weber, and Yu [2008] find that escalator provisions—provisions where net income and equity issuance proceeds "build up" the thresholds of net worth covenants—are pervasive in practice.

inferences in support of the hypothesis. Third, there have recently been innovations in the statistical testing of histogram discontinuities by Byzalov and Basu [2019] that allow us to further assess the validity of past findings. Fourth, the findings in DS are from the period 1989–1999. There have been considerable economic and financial reporting changes, however, since this period. The passage of the Sarbanes–Oxley Act, the global financial crisis of 2007 and 2008, and the rise of securitization have potentially shifted the incentives and ability of borrowers to manage financial reporting outcomes to avoid debt covenant violation. We believe that replicating and extending DS's research, taking into consideration these factors, will further our understanding of the influence of DCH on managerial behavior.

3. DS Replication

To reexamine the evidence in support of DCH, we start by attempting to replicate DS. In subsection 3.1, we describe our procedure to replicate their sample, including some data constraints and how we address them. In subsection 3.2, we discuss the DS research design, and in subsection 3.3, we present our replication results, including descriptive statistics of our replication samples and discontinuity evidence from histograms.

3.1 SAMPLE

The sample in DS includes loans initiated between 1989 and 1999. DS collected loan data from Dealscan, which at the time of their study had data for approximately 60,000 loans.[5] Dealscan provided certain loan details, including the loan amount, interest rate, maturity, and covenants. In terms of financial covenant data, Dealscan provided the covenant category (12 different categories in total) and the minimum or maximum threshold at the time of loan initiation. The database did not provide precise measurement data beyond the covenant category, so, as we discuss earlier, DS limit their analysis to the current ratio and net worth covenants.[6] Dealscan also provided limited details on ex ante negotiated changes in the covenant threshold that follow loan initiation, which we describe in the next section.

The tests of DCH require accounting information to measure quarterly covenant compliance; thus, DS match Dealscan loan data with quarterly accounting data from Compustat. Because there was no consistent identifier between the two databases,[7] the authors used tickers included in the

[5] As we describe in subsection 3.3, aspects of Dealscan coverage have changed since DS. In this section, we discuss items specific to the iteration of the database that DS used in the past tense, as in "this database *had* 60,000 loans."

[6] DS also note that, based on the evidence available at the time (Beneish and Press [1993], Chen and Wei [1993], Sweeney [1994]), these covenants were also the most likely to be violated, which increased the power of the analysis.

[7] Recent work, including our replication, uses the linking table provided by Chava and Roberts [2008], now provided by Wharton Research Data Services (WRDS).

Dealscan loan record to match with Compustat. The authors also completed a "manual match" of tickers between the databases; this was necessary when Compustat made slight changes to companies' stock tickers.

3.2 RESEARCH DESIGN

The analysis in DS follows Burgstahler and Dichev [1997] by identifying discontinuities in distributions. Three elements are required in this style of analysis. First, the researchers must select a benchmark whereby the discontinuity is predicted to exist. Second, the researchers must select bin widths for the histograms. Third, there must be a test statistic to assess the significance of any discontinuity that is identified.

The variable of interest in each test is the distance between the covenant threshold and the actual quarterly realization of the metric, which DS operationalize as *covenant slack*. DS measure slack differently depending on the covenant. For the current ratio, slack is measured as the actual value of the ratio minus the threshold, with no scaling. For net worth, slack is measured as the actual net worth minus the threshold, all scaled by total assets.[8] The scalar allows for comparisons of slack between companies of different sizes. Larger values of slack indicate a borrower further from technical default, while lower values indicate closer proximity. A negative value of slack indicates that the borrower has violated the financial covenant. DS use a slack of zero as their discontinuity benchmark.

As discussed in Burgstahler and Dichev [1997], the selection of a histogram bin width is important for the detection of discontinuities. The selection trades off precision and fineness: It requires bins that are not so wide as to fail to detect subtle discontinuities and not so narrow as to be susceptible to statistical noise. DS use a width of 0.2 for the current ratio sample and 0.045 for net worth.

DS test the statistical significance of the observed discontinuities using the *standardized difference* developed in Burgstahler and Dichev [1997]. Under the null hypothesis of a smooth distribution, the standardized difference is calculated as the difference between the bin under study and the average of the adjacent bins scaled by the estimated standard deviation of the difference.[9] DCH predicts an unusually low number of observations in bin –1 (the bin with slightly negative slack, i.e., those observations that just miss their covenant thresholds) and an unusually high number of observations in bin 0 (the bin with slightly positive slack, i.e., just barely in compliance with covenants).

---

[8] Following DS, we scale by the total assets for the quarter immediately following loan initiation so that the total assets include the loan proceeds. We use this total asset figure as the scalar for all subsequent quarterly measurements of net worth slack.

[9] The estimated total variance of the difference is $n\sigma_0(1-\sigma_0) + \frac{1}{4}\ [n\ (\sigma_{-1} + \sigma_1)\ (1 - (\sigma_{-1} + \sigma_1))]$, where n is the number of observations, and σ_k is the number of observations in bin k divided by n.

3.3 REPLICATION

3.3.1. Sample Description. We face two challenges in replicating the results of DS: sample reproduction and slack measurement. With regard to sample reproduction, there have been several changes to the Dealscan database that make it impossible to precisely replicate their sample. We next describe their procedure and the additional steps we took, beginning with the current ratio sample and followed by the net worth sample.

When DS accessed loan data on Dealscan, the database was partitioned by an "automated search feature"; loans after 1993 were included when the search feature was used, while earlier loans were not. To collect the latter portion of their sample, DS used the automated search feature and manual ticker matching, yielding a sample of 805 loans. The authors supplemented this sample with a manual search of Dealscan; this search yielded 508 additional loans, for a total of 1,313. The more current version of Dealscan, which we use in our study (accessed via WRDS on April 10, 2020), includes very few loans initiated prior to 1993. That is, the "manual search" portion of the DS current ratio sample is, for the most part, not accessible to us.[10] Relative to DS's current ratio sample, our current ratio sample has fewer observations (955 loans), and these observations are concentrated in the later part of the DS sample period.

When DS collected their net worth sample, they required loans to have data on TearSheets. TearSheets was a supplemental data set that covers a portion of the Dealscan database (typically, "bellwether" loans) that provided detailed covenant information, including information on ex ante negotiated threshold changes. The requirement to have a TearSheets record significantly reduced the DS net worth sample size. Following a similar process as for their current ratio sample (and requiring TearSheets data) yielded a sample of 288 loans. TearSheets is no longer attached to Dealscan, so we cannot apply this sample restriction. By not limiting our sample to the sparser coverage of Tearsheets, we collect a considerably larger net worth sample than did DS.[11] We also note that this sample is mostly composed of loans from 1993 onward for the same reason as the current ratio sample.

The second challenge in replicating DS is identifying the covenant threshold to calculate slack. DS describe how the thresholds of some current ratio covenants change over time and note that they "manually search for such adjustments and adjust our data accordingly."[12] Similar to DS, in our replication, manual adjustment of thresholds is necessary due to limitations of Dealscan: the database indicates the initial level of the threshold and the final level, but it does not indicate the intermediate steps in the

[10] In section 1 of the online appendix, we document our sample loan observations by year.

[11] As we describe later in this section, we hand-collect contract details for net worth covenants from SEC filings. This hand-collected data substitutes for the Tearsheets data that we cannot access and yields a larger sample than DS.

[12] DS do not indicate how many current ratio covenants have dynamic thresholds and from where they collect the information they use for threshold adjustments.

threshold. To emulate the DS adjustment, we hand-collect the loan terms from publicly available filings with the SEC. Private debt agreements, such as the ones that we study here, are considered material definitive agreements and are subject to mandatory disclosure. In many cases, the borrower submits the actual loan contract as an exhibit along with a filing.

We first identify all observations with a current ratio covenant in our replication sample that Dealscan indicates as having a dynamic covenant. Of the 955 loans in our sample, 98 have dynamic thresholds.[13] We search the SEC EDGAR system using the loan start date from Dealscan to search for relevant filings. Ideally, we find the necessary contract in an 8-K filed near the loan inception date, but we examine subsequent 10-Q, 10-K, and registration statements (S-3) if we do not find an 8-K with the information. Because the sample predates SEC Rule 33–8400, which mandates the timely disclosure of material contracts in 8-K filings, most of the contracts we collected are from 10-K or 10-Q filings. As detailed in Caskey, Huang, and Saavedra [2021], even after Rule 33–8400 mandated timely 8-K disclosure, only four out of five loans are disclosed in a timely manner, and some are not disclosed at all. We find contract details for approximately 80% (78/98) of loans with dynamic current ratio thresholds. Notably, all 78 loans for which we collect contract details have thresholds that increase over time. This suggests that exclusion of dynamic threshold information introduces systematic upward bias into the slack calculation. We record these specific covenant thresholds by quarter, from loan inception to maturity. We also randomly test from the static-threshold current ratio covenant sample to confirm the accuracy of Dealscan's data collection process. We conclude that Dealscan reports static covenant thresholds accurately.

DS also describe adding "build up" provisions—including cumulative positive net income and equity issuance proceeds—to the thresholds of net worth covenants using data from Tearsheets. We follow their adjustment method but use data from hand-collected SEC filings. For our initial sample of 1,112 loans with net worth covenants between 1989 and 1999, we find loan contract details for 92% (1,024) from SEC filings, including 10-K, 10-Q, and 8-K filings. We find that 310 net worth covenants have net income escalators, whereby a percentage of net income is added to the covenant threshold each quarter. Of these, most (272) add only positive net income, while the other 38 add either positive or negative net income. We also observe escalators that add the proceeds of equity issuances to the net worth threshold in 65 loans; most of these (52) accompany a net income escalator. We find that net income and equity proceeds escalators range from 10% to 100%, with modal values of 50% and 100%, respectively. Finally, we find that 13 net worth covenants have dynamic thresholds that increase by a fixed amount (i.e., not conditional on net income or equity issuance

[13] We note that we retain fewer observations in the analysis due to missing variables from Compustat.

proceeds). Once we verify the escalator percentage, we multiply it by the appropriate value from Compustat (NIQ for income escalators and SSTK for equity escalators) and add this product to the baseline amount to estimate the actual realization of net worth to calculate slack. Escalator clauses accumulate over time, so we make similar calculations over the full term of the loan.

We draw a few conclusions from this hand-collection process. First, the Dealscan data generally identify the correct threshold at contract inception but provide insufficient information to determine the quarterly changes, when present, in thresholds. We find that the Dealscan final threshold level tends to be accurate for the current ratio sample. Our verification process, however, identified many inconsistencies between the net income and equity issuance proceeds escalators recorded in Dealscan and those we found in disclosed contracts for the net worth sample. Accordingly, we hand-collect data on all net worth covenant observations, not just those listed as having build-up in Dealscan. We urge researchers to exercise caution when relying on Dealscan to calculate nonstatic covenant thresholds. Second, the idiosyncratic nature of dynamic covenant thresholds impedes systematically programming quarter-by-quarter thresholds for data analysis. That is, within the dynamic covenant set in our sample, it appears that lenders do not use boilerplate terms. The nature of these dynamic covenants can vary along dimensions of the timing, pattern, and magnitude of the change in threshold. For example, some loans show a systematic tightening each quarter, while others show a delay before any tightening occurs, and yet others reveal seasonal patterns. Thus, hand-collection and verification are essential steps to appropriately measure dynamic covenant thresholds.[14]

We begin our analysis by assessing whether sample selection differences yield substantively different sample characteristics from those reported in DS. In table 1, we reproduce table 2 from DS, which provides sample median values on loans and borrowers by covenant type. In panel A, we report column 3 from DS's table 2 ("Current ratio sample") with similar statistics reported for our current ratio sample. Based on the reported statistics, the samples have similar median values for the reported variables. Compared to DS, borrowers in our sample have slightly higher *total assets* ($113 vs. $107, in millions), slightly lower *leverage* (0.27 vs. 0.30), and an almost identical *market-to-book ratio* (2.04 vs. 2.01). In terms of loan features, we report similar *loan maturity* (36 vs. 34 months) and *loan amount* ($22.5 vs. $23, in millions). The largest difference is in the ratio of the loan size to long-term debt (0.98 vs. 0.78). Although we lack sufficient statistical data from DS to

[14] In section 2 of the online appendix, we provide excerpts from disclosed contracts that contain static and dynamic threshold patterns in financial covenants. The examples include the current ratio with a static threshold, current ratio with a monotonically tightening dynamic threshold, current ratio with a seasonally varying threshold, net worth with a static threshold, net worth with net income and equity issuance escalator clauses, and net worth with a predetermined "step-up" pattern.

TABLE 1
Descriptive Statistics

Panel A: Current ratio sample

	DS Sample	Replication Sample
Total assets	107	113
Leverage	0.30	0.27
Market-to-book ratio	2.01	2.04
Loan maturity	34	36
Loan amount	23	22.5
Loan amount/ total debt	0.78	0.98
Observations	1,313	894

Panel B: Net worth sample

	DS Sample	Replication Sample
Total assets	786	412
Leverage	0.33	0.34
Market-to-book ratio	2.19	2.02
Loan maturity	33	37
Loan amount	250	65
Loan amount/total debt	0.83	0.54
Observations	288	827

In this table, we provide descriptive statistics for two subsamples, loans with current ratio covenants (panel A) and loans with net worth covenants (panel B). Variables include *total assets* (Compustat: ATQ), *leverage* ((DLTTQ + DLCQ)/ATQ), *market-to-book ratio* ((CSHOQ*PRCCQ) + (DLTTQ + DLCQ)/ATQ), *loan maturity* in months, *loan amount* in millions of dollars, the ratio of *loan amount to total debt*, and the number of observations in the subsample. We include data reported in DS drawn from their tables 1 and 2 (DS sample) and our corresponding values (replication sample); all reported values are medians.

measure whether these differences are statistically meaningful, our current ratio sample resembles that used by DS.

In panel B, we report column 4 from DS's table 2 ("Net worth sample") with a comparison to statistics from our replication. The median borrower in our sample is smaller based on *total assets* ($412 vs. $786, in millions) but has similar *leverage* (0.34 vs. 0.33) and *market-to-book ratio* (2.02 vs. 2.19). This is not surprising because Tearsheets included bellwether firms that are larger, while our collection of contract details from SEC filings likely introduced smaller borrowers into our sample. This is borne out in the loan features: Although our maturities are similar (37 vs. 33 months), the average loan in our sample is smaller ($65 vs. $250, in millions), leading to a lower ratio of loan to long-term debt (0.54 vs. 0.83). Although we do not have reason to think that smaller borrowers with smaller loans have different incentives to maintain net worth covenant thresholds, we are careful to note that our net worth sample differs from that of DS, and the results we report must be interpreted with this caveat.[15]

[15] To further examine the implications of sample differences, we replicate our main analysis using the 288 largest loans with net worth covenants (to mirror the DS sample size). The results, reported in section 3 of the online appendix, show that this sample yields summary

TABLE 2
Replication Results

Panel A: Current ratio

	[1]		[2]		[3]		[4]		[5]	
	Bin -1	Bin 0	Bin -1	Bin 0	Bin -1	Bin 0	Bin -1	Bin 0	Bin -1	Bin 0
Actual	380	771	160	557	220	214	93	339	67	218
Predicted	523	617	311	444.5	212	172.5	193.5	228.5	117.5	216
Error	−143	154	−151	112.5	8	41.5	−100.5	110.5	−50.5	2
Standardized Diff.	−5.9***	5.0***	−8.3***	4.3***	0.5	2.6***	−7.6***	5.6***	−4.6***	0.1
DS Standardized Diff.	−3.5***	8.0***	−12.5***	7.4***	n.r.	n.r.	−7.6***	7.7***	−10.3***	2.9***
Observations	6,871		5,460		1,411		2,876		2,584	
DS Observations	13,052		9,546		3,506		4,298		5,248	

Panel B: Net worth

	[1]		[2]		[3]		[4]		[5]	
	Bin -1	Bin 0	Bin -1	Bin 0	Bin -1	Bin 0	Bin -1	Bin 0	Bin -1	Bin 0
Actual	315	796	107	715	208	81	70	491	37	224
Predicted	495.5	688.5	381.5	575	114	113.5	262	352.5	119.5	222.5
Error	−180.5	107.5	−274.5	140	94	−32.5	−192	138.5	−82.5	1.5
Standardized Diff.	−8.0***	3.5***	−16.8***	4.8***	6.4†††	−3.0†††	−14.6***	6.0***	−8.7***	0.1
DS Standardized Diff.	−10.8***	8.9***	−16.6***	9.8***	n.r.	n.r.	n.r.	n.r.	n.r.	n.r.
Observations	6,122		4,978		1,144		2,613		2,365	
DS Observations	2,339		1,900		439		1,016		884	

In this table, we present replication results of DS. Panel A includes the results from testing the current ratio sample. We present the results for the bin to the left of the threshold (bin –1) and the bin to the right of the threshold (bin 0). The results include the number of observations in the bin (*Actual*), the average of the bin to the left and right (*Predicted*), the difference between the two (*Error*), the standardized difference of the error (*Standardized diff.*), and the total number of observations in the histogram (*Observations*). We generate histograms for five samples: [1] all observations, [2] all observations up to and including the first violation, [3] all observations following the first violation, [4] all observations up to and including the first violation that occur during the first year of the contract, and [5] all observations up to and including the first violation that occur after the first year of the contract. We also report the standardized difference and observations from DS from the corresponding histogram (when available) for comparison purposes. We report similar statistics testing the net worth covenant in panel B. *** indicates statistical significance at the 1% level; ††† indicates statistical significance in the opposite of the predicted direction at the 1% level.

3.3.2. Histogram and Statistical Evidence. DS tests DCH in the style of Burgstahler and Dichev [1997] by visually inspecting the histograms and statistically testing for discontinuities under the null hypothesis of smoothness. DS generate five histograms for each covenant type using the following five samples:

1) All available observations.
2) Observations up to and including the first violation.
3) Observations following the first violation.
4) Observations up to and including the first violation that occurs *during* the first year of the contract.
5) Observations up to and including the first violation that occurs *after* the first year of the contract.

We replicate these five histograms for the current ratio and net worth samples and present them in figures 1 and 2. The histograms yield inferences that are consistent with those of DS. In figure 1(a), which provides histogram data for all available observations of the current ratio covenant, we find a discrete increase in the number of observations from bin –1 (380) to bin 0 (771). When restricting our scope to observations up to and including the first violation, the jump is even more dramatic, from 160 to 557 (figure 1(b)). Similar to DS, we find little support for DCH when examining observations after the first violation; there are nearly the same number of observations, 220 and 214, in bins –1 and 0, respectively (figure 1(c)). In figures 1(d) and (e), we partition observations up to and including the first violation into those during the first year of the life of the loan and those after the first year. Similar to DS, we find discontinuities in both (figure 1(d): 93 to 339; 1(e): 67 to 218). In total, our replicated histograms provide inferences consistent with those of DS, despite sample selection differences.

In figure 2, we provide similar histograms for net worth covenants. Our findings here are also consistent with those of DS in that we show more pronounced discontinuities than for corresponding specifications for the current ratio sample. In figure 2(a) (all observations), observations between bin –1 and bin 0 increase from 315 to 796; for observations up to and including the first violation, the increase is larger, from 107 to 715 (figure 2(b)). Similar to DS, when looking at observations after the first violation, we find *more* observations in bin –1 than in bin 0 (208 to 81, respectively, figure 2(c)). Figures 2(d) (70 to 491) and (e) (37 to 224) show patterns similar to those in DS and consistent with those in DCH.

To compare the statistical magnitude of our results with those reported in DS, we tabulated the standardized differences from the histograms in table 2. We report results for the current ratio sample in panel A. For each of the five histograms, we test bin –1 and bin 0 separately, including the

statistics closer to DS. The histogram and statistical results yield similar inferences as our main sample.

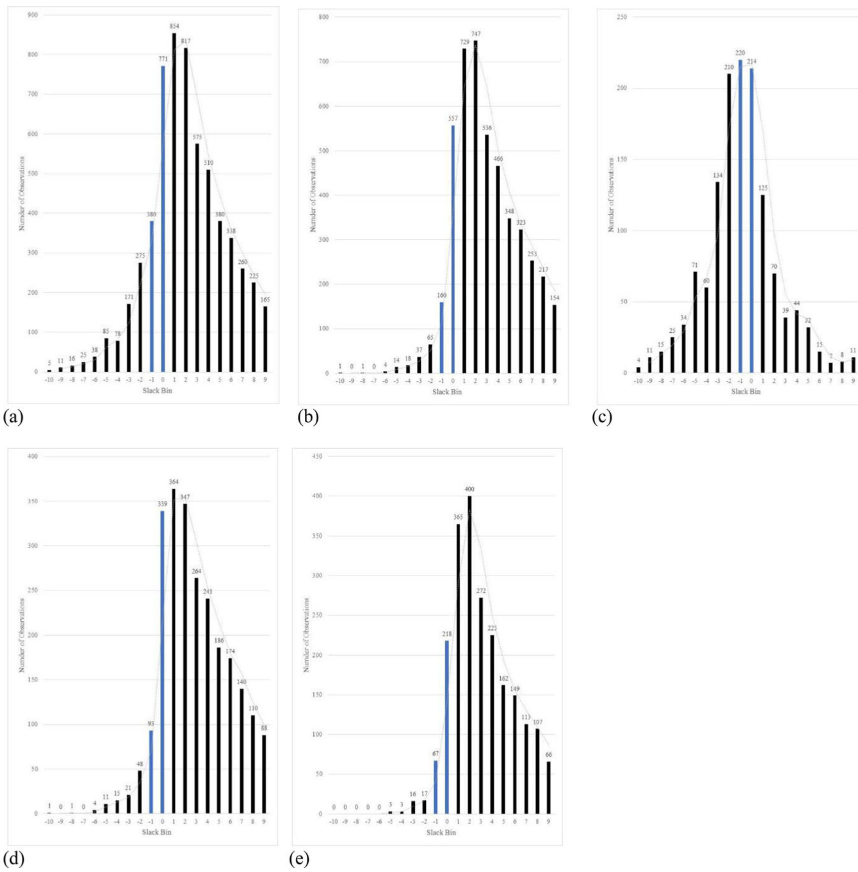

FIG 1.—Current ratio. This figure presents histograms of current ratio slack for the period 1989–1999. We report figures for five samples: (a) all observations, (b) all observations up to and including the first violation, (c) all observations after the first violation, (d) all observations up to and including the first violation in the first year of the contract, and (e) all observations up to and including the first violation after the first year of the contract. The bins of interest for DCH are shaded blue.

number of observations in the bin (*Actual*), the average of the observations in the adjacent bins (*Predicted*), the difference (*Error*), the test of the statistical magnitude of the error (*Standardized Difference*), and the observations for the test. We also report standardized differences and observations from DS for comparison purposes, although in some cases, DS did not report the standardized difference when the histogram visually suggested no discontinuity.[16] Using the findings from the DS of statistically significant discontinuities in all histograms except the third (observations after the

[16] This includes the third histogram for current ratio and the third, fourth, and fifth for net worth.

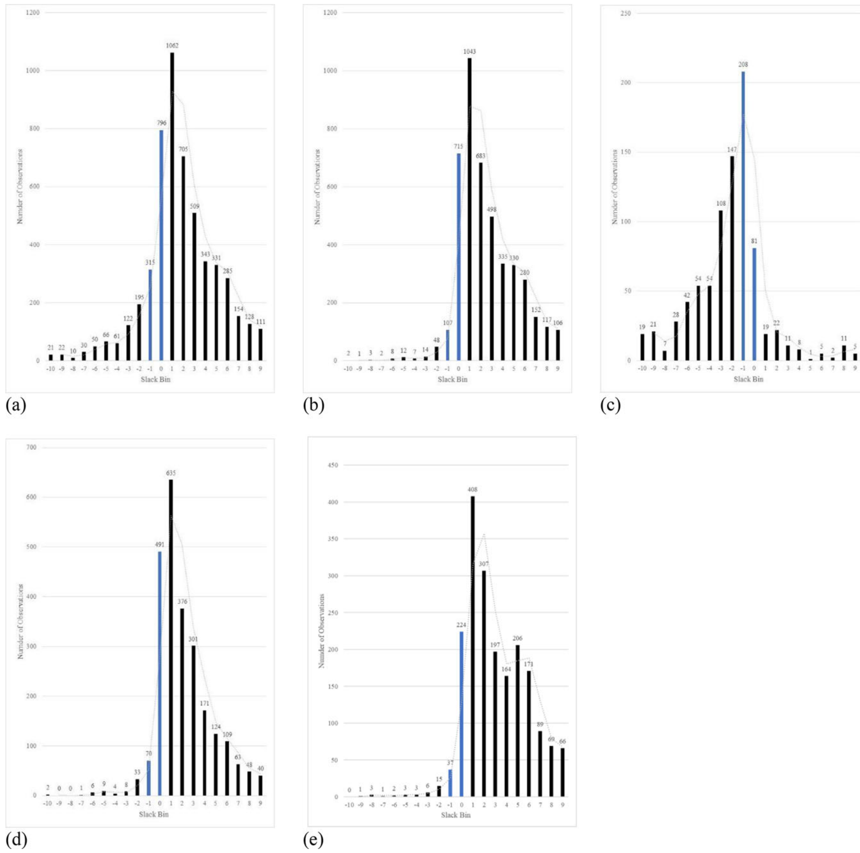

FIG 2.—Net worth. This figure presents histograms of net worth slack for the period 1989–1999. We report figures for five samples: (a) all observations, (b) all observations up to and including the first violation, (c) all observations after the first violation, (d) all observations up to and including the first violation in the first year of the contract, and (e) all observations up to and including the first violation after the first year of the contract. The bins of interest for DCH are shaded blue.

first violation) as a benchmark, our statistical tests yield findings that are consistent with those of the DS. For bin –1, we find negative, significant standardized differences in all histograms except the third. For bin 0, where we expect a positive coefficient, our results are also consistent with DS. The only exception is column 5 (all observations up to and including the first violation, after the first year of the contract), where DS finds a significant statistic (2.9) and ours is insignificant (0.1). On balance, our results of the current ratio covenant sample are consistent with those of DS and support DCH.

We present statistics for net worth in table 2, panel B. DS do not report statistics for the final three partitions due to a small number of observations and unevenness in the histograms. With our larger sample size, we calculate and report the statistics. Our evidence, again, is consistent with that of DS. We find negative, significant standardized differences for bin –1 for all the histograms except the third. For bin 0, we find the expected positive differences for all but the third, although column 5 is insignificant. The third histogram, including all observations after the first violation, reveals a pattern of statistics the opposite of that predicted by DCH; that is, we find a positive, significant statistic in bin –1 and a negative, significant coefficient in bin 0. These findings confirm the idea from DS that once a borrower has violated a covenant, it is increasingly difficult and costly to avoid future violations (and, thus, the borrower lacks the ability and incentive to do so). Figures 1 and 2 and table 2 show that, even with possible differences in samples, our replication of DS shows support for DCH.[17]

4. *Research Design and Sample Period Extensions*

Having replicated DS as closely as possible, we turn our attention to the sensitivity of these findings to alternative research design choices. Our extensions vary in three aspects of the research design: bin width, calculation of slack, and the statistical test of significance of the discontinuities. We also examine the results for an out-of-sample period, from 2000 to 2019. In evaluating the influence of each of these alternatives, we use the findings from our replication of DS as a benchmark, focusing on the subsample of observations up to and including the first covenant violation (reported in column 2 of table 2). As discussed in DS and highlighted in Burgstahler and Chuk [2017], evidence of a discontinuity around a threshold requires both a peak (in bin 0) and a trough (in bin –1) in the distribution. We interpret findings of a discontinuity for one, but not both, bin as partial, relatively weak evidence in support of DCH.

4.1 BIN WIDTH

As discussed in DS, there is no precise method for calculating the "correct" bin width; bin width selection is fundamentally an ad hoc process. To summarize the discussion in DS, bin width requires balancing the signal and the noise that the distribution provides.[18] Overly narrow bin widths lead to weak signals, for which the interval excludes too many observations whereby the borrower has an incentive to avoid violation. Overly wide bin widths, which include observations unrelated to covenant management incentives, introduce noise. Bin width selection should include the researcher's subjective assessment of the economic reasonableness of these assumptions. In other words, it must be reasonable to consider that a manager whose firm is X% below a threshold could and would manage

[17] DS also assess the statistical significance of their results using randomization tests (see DS table 4). We reproduce these tests and find results, reported in section 4 of the online appendix, consistent with our replication results.

[18] They discuss this as balancing "fineness" and "precision."

the covenant metric to get above the threshold and that a slack value Y% above the threshold could represent a plausible outcome of covenant management behavior. Assessing the distributional evidence under alternative bin width specifications that consider these factors can help to strengthen any inferences that can be drawn on DCH.

DS use bin widths of 0.2 for current ratio and 0.045 for net worth. In our extension, we examine the implications of narrower and wider bin widths on our test of DCH. Using the DS bin widths as a base, we add and subtract half of their bin width; that is, we use bin widths of 0.1 (narrow) and 0.3 (wide) for the current ratio and bin widths of 0.0225 (narrow) and 0.0675 (wide) for net worth. We report these results in the first two columns of table 3. In panel A, where we report current ratio results, column 1 (DS Slack: Narrow) shows partial support for DCH; the standardized difference for bin –1 is negative and significant, but the standardized difference for bin 0 is insignificantly different from zero. In column 2 of panel A (DS Slack: Wide), we present the results for the current ratio using wide bin widths. Here, our results robustly support DCH, with a significantly negative (positive) standardized difference in bin –1 (bin 0).

In panel B, we present similar tests for net worth covenants. The results mirror those in panel A, with evidence weakly supporting DCH for the narrow bin width and strongly supporting it for the wide bin width. The key inference from these results is that increasing bin width leads to greater statistical significance in the measured standardized differences. This is due, in large part, to the positive skew in the distribution of slack for both current ratio and net worth (see figures 1 and 2). Positive skew means a greater density of observations in the bins to the right of bin 0 than to the left. As the bin widths expand, the observations in bin 1 increase dramatically relative to bin 0, resulting in stronger evidence of a discontinuity.[19]

We also considered using the approach of Degeorge, Patel, and Zeckhauser [1999], which builds on the intuition that bin width should be negatively related to sample size but positively related to the variability of the underlying data. Using the Degeorge et al. formula yields bin widths of 0.12 (current ratio) and 0.024 (net worth); both are very close to the narrow bin width amount. The analysis that we report in section 5 of the online appendix shows that histograms that use these bin widths yield inferences consistent with the narrow bin width results that we report in table 3, column 1.

[19] In an untabulated analysis, we used bin widths that double the DS figures (0.4 for current ratio and 0.09 for net worth.) These results show robust and highly significant evidence of discontinuities for both covenants.

TABLE 3
Research Design Extensions

Panel A: Current ratio

	[1] DS Slack: Narrow Bin Width = 0.1		[2] DS Slack: Wide Bin Width = 0.3		[3] $Slack_T$: Bin Width = 0.086		[4] $Slack_T$: Wide Bin Width = 0.172	
	Bin -1	Bin 0	Bin -1	Bin 0	Bin -1	Bin 0	Bin -1	Bin 0
Actual	92	214	202	926	101	253	179	669
Predicted	141	217.5	493	654.5	165.5	258.5	366.5	524
Error	−49	−3.5	−291	271.5	−64.5	−5.5	−187.5	145
Standardized Diff.	−3.9***	−0.2	−14.6***	8.5***	−4.8***	−0.3	−10.3***	5.1***
Observations	5,460		5,460		5,460		5,460	

Panel B: Net worth

	[1] DS Slack: Narrow Bin Width = 0.0225		[2] DS Slack: Wide Bin Width = 0.0675		[3] $Slack_T$: Bin Width = 0.084		[4] $Slack_T$: Wide Bin Width = 0.168	
	Bin -1	Bin 0	Bin -1	Bin 0	Bin -1	Bin 0	Bin -1	Bin 0
Actual	70	267	133	1,286	69	285	111	841
Predicted	152	259	661	644	163.5	312.5	446.5	596.5
Error	−82	8	−528	642	−94.5	−27.5	−335.5	244.5
Standardized Diff.	−6.9***	−0.4	−27.4***	18.6***	−7.9***	−1.4	−19.6***	8.0***
Observations	4,978		4,978		4,978		4,978	

In this table, we present the results of research design extensions. Panel A includes results testing the current ratio sample, and panel B includes results testing the net worth sample. We present the results for the bin to the left of the threshold (bin –1) and the bin to the right of the threshold (bin 0). The results include the number of observations in the bin (*Actual*), the average of the bin to the left and right (*Predicted*), the difference between the two (*Error*), the standardized difference of the error (*Standardized diff.*), and the total number of observations in the histogram (*Observations*). We report statistics for four research design extensions: [1] DS Slack: Narrow calculates bin width subtracting half of the DS bin width, [2] DS Slack: Wide calculates bin width adding half of the DS bin width, [3] $Slack_T$ calculates slack as the actual covenant metric value minus threshold, all scaled by the threshold, and uses bin width based on the formula in Degeorge et al. [1999], and [4] $Slack_T$: Wide calculates bin width as double the bin width in column 3. *** indicates statistical significance at the 1% level.

4.2 SLACK MEASUREMENT

In this section, we test the sensitivity of the results in DS using a threshold-scaled measure of slack from El-Gazzar and Pastena [1991]:

$$\text{Slack}_{\text{T}} = (\text{actual value} - \text{threshold value}) / \text{threshold value}$$

Unlike the measures used in DS, this formula is general and can be applied to any minimum threshold covenant (including the current ratio, even though it already features scaling); it converts slack into a percentage above the threshold.

We note that because this definition of slack is different, we cannot use the bin widths set in DS. Rather, we use the formulaic bin widths, following Degeorge, Patel, and Zeckhauser [1999]. We note above that the formulaic approach results in narrower bin widths than those used in DS; for example, using the DS definition of current ratio slack, the formula yields a bin width that is approximately half the width of DS. This suggests that using Slack_{T} and the formulaic bin width comprises a joint test. To isolate the effects of slack calculation only, we report results for two bin widths. The first is the formulaic bin width, following Degeorge et al. For the second, we double this bin width. This is our effort to have test results that serve as direct analogues to the results reported in DS.

Table 3 presents our results using Slack_{T}, with panel A providing the current ratio findings. The results in column 3 (Slack_{T}), using the formulaic approach, mirror the results in column 1 closely; the number of observations in bin –1 are abnormally low, consistent with DS, while bin 0 does not show statistically significant standardized differences. In column 4 (Slack_{T}: Wide), the results where we have doubled the formulaic bin width provide strong support for DCH, with a negative and positive standardized difference for bin –1 and bin 0, respectively. These results, analogous to those reported in column 2 of table 2, panel A, are consistent with DS.[20]

In table 3, panel B, columns 3 and 4, we report results for the net worth sample using formulaic and double-formulaic bin widths for Slack_{T}. Similar to the current ratio results, the results in column 3 mirror the narrow-bin width results in column 1 (weakly supportive of DCH, with a significant difference for bin –1 but an insignificant difference for bin 0), while the results in column 4 strongly support DCH and mirror the replication results in table 2, panel B, column 2. Considering these results, particularly those in column 4, we believe that the results in DS are robust to a threshold-scaled measure of slack.

[20] In our replication of DS, the second specification tests a total of 717 observations: 160 in bin –1 and 557 in bin 0. The analysis using Slack_{T} and formulaic bin width tests a total of 354 observations (101 in bin –1 and 253 in bin 0.) When we double the bin width, this increases to 848 observations (179 in bin –1 and 669 in bin 0), much closer to the total number of bins from our replication results. This further justifies the inclusion of the double-formulaic bin width to isolate the effect of slack formula. The same difference holds for net worth covenants.

TABLE 4
Research Design Extensions: Alternate Statistical Test

	[1] Current Ratio	[2] Net Worth
Coefficient	0.513***	0.827***
t-Statistic	6.92	9.45
Adjusted R^2	0.047	0.057
Observations	4,418	4,463

In this table, we report an analysis of the discontinuity in the distribution of the current ratio and net worth slack using the regression-based test of Byzalov and Basu [2019]. We report the coefficient on the "kink" in the regression; a positive coefficient indicates a significant discontinuity where there are disproportionately few (many) observations below (above) the threshold. We also report the statistical significance of the coefficient (*t*-statistic), the adjusted R^2, and the observations. We measure slack and bin width as in DS and use the period 1989–1999. *** indicates statistical significance at the 1% level.

An alternative method of calculating slack is to scale the difference between the actual value and the covenant threshold by the borrower-level, time-series variability of the covenant metric (Demerjian and Owens [2016], Murfin [2012]). We consider this measurement but opt not to include it in this study. Our reasoning is that variance-scaled slack may conceptually indicate closeness to covenant violation, but it is less likely to be salient to borrowers than is a threshold-scaled measure (such as Slack_T). Although variance-scaled slack makes sense for researchers who attempt to understand financial covenant strictness, we believe that Slack_T better measures borrowers' incentives to avoid covenant violation.

4.3 STATISTICAL TEST

Our analysis up until this point has assessed the statistical significance of the standardized differences for bins –1 and 0 using the method outlined in Burgstahler and Dichev [1997]. To test the sensitivity of the results to this testing method, we use a recently developed method from Byzalov and Basu [2019]. Byzalov and Basu use local polynomial approximation to model whether distributions are smooth. Because this is meant to capture a discontinuity in the subject distribution, the regression is a joint test of bin –1 and bin 0. The results from the estimation include coefficient estimates for each polynomial term and a coefficient estimate for the "kink" in the distribution, where a positive coefficient indicates a disproportionately low number of observations in bin –1 and a disproportionately high number of observations in bin 0. We note, however, that the Byzalov and Basu method requires several parameter choices, introducing substantial judgment on the part of the researcher. We therefore urge caution in interpreting results using this method, particularly in economic terms. In section 6 of the online appendix, we describe this method in more detail, including alternate parameter choices and result sensitivity.

We report the estimation results in table 4. Following our other research design extensions, we report the results only for observations up to and including the first covenant violation. We use the definitions of slack and bin widths from DS. We show the estimation results for the current ratio in column 1. The coefficient is 0.513, which is positive and significant at the 1% level, indicating a disproportionately higher number of observations in bin 0 and a lower number in bin –1. In column 2, we report the results for the net worth estimation. This coefficient is also positive and significant (0.827). These findings suggest that the consistent results we observe in our replication are not an artifact of the statistical test employed by DS.

4.4 OUT-OF-SAMPLE REPLICATION

We next examine whether DCH holds in a more recent period, 2000–2019. We anticipate that significant economic and regulatory changes, such as the passage of Sarbanes-Oxley, the global financial crisis of 2007 and 2008, and the rise of loan securitization, could have affected borrowers' incentives and ability to avoid debt covenant violations. To be consistent with our prior extensions, we analyze loans from this period using five different research designs: the original assumptions of DS; slack calculated using their definitions with narrow and wide bin widths; and Slack_{T} with formulaic and wide bin widths. In each test, the sample includes observations up to and including the first covenant violation, so the results are comparable to those in column 2 of tables 2 and 3. We report these results in table 5, starting with the current ratio in panel A. The results in this table are consistent with DCH; in fact, we find negative, significant differences for bin –1 and positive significant differences for bin 0 in each specification. The evidence for DCH for the current ratio has become stronger since 2000.

We present results for net worth covenants in panel B. The results here provide less robust evidence of DCH: We find negative, significant differences in bin –1 in each specification but positive, significant differences in bin 0 for only two (the original DS assumptions in column 1 and wide bin width in column 3). This mirrors our extension results, where tests with narrow bin widths (columns 2 and 4) yield insignificant results for net worth for bin 0. In total, these results suggest that the findings in support of DCH are consistent in our out-of-sample period, remaining similar (net worth) or becoming stronger (current ratio).

5. Additional Analysis: Debt-to-EBITDA Covenants

5.1 EVIDENCE OF DCH FOR DEBT-TO-EBITDA COVENANTS

The rationale in DS for examining only current ratio and net worth covenants is their ease of measurement (to facilitate large sample measurement of covenant slack) and frequency of default (indicating economic importance of the covenants). However, current ratio and net worth covenant use have changed in the years since DS constructed their sample. In this section, we examine this change and conduct additional analyses with the

TABLE 5
Out-of-Sample Replication

Panel A: Current ratio

	[1]		[2]		[3]		[4]		[5]	
	DS Slack: Bin Width = 0.2		DS Slack: Narrow Bin Width = 0.1		DS Slack: Wide Bin Width = 0.3		$Slack_T$: Bin Width = 0.078		$Slack_T$: Wide Bin Width = 0.156	
	Bin -1	Bin 0	Bin -1	Bin 0	Bin -1	Bin 0	Bin -1	Bin 0	Bin -1	Bin 0
Actual	266	729	174	357	350	1184	154	314	237	668
Predicted	439.5	560.5	224.5	273	668.5	780.5	198.5	254	402	547
Error	−173.5	168.5	−50.5	84	−318.5	403.5	−44.5	60	−165	121
Standardized Diff.	−8.2***	5.7***	−3.1***	3.9***	−13.1***	11.4***	−2.9***	2.9***	−8.2***	4.2***
Observations	6,368		6,368		6,368		6,368		6,368	

Panel B: Net worth

	[1]		[2]		[3]		[4]		[5]	
	DS Slack: Bin Width = 0.045		DS Slack: Narrow Bin Width = 0.0225		DS Slack: Wide Bin Width = 0.0675		$Slack_T$: Bin Width = 0.045		$Slack_T$: Wide Bin Width = 0.09	
	Bin -1	Bin 0	Bin -1	Bin 0	Bin -1	Bin 0	Bin -1	Bin 0	Bin -1	Bin 0
Actual	271	1559	187	601	320	2,780	148	408	228	1039
Predicted	820.5	1386.5	342.5	572.5	1436.5	2144	244	389.5	565.5	1097
Error	−549.5	172.5	−155.5	28.5	−1116.5	636	−96	18.5	−337.5	−58
Standardized Diff.	−21.8***	3.9***	−8.3***	1.0	−37.0***	11.4***	−5.9***	0.8	−15.3***	−1.5
Observations	16,926		16,926		16,926		16,926		16,926	

In this table, we present the results for the period 2000–2019. Panel A includes results testing the current ratio sample, and panel B includes results testing the net worth sample. We present the results for the bin to the left of the threshold (bin –1) and the bin to the right of the threshold (bin 0). The results include the number of observations in the bin (*Actual*), the average of the bin to the left and right (*Predicted*), the difference between the two (*Error*), the standardized difference of the error (*Standardized Diff.*), and the total number of observations in the histogram (*Observations*). We report statistics for five research specifications: [1] DS Slack follows the DS slack and bin width design, [2] DS Slack: Narrow calculates bin width subtracting half of the DS bin width, [3] DS Slack: Wide calculates bin width adding half of the DS bin width, [4] $Slack_T$ calculates slack as the actual covenant metric value minus threshold, all scaled by the threshold, and uses bin width based on the formula in Degeorge et al. [1999], and [5] $Slack_T$: Wide calculates bin width as double the bin width in column 4. *** indicates statistical significance at the 1% level.

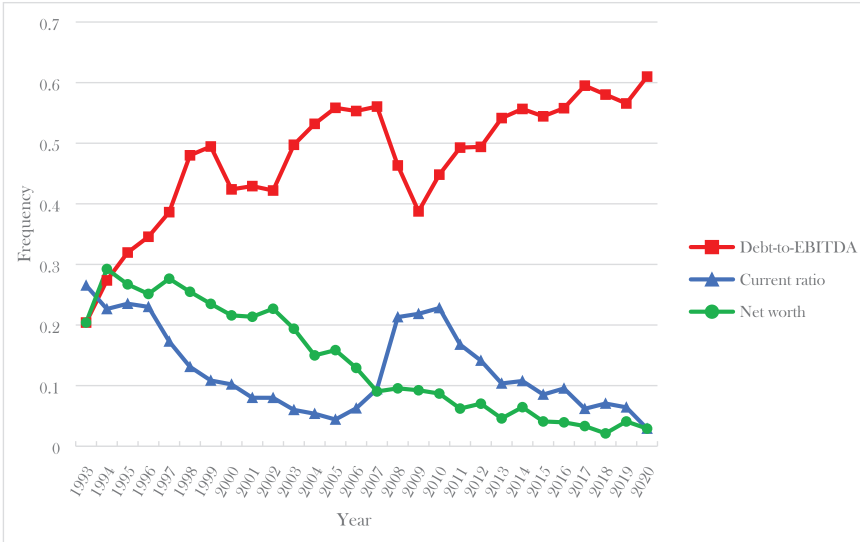

FIG 3.—Frequency of financial covenant use. This figure presents the frequency of inclusion of current ratio, net worth, and debt-to-EBITDA covenants in Dealscan loans from 1993 to 2020.

most common covenant in the Dealscan population of loans, maximum debt-to-EBITDA.

Demerjian [2011] examines changes in financial covenant use over the period 1996–2007. The study shows that the use of covenants written exclusively on balance sheet variables, including current ratio and net worth, has been declining over time. In contrast, the use of covenants whose measurement includes income statement values, such as interest coverage, fixed charge coverage, and debt-to-EBITDA, has been steady or increased over time. The declining usage of the current ratio and net worth, particularly if it has persisted in recent years, calls into question the economic importance of these covenants for our current understanding of DCH. Given this, we believe that understanding DCH now requires an examination of more frequently used covenants.

To assess trends in financial covenant use, we graph the current ratio and net worth covenant frequency by year. We present these results in figure 3, reporting the frequency of covenant use among all Dealscan loans. We find that the frequency of use of each is approximately 20%–25% in 1993 (the first year for which we have comprehensive data). Current ratio covenant use declines to about 5% of Dealscan loans by 2005 and recovers following the financial crisis to over 20% in 2010. Usage then declines again, falling under 10% by 2017. Net worth covenant usage has an even more dramatic decline, falling from over 20% usage in 1993 to less than 4% of Dealscan loans issued in 2019.[21] By way of comparison, we also report the frequency of the debt-to-EBITDA covenant over the same period. The frequency of use of this covenant is lower than either the current ratio or net worth in 1993 but rises to be used in over 50% of loans, a usage rate that persists to the current period.

Based on this descriptive analysis, debt-to-EBITDA appears to be growing in prominence and economic importance; it is, in fact, the most frequently used financial covenant in recent years.[22] Therefore, we reproduce our analysis using debt-to-EBITDA covenant slack. One concern with using debt-to-EBITDA is heterogeneity in measurement. Demerjian and Owens [2016] shows, however, a relatively homogeneous measurement of debt-to-EBITDA in covenants, with the "standard definition" (total long-term debt divided by EBITDA) being used for over 90% of these covenants. Furthermore, deviations from the standard definition do not introduce systematic measurement bias. Based on this evidence, we use the standard definition to measure debt-to-EBITDA slack.[23]

Debt-to-EBITDA covenants often have dynamic thresholds that tighten the covenant over time. As discussed earlier, Dealscan provides insufficient data to determine the thresholds between inception and maturity. To reflect the intermediate thresholds more accurately, we follow the procedure described in subsection 3.3 and hand-collect debt contract details from SEC filings. Of 4,263 contracts that report debt-to-EBITDA covenants with dynamic thresholds, we match 3,437 SEC filings (80.6%). We exclude observations with dynamic thresholds indicated but where we cannot match a filing to the sample observation.

Because debt-to-EBITDA is a maximum threshold covenant (as opposed to current ratio and net worth, which both have minimum thresholds), our main measure of slack is reversed, measured as the threshold value minus the actual value. We use the following measurements:

1) We measure slack as an unscaled (threshold minus actual) and as a scaled metric, dividing the unscaled slack by the threshold (which we again call Slack_{T}).

---

[21] We note that the minimum tangible net worth covenant displays a similar trend as net worth (unreported), suggesting that contracts did not shift usage between these two similar covenants.

[22] In the period 1994–2019, debt-to-EBITDA is the most used financial covenant in 32,900 loan packages reported in Dealscan, with a frequency of 48.0%. The next most used financial covenants are interest coverage (39.6%) and fixed charge coverage (29.8%). On a yearly basis, debt-to-EBITDA is the most used covenant in 21 of 26 years; it is less used than interest coverage in 1994–1996 and again in 2009 and 2010.

[23] Li [2010] provides extensive evidence on the measurement of EBITDA in debt contracts. We summarize these findings, including implications for measurement error in our setting, in section 7 of the online appendix.

2) We calculate the bin width using the formula from Degeorge, Patel, and Zeckhauser [1999] and the wide bin widths using double the formula value.
3) We report the results for the period 2000–2019.[24]

Focusing on the subsample of observations up to and including the first violation, the different slack and bin widths yield four tests of discontinuities.

We present histograms in figure 4. Visual inspection yields several insights about differences in the distribution of debt-to-EBITDA compared to the current ratio or net worth. First, where the distributions of current ratio and net worth slack have their highest observations relatively close to the threshold—typically, bin 1 or 2—the distributions for debt-to-EBITDA slack have their highest observations further to the right, in bin 5 (figure 4(d)), bin 8 (figure 4(b)), or above bin 9 (figures 4(a) and (c)). Second, there is a significant massing of observations to the right of bin 0 that is more dramatic than in histograms for current ratio or net worth. Third, the histograms appear to be smooth through the region of the threshold, showing no visual signs of discontinuities.

We continue the analysis with statistical evidence, reporting *actual* and *predicted* observations, *error*, and *standardized differences* in table 6. As with the histograms, we only report results for the period 2000–2019, as this period is most relevant for the debt-to-EBITDA covenant; we report statistical results for the DS period (1989–1999) in section 8 of the online appendix. The results on standardized differences are inconclusive for DCH. In three of the four columns (all but Slack_{T}, standard bin width), the difference for bin –1 is negative and significant, consistent with a discontinuity and DCH. In contrast, three of the four columns (all but unscaled slack, standard bin width) show a *negative* and significant difference for bin 0—inconsistent with DCH. In total, the findings in figure 4 and table 6 suggest inconclusive evidence of a discontinuity in the distribution of debt-to-EBITDA slack around the threshold.

5.2 DISCUSSION

Comparing the evidence of DCH for current ratio and net worth covenants against that for debt-to-EBITDA presents apparently contradictory results: We find consistent, current evidence of the discontinuity in the distribution of current ratio and net worth slack but not for debt-to-EBITDA slack. In this section, we present reasons why DCH may hold for current and net worth covenants but not for debt-to-EBITDA. Our discussion centers on the idea that different types of financial covenants serve different roles in debt contracts, drawing on the findings of Christensen and Nikolaev [2012]. Christensen and Nikolaev partition financial covenants

[24] We report results for the DS period (1989–1999) in section 8 of the online appendix, including histograms (figure A2) and tabulated standardized differences (table A5).

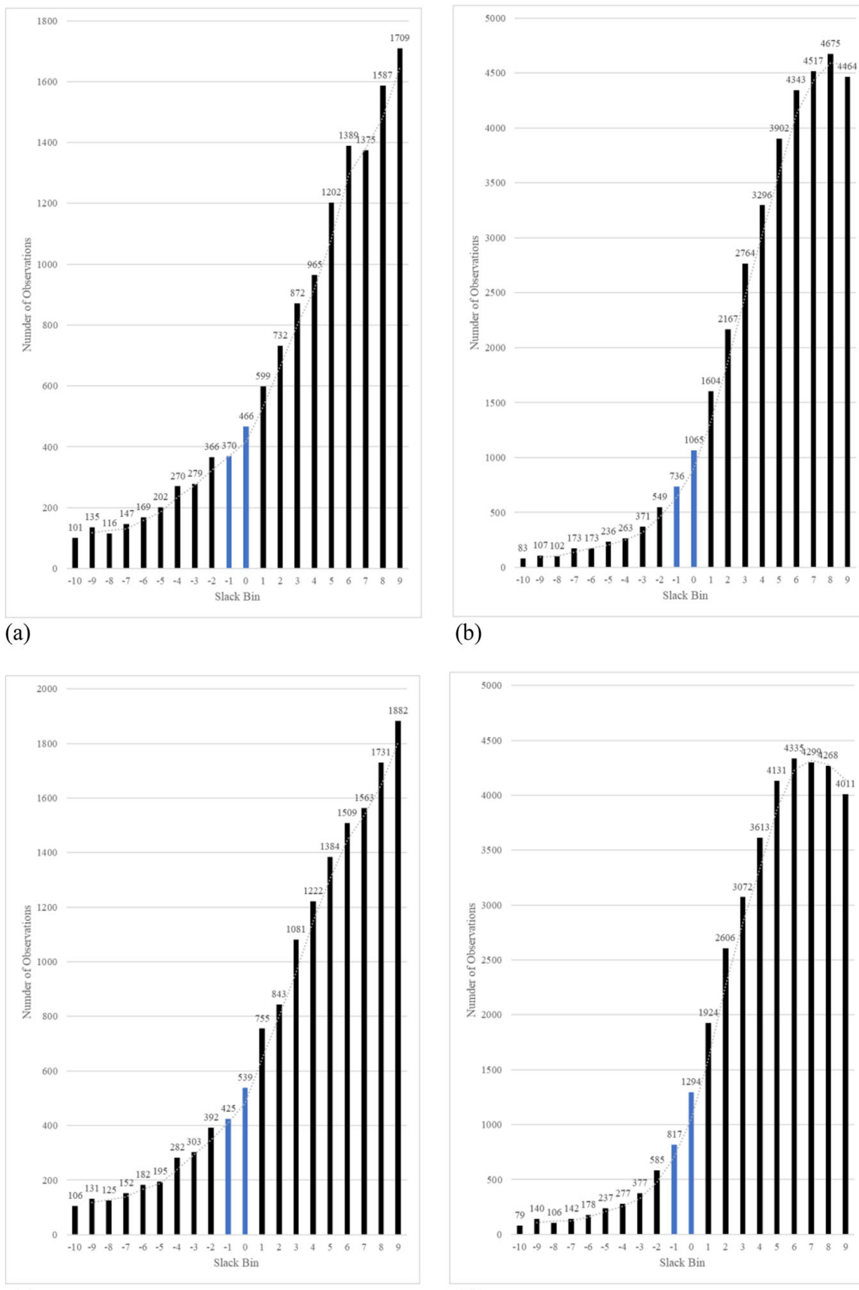

Fig 4.—Debt-to-EBITDA. This figure presents histograms of debt-to-EBITDA slack for the period 2000–2019. We report four figures: (a) unscaled slack, formula bin width, (b) unscaled slack, double formula bin widths, (c) Slack_T, formula bin width, and (d) Slack_T, double formula bin width. The bins of interest for DCH are shaded blue.

TABLE 6

Debt-to-EBITDA

	[1]		[2]		[3]		[4]	
	Unscaled Slack: Formula Bin Width = 0.089		Unscaled Slack: Wide Bin Width = 0.178		$Slack_T$: Formula Bin width = 0.028		$Slack_T$: Wide Bin width = 0.056	
	Bin -1	Bin 0	Bin -1	Bin 0	Bin -1	Bin 0	Bin -1	Bin 0
Actual	370	466	736	1065	425	539	817	1294
Predicted	416	484.5	807	1170	465.5	755	939.5	1370.5
Error	−46	−18.5	−71	−105	−40.5	−51	−122.5	−76.5
Standardized Diff.	−1.9*	−0.7	−2.1**	−2.6†††	−1.6	−1.8†	−3.4***	−1.7†
Observations	64,837		64,837		64,837		64,837	

In this table, we present the results for debt-to-EBITDA slack for the period 2000–2019. We present the results for the bin to the left of the threshold (bin –1) and the bin to the right of the threshold (bin 0). The results include the number of observations in the bin (*Actual*), the average of the bin to the left and right (*Predicted*), the difference between the two (*Error*), the standardized difference of the error (*Standardized diff.*), and the total number of observations in the histogram (*Observations*). We report statistics for four research design extensions: [1] Unscaled Slack: Formula calculates slack as the threshold minus the actual value, with bin width calculated using the formula from Degeorge et al. [1999], [2] Unscaled Slack: Wide calculates slack as in column 1, with the bin width doubled, [3] $Slack_T$: Formula calculates slack as the threshold minus the actual value, all scaled by the threshold, with bin width calculated using the formula from Degeorge et al., and [4] $Slack_T$: Wide calculates slack as in column 3, with the bin width doubled. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively; ††† and † indicate statistical significance in the opposite of the predicted direction at the 1% and 10% levels, respectively.

into two categories: capital covenants, which align borrower and lender incentives ex ante, and performance covenants, which allocate control rights ex post.[25] Both types of covenants address agency conflicts but in different ways, and Christensen and Nikolaev provide evidence on when covenants from each group are more likely to be used.

It is notable that our replication and extension analysis finds evidence in support of DCH for two capital covenants (current ratio and net worth) but a lack of support for a performance covenant (debt-to-EBITDA). One possible explanation of this discrepancy is the difference in the ability of the borrower to avoid violation. As Christensen and Nikolaev [2012] discuss, capital covenants will not necessarily be violated when borrower performance is poor, as long as the borrower can increase equity (i.e., by issuing new shares or reducing distributions). In contrast, technical default is costlier to avoid in performance covenants when economic outcomes are poor; they are, as Christensen and Nikolaev note, timely signals of impending financial distress. Hence, we may fail to detect evidence of a discontinuity in the distribution of debt-to-EBITDA because the borrowers' actions in this case are costlier than the actions borrowers can take to avoid default with the current ratio and net worth.

A second, nonmutually exclusive explanation is that the two covenant types provide different incentives to avoid technical default. Because performance covenants are designed to allocate control rights ex post, these covenants must be violated to serve their purpose. It follows that the consequences of technical default with performance covenants may not be too severe. In contrast, capital covenants do not need to be violated to serve their purpose. In fact, if they are effective, they will *not* be violated but rather align borrower and lender incentives ex ante. This suggests a higher price of technical default for capital covenants. If the consequences of violation differ, this could lead to different incentives related to avoiding violation. DS describe financial covenants as "trip wires" that are set tightly, are violated frequently, and typically have violations waived. This description conforms to performance covenants, suggesting a relatively low cost of violation. These violations are likely to act as a mechanism to facilitate renegotiation if borrower performance declines too much. Violation of capital covenants, in contrast, may induce more severe consequences; technical default of these covenants represents a breakdown of incentive alignment that is likely to be costly for lenders. This suggests a stronger incentive for borrowers to avoid violating capital covenants and an explanation for these apparently contradictory results.

[25] Capital covenants are written on balance sheet information only and include current and quick ratios, net worth and its variants, and balance sheet measures of leverage (e.g., debt-to-asset ratio, debt-to-equity ratio). Performance covenants typically feature a measure of earnings, and include coverage covenants (e.g., interest coverage, fixed charge coverage), income statement leverage measures (e.g., debt-to-EBITDA), and EBITDA covenants.

Our findings related to debt-to-EBITDA suggest a more nuanced perspective on DCH. Building on the work of Christensen and Nikolaev [2012], our evidence suggests that the inference of DS related to capital covenants continues to hold. It does not indicate, however, that DCH holds for all covenants. Our findings on the debt-to-EBITDA covenant suggest that the ability or incentive of the borrower to avoid technical default is sufficiently low to yield inconclusive evidence on DCH. These findings are exploratory in the sense that there is potential measurement error in calculating slack for debt-to-EBITDA. There are also other performance covenants, including interest coverage, that are commonly used in debt contracts. Analyzing the full set of financial covenants, including refinements on measuring covenant slack, would shed further light on DCH.

In interpreting the debt-to-EBITDA results, it is important to consider not only the evidence for DCH for this covenant, but also the implications of changing patterns in financial covenant use. That is, our analysis takes the change in use of debt-to-EBITDA covenants as given and examines the distribution of slack. Although it is outside the scope of this study, exploring broader changes in the institutional environment of debt contracting—particularly those changes that affect financial covenant use as well as borrowers' ability and incentive to avoid technical default—is a promising direction for future research. Here, we describe some significant regulatory and economic changes since 2000 and speculate on their effect on DCH.

The Sarbanes–Oxley Act was passed by congress in 2002 in the wake of the Enron and Worldcom accounting scandals. Cohen, Dey, and Lys [2008] show that the passage of this law shifted the costs and benefits of accruals earnings management, which is consistent with technical default becoming more difficult and costly for borrowers to avoid. The global financial crisis of 2007 and 2008 and the long recovery that followed upended financial markets, left banks thinly capitalized, and led to credit rationing (Duchin, Ozbas, and Sensoy [2010]). The constriction of credit and increase in borrower default risk likely shifted negotiating power in favor of lenders. Potential consequences of such a shift include more financial covenants, a different mix of financial covenants, and more tightly set financial covenants. Another emergent innovation in debt markets, starting around 2005 but taking stronger hold in the post-crisis recovery period starting in 2010, is the securitization of commercial loans in collateralized loan obligations (CLOs). Wang and Xia [2014] show that banks apply looser monitoring and grant more waivers for technical defaults when loans can be securitized. This finding is consistent with Bolton and Oehmke [2011] and Parlour and Winton [2013], both of which illustrate how the availability of credit default swaps changes lenders' monitoring incentives and leads to the "empty creditor" problem. To the extent that the increase in CLOs changed lenders' monitoring, incentives for DCH could have changed.

6. Conclusion

We revisit the DCH, examining the distribution of the current ratio and net worth covenant slack following the research design of DS. After reproducing the results in DS using that study's period (1989–1999) and research design choices, we confirm support for DCH, although we acknowledge that our samples do not perfectly overlap. We extend their results by varying three aspects of the analysis: bin width, measurement of covenant slack, and the statistical test of discontinuity. Considering the key finding of DS—discontinuities in the slack distributions for observations up to and including the first violation—our extensions are generally consistent with the results in DS, although they do reveal some sensitivity to inferences regarding bin width. We also extend the DS sample period through 2019 and continue to find robust support for DCH.

We note, in replicating and extending DS, that the use of the current ratio and net worth covenants has declined in recent years. Although our evidence, even in the recent period, supports DCH, we believe that analysis also must explore more commonly used covenants to draw more generalizable conclusions. Our final analysis examines the distribution of slack for the maximum debt-to-EBITDA covenant, the most frequently used financial covenant in our sample period. Our tests for this covenant are inconclusive; depending on research design choices, we find results that are either consistent or inconsistent with DCH.

These inconclusive results can motivate additional research on DCH. Expanding the set of covenants under study, including both performance and capital covenants, can shed further light on borrowers' ability and incentive to avoid covenant violation. We also believe that examining changes in financial reporting and debt contracting provides a particularly promising direction for future research. For example, changes in monitoring incentives when loans are securitized and sold in CLOs are likely to shift the costs and benefits of avoiding technical default. Finally, changes in regulatory aspects of accounting, including changes in accounting standards and their enforcement, are likely to affect manager's reporting incentives related to debt contracts.

REFERENCES

Beatty, A., J. Weber; and J. Yu. "Conservatism and Debt." *Journal of Accounting and Economics* 45 (2008): 154–74.

Begley, J. "Debt Covenants and Accounting Choice." *Journal of Accounting and Economics* 12 (1990): 125–39.

Beneish, M. D., and E. Press. "Costs of Technical Violation of Accounting-Based Debt Covenants." *The Accounting Review* 68 (1993): 233–57.

Bolton, P., and M. Oehmke. "Credit Default Swaps and the Empty Creditor Problem." *Review of Financial Studies* 24 (2011): 2617–55.

Burgstahler, D. and E. Chuk. "What Have We Learned About Earnings Management? Integrating Discontinuity Evidence." *Contemporary Accounting Research* 34 (2017): 726–49.

Burgstahler, D. and I. Dichev. "Earnings Management to Avoid Earnings Decreases and Losses." *Journal of Accounting and Economics* 24 (1997): 99–126.
Byzalov, D., and S. Basu. "Modeling the Determinants of Meet-Or-Just-Beat Behavior in Distribution Discontinuity Tests." *Journal of Accounting and Economics* 68 (2019).
Caskey, J., K. Huang, and D. Saavedra. "Noncompliance with SEC Regulations: Evidence from Timely Loan Disclosures." *Review of Accounting Studies* (2021): Forthcoming.
Chava, S. and M. Roberts. "How Does Financing Impact Investment? The Role of Debt Covenants." *The Journal of Finance* 63 (2008): 2085–121.
Chen, K., and J. Wei. "Creditors' Decisions to Waive Violations of Accounting-Based Debt Covenants." *The Accounting Review* 68 (1993): 218–32.
Christensen, H., and V. Nikolaev. "Capital Versus Performance Covenants in Debt Contracts." *Journal of Accounting Research* 50 (2012): 75–116.
Cohen, D., A. Dey; and T. Lys. "Real and Accrual-Based Earnings Management in the Pre- and Post-Sarbanes-Oxley Periods." *The Accounting Review* 83 (2008): 757–87.
Deangelo, H., L. Deangelo; and D. Skinner. "Accounting Choice in Troubled Companies." *Journal of Accounting and Economics* 17 (1994): 113–43.
Defond, M., and J. Jiambalvo. "Debt Covenant Violation and Manipulation of Accruals." *Journal of Accounting and Economics* 17 (1994): 145–76.
Degeorge, F., J. Patel; and R. Zeckhauser. "Earnings Management to Exceed Thresholds." *Journal of Business* 72 (1999): 1–33.
Demerjian, P. "Accounting Standards and Debt Covenants: Has the 'Balance Sheet Approach' Led to a Decline in the Use of Balance Sheet Covenants?" *Journal of Accounting and Economics* 52 (2011): 178–202.
Demerjian, P., and E. Owens. "Measuring the Probability of Financial Covenant Violation in Private Debt Contracts." *Journal of Accounting and Economics* 61 (2016): 433–47.
Dichev, I., and D. Skinner. "Large-Sample Evidence on the Debt Covenant Hypothesis." *Journal of Accounting Research* 40 (2002): 1091–123.
Drucker, S., and M. Puri. "On Loan Sales, Loan Contracting, and Lending Relationships." *The Review of Financial Studies* 22 (2008): 2835–72.
Duchin, R., O. Ozbas; and B. Sensoy. "Costly External Finance, Corporate Investment, and the Subprime Mortgage Credit Crisis." *Journal of Financial Economics* 97 (2010): 418–35.
Duke, J., and H. Hunt. "An Empirical Examination of Debt Covenant Restrictions and Accounting-Related Debt Proxies." *Journal of Accounting and Economics* 12 (1990): 45–63.
EL-Gazzar, S., and V. Pastena. "Factors Affecting the Scope and Initial Tightness of Covenant Restrictions in Private Lending Agreements." *Contemporary Accounting Research* 8 (1991): 132–51.
Franz, D., H. Hassabelnaby; and G. Lobo. "Impact of Proximity to Debt Covenant Violation on Earnings Management." *Review of Accounting Studies* 19 (2014): 473–505.
Hail, L., M. Lang; and C. Leuz. "Reproducibility in Accounting Research: Views of the Research Community." *Journal of Accounting Research* 58 (2020): 519–43.
Kim, B., L. Lei; and M. Pevzner. "Debt Covenant Slack and Real Earnings Management." Working paper, American University and George Mason Univeriesty. 2010.
Li N. "Negotiated Measurement Rules in Debt Contracts." *Journal of Accounting Research* 48 (2010): 1103–144.
Li, N., F. Vasvari; and R. Wittenberg-Moerman. "Dynamic Threshold Values in Earnings-Based Covenants." *Journal of Accounting and Economics* 61 (2016), 605–29.
Murfin, J. "The Supply-Side Determinants of Loan Contract Strictness." *Journal of Finance* 67 (2012): 1565–601.
Parlour, C., and A. Winton. "Laying Off Credit Risk: Loans Sales Versus Credit Default Swaps." *Journal of Financial Economics* 107 (2013): 25–45.
Press, E., and J. Weintrop. "Accounting-Based Constraints in Public and Private Debt Agreements: Their Association with Leverage and Impact on Accounting Choice." *Journal of Accounting and Economics* 12 (1990): 65–95.
Sweeney, A. "Debt-Covenant Violations and Managers' Accounting Responses." *Journal of Accounting and Economics* 17 (1994): 281–308.

Wang, Y., and H. Xia. "Do Lenders Still Monitor When They Can Securitize Loans?" *The Review of Financial Studies* 27 (2014): 2354–91.

Watts, R., and J. Zimmerman. "Towards a Positive Theory of the Determination of Accounting Standards." *The Accounting Review* (1978): 53(1) 112–34.

Watts, R., and J. Zimmerman. *Positive Accounting Theory.* New Jersey: Prentice Hall, 1986.