Measurement Error when Estimating Covenant Violations

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ABSTRACT

We hand collect true covenant thresholds and realizations from SEC filings and show that estimating covenant slack using data from commercial databases frequently overestimates but rarely underestimates violations. This asymmetric measurement error, largely driven by differences between true and estimated realizations, meaningfully affects research in at least two settings: (1) regression discontinuity designs that seek to precisely identify covenant realizations around a threshold, and (2) research that infers lenders' enforcement or forbearance of estimated violations. We show that true violations, but not estimated violations, are associated with stock market reactions and renegotiations. Finally, we investigate ways to reduce measurement error.

Keywords: Debt Covenants, Covenant Slack, Covenant Violations

JEL Classification: G30, M40, M41

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1. Introduction

Financial covenants are ubiquitous in debt contracts and facilitate lending by mitigating conflicts between firms and capital providers (Smith and Warner 1979; Roberts and Sufi 2009a; Bradley and Roberts 2015). An extensive literature examines the determinants and consequences of covenant violations (e.g., Chava and Roberts 2008; Demiroglu and James 2010; Murfin 2012; Nini et al. 2012; Falato and Liang 2016; Ferreira et al. 2018; Gustafson et al. 2021), as well as the extent to which covenants create incentives for firms to manipulate reported numbers (Watts and Zimmerman 1978; Dichev and Skinner 2002; Bordeman and Demerjian 2022). Covenant violations occur when the realization of a contractually defined financial measure, such as debt-to-EBITDA or tangible net worth, violates a threshold specified in the debt contract; the distance between a covenant realization and a covenant contractual threshold often is referred to as covenant slack or covenant tightness.

To calculate covenant slack and covenant violations, researchers often use accounting information from Compustat to measure covenant realizations (EBITDA, debt, etc.) and loan information from Thomson Reuters's Dealscan to measure covenant thresholds.[1] The appropriateness of this approach rests on two assumptions: (1) covenants are defined identically, or nearly identically, across firms and contracts in a manner consistent with U.S. GAAP and the associated formulas used by Compustat or that any measurement error is randomly distributed or immaterial; and (2) covenant thresholds in Dealscan accurately identify the contractual (true) covenant thresholds or that differences between Dealscan and true thresholds are randomly distributed and immaterial.

Violations of these assumptions can result in two types of measurement error: realization measurement error and threshold measurement error. Realization measurement error arises when covenant realizations measured with Compustat data (hereafter, estimated realizations) differ from

[1] Some papers identify covenant violations using violations disclosed in SEC filings (Nini et al. 2012). If firms comply with disclosure regulations, this approach would mitigate measurement error associated with identifying the occurrence of a covenant violation, but it cannot address measurement error issues associated with estimated slack. Thus, this approach is limited in its ability to mitigate measurement error for research questions that require the precise identification of a borrower's proximity to covenant thresholds (e.g., regression discontinuity designs).

true realizations. These differences exist because financial covenants are commonly defined using non-GAAP numbers that are not available in commercial databases such as Compustat (Leftwich 1983; Christensen and Nikolaev 2017). In addition, covenant definitions for the same covenant type (e.g., debt-to-EBITDA) can vary widely across debt contracts (Beatty et al. 2019; Badawi et al. 2022), yet researchers often rely on standardized measures to estimate covenant realizations (Demerjian and Owens 2016).[2] Threshold measurement error arises when covenant thresholds from Dealscan (hereafter, estimated thresholds) differ from true thresholds. These differences could arise because Dealscan fails to capture planned changes in thresholds over the life of the loan or threshold adjustments from contractual amendments (Roberts 2015; Li et al. 2016).[3]

In this paper, we use a hand-collected dataset of firms' true covenant realizations and true thresholds to: (1) investigate the measurement error associated with traditional measures of covenant slack and covenant violations; (2) identify the source of the measurement error; (3) assess whether using true versus estimated measures of slack or violations yields different inferences or violates identifying assumptions; and (4) investigate ways to reduce measurement error. We obtain firms' true covenant realizations and true thresholds with the following process. We begin with the universe of firm-quarter observations from 2000 to 2016 that can be linked to both Dealscan (for estimated thresholds) and Compustat (for estimated realizations). We then manually review a random sample of 1,000 10-K filings from the universe of over 90,000 firm-quarter observations to identify whether firms report both true covenant thresholds and realizations (i.e., true covenant information) and to examine the language used in these disclosures. We use this information to develop a text-search algorithm that we apply to the full sample of firm-quarter observations to identify firms' disclosure of true covenant information. Finally, we hand collect true covenant information from the resulting sample for eight covenant types: interest coverage ratios, fixed

[2] In the Internet Appendix we illustrate these measurement issues using an example from Ruby Tuesday's filing for the quarter ending December 2, 2008. Ruby Tuesday, Inc. notes that "because not all companies use identical calculations" its debt-to-EBTIDA covenant "may not be comparable to similarly titled measures of other companies."

[3] Roberts (2015) shows that 73% of loans in his sample are renegotiated at least once before maturity and that approximately half of the renegotiations modify covenants. Similarly, Nikolaev (2018) documents that 37% of firm-years experience a renegotiation. Li et al. (2016) show that nearly half of syndicated loans include dynamic covenant thresholds in earning-based covenants, yet Dealscan frequently fails to capture these changes.

charge coverage ratios, debt-to-EBITDA ratios, senior debt-to-EBITDA ratios, current ratios, leverage, net worth, and tangible net worth.

This process results in a primary sample of 18,217 unique covenants corresponding to 9,799 unique firm-quarters (True Slack Sample). We observe that the likelihood that a firm discloses true covenant information is increasing in firm size and leverage. Notably, prior disclosure is by far the strongest determinant of current disclosure, suggesting that once firms start providing true covenant information, they do so persistently. To enhance the generalizability of our findings, we also collect information about firms' covenant compliance status. We observe that approximately two-thirds of the observations in our initial sample (61,303 firm-quarters) qualitatively discuss their compliance status with financial covenants (Compliance Sample). Although these disclosures do not provide information about true covenant slack, they reveal whether firms are compliant with covenants. Thus, we can use these disclosures to evaluate the consequences of one type of measurement error in estimated covenant violations – the identification of false covenant violations – in a broader sample relative to our true slack sample.

We find that the difference between true slack (calculated as the distance between true covenant realizations and true thresholds) and estimated slack (calculated as the distance between estimated covenant realizations and estimated thresholds) is both large and frequent. This measurement error considerably impairs researchers' ability to identify covenant violations. Relying on estimated slack overstates the number of covenant violations for all eight covenant types, with nearly 96 out of every 100 estimated violations being Type I Errors (i.e., estimated slack identifies a violation while true slack does not). On average, estimated violations occur 1,700 percent more frequently than true violations, and the number spikes to 6,300 percent for senior debt-to-EBITDA covenants. Importantly, measurement error in estimated slack is not symmetric: Type I errors occur about 78 times as frequently as Type II measurement errors (i.e., true slack identifies a violation while estimated slack does not).

Prior research is not entirely unaware of these measurement error concerns, and some researchers have adopted solutions, such as focusing on current ratio and net worth covenants (e.g., Dichev and Skinner 2002; Chava and Roberts 2008), which some consider less susceptible to measurement error. This commonly held belief is unsubstantiated by our data. We show that current ratio and net worth covenants are subject to measurement error that is similar in frequency and magnitude to measurement error in other covenant types. For example, approximately 9 out of 10 estimated violations for net worth and current ratio covenants are Type I errors.

We next investigate whether the documented measurement error arises primarily because of realization measurement error, threshold measurement error, or a combination of the two. We show that both sources of measurement error play a considerable role, although the measurement error is most prominent when Compustat data are used to estimate contractual realizations. We conclude that even when researchers hand collect data on thresholds from original contracts or contractual amendments (e.g., Chava and Roberts 2008; Bordeman and Demerjian 2022) their measure of covenant violations can overestimate the frequency of violations by more than tenfold.

To better understand the nature of measurement error, we investigate its correlation with various borrower, lender, and contract characteristics. Measurement error can lead to biased inferences if the error is correlated with observed or unobserved explanatory variables (Roberts and Whited 2013). Therefore, this analysis is important because it can inform researchers about potential sources of bias in their estimates. We find that measurement error does not have significant correlations with many borrower, lender, or loan characteristics. Nonetheless, three important determinants of such errors emerge: a borrower's incurrence of a loss, a borrower's debt-to-EBITDA ratio, and the number of covenants included in the loan contract. This last determinant, in particular, has important implications for researchers, and we discuss them in Section 3.2. We also find that threshold realization error increases in the elapsed time since contract origination, while realization measurement error does not change throughout the contract life.

The presence of frequent, large, and non-random measurement error engenders concerns that inferences from prior research may be incorrect or misleading when such research has relied on estimated realizations and estimated thresholds. For this reason, we investigate the extent to which findings from prior research are sensitive to measurement error. We start by analyzing two types of papers that examine the consequences of covenant violations.

First, we focus on papers that rely on regression discontinuity designs (RDDs), which require the precise identification of firms in close proximity to the covenant threshold. To do so, we reexamine Chava and Roberts (2008), the influential paper that helped introduce this research design to the literature. Using estimated realizations and thresholds, we replicate the paper's finding that investment declines following a covenant violation, and show consistent results using our true covenant data. We then use our true covenant data to assess whether the two main identifying assumptions underlying RDDs hold true in the data. We find that results are quite sensitive to bandwidth selection and that the decline in investment following a covenant violation is driven by observations far away from, not close to, the threshold. These findings are worrisome because they indicate a potential violation of the first identifying assumption for RDD – namely, that firms are assigned to treatment solely on the forcing variable, which allows for causal inference only near the threshold (Bakke and Whited 2012). We also observe substantial bunching of observations near true covenant thresholds. This finding is also concerning because it indicates a potential violation of the second identifying assumption in RDD: that there is no manipulation around the threshold. We use formal tests from Cattaneo et al. (2018) to verify the presence of bunching using either estimated slack or true slack. Although we do not find evidence of threshold manipulation using estimated slack, we do find evidence of threshold manipulation with true slack, which implies that firms may endogenously sort into violators and non-violators. We conclude that while the findings in Chava and Roberts (2008) are not driven by measurement error, they should be interpreted cautiously. More broadly, these findings serve as a warning to researchers about using RDDs in the context of covenant violations with estimated data.

Second, we focus on papers that study lender forbearance, for which precisely identifying instances of covenant violations is critical. Using our sample and the same measurement and design choices as the authors, we replicate the main findings both in Bird et al. (2022a) – that lenders appear to forbear most violations – and in Bird et al. (2022b) – that lenders enforce more covenant violations when they face short-term earnings pressure. We then show that these findings fail to hold when using alternative measures of covenant violations that reduce measurement error.

Therefore, we conclude that prior evidence of lender forbearance largely reflects Type I measurement error in covenant violations measured with commercial databases.

Research on the consequences of covenant violations is not the only line of work that relies on estimated covenant realizations and thresholds. A related stream of research investigates the determinants of initial covenant slack (or tightness) and documents how slack varies with borrower characteristics (Demiroglu and James 2010; Rauh and Sufi 2010), lender characteristics (Murfin 2012), and the relationship between the two (Prilmeier 2017). This research also suffers from measurement error in estimated slack. However, measurement error should be less problematic and possibly introduce only an attenuation bias for two reasons. First, in these papers, covenant slack appears as a dependent variable. Second, estimated and true covenant slack display a positive and relatively high correlation in the data. We explore the extent to which measurement error is a problem for this research by reexamining the finding by Prilmeier (2017) that firm risk and lender relationships explain variation in covenant tightness. Our true covenant data show that these findings are robust. For example, we find that the duration of a lending relationship is associated with covenant slack, as in Prilmeier (2017), and that this finding becomes both economically and statistically stronger when replacing estimated slack with true slack. Moreover, consistent with prior literature (e.g., Demiroglu and James 2010; Rauh and Sufi 2010), we observe that riskier borrowers receive tighter covenants. Collectively, our evidence indicates that research on the determinants of covenant slack is likely less susceptible to measurement error concerns.

One of the insights that emerges from our reexamination of Prilmeier (2017) is that, under certain circumstances, the large and frequent measurement errors in estimated covenant slack and covenant violations could attenuate economic effects, thereby impairing researchers' ability to find empirical support for otherwise sound economic relations. Consistent with this possibility, we reveal two insights when using our data on true covenant violations that otherwise would be concealed by measurement error in estimated covenant violations. Specifically, we show that although estimated violations do not engender negative stock market reactions and higher likelihood of future loan renegotiation, true covenant violations do.

We conclude our analyses by investigating the extent to which adjustments to the Compustat variable used to estimate contractual EBITDA – an integral component of the most common covenant types (Demerjian 2011; Chava et al. 2021; Griffin et al. 2021) – may reduce measurement error. We find that we can reduce measurement error by 20–30% by adding non-cash compensation expenses back into EBITDA; such expenses commonly are excluded from contractual EBITDA but are included in the Compustat variable most frequently used to measure EBITDA. Note that despite these adjustments, estimated covenant violations continue to display substantial measurement error.

Our study makes several contributions to the literature. Although prior research acknowledges potential measurement issues associated with estimated covenant slack (Dichev and Skinner 2002; Chava and Roberts 2008; Demerjian and Owens 2016), we are, to the best of our knowledge, the first to document that this measurement error is common and large and that it extends to all covenant types. Furthermore, we are also the first to characterize the nature of this measurement error by showing that it frequently results in the overestimation of covenant violations (Type I error) but rarely in their underestimation (Type II error), which is important because it implies that measurement error in covenant violations cannot be assumed to be white noise. We also provide evidence on when measurement error is more concerning by showing which borrower and loan contract characteristics are correlated with covenant measurement error.

Our second contribution is to identify situations where measurement error issues may create large biases and where these issues may be less problematic. We show that measurement error possibly invalidates the use of RDDs around covenant violations and that its presence is particularly problematic for research that requires the precise identification of covenant violations, such as studies on lender forbearance. We base these conclusions on three grounds: (1) Estimated slack severely overestimates covenant violations; (2) estimated slack does not allow researchers to precisely measure firms' slack, making it nearly impossible to study intervals near the threshold; and (3) firms appear to engage in threshold manipulation. In view of this evidence, we caution researchers against relying on RDDs to study the consequences of covenant violations, and against relying on estimated covenant violations to study lender forbearance. We then show that measurement error in estimated slack is less problematic for research interested in the relative magnitude of slack, such as papers on the determinants of covenant tightness. The reason is that estimated and true slack are fairly highly correlated in the data and because measurement error appears in the dependent variable and is therefore less problematic in these studies.

Third, we offer researchers some (admittedly imperfect) solutions to these measurement error issues. We suggest adjustments that researchers can apply to reduce measurement error in their estimates of covenant slack, with the caveat that considerable measurement error remains. We also plan to make our two new databases available. The first database, which reports firms' qualitative disclosures about their compliance status with covenants, can be used to remove many instances of Type I errors in estimated covenant violations, thereby improving the quality of the measure. Researchers can use the second database, which reports true covenant realizations and thresholds, to confirm their findings in a smaller sample free of measurement error, as well as to gauge the significance of measurement error problems in their study – for example, by investigating the correlation between their variable(s) of interest and measurement error.

These contributions notwithstanding, our study is subject to some caveats and limitations. First, our data indicate that many firms do not report true covenant information, and our determinants analysis suggests that these disclosures may be non-random. Second, our sample with true covenant information is smaller than those used in many existing studies and is unevenly balanced over time because the disclosure of true covenant information has become more common in recent years. Thus, researchers relying on these data (our own paper included) should exercise modesty when extrapolating their inferences to alternative settings or to the broader population.

2. Sample Construction

To analyze measurement error when estimating covenant slack and covenant violations, we need information on both true covenant thresholds and realizations. However, to the best of our knowledge, no publicly available database contains this information. Therefore, we use firms' SEC filings combined with textual analysis and manual collection to construct one.

We begin with the population of loan packages (or deals) with deal dates outstanding between January 1, 2000, and December 31, 2016. We create a firm-quarter sample by linking these loan packages to borrowers' financial information in Compustat using the Chava and Roberts (2008) linking table. We then merge this dataset with qualitative violation data from Nini et al. (2012).[4] After eliminating firm-quarters with insufficient data to compute estimated covenant realizations or thresholds, we have a sample comprising 93,092 observations (Full Sample).

From these 93,092 observations, we select a random sample of 1,000 10-K filings. We review each of these filings to determine whether the firm discloses both true covenant thresholds and realizations, and find this occurs in 139 cases. We use these 139 cases to establish the language that firms use in their disclosure of true covenant thresholds and realizations, and use this language to develop a text-based algorithm to identify these disclosures.[5] When we deploy the algorithm to the Full Sample, more than 28,000 of the initial 93,092 observations are identified as potentially disclosing true covenant information. We manually review the SEC periodic filing associated with each of these identified observations, finding and collecting data for 18,217 unique covenants (covenant-firm-quarters), corresponding to 9,799 unique firm-quarters, which serves as the basis for our main sample (True Slack Sample). Table 1 Panel A describes the sample construction.[6]

In Table 1 Panel B, we report the number of firm-quarter observations with true covenant realizations and thresholds for the 12 most common covenant types. The table shows that certain covenant types are used infrequently, so we focus our attention on the eight most common covenants: Debt-to-EBITDA (DBEBD), Interest Coverage (ICVR), Fixed Charge Coverage (FCVR), Leverage (DBAT), Senior Debt-to-EBITDA (SDBEBD), Net Worth (NW), Tangible Net Worth (TNW), and Current Ratio (CRTO) covenants. This restriction reduces our sample to 9,702 unique firm-quarters and 17,718 covenant-firm-quarter observations.

Figure 1, Panel A reports the number of unique firms that report true covenant information by year. This number increases from 20 in 2000 to 317 in both 2009 and 2010, before decreasing

[4] We thank Greg Nini for sharing covenant violation data with us that covers fiscal quarters ending through 2016.
[5] In developing this algorithm, we sought to maximize our ability to correctly identify the presence of true covenant information while minimizing false positives. When tested against our training sample of 1,000 10-Ks, our algorithm correctly classified 91% (127 of 139) of the observations disclosing true covenant information but also generated 245 false positives. These rates are similar to prior studies (e.g., Nini et al. 2012).
[6] For additional information about the sample selection procedures, please see the Internet Appendix.

each year thereafter. We next compare the number of unique firms that report true covenant information to the number of unique firms in our Full Sample of more than 90,000 firm-quarters. During our sample period (2000-2016), we find that approximately 10.5% of firm-quarters disclose true covenant information. Figure 1, Panel B shows that the proportion of firms in the full sample that report true covenant information increases since 2000, peaking in 2012 at nearly 21%. Since 2012, this figure falls modestly to approximately 18% by 2016.[7]

We complement our information about true covenant realizations and thresholds with firms' qualitative disclosures about their covenant compliance status, which is more common than quantitative true covenant information.[8] Although this information does not allow us to know true slack, it helps us to identify instances where estimated slack indicates a violation but no true violation occurred (i.e., Type I error) for a large sample of firms. We identify observations that disclose covenant compliance status in our full sample by manually reviewing more than 3,000 periodic filings to determine: 1) whether the filing discloses covenant compliance status and 2) the common language used in this disclosure. Using this information, we create a text-search algorithm to identify instances of covenant compliance status disclosure and apply the algorithm to the Full Sample, finding that approximately 66% (61,303) of the 93,092 periodic filings contain qualitative disclosure of firms' covenant compliance status (see Compliance Sample in Table 1, Panel A).[9]

2.1 Determinants of Covenant Disclosure

Our sample relies on hand-collected disclosures of true covenant information from SEC filings. The SEC offers specific guidance about discussions concerning debt covenants (SEC FRM 9210.2), which are required under the following two circumstances: (1) The registrant is, or is

[7] One potential reason for the decline in our sample after 2012 is the rise of covenant-lite debt contracts that require borrowers to comply with covenants only when the borrower pursues a significant event, such as an acquisition (Becker and Ivashina 2016). These covenant-lite loans reduce the relevance and materiality of covenants, thereby diminishing firms' need and incentives to disclose covenant information.

[8] For example, in its 10-K for fiscal 2015, BWX Technologies, Inc. fails to report true covenant realizations and thus cannot be included in our True Slack Sample. However, the firm does explicitly state that "At December 31, 2015, we were in compliance with all covenants set forth in the Credit Agreement."

[9] Untabulated analyses show that the proportion of firms qualitatively disclosing their covenant compliance relative the Full Sample increases over time from 35% in 2000 to 85% in 2016. These results indicate that while disclosure of true covenant information declined over the second half of our sample period, compliance disclosure increased.

reasonably likely to be, in breach of debt covenants; or (2) covenants affect the registrant's ability to obtain additional debt or equity financing. Assuming that firms comply with SEC disclosure requirements, which seems likely because the SEC can question firms that fail to comply (Deloitte 2017; EY 2017), our True Slack Sample likely contains cases where covenants have a meaningful effect on financing and investment. Thus, our ability to generalize results beyond this sample may be limited, and the typical sample selection caveats apply.

To better convey the nature of our sample, in Table 2, Panel A, we provide descriptive statistics for: 1) observations that disclose true covenant information (True Slack Sample); 2) observations that disclose covenant compliance status (Compliance Sample); and 3) observations that do not disclose either (i.e., No Disclosure). We observe that firms that disclose true covenant information are larger than compliance disclosers, which are themselves larger than non-disclosers, with all univariate differences significant at the 0.01 level. Firms that disclose true covenant information or covenant compliance status are, on average, more highly leveraged than non-disclosers. Perhaps not surprisingly, firms that disclose true covenant information also are riskier, as indicated by lower Z-scores, current ratios, interest coverage ratios, and higher debt-to-EBITDA ratios. Collectively, the evidence suggests that firms provide disclosure when covenants are more likely to be material to financing, consistent with the SEC guidelines. Moreover, the degree of disclosure (i.e., true covenant information vs. compliance status) appears to become more detailed as covenants become more relevant.

We offer additional insights into the nature of our sample by studying the determinants of disclosing true covenant information in Table 2, Panel B. We find that, consistent with the univariate evidence, larger and more leveraged firms are more likely to disclose true covenant information. Table 2 Panel B also indicates that, all else equal, firms are 0.8 percent more likely to disclose true covenant information when they report (qualitatively) a contemporaneous violation. Notably, the strongest predictor of whether firms disclose true covenant information is the lagged disclosure of the same information, indicating that once firms begin to disseminate true covenant information, they rarely reverse that decision. As such, it appears unlikely that firms strategically oscillate between disclosure and non-disclosure.[10]

3. Measurement Error Analysis

3.1 Measurement Error when Estimating Slack and Violations

We begin our analysis by examining when measurement error leads to the incorrect identification of covenant violations, the primary sources of measurement error (threshold vs. realization measurement error), and the frequency of large measurement error (regardless of whether it results in incorrect identification of a violation). We report these results in Table 3.

Table 3, Panel A compares the frequency of violations (*Violation %*) for our eight covenant types when using true versus estimated slack, while Figure 2 provides a graphical illustration of the same frequencies.[11] We identify a true violation as occurring when true slack is negative and an estimated violation as occurring when estimated slack is negative.[12] Table 3 indicates that true violations occur infrequently in our True Slack Sample, with the *Violation %* ranging from 0.4% (DBAT) to 4.8% (CRTO). In contrast, estimated violations are much more common across all covenant types and range from 1.7% (DBAT) to 57.3% (SDBEBD). Moreover, while the frequency of true violations is never above 5%, the frequency of estimated violations is greater than 10% for six of the eight covenant types. These statistics indicate that nearly 96 of every 100 estimated violations is a false positive (i.e., estimated slack identifies a violation while true slack does not); that the overestimation problem is spread across all covenant types, with seven of the eight covenant types having false positive rates greater than 90%; and that this problem is severe

[10] One potential concern in our setting is strategic disclosure. The persistence of disclosure helps alleviate this concern. To better understand firm disclosure choices, we show in the Internet Appendix that 80% of firms that disclose true covenant information never incur a violation, 12% begin reporting true covenant information at least two quarters before incurring a violation, and 8% begin disclosing true covenant information within one quarter of a violation.

[11] When matching our observations in our True Slack Sample with Compustat and Dealscan data for determining estimated slack, we drop firm-quarter-covenant observations that have insufficient data for computing estimated covenant realizations or that are lacking data on covenant thresholds. This step reduces our sample from 17,718 unique covenant-firm-quarter observations (Table 1 Panel B) to 14,623 (Table 3, Panel A).

[12] For covenants with a minimum threshold, covenant slack is computed as the covenant realization less the threshold. For covenants with a maximum threshold, covenant slack is computed as the covenant threshold less the realization.

because estimated slack overstates violations relative to true slack by over 1,700% in aggregate.[13]

Prior research often acknowledges the measurement error that comes with using estimated slack to identify covenant violations. In some cases, researchers assert that this measurement error is likely random and thus unlikely to bias inferences (Bird et al. 2022b). We recognize that measurement error in estimated slack does not, on its own, imply that estimated slack is biased because measurement error could be symmetrically distributed (Trochim et al. 2015). However, if measurement error is not symmetric and generates more overestimations (i.e., Type 1 errors) than underestimations (i.e., Type II errors) of violations, then estimated slack is a biased proxy for true slack, which could meaningfully affect inferences. We assess whether this bias exists by examining the frequency of Type I and Type II errors that using estimated slack entails. Table 3, Panel A shows that Type I errors are much more common than Type II errors across all covenant types, with more than 98% of errors coming from overestimations of violations. We conclude that using estimated slack to identify covenant violations is subject to non-random (or systemic) measurement error, with Type I errors occurring more than 70 times as frequently as Type II errors.

Our analysis results in three findings: (1) using estimated slack grossly overestimates the frequency of violations; (2) the resulting measurement error is not random, with Type I errors being much more common than Type II errors; and (3) measurement error exists across all eight covenant types. This latter finding, is relevant because researchers often assume that a subset of covenants, such as current ratio and net worth, are less subject to measurement error. For this reason, researchers often focus their analyses on these covenants (e.g., Chava and Roberts 2008; Falato and Liang 2016). However, we find that nearly 90% of estimated violations associated with current ratio and net worth covenants are false positives, which is similar to the frequency observed with other covenant types and signifies that relying on these covenants is unlikely to resolve the measurement error problem that our paper reveals.

[13] Some studies (Nini et al. 2012; Ferreira et al. 2018) focus on "new violations" – that is, covenant violations by firms without a violation in any of the previous four quarters. In untabulated results, we remove true violations that fail to qualify as "new violations." Taking this step leads to a decrease in true violations that is much smaller than the decrease in estimated violations, causing the *Overestimation %* to climb above 6,000%.

Readers may find our evidence of substantial measurement error in current ratios surprising because these ratios are perceived as being fairly simple to calculate and because their definitions are supposedly highly standardized (Dichev and Skinner 2002; Chava and Roberts 2008; Demerjian and Owens 2016). To better understand the source of this measurement error, we examine the original debt contracts for all loans with Type I errors associated with current ratios.[14] We find that nearly all contractual current ratio covenants are defined as current assets divided by current liabilities, consistent with the maintained assumption in prior research. However, in most cases (more than 95% of our false positives), current assets are contractually defined to include unused borrowing capacity (e.g., unused amount on a line of credit), which is unobservable in Compustat. As such, measuring current assets with Compustat data systematically understates the covenant's numerator (see Figure IF6 in the Internet Appendix), leading to a high incidence of false positives. We provide an illustrative example of this problem in Appendix B.

3.1.2 Sources of Measurement Error

Measurement error in estimated covenant slack and estimated covenant violations can originate from differences between true and estimated realizations, differences between true and estimated thresholds, or both. In Table 3, Panel B we attempt to determine the primary source of measurement error by comparing violation frequencies across slack measures calculated with various combinations of true and estimated covenant realizations and thresholds.

The first row of this table shows that there are 158 true violations across our 14,623 firm-quarter-covenants based on true slack, which is a *Violation %* of 1.08%. When we combine true realizations with estimated thresholds, which enables us to isolate the measurement error induced by differences between true and estimated thresholds, *Violation %* amounts to 6.45%, an overestimation of 497%.[15] When we combine estimated realization with true thresholds, which

[14] We identify current ratio definitions from SEC filings rather than from Dealscan tearsheets because tearsheets merely provide a summary of the original contract, and this summary information may differ from the contract in meaningful ways.

[15] One concern is that thresholds are preemptively adjusted to allow a borrower to avoid a violation. We investigate this issue by calculating covenant violations using true realizations from the current period (i.e., period *t*) and true thresholds from the prior period (i.e., period *t-1*). Results are reported in the Internet Appendix. We find moderate

enables us to isolate the measurement error induced by differences between true and estimated realizations, *Violation %* amounts to 16.80%, an overestimation of 1,455%. Finally, relying on both estimated realizations and thresholds results in a *Violation %* (*Overestimation %*) of 19.52% (1,706%). In all cases, Type I errors are much more common than Type II errors.

These findings yield important insights. First, the overestimation in covenant violations is driven primarily by differences between true and estimated realizations – namely, by using Compustat financial data and standardized covenant definitions. Second, even if one were able to eliminate one of the two sources of measurement error, considerable measurement error in estimated slack – and therefore substantial overestimation of covenant violations – would remain. For example, hand-collecting threshold changes from renegotiation contracts filed with the SEC, which could potentially eliminate threshold measurement error, would still result in an overestimation of the frequency of violations by more than 1,400%.

3.1.3 Large Measurement Error

Our analyses so far indicate that estimated slack significantly overstates the frequency of (Type I error) covenant violations. However, this evidence does not necessarily imply that measurement error in estimated slack is large in absolute terms. We investigate whether this is the case by calculating *Difference %*, the absolute value of the difference between the true and estimated measure scaled by the true measure. Table 3 Panel C reports the frequency of cases where this difference is equal to or greater than both 10 percent and 25 percent.

Column (1) shows that the proportion of observations with *Difference %* greater than 10 percent for covenant slack ranges from a low of 77% (interest coverage and debt-to-EBITDA ratios) to a high of 95% (current ratios). A similar picture emerges if we consider observations with measurement error larger than 25%. We then apply the same calculation separately to threshold measurement error (Columns (3) and (4)) and realization measurement error (Columns (5) and (6)). We find that realization measurement error is pervasive and quite large: Six of the eight covenant types register realization differences greater than 10% in at least 50% of

evidence that some preemptive altering occurs; however, this altering does not appear to be a primary reason for why violations are grossly overestimated when using estimated slack relative to true slack.

observations; meanwhile, net worth and tangible net worth covenants experience lower levels of realization measurement error. We also find that large realization measurement error is more common than large threshold measurement error for all covenant types other than net worth and tangible net worth covenants.[16] Overall, Panel C shows all covenant types are subject to frequent and large slack measurement error; however, the underlying source of the measurement error varies across covenant type.

We also study whether the extent of measurement error varies in the time elapsed since contract inception. In Figure 3 we display the proportion of firm-quarter observations that register a *Difference %* larger than 10% or 25% for covenant realizations (Panel A) and thresholds (Panel B) for each of the eight quarters following contract origination.[17] The figures indicate that realization differences are common, yet their frequency does not change considerably in the time elapsed since contract origination. Meanwhile, the frequency of threshold measurement error is relatively low at contract inception but increases sharply thereafter, which is consistent with prior research documenting frequent contract renegotiations that modify covenant thresholds but are not reported in Dealscan (Roberts and Sufi 2009b; Roberts 2015).

3.2 Determinants of Measurement Error

Our evidence of pervasive and large measurement error in estimated slack may be important for research related to covenant slack or tightness (e.g., Demiroglu and James 2010; Prilmeier 2017) because we show that these constructs cannot be precisely measured. However, whether this error affects economic inferences depends on its correlation with other variables of interest. In this section, we examine the determinants of slack measurement error and Type I errors at the firm-quarter level. This analysis provides insights into firm and contract characteristics that are associated with measurement error in estimated slack, which could be informative for

[16] A plausible explanation for this pattern is that net worth covenants commonly include income escalators that adjust the required threshold over time in relation to prior income generated by the borrower (Beatty et al. 2008).

[17] For Figure 3, we identify a firm-quarter as having a *Difference %* greater than 10 percent and 25 percent if the *Difference %* for any of the covenant types to which a borrower is subject during the quarter exceeds these thresholds. In the Internet Appendix, we report figures separately for each covenant type.

researchers. In Section 4.3, we also assess whether measurement error in covenant slack influences our ability to draw inferences about its determinants.

We report our findings in Table 4, where the dependent variables are *Large Slack Error* – an indicator set to one if *Difference %* is greater than 25 percent – in Columns (1) to (3) and *Type I Error* – an indicator set to one if estimated slack is negative but true slack is not – in Columns (4) to (6).[18] We find that *Large Slack Error* and *Type I Error* are positively correlated with financial losses, debt-to-EBITDA ratios and the number of covenant included in the loan contract, while other borrower, lender, and loan characteristics do not consistently display economically meaningful correlations. These findings have at least two implications for researchers. First, the findings suggest that measurement error may be greatest for firms likely to be closest to covenant thresholds, namely firms more likely to be included by studies on the consequences of covenant violations. Second, researchers should avoid including both estimated measures of covenant slack (e.g., tightness or probability of violation) and the number of covenants contemporaneously as explanatory variables in their empirical models because the correlation between these two variables could bias coefficient estimates.[19] Moreover, researchers should not necessarily interpret a positive correlation between the number of covenants and their measure of covenant slack as validation of the measure (e.g., Demerjian and Owens 2016) because the correlation could be driven by measurement error.

4. Does Measurement Error Change Inferences from Prior Research?

Our evidence of significant measurement error in estimated slack engenders the concern that inferences from prior research that rely on estimated slack may be incorrect or misleading. In this section, we investigate the extent to which relevant conclusions from prior research are sensitive to the documented measurement error. We also endeavor to help researchers deal with

[18] In the Internet Appendix, we report determinant analyses at the covenant level.

[19] Whether the number of covenants is correlated with measurement error in estimated slack depends on the measurement error in each covenant type and on the covariance among these errors. Because all covenants have substantial measurement error, the strong association between measurement error and number of covenant types is not surprising. If each covenant has a false positive rate of 10%, then the likelihood of a false positive in a firm quarter is 10% when there is only 1 covenant, but 27.1% (1-.9^3) if the firm has 3 covenants.

these measurement error issues by highlighting the settings and research design choices where measurement error is more or less likely to be problematic.

Debt covenant research examines both the determinants and consequences of covenant slack and covenant violations, meaning that estimated slack and estimated violations (with their measurement error) may reside in either the dependent variable or the independent variable. From an econometric standpoint, measurement error can lead to biased inferences if it is correlated with observed or unobserved explanatory variables (Roberts and Whited 2013). As Wooldridge (2010, pg. 73) notes, "… traditionally, measurement error in an explanatory variable has been considered a much more important problem than measurement error in the response variable."

For these reasons, we first revisit two sets of studies that investigate the consequences of estimated covenant violations and are likely sensitive to measurement error concerns because estimated slack is used as an independent variable. We start with research that relies on RDDs, focusing on Chava and Roberts' (2008) paper that helped introduce discontinuity designs to the covenant violation literature. In addition to assessing the robustness of Chava and Roberts' (2008) findings to measurement error, we also verify the validity of the two main identifying assumptions behind RDDs in the covenant violation setting. This verification is important because the measurement error we document makes it impossible for researchers that rely on estimated slack to accurately restrict their sample to observations near the threshold and to gauge the presence of threshold manipulation. We next focus on Bird et al. (2022a) and Bird et al. (2022b), papers in the stream of research that focuses on lenders' proclivity to enforce violations. These studies rely on the precise identification of covenant violation events, yet they use estimated slack to do so. Because we document that estimated slack grossly overstates the occurrence of covenant violations relative to true slack, it is plausible that these studies may overestimate the occurrence of lender forbearance as well.

Second, we examine research on the determinants of covenant slack. This research is likely less susceptible to measurement error because estimated slack features as the dependent variable, and there exists a fairly high positive correlation between estimated and true slack in our data (0.47). To analyze the extent to which measurement error in estimated slack affects researchers' ability to identify important determinants of covenant slack, we reexamine the finding in Prilmeier (2017) that covenant tightness is relaxed over the duration of a lending relationship.

Overall, our analysis leads to three insights: (1) important assumptions underlying RDDs appear violated in the financial covenant setting, which potentially makes the use of this design unwarranted for this setting; (2) the evidence of considerable lender forbearance is largely driven by Type I errors in estimated covenant violations; and (3) measurement error is not as problematic for research about the determinants of covenant slack; if anything, measurement error may obscure important economic determinants of covenant slack in the data.

4.1 Regression Discontinuity Designs around Covenant Violations

4.1.1 Reexamination of Chava and Roberts (2008)

We begin by reexamining in our sample period the finding of Chava and Roberts (2008) that firms' investment declines following a covenant violation. In the original paper, the authors focus solely on net worth and current ratio covenants. However, prior research shows a shift away from balance sheet covenants (i.e., current ratio and net worth covenants) that starts near the beginning of our sample period (Demerjian 2011; Christensen and Nikolaev 2012). Perhaps because of this trend, research subsequent to Chava and Roberts (2008) often includes debt-to-EBITDA covenants as well (Ferreira et al. 2018). Accordingly, we conduct our reexamination using current ratio, net worth, and debt-to-EBITDA covenants. We follow the authors and regress corporate investment on an indicator variable for negative slack (*Bind*), control variables, and both firm and year-quarter fixed effects. The inclusion of firm fixed effects means we are identifying the effect of covenant violations from firms that have at least one estimated violation. As such, we restrict the sample to firms with at least one estimated violation, as Chava and Roberts (2008) do.

Table 5, Panel A presents the associated coefficient estimates. Column (1), where we rely on the full sample and measure *Bind* using estimated slack, reports a negative and statistically significant (p-value < 0.01) coefficient, consistent with the evidence in Chava and Roberts (2008) that firms decrease their investment following a covenant violation. In Column (2), we modify the estimation in two ways: (1) We restrict the sample to firms that qualitatively discuss their compliance with covenants (Compliance Sample); and (2) we redefine *Bind* to be equal to zero if estimated slack is negative but the firm explicitly discloses that it complies with all covenants, which should significantly reduce Type I errors. We continue to observe a negative and statistically significant (p-value < 0.01) coefficient. Finally, in Column (3) we re-estimate the same regression, but this time we measure *Bind* using true slack, which restricts this estimation to our hand-collected true covenant information sample (True Slack Sample). In this smaller sample, we continue to observe a negative and marginally significant (p-value < 0.10) coefficient, consistent with the notion that covenant violations are associated with declines in investment. Therefore, we conclude that the evidence in Chava and Roberts (2008) is not driven solely by measurement error in estimated slack and estimated violations.[20]

4.1.2 Validity of identifying assumptions in RDD around covenant violations

The evidence that the findings in Chava and Roberts (2008) are robust to measurement error does not necessarily mean that they can be interpreted causally. As discussed by Bakke and Whited (2012), a sharp RDD has two key identifying assumptions. The first assumption requires that firms be assigned treatment solely based on an observed, continuous running variable (also called a forcing, selection, or assignment variable). Under this assumption, causal inference holds only near the threshold.[21] The second assumption requires that the running variable has a positive density around the cutoff, which formalizes the intuition that there should be no bunching of observations on either side of the threshold due to threshold manipulation (e.g., McCrary 2008; Cattaneo et al. 2018). Applied to the covenant setting, these assumptions stipulate that researchers must: (1) be able to observe the running variable (covenant slack); (2) restrict the sample to observations near the threshold; and (3) show evidence of no threshold manipulation.

Our measurement error analysis raises concerns about the validity of these assumptions. We show that slack is measured with significant error, suggesting that researchers cannot observe the true running variable or accurately identify observations near the true threshold. We also show

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[20] We find qualitatively similar but statistically weaker results if we conduct our reexamination using only net worth and current ratio covenants.

[21] The continuity assumption implies that as observations move farther away from the threshold, they are less comparable to one another, thereby reducing the validity of the design.

a low density of true violations below the threshold, raising the possibility of threshold manipulation. For these reasons, we use our reexamination of Chava and Roberts (2008) and our True Slack Sample to test the appropriateness of a sharp RDD in a covenant violation setting.[22]

Investment Around the Covenant Threshold

Conceptually, an RDD seeks to find a discontinuous jump in the outcome variable when crossing the covenant threshold. For example, if investment usually declines by 0.25% as firms experience a deterioration in financial performance and approach the covenant threshold, then we would expect a larger drop (say 0.75% or 1%) when crossing the threshold. We explore the behavior of investment around the threshold to verify whether the effect is concentrated in intervals close to the true covenant thresholds, as required by RDD, and find the opposite pattern in the data.

First, we repeat our reexamination of Chava and Roberts (2008) but focus on narrow bandwidths around the covenant threshold by restricting the sample to observations where *True Slack %* – the difference between the true realization and true threshold scaled by the true threshold – is equal to or less than 100%; this restriction eliminates 137 observations. Column (4) of Table 5, Panel A shows that, relative to Column (3), the point estimate declines from -0.007 to -0.002, and the test-statistic falls from -1.7 to -0.57. We next restrict the bandwidth to observations where *True Slack %* is equal to or less than 50%, which cuts the sample down to 4,181 observations. Column (5) of Table 5, Panel A indicates that the point estimate shrinks even further and turns positive (0.001), while the t-statistic drops to 0.229.[23]

In Table 5, Panel B, we report the number of observations and average investment for four different bins above (Bins 1–4) and four different bins below (Bins 5–8) the covenant threshold. In the first four columns, we present statistics using estimated slack. The table shows that investment is 0.84% lower for observations that have negative estimated slack (Bins 5–8).

[22] As noted in this discussion, a RDD requires the researchers to observe the running variable. Our compliance sample provides less benefit in this setting because it allows us to determine which observations cross the threshold (i.e., violations or not) but not to determine proximity to the threshold.

[23] In untabulated analyses, we verify whether the same patterns extend to estimated covenant slack. We find that using narrower bandwidths attenuates the coefficient on estimated covenant violations, but we believe that this finding is difficult to interpret. Estimated slack contains significant measurement error, so narrower bandwidths do not necessarily entail observations closer to the true threshold.

However, this difference is driven by observations further from the threshold. For example, investment drops by 0.22% when moving from Bin 1 to Bin 2, and by 1.05% when moving from Bin 2 to Bin 3; meanwhile, when crossing the covenant threshold (moving from Bin 4 to Bin 5), investment displays a relatively small increase of 0.09%. This lack of a discontinuous change in investment around the threshold could reflect measurement error. To mitigate this concern, we repeat this analysis using our True Slack Sample in the last four columns. A discontinuous change in investment around the covenant threshold seems to be lacking here as well: The two largest declines in investment occur when moving from Bin 2 to Bin 3 (-1.03%) and from Bin 5 to Bin 6 (-0.71%), respectively. Overall, this evidence implies that the negative effect of covenant violations on investment is primarily driven by observations that are not close to the threshold.

Table 5, Panel B displays a second noteworthy pattern: There exists extreme bunching just above the threshold for true slack: 30.0% of observations belong to the bin right above the covenant violation threshold (Bin 4), but only 0.6% belong to the bin right below it (Bin 5). This evidence suggests the prospect of manipulation around the threshold, possibly invalidating the second key RDD identifying assumption. We explicitly test for such manipulation in the next section.

Bunching Around the Covenant Threshold and Threshold Manipulation

In Figure 4, we plot the density distribution of true and estimated covenant slack. The figure does not show a significant discontinuity around covenant thresholds when relying on estimated slack, but the discontinuity exists and is quite dramatic for true slack. As discussed in Cattaneo et al. (2018), a discontinuity in the density of observations around a threshold is "interpreted as empirical evidence of self-selection or non-random sorting of units into control and treatment status," which limits the ability of researchers to assign causal interpretation to a design. Consistent with this interpretation, we see that Vishay Intertechnology, Inc. writes in its 10-K for the fiscal year 2010: "We expect to continue to be in compliance with these covenants based on current projections. We also have mechanisms, including deferral of capital expenditures and other discretionary spending, to facilitate on-going compliance." Nonetheless, this graphical and anecdotal evidence is only circumstantial evidence of manipulation around the threshold.

To more formally test the assumption of no manipulation of the running variable at the threshold, we follow Cattaneo et al. (2018), who offer researchers manipulation tests to gauge the likelihood that the no-manipulation assumption holds in the data, similar to the way researchers can test for parallel trends in the outcome variable in a difference-in-differences analysis. We present the results of this analysis graphically in Figure 5. The null hypothesis of this test is that the density of the running variable is continuous around the threshold. We find no evidence of manipulation around covenant thresholds when we perform these tests using estimated slack in Figure 5, Panel A (p-value > 0.1). In contrast, we find consistent and robust evidence of threshold manipulation when using true slack in Figure 5, Panel B (p-value < 0.01). That is, firms appear to endogenously sort into violators and non-violators, infracting the no manipulation assumption.[24]

Overall, our evidence indicates that the two main identifying assumptions of RDDs do not seem to hold in the true covenant information data. Thus, we caution researchers against interpreting evidence based on a RDD in the context of covenant violations as causal.

4.2 Lender Forbearance

A nascent research stream investigates lenders' proclivity to enforce estimated violations (e.g., Colonnello et al. 2021; Haque and Kleymenova 2023). This lender forbearance research relies predominantly on the research design used in Bird et al. (2022a) and Bird et al. (2022b), where an estimated violation is considered enforced if it is qualitatively disclosed by the borrower. In this section, we reexamine the main finding of these two papers and assess their sensitivity to measurement error. The structure of the two reexaminations follows the same pattern. We first replicate the result using estimated covenant violations within our three samples: the Full Sample, the Compliance Sample, and the True Slack Sample. We then verify the robustness of the finding to two alternative measures of covenant violation that reduce measurement error.

[24] Recent research by Bordeman and Demerjian (2022) uses estimated slack to document that no bunching exists just above the covenant threshold for debt-to-EBITDA covenants, which the authors interpret as evidence that firms do not engage in threshold manipulation. In untabulated analyses, we confirm the evidence of no bunching for these covenants with estimated slack but also find strong evidence of bunching with true slack.

In both cases, we can replicate the papers' results when applying their methodology to our three samples, mitigating concerns that our evidence is driven by lack of power or by the uniqueness of these samples. Meanwhile, we find that adjusting for measurement error considerably weakens the evidence of lender forbearance in terms of both statistical significance and economic magnitude. In light of these findings, we encourage researchers to use caution when drawing inferences about lender forbearance from estimated covenant violations.

4.2.1 Reexamination of Bird et al. (2022a)

Bird et al. (2022a) regress an indicator that identifies the disclosure of covenant violation in firms' SEC filings *(Enforcement)* on estimated covenant violations (*Negative Estimated Slack*), which they measure using realizations from Compustat and thresholds from Dealscan. The estimated coefficient from this regression, which they report in Table 3 Column (1) of their paper, is 10.534. This finding suggests that approximately 90% of estimated violations remain undisclosed in firms' SEC filings, which the authors interpret as evidence that lenders enforce approximately 10% of violations. We replicate the authors' findings, applying the same measurement choices and design to our three samples – Full Sample, Compliance Sample, and True Slack Sample – and report the results in Columns (1) to (3) of Table 6, Panel A, respectively.[25] The estimated coefficient is statistically different from zero at the 1% significance level, and its magnitude is between 7 and 11 – namely, in the neighborhood of 10.

We next verify the robustness and stability of this finding to measurement error. We first use a modified version of *Negative Estimated Slack*, where we reset *Negative Estimated Slack* to zero if estimated slack is negative but the firm explicitly discloses that it complies with all covenants (*Negative Estimated Slack2*). Column (4) of Table 6, Panel A shows that the coefficient estimate associated with *Negative Estimated Slack2*, which is less subject to Type I errors, is 94.172 (p-value < 0.01), suggesting that lenders "enforce" more than 94% of true violations. A

[25] The sample period in Bird et al. (2022a) is 1998–2006. The samples in both Bird et al. (2022a) and Bird et al. (2022b) are constructed at the loan package-quarter level. Because a firm may be subject to more than one covenant for a given quarter, we follow Bird et al. (2022a, 2022b) and determine firm-quarter measures of slack, including *Negative Estimated Slack*, based on the minimum value of slack across all covenant types.

similar picture emerges when we rely on our true covenant information, which further reduces measurement error in covenant violations, although with a much smaller sample: Column (5) reports an estimated coefficient of 94.37 (p-value < 0.01).[26]

Overall, we interpret our findings as suggesting that the evidence of considerable lender forbearance in Bird et al. (2022a) largely reflects measurement error in estimated slack, which induces a considerable overestimation of the frequency of covenant violations, rather than lenders' reluctance to enforce those violations.

4.2.2 Reexamination of Bird et al. (2022b)

Building on Bird et al. (2022a), Bird et al. (2022b) predict that lenders are more likely to enforce violations when facing incentives to meet short-term earnings targets. To test this prediction, they regress whether violations are disclosed in SEC filings (*Enforcement*) on the interaction between covenant violations (*Negative Estimated Slack*) and *STLender*, an indicator set to one if the lender displays an earnings-per-share (EPS) surprise of zero or one cent, and zero otherwise, to measure lenders' incentives to meet short-term earnings targets.

The authors' analysis lends support to this prediction: Column (2) of Table 2 of their paper reports a positive and statistically significant interaction term (coefficient = 0.29; p-value < 0.01), suggesting that lenders facing short-term incentives are 2.9 percentage points more likely to enforce violations, all else equal. We replicate the authors' findings applying the same measurement choices and design to our three samples – Full Sample, Compliance Sample, and True Slack Sample – and report the results in Columns (1) to (3) of Table 6, Panel B, respectively. The coefficient on the interaction term is always positive, with a magnitude between 0.035 and 0.053, and statistically different from zero at conventional significance levels.

[26] A coefficient smaller than 100 can result from lenders' not "enforcing" some violations or from differences in true violations and reported violations (i.e., qualitative violation disclosures). Nini et al. (2012) identify violations using a text-search algorithm. For a few observations in our sample, *True Slack* is negative, but no qualitative violation is identified. For example, for its fiscal quarter ending December 31, 2006, Fountain Powerboat Industries, Inc. reports a required fixed-charge coverage ratio threshold of 1.75 and an actual ratio of 0.37, indicating a violation. However, the Nini et al. (2012) text-search algorithm fails to identify a violation.

In Columns (4) and (5) of Table 6, Panel B, we report whether these results remain after adjustments that reduce measurement error. Column (4), where we replace *Negative Estimated Slack* with *Negative Estimated Slack2*, as previously defined, displays a *negative* and statistically insignificant coefficient of interest, which is the opposite of the predictions and findings of Bird et al. (2022b). Column (5), where we replace *Negative Estimated Slack* with *Negative True Slack*, displays a positive coefficient, but the estimate is statistically indistinguishable from zero.

Overall, we conclude that, after correcting measurement error issues, lenders' enforcement rates do not vary with short-term incentives.

4.3 Research on the Determinants of Covenant Slack

Papers on the consequences of covenant violations are not the only research that relies on estimated slack. A separate stream of studies focuses on the determinants of covenant slack or tightness (Billett et al. 2007; Demiroglu and James 2010; Rauh and Sufi 2010; Prilmeier 2017). Although this research relies on estimated slack as a measure of covenant slack, we expect its findings to be less sensitive to measurement error concerns compared to research on the consequences of covenant violation for two reasons. First, this research does not rely upon researchers' ability to precisely measure distance from the threshold or the occurrence of violations but simply to rank firms based on covenant slack, and estimated slack exhibits a relatively high correlation with true slack (0.47, significant at the 1% level in our True Slack Sample). This high correlation should allow researchers to use estimated covenant slack to identify the general association between slack and various firm, loan, and lender characteristics. Second, research on the determinants of slack uses covenant slack as the dependent variable, and measurement error in the dependent variable is typically considered less problematic than measurement error in independent variables (Wooldridge 2010). To illustrate these points, we reexamine the primary finding in Prilmeier (2017) that covenant tightness decreases over the duration of a lending relationship, and report our results in Table 7.

Following the design used by Prilmeier (2017), we regress average covenant tightness, based on estimated slack (i.e., *Estimated Tightness*) on relationship duration and a set of controls. In Table 7, Column (1), we present the results using our Full Sample. The coefficient on the variable of interest – *Log(Relation (Duration))* – is negative (-0.016) and statistically significant at the 5% level, consistent with the finding by Prilmeier (2017) that tightness decreases (or, equivalently, the slack increases) over the duration of a lending relationship. In Table 7, Column (2), we report results using our True Slack Sample.[27] Note that the coefficient of interest continues to be negative (-0.012), but it becomes insignificantly different from zero at conventional levels (p-value > 0.1). Finally, in Column (3), we replace *Estimated Tightness* with *True Tightness*, which is based on true slack, as the dependent variable. The coefficient of interest increases in economic magnitude (-0.024) and becomes statistically significant at the 5% level, consistent with a decrease in true covenant tightness over the duration of a lending relationship.

The results in Table 7 yield important insights. First, we show that similar inferences on the coefficient of interest are drawn from *Estimated Tightness* (Column (1)) and *True Tightness* (Column (3)). Further, the sign and magnitude of coefficients on *Log (Interest Coverage Ratio)*, *Rating* and *Not Rated* are nearly identical across Column (1) and Column (3), which is consistent with research documenting tighter covenants for riskier borrowers (Rauh and Sufi 2010; Demiroglu and James 2010). This suggests that determinants of estimated slack and true slack are similar, and that research interested in the determinants of covenant slack is likely less susceptible to measurement error concerns. Second, the results in Column (2) and Column (3) highlight that measurement error could not only lead researchers to incorrectly reject a null hypothesis, as we documented in Section 4.2, but in some cases may cause researchers to fail to reject a null hypothesis that should be rejected. We further explore this insight in Section 5.

5. Does Measurement Error Obscure Economic Phenomena?

One of the insights from Table 7 is that measurement error may obscure economic phenomena in the data and impair researchers' ability to find evidence in support of economic relations that do, in fact, exist. To further illustrate this point, we examine two outcomes – stock market responses and renegotiations – that economic theory predicts are associated with covenant violations but that researchers have struggled to document in the data.

[27] Please note that our Compliance Sample cannot be used in this setting because it simply tells us whether a firm is in compliance with its loan covenants; it does not include the value of their covenant realizations and thresholds.

Covenants serve as trip-wires that transfer decision rights from borrowers to lenders when borrowers may have incentives to make decisions that favor shareholders at the expense of lenders. As such, covenant violations (i.e., technical defaults) are material events that shift decision rights from the shareholders to the lenders (Beneish and Press 1993; DeAngelo et al. 2002; Falato and Liang 2016). These events can be costly for the borrower, creating incentives for managers to undertake both accounting and real actions to avoid violations (DeFond and Jiambalvo 1994; Dichev and Skinner 2002). Consequently, there are strong conceptual reasons to expect covenant violations to elicit significant negative stock market reactions.

In Table 8, Columns (1), we report our study of the market reaction over a three-day window around the release date in which the borrower's periodic filings indicate that an estimated covenant violation has occurred (*Estimated Violation*). Note that the coefficient on *Estimated Violation* is 0.002 and statistically indistinguishable from zero (p-value > 0.10). In Column (2), we replace estimated violations with true violations. The coefficient on *True Violation* is -0.039 and statistically significant (p-value < 0.01), consistent with a negative market response to true violations of 3.9%. We interpret the evidence as suggesting that measurement error in estimated violations biases the coefficient of interest toward zero, likely because the majority of these violations are Type I errors and thus do not warrant a negative market response.[28] This finding indicates that relying on estimated violations impairs researchers' ability to detect the large and statistically significant negative market reaction to covenant violations.

Once a covenant is violated, the lender can decide to recall the loan, renegotiate contractual terms to better reflect the borrower's new economic conditions, or waive the violation. To the extent that renegotiating the contract at least on some occasions is optimal for the contracting parties, there are strong conceptual reasons to expect a positive association between covenant violations and loan renegotiations in the data. Nonetheless, Denis and Wang (2014) document that as capital expenditures approach the covenant limit, the likelihood of covenant relaxation

[28] In untabulated analyses, we find similar evidence if we use alternative estimation periods of two days (0 to +1) or five days (-2 to +2) around the filing. We also find a statistically negative market response to covenant violations if we reset *Estimated Violation* to zero when the firm qualitatively reports that it is in compliance with covenants.

increases; meanwhile, after the covenant has been breached, the magnitude of the breach does not affect the likelihood of a renegotiation. Furthermore, the authors find no evidence that the likelihood of renegotiation is related to covenant slack. However, an alternative interpretation of this finding is that measurement error in covenant violations prevents researchers' from precisely measuring the timing of covenant violations and, thereby, their association with renegotiations.

In Table 8, Columns (3) and (4), we report the association between loan contract renegotiations and borrowers' estimated and true violations. We regress an indicator set to one if a loan is renegotiated in quarter *t+1* on *Estimated Violation* (Column (3)) and on *True Violation* (Column (4)), measured in quarter *t*. We note that the coefficient on *Estimated Violation* is negative and statistically insignificant (coefficient = -0.005; p-value = 0.117), indicating that current violations do not predict future renegotiations. However, the coefficient on *True Violation* is positive (coefficient = 0.020), statistically different from zero (p-value < 0.05), and economically meaningful: Firms with true violations are two and a half times more likely to renegotiate their loan in the next quarter, relative to the unconditional probability of renegotiation in our sample.[29] Thus, estimated violations hide in the data the real economic association that exists between violations and future renegotiations.

Overall, Table 8 indicates the measurement error in estimated violations may hinder researchers' ability to detect economically sound associations between covenant violations and outcomes predicted by theory.

6. Proposed Adjustments to Reduce Measurement Error

The evidence in Sections 4 and 5 highlights several problems with using estimated slack to identify covenant violations. In this section, we explore whether researchers could continue using Compustat data to measure covenant realizations but adjust standard covenant definitions in ways that reduce measurement error in estimated slack. We focus our analysis on covenants that use EBITDA (i.e., ICVR, FCVR, DBEBD, and SDBEBD) because they are the most common covenant types (e.g., Demiroglu and James 2010; Griffin et al. 2021) and because they contain

[29] We also find a positive association between covenant violations and future renegotiations if we reset *Estimated Violation* to zero when the firm qualitatively states that it is in compliance with covenants.

substantial realization measurement error, as we show in Section 3. For these covenants, we start from the Compustat proxy for EBITDA and consider various adjustments that should move this proxy closer to contractually defined EBITDA, thereby reducing measurement error.

As suggested by Demerjian and Owens (2016), the Compustat item that researchers should use to proxy for contractual EBITDA is OIBDP, which has several appealing features. The variable is widely populated, which reduces sample attrition; it is available on both a quarterly and annual basis; and importantly, it excludes depreciation, amortization, non-operating income, taxes, and special items, which are commonly excluded from the definition of EBITDA in debt contracts (Li 2010, 2016; Dyreng et al. 2017; Beatty et al. 2019). These benefits notwithstanding, contractual EBITDA definitions often exclude additional items that Compustat includes in OIBDP. In the Internet Appendix we present a standard Compustat income statement and eight common EBITDA adjustments found in debt contracts. The following seven adjustments are already applied to OIBDP and thus require no further action: (1) extraordinary, unusual, or nonrecurring items; (2) asset sales or dispositions; (3) asset write-downs; (4) restructuring charges; (5) non-operating income; (6) equity method earnings; and (7) adjustments related to insurance. However, non-cash compensation expense, a very frequent adjustment in contractual EBITDA, is not excluded from OIBDP in Compustat. Meanwhile, U.S. GAAP requires compensation expense to be reported as an operating expense, which Compustat excludes from OIBDP. The reason is that although some forms of compensation are paid in cash (salaries and wages), other forms, such as stock-based compensation expense (Compustat = STKCO) and pension expense (Compustat = XPR), are non-cash items and thereby are included in OIBDP. Ergo, adding back these expenses to OIBDP conceivably could reduce the measurement error in the four EBITDA-based covenants (i.e., ICVR, FCVR, DBEBD, and SDBEBD).

We present the results of this analysis in Table 9. Panel A reports the frequency of true versus estimated violations when using OIBDP and two adjusted versions of OIBDP for the four EBITDA-based covenants in our sample. When EBITDA is defined using OIBDP, the *Estimated Violation %* for these covenant types ranges from 10.3% (ICVR) to 57.3% (FCVR) – a repeat of the findings in Table 3, Panel A. Adding back stock-based compensation to OIBDP decreases the

Estimated Violation % for all four EBITDA-based covenants and reduces measurement error by 4.2%, to 10.0%, relative to standardized measures. Adding back both stock-based compensation and pension expense further reduces the *Estimated Violation %* for all four covenants; the improvement over the Demerjian and Owens (2016) baseline ranges from 6.9% (SDBEBD) to 19.4% (DBEBD). We conclude that adjusting OIBDP for non-cash compensation items reduces measurement error in EBITDA-based covenants to some degree, although estimated violations continue to significantly overstate the true number of violations for all four covenant types, even after making these adjustments. As a result, substantial measurement error remains.

We next verify whether the reduction in Type I errors associated with our proposed adjustments comes at the expense of increasing Type II errors. Table 9, Panel B shows that, as expected, our proposed adjustments decrease Type I errors across all four covenant types. This decrease ranges from 4.2% to 9.9% when adding back stock compensation, and from 6.9% to 19.2% when adding back both stock-based compensation and pension expense. In contrast, we fail to find any evidence of an increase in Type II errors. Thus, it appears that these adjustments are net positive. Meanwhile, we would emphasize once more that the improvements we document are economically modest, and Type I errors remain very common.[30] On this ground, we conclude that Compustat-based adjustments are not likely to substantially mitigate measurement error, and that any Compustat-based measure of estimated covenant violations can still produce biased inferences. We thus encourage researchers to use caution or rely on other approaches to identify covenant violations, such as the Nini et al. (2012) text-based approach or our hand-collected data.

7. Conclusion

Financial covenants help mitigate agency problems that arise between borrowers and lenders. A large literature discusses and examines the role that financial covenants play in debt markets. This literature frequently estimates covenant slack and violations using financial data from Compustat and covenant thresholds from Dealscan. In this paper, we use a unique, hand-

[30] Moreover, we acknowledge that our proposed adjustments may be inconsistent with the true covenant definition. Additionally, for periods prior to 2006, accounting standards did not require firms to report share-based compensation (Hayes et al. 2012); thus, these amounts are frequently zero or missing in Compustat prior to 2006.

collected dataset of firms' true covenant realizations and thresholds to investigate the frequency and magnitude of measurement error in traditional estimates of covenant slack and covenant violations. We show that using estimated covenant realizations and thresholds induces high levels of measurement error and frequently overstates the occurrence of covenant violations. Our evidence suggests that the measurement error is not random and that Type I errors occur approximately 78 times more frequently than Type II errors. We document that measurement error associated with estimated slack arises from both realization and threshold measurement errors, although measurement error associated with covenant realizations is more severe.

We also show that this measurement error influences the viability of RDDs around covenant violations, changes inferences in prior research, and hinders researchers' ability to document true economic relations. Regarding RDD, we find that while the evidence in prior research may be robust to measurement error, these studies likely violate the assumption that the documented effect is concentrated near the covenant thresholds and the assumption of no manipulation around the threshold. Therefore, their findings should be interpreted cautiously, and perhaps not causally. Regarding prior inferences, we show that measurement error is most problematic when researchers need to precisely identify instances of covenant violation, such as for lender forbearance research. At the same time, we find that measurement error is likely less concerning for research that neither relies on RDD nor requires the precise identification of covenant violations, such as studies on the determinants of covenant slack. Finally, we show that true violations are associated with a negative market response and renegotiations while estimated violations are not, demonstrating that reducing measurement error can yield new insights.

Given our findings, we propose adjustments to Compustat-based covenant realization measures to reduce slack measurement error. We find that our proposed adjustments decrease Type I errors without increasing Type II errors, but also that these improvements are modest. Thus, we plan to make our hand-collected dataset of true covenant slack publicly available, with the hope that researchers can validate their findings using these data or determine the extent to which slack measurement error correlates with their variables of interest.

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Appendix A – Variable Definitions

Variable	Description	Source
CFO Volatility	The variance of cash flow from operating activities for the previous 12 quarters scaled by total assets	Compustat
EPU Index	The Economic Policy Uncertainty Index	Baker et al. (2016)
Estimated Realization	The covenant ratio or amount estimated by using accounting numbers from Compustat based on covenant definitions from Demerjian and Owens (2016)	Compustat
Estimated Slack	For covenant types subject to a minimum threshold, it is the estimated realization less the estimated threshold. For covenant types subject to a maximum threshold, it is the estimated threshold less the estimated realization.	Compustat & Dealscan
Estimated Threshold	Required covenant threshold specified in the original debt agreement. We require fiscal period date to be between a facility's beginning and ending date.	Dealscan
Estimated Violation	An indicator variable equal to one if *Estimated Slack* is negative, and zero otherwise	Compustat & Dealscan
Large * Error	An indicator equal to one if the absolute value of the difference between the true and estimated measure scaled by the true measure exceeds 25%	Compustat & Dealscan
Leverage	Total long-term debt (DLCQ + DLTTQ) scaled by Total Assets (ATQ)	Compustat
Log (Assets)	Natural log of one plus total assets ($Ln(1 + ATQ)$)	Compustat
Log (Loan Amount)	Natural log of one plus the total funded amount of the underlying deal package	Dealscan
Log (Maturity)	The natural log of one plus the length, in months, between the activation date of the credit agreement and maturity date.	Dealscan
Loss	An indicator equal to one if quarterly net income is negative (NIQ), and zero otherwise	Compustat
MB	Market-to-book ratio computed as the ratio of market value of equity (PRCCQ × CSHOQ) to book value of equity (CEQQ)	Compustat
N_Covenants	Number of financial covenants that a firm discloses it is subject to in its periodic filings	EDGAR
Relationship Lender	An indicator variable equal to one if part of the loan package contains a relationship loan. Following Bharath et al. (2011), we define a loan (i.e., facility) as a relationship loan if any of the lead arrangers for the loan had previously been a lead arranger on any loans to the same borrower in the previous five years.	Dealscan
Renegotiation	An indicator equal to one if a loan is renegotiated in quarter t+1, and zero otherwise	Thomson One
Reported Violation	An indicator to one if a borrowing firm qualitatively discloses a violation in its periodic filing for a given firm-quarter (e.g., Nini et al. 2012)	Nini et al. (2012)

ROA	Income before extraordinary items (IBQ) scaled by total assets (ATQ)	Compustat
Sales Growth	Sales for the current quarter divided by sales for the same fiscal quarter from the previous year (i.e., lagged four quarters sales) less one $\left(\frac{SALEQ_q}{SALEQ_{q-4}}\right) - 1$	Compustat
S&P Rated	Indicator equal to one if the firm has an S&P Credit Rating	Compustat
Tangibility	Net Property, Plant and Equipment scaled by total assets ($\frac{PPENTQ}{ATQ}$)	Compustat
True Realization	The covenant ratio or amount disclosed in a firm's periodic filing	EDGAR
True Slack	For covenant types subject to a minimum threshold, it is the true realization less the true threshold. For covenant types subject to a maximum threshold, it is the true threshold less the true realization.	EDGAR
True Threshold	Required covenant threshold disclosed in a firm's periodic filing	EDGAR
True Violation	Indicator equal to one if *True Slack* is negative, and zero otherwise	EDGAR
WhitedWu	Whited and Wu (2006) Index	Compustat
ZScore	Following Graham et al. (2008), we calculate a modified Altman (1968) Z-score as follows: $ZScore = (1.2 * WCAPQ + 1.4 * REQ + 3.3 * PIQ + 1 * SALEQ)/AT$, where WCAP is working capital (current assets less current liabilities plus the current portion of long-term debt), REQ is retained earnings, and PIQ is pretax income, SALE is total sales, and ATQ is total assets.	Compustat
Covenant Definitions from Demerjian and Owens (2016)		
Interest Coverage Ratio (ICVR)	$OIBDPQ/XINTQ$	Compustat
Fixed Charge Coverage Ratio (FCVR)	$\frac{OIBDPQ_q}{XINTQ_q + DLCQ_{q-1} + XRENT}$	Compustat
Debt-to-EBITDA (DBEBD)	$(DLTTQ + DLCQ)/OIBDPQ$	Compustat
Senior Debt-to-EBITDA (SDBEBD)	$DLTTQ + DLCQ / OIBDPQ$	Compustat
Net Worth (NW)	$ATQ - LTQ$	Compustat
Tangible Net Worth (TNW)	$ATQ - INTANQ - LTQ$	Compustat
Capitalization Ratio or Leverage (DBAT)	$(DLTTQ + DLCQ)/ATQ$	Compustat
Current Ratio (CRTO)	$ACTQ / LCTQ$	Compustat
Unique Variables in Chava and Roberts (2008) Reexaminations		
Bind	An indicator equal to one if *True Slack* is negative and zero otherwise. If a borrower is subject to more than one of the covenant types in Chava and Roberts (2008) then we use the minimum slack value amongst the relevant covenant types.	EDGAR
Cash Flow	Ratio of income before extraordinary items plus depreciation and amortization to start-of-period net property, plant, and equipment	Compustat

EBind	An indicator equal to one if *Estimated Slack* is negative and zero otherwise. If a borrower is subject to more than one of the covenant types in Chava and Roberts (2008) then we use the minimum slack value amongst the relevant covenant types.	Compustat & Dealscan
ESlack	The minimum *Estimated Slack %* value across all covenant types that a borrower is subject to at a given firm-quarter, where *Estimated Slack %* is the difference between the estimated realization and estimated threshold scaled by the estimated threshold.	Compustat & Dealscan
Investment	Ratio of capital expenditures to the start-of-period net property, plant, and equipment	Compustat
Macro q	Sum of total book debt and market equity less total inventories divided by start-of-period net property, plant, and equipment	Compustat
TSlack	The minimum *True Slack %* value across all covenant types that a borrower is subject to at a given firm-quarter, where *True Slack %* is the difference between the true realization and true threshold scaled by the true threshold.	EDGAR
Unique Variables in Bird et al. (2022a, 2022b) Reexaminations		
Enforcement	An indicator equal to one if a borrowing firm qualitatively discloses a violation in its periodic filing for a given firm-quarter and zero otherwise	Nini et al. (2012)
Negative Estimated Slack	An indicator equal to one if Estimated Slack is negative and zero otherwise. Because a firm may be subject to more than one covenant at a time, we follow Bird et al. (2022a) and determine Estimated Slack based on the minimum slack value across all covenant types that a borrower is subject to at a given point in time.	Compustat & Dealscan
Negative Estimated Slack2	An indicator equal to one if Estimated Slack is negative and the borrowing firm does not disclose that they are in compliance with all financial covenants, and zero otherwise	Compustat & Dealscan
Negative True Slack	An indicator equal to one if True Slack is negative, and zero otherwise	EDGAR
STLender	An indicator equal to one if the Lender EPS Surprise equals zero or one cent and zero otherwise. Lender EPS Surprise is the realized earnings per share (EPS) minus the median analyst EPS forecast.	I/B/E/S
Unique Variables in Prilmeier (2017) Reexamination		
Estimated (True) Tightness	Average tightness of the loan's financial covenants, where tightness is determined by evaluating the cumulative normal distribution function using the *Estimated (True) Slack* of the covenant in the quarter immediately prior to the start date of the loan divided by the standard deviation of the corresponding financial ratio (or amount) over the previous 12 quarters.	Compustat & Dealscan (& EDGAR)
Log (Lenders)	Natural log of one plus the number of lenders in the loan syndicate	Dealscan
Log (Relation (Duration))	Natural log of one plus the number of years elapsed since the borrower first obtained a loan arranged by the same bank.	Dealscan
Not Rated	Indicator variable equal to if the firm has no S&P rating	Compustat
Rating	Categorical variable that equals zero if the firm has no S&P long-term issuer credit rating, 1, 2, 3, 4, if the rating is AAA, AA+, AA, AA-, respectively, and so on	Compustat
S&P 500	An indicator equal to one if the borrower is a member of the S&P 500 Index, and zero otherwise	Compustat

Appendix B – Examples of Measurement Error in Current Ratios

We examine Chaparral Energy, Inc.'s 10-K Filing for the year ended December 31, 2007 to elucidate the measurement issues in current ratios. Chaparral Energy reports its required current ratio threshold (1.0) and then provides a detailed breakdown of how the credit agreement defines current assets and liabilities, noting that a current ratio based on the US GAAP definition of current assets and liabilities would lead to the incorrect conclusion that the firm is violating their current ratio covenant (0.88 on 12/31/2006 and 0.69 on 12/31/2007). In contrast, the true current ratios used in their covenants are well above the required threshold (2.15 on 12/31/2006 and 1.49 on 12/31/2007), with the primary reason for the differences being the availability under the Credit Agreement (i.e., unused capacity). In our review of false positives, we found that over 95% of false positives had a credit agreement that included unused capacity in the definition of current assets.

Our Credit Agreement requires us to maintain a Current Ratio, as defined in our Credit Agreement, of not less than 1.0 to 1.0. The definition of current assets and current liabilities used for determination of the current ratio computed for loan compliance purposes differs from current assets and current liabilities determined in compliance with generally accepted accounting principles. Since compliance with financial covenants is a material requirement under our Credit Agreement, we consider the current ratio calculated under our Credit Agreement to be a useful measure of our liquidity because it includes the funds available to us under our Credit Agreement and is not affected by the volatility in working capital caused by changes in the fair value of derivatives. At December 31, 2006 and 2007, our current ratio as computed using generally accepted accounting principles was 0.88 and 0.69, respectively. After giving effect to the adjustments, our current ratio computed for loan compliance purposes was 2.15 and 1.49, respectively. The following table reconciles our current assets and current liabilities using generally accepted accounting principles to the same items for purposes of calculating the current ratio for our loan compliance:

(Dollars in thousands)	December 31, 2006	December 31, 2007
Current assets per GAAP	$ 91,863	$ 120,704
Plus—Availability under Credit Agreement	112,136	76,311
Less—Deferred tax asset on derivative instruments and asset retirement obligation	(847)	(19,123)
Less—Short-term derivative instruments	(7,599)	—
Current assets as adjusted	$ 195,553	$ 177,892
Current liabilities per GAAP	$ 104,255	$ 174,980
Less—Short-term derivative instruments	(12,376)	(54,307)
Less—Short-term asset retirement obligation	(749)	(1,000)
Current liabilities as adjusted	$ 91,130	$ 119,673
Current ratio for loan compliance	2.15	1.49

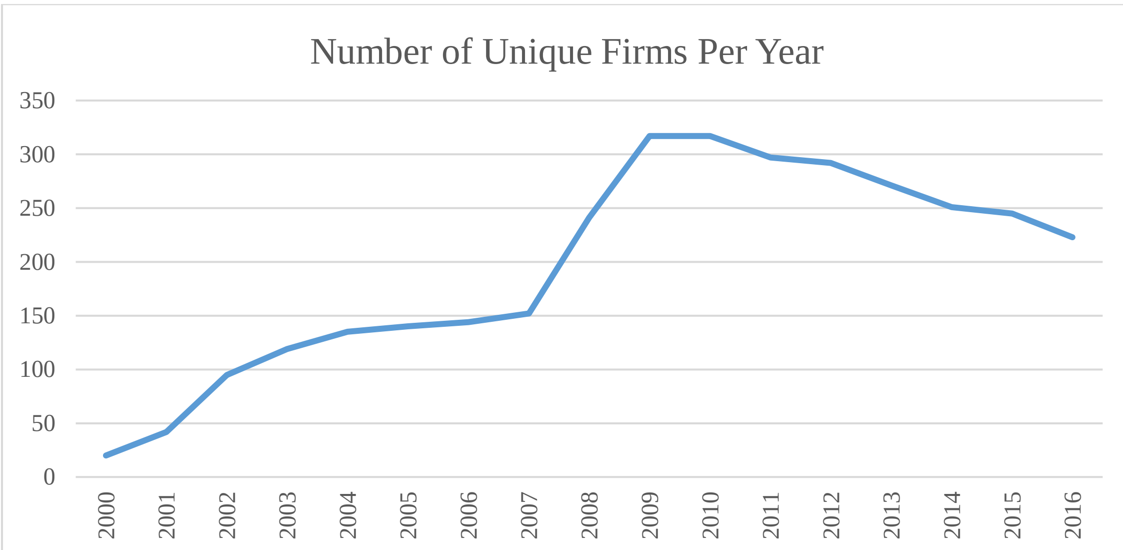
In the loan agreements we reviewed, current ratios are typically defined in one of two ways. In some cases, the definition of the current ratio explicitly mentions unused borrowing capacity.[31] In other cases, the current ratio definition refers to the ratio of current assets to liabilities, and current assets is subsequently defined as including unused borrowing capacity (and other adjustments).[32]

[31] For example, Asbury Automotive Group's 2008 loan agreement defines current ratio as "the ratio of (a) the sum of Consolidated Current Assets *plus Available Unused Commitments* to (b) Consolidated Adjusted Current Liabilities."
[32] For example, Abraxas Petroleum's 2009 loan agreement states "the Borrower shall not permit, as of the end of any fiscal quarter, the ratio of (a) its consolidated current assets to (b) its consolidated current liabilities, to be less than 1.00 to 1.00. For purposes of this calculation, "current assets" shall include, as of the date of calculation, the aggregate Unused Revolving Commitment Amounts but shall exclude, as of the date of calculation (A) any cash deposited with or at the request of a counterparty to any Hedge Contract … and (B) any assets of Borrower, … representing a valuation account arising from the application of SFAS 133 and 143."

Figure 1: Sample Overview

This figure reports details about the sample size over time. Panel A reports the number of unique firms that report both actual realizations and actual thresholds for at least one quarter in a given year. Panel B reports the number of unique firms in our sample for a given year compared to the total number of unique firms at the intersection of Compustat and Dealscan data (with non-missing financial covenant information). These ratios are plotted as a percentage for each year in our sample (2000–2016).

Panel A – Sample Size Over Time



Panel B – Sample Comparison Over Time

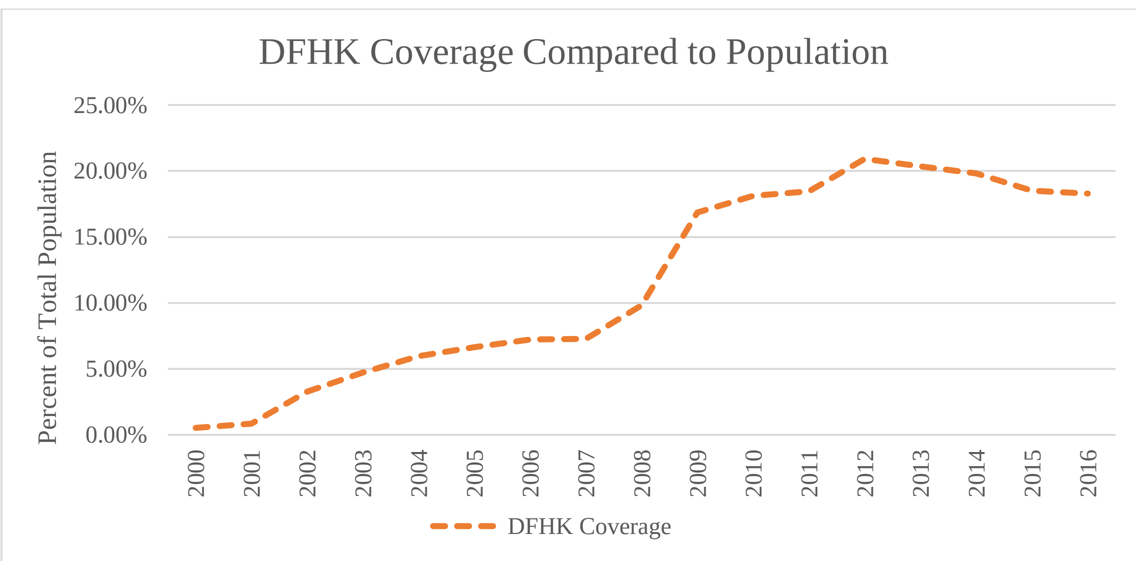
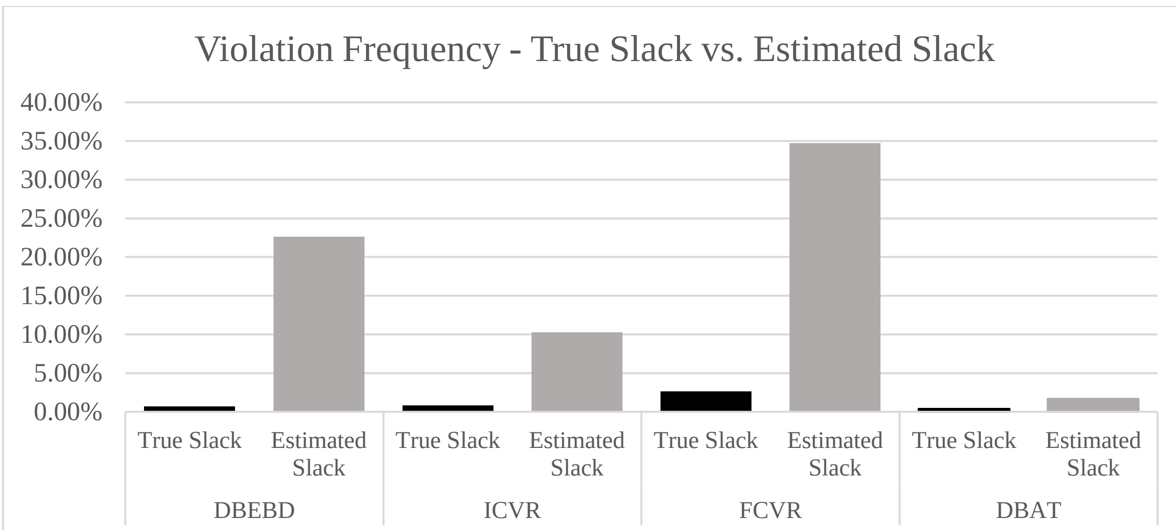


Figure 2: Violation Frequency (True Slack versus Estimated Slack)

This figure compares the frequency of violations calculated from *True Slack* relative to *Estimated Slack*. Panel A reports the comparison for the four most common covenants. Panel B reports the comparison of the fifth to eighth most common covenants. *True Slack* is computed as true realization less true threshold (true threshold – true realization) for covenants with a minimum (maximum) threshold. *Estimated Slack* is computed as estimated realization less estimated threshold (estimated threshold – estimated realization) for covenants with a minimum (maximum) threshold, where estimated realizations are determined using Compustat data and standardized covenant definitions from Demerjian and Owens (2016), and where estimated thresholds are based on the initial covenant thresholds specified in Dealscan.

Panel A – Four Most Common Covenants



Panel B – Other Common Covenants

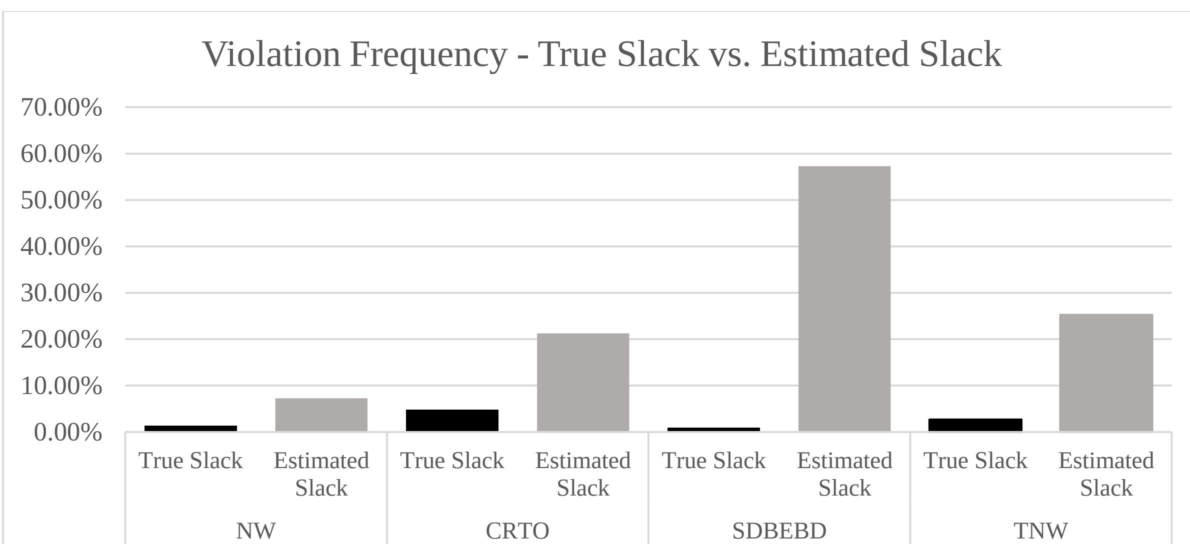
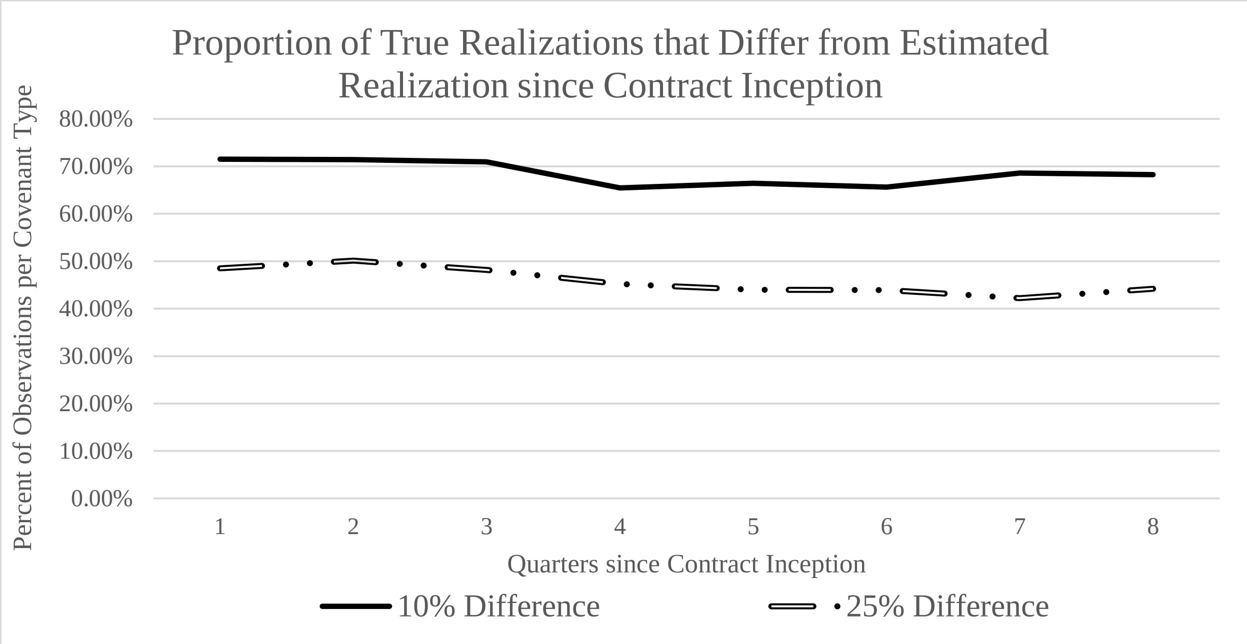


Figure 3: Realization and Threshold Measurement Error Since Contract Origination

This figure reports the frequency of firm-quarter observations from our True Slack Sample that are subject to realization measurement error (Panel A) or threshold measurement error (Panel B) in the eight quarters since contract origination. Realization or threshold measurement error occurs when *Difference %* exceeds 10 or 25 percent, where *Difference %* is equal to $\frac{|True\ Measure - Estimated\ Measure|}{True\ Meaure}$, and measure is covenant realization or threshold. Covenant calculations and definitions are reported in Appendix A.

Panel A – Threshold Measurement Error since Contract Inception



Panel B – Realization Measurement Error since Contract Inception

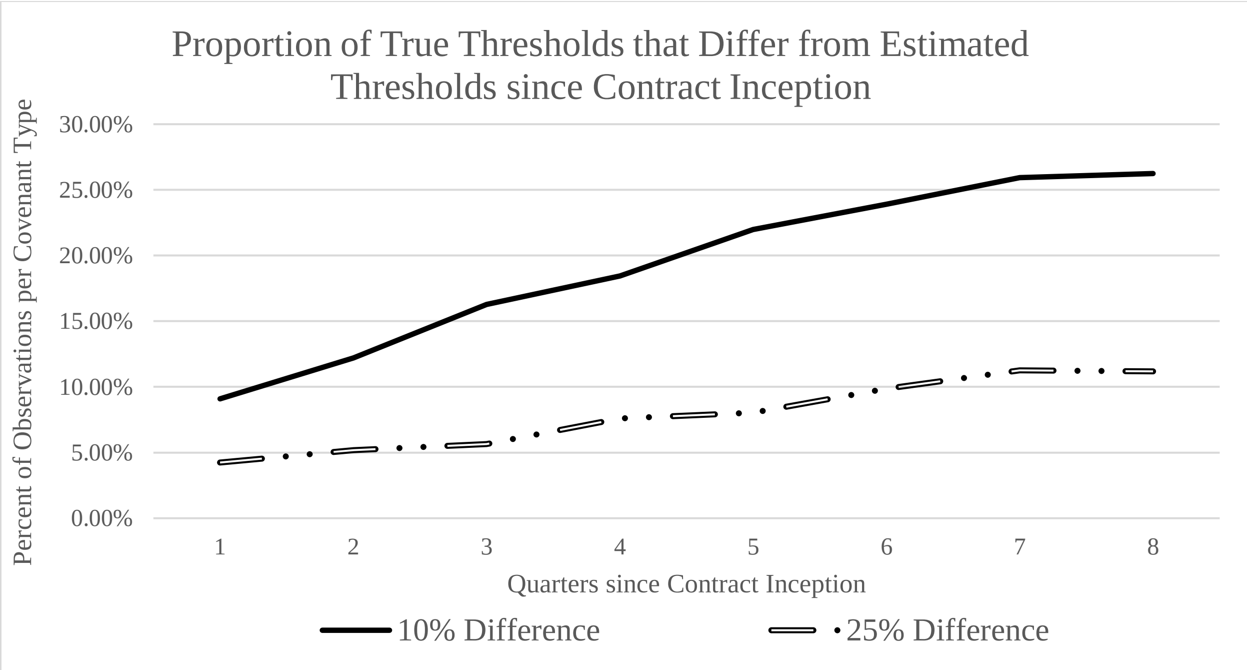


Figure 4 – Density Distribution of True Slack versus Estimated Slack

This figure reports the density distribution of *True Slack* relative to *Estimated Slack*. For covenant types with a minimum threshold, covenant slack is computed as the covenant realization less the covenant threshold, scaled by the covenant threshold. For covenant types with a maximum threshold, covenant slack is computed as the covenant threshold less the covenant realization, scaled by the covenant threshold. For these computations, *True Slack* relies on true realization and true thresholds, whereas *Estimated Slack* relies on estimated realizations and estimated thresholds. The figure reports results at the firm-quarter level. For firms subject to more than one covenant type, *Estimated Slack* and *True Slack* are the minimum values of *Estimated Slack* and *True Slack*, respectively, across all covenant types. For presentation purposes, we winsorize both True Slack and Estimated Slack at (-2, +2).

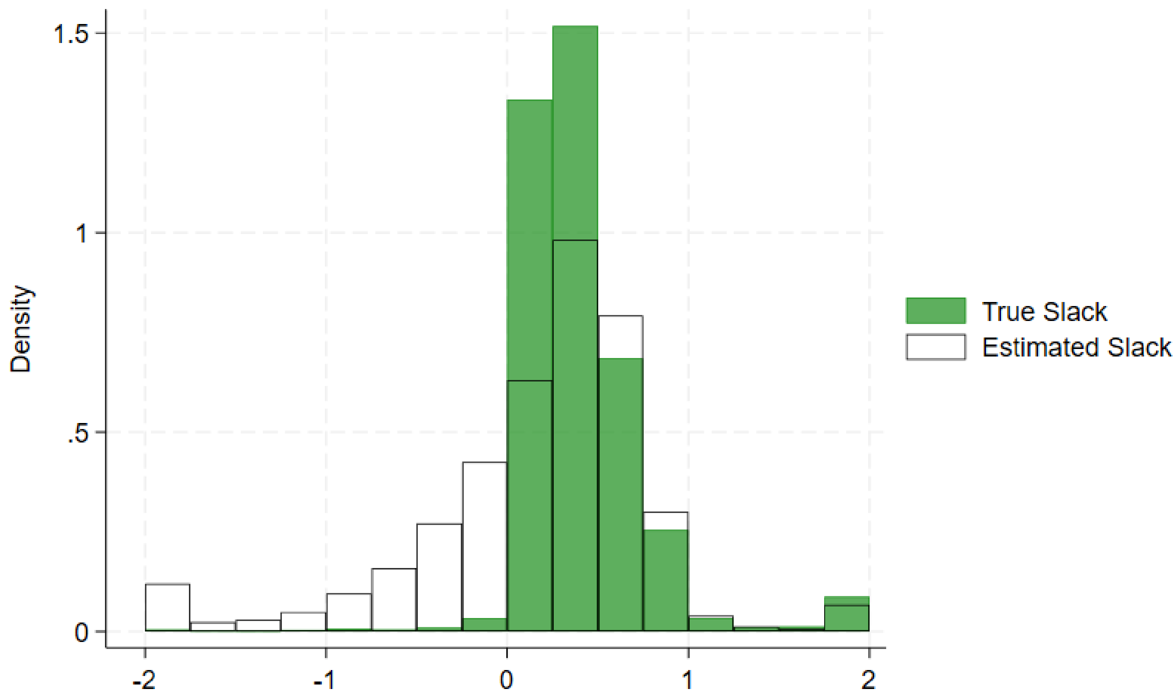
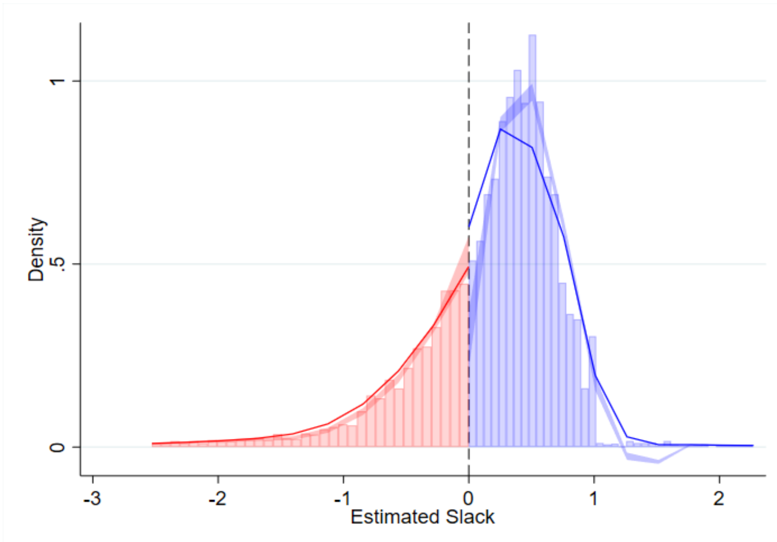


Figure 5: Threshold Manipulation using Estimated and True Slack

This figure provides the Cattaneo et al. (2018) threshold manipulation test using estimated slack (Panel A) and true slack (Panel B). *True Slack* is computed as true realization less true threshold (true threshold – true realization) for covenants with a minimum (maximum) threshold. *Estimated Slack* is computed as estimated realization less estimated threshold (estimated threshold – estimated realization) for covenants with a minimum (maximum) threshold, where estimated realizations are determined using Compustat data and standardized covenant definitions from Demerjian and Owens (2016) and where estimated thresholds are based on the initial covenant thresholds specified in Dealscan.

Panel A – Threshold Manipulation using Estimated Slack



Panel B – Threshold Manipulation using True Slack

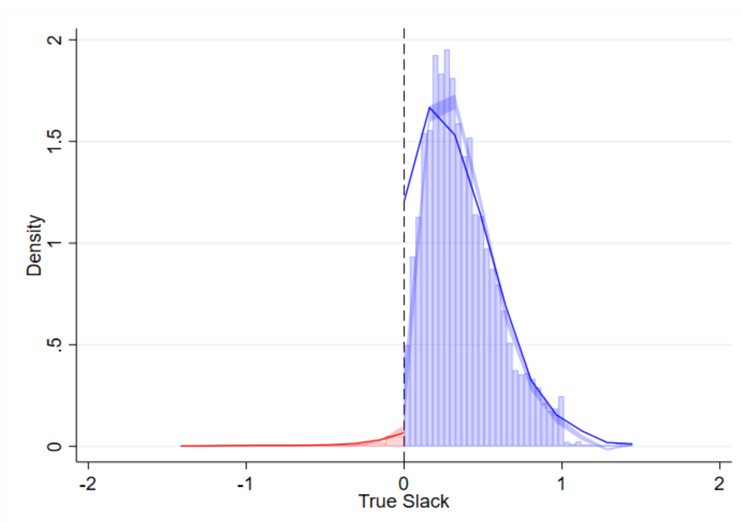


Table 1: Sample Construction

Panel A – Sample Selection

Sample Description	Unique Firm-Quarters	Unique Deals	Unique Firms
Dealscan Packages with Dealdates outstanding between 1/1/2000 and 12/31/2016 and at least one reported covenant	-	26,896	10,991
With GVKEY Match using Roberts Linking Table	-	24,094	8,899
Unique Firm-Quarters ending between 1/1/2000 and 12/31/2016 with Compustat Data for Realizations	128,722	18,357	6,244
Unique Firm-Quarters ending between 1/1/2000 and 12/31/2016 with Compustat Data for Realizations and Qualitative Violation Data (e.g., Nini et al. 2012)) [**Full Sample**]	93,092	9,906	4,493
Disclose Compliance Status [**Compliance Sample**]	61,303	8,603	4,052
Disclose True Covenant Realizations and Thresholds [**True Slack Sample**]	9,799	1,431	712

Panel B – Sample by Covenant Type

		Disclose Covenant Details	
Covenant Type	**Abbreviation**	**Observations**	**Unique Firms**
Debt-to-EBITDA*	DBEBD	6,640	469
Interest Coverage Ratio*	ICVR	4,309	328
Fixed Charge Coverage Ratio*	FCVR	2,296	222
Leverage (Capitalization) Ratio*	DBAT	2,102	112
Senior Debt-to-EBITDA	SDBEBD	848	93
Net Worth*	NW	705	73
Tangible Net Worth*	TNW	411	52
Current Ratio	CRTO	407	42
Debt-to-Tangible Net Worth	DBTNW	183	15
EBITDA	EBITDA	133	32
Debt Service Coverage Ratio	DSCVR	129	20
Debt-to-Net Worth	DBNW	54	6
Total Covenant Observations (Top 8 Covenant Types)		18,217 (17,718)	

This table provides sample selection. In Panel A, we show sample construction. We begin our sample with the intersection of Compustat and Dealscan data and keep all observations with necessary data. We then identify three different samples: first, the full sample of firm-quarter observations with necessary Compustat and Dealscan data (Full Sample); second, the sample of firm-quarter observations with discussion of compliance with covenants (Compliance Sample); and third, the sample of firms that provide true thresholds and realizations, allowing us to measure true covenant slack (True Slack Sample). In Panel B, we report the number of firm-quarter observations and number of unique firms per covenant type for 12 unique covenants for which we are able to hand-collect data on reported true realizations and true thresholds for 2000–2016. The table also reports the total number of unique covenant observations included in our sample. In subsequent analysis, we focus our attention on the eight most common covenant types: Debt-to-EBITDA (DBEBD), Interest Coverage (ICVR), Fixed Charge Coverage (FCVR), Leverage (DBAT), Senior Debt-to-EBITDA (SDBEBD), Net Worth (NW), Tangible Net Worth (TNW), and Current Ratio (CRTO) covenants.

Table 2: Determinants of Disclosing True Covenant Information

Panel A – Descriptive Statistics

	1	2	3	4	5	6
	Disclose True Covenant Info.	*Disclose Compliance*	*No Disclosure*	*Difference 1-2*	*Difference 1-3*	*Difference 2-3*
Log(Assets)	7.74	6.82	6.72	0.92***	1.02***	0.10***
Leverage	0.36	0.31	0.28	0.05***	0.08***	0.03***
Current Ratio	1.73	1.98	2.14	-0.25***	-0.41***	-0.16***
Tangibility	0.35	0.31	0.31	0.04***	0.04***	0.00
Loss	0.22	0.28	0.23	-0.06***	-0.01**	0.05***
Sales Growth	0.06	0.10	0.13	-0.04***	-0.07***	-0.03***
Zscore	1.16	1.23	1.30	-0.07***	-0.14***	-0.07***
Debt-to-EBITDA	3.23	2.71	2.36	0.52***	0.87***	0.35***
Interest Coverage	13.79	20.63	21.11	-6.84***	-7.32***	-0.48

Panel B – Multivariate Regression

	Disclose True Covenant Information =1				
	(1)	(2)	(3)	(4)	(5)
Log(Assets)	0.029***	0.008*	0.011**	0.003***	0.003***
	(8.207)	*(1.888)*	*(2.368)*	*(3.407)*	*(3.576)*
Leverage	0.072***	0.054***	0.055***	0.014***	0.014***
	(4.063)	*(3.172)*	*(2.752)*	*(3.638)*	*(3.529)*
Current Ratio	-0.004**	-0.006***	-0.008***	-0.001**	-0.001**
	(-2.076)	*(-2.966)*	*(-2.771)*	*(-2.282)*	*(-2.036)*
Tangibility	0.007	0.003	-0.012	-0.003	-0.003
	(0.337)	*(0.093)*	*(-0.375)*	*(-0.656)*	*(-0.595)*
Loss	-0.011**	-0.003	-0.007	0.000	-0.001
	(-1.968)	*(-0.625)*	*(-1.155)*	*(0.051)*	*(-0.425)*
S&P Rated	-0.001	0.019	0.015	-0.001	-0.001
	(-0.121)	*(1.621)*	*(1.177)*	*(-0.418)*	*(-0.351)*
CFO Volatility	-0.000	-0.000	-0.000**	-0.000*	-0.000**
	(-1.244)	*(-1.032)*	*(-1.986)*	*(-1.906)*	*(-1.981)*
Sales Growth			-0.000	-0.000	-0.000
			(-1.344)	*(-0.056)*	*(-0.030)*
ZScore			0.000	0.000	0.000
			(0.271)	*(0.874)*	*(0.749)*
Debt-to-EBITDA			-0.000	0.000	0.000
			(-1.012)	*(0.206)*	*(0.223)*
Interest Coverage			0.000	0.000	0.000
			(0.223)	*(0.532)*	*(0.543)*
EPU Index				0.000***	0.000***
				(3.511)	*(3.487)*
$Disclose_{t-1}$				0.885***	0.885***
				(119.348)	*(119.289)*
Reported Violation					0.008***
					(2.722)
Observations	89,856	86,434	72,241	72,241	72,241
Adjusted R^2	0.0326	0.137	0.141	0.789	0.789
Fixed Effects	None	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender

Panel A provides descriptive statistics for firms that disclose true covenant information (Column (1)), disclose compliance (Column (2)), and provide no disclosure (Column (3)). Columns (4) through (6) provide a pairwise comparison of means for each of the groups. Panel B provides the determinants of full disclosure using a linear probability model. The dependent variable equals 1 if the firm discloses both the true threshold and true realization. We winsorize continuous variables at the 1st and 99th percentiles. *, **, and *** represent statistical difference at the 10%, 5%, and 1% levels, respectively, using a two-tailed t-test. Appendix A defines all variables.

Table 3: True and Estimated Covenant Violations

Panel A – Comparison of True Violations to Estimate Violations

		True Violations		*Estimated Violations*				*Type I & Type II Errors*		
Covenant	**N**	**Violations**	**Violation %**	**Violations**	**Violation %**	**False Positive Error Rate**	**Overestimation %**	**Type I Error**	**Type II Error**	**Percent Type I Error**
DBEBD	6,011	42	0.7%	1,357	22.6%	97.6%	3131.0%	1,324	9	99.3%
ICVR	3,589	29	0.8%	369	10.3%	94.6%	1172.4%	349	9	97.5%
FCVR	1,708	45	2.6%	593	34.7%	93.6%	1217.8%	555	7	98.8%
DBAT	1,855	7	0.4%	31	1.7%	93.5%	342.9%	29	5	85.3%
NW	456	6	1.3%	33	7.2%	87.9%	450.0%	29	2	93.5%
CRTO	372	18	4.8%	204	54.8%	91.7%	1033.3%	187	1	99.5%
SDBEBD	335	3	0.9%	192	57.3%	98.4%	6300.0%	189	0	100.0%
TNW	297	8	2.7%	75	25.3%	92.0%	837.5%	69	2	97.2%
Totals	14,623	158	1.08%	2,854	19.5%	95.7%	1706.3%	2,731	35	98.7%

Panel B – Measurement Error Decomposition

	Covenant (N=14,623)	**Violations**	**Violation %**	**Overestimation %**	**Type I Error**	**Type II Error**
1	True Slack	158	1.08%	-	-	-
2	True Realization & Estimated Threshold	943	6.45%	497%	807	22
3	Estimated Realization & True Threshold	2,457	16.80%	1455%	2,330	31
4	Estimated Slack	2,854	19.52%	1706%	2,731	35

Panel C – Large Measurement Error by Covenant Type

	Slack Measurement Error		*Threshold Measurement Error*		*Realization Measurement Error*	
Covenant	**Greater than 10%**	**Greater than 25%**	**Greater than 10%**	**Greater than 25%**	**Greater than 10%**	**Greater than 25%**
DBEBD	77%	61%	40%	15%	57%	34%
ICVR	77%	56%	31%	13%	65%	33%
FCVR	94%	87%	33%	15%	89%	73%
DBAT	91%	81%	9%	2%	92%	69%
NW	84%	74%	67%	42%	16%	5%
CRTO	95%	90%	4%	3%	82%	69%
SDBEBD	93%	81%	51%	27%	78%	70%
TNW	93%	79%	62%	38%	28%	17%

Panel A reports violations (negative slack) for eight covenant types based on *True Slack* and *Estimated Slack*. *Violation %* is the proportion of observations per covenant type for which slack is negative, indicating a violation. *False Positive Error Rate* is the percentage of estimated violations that are not, in fact, true violations. *Overestimation %* is calculated as the difference between *Violation %* based on *Estimated Slack* and *Violation %* based on *True Slack*, scaled by the *Violation %* based on *True Slack*. In the last two columns, we report the number of observations that are misclassified as violations (Type 1 errors) or non-violations (Type II errors) when using *Estimated Slack* as a proxy for *True Slack*. Type I errors are errors in which the researcher identifies a violation using *Estimated Slack* but no violation has actually occurred (based on *True Slack*). Type II errors are errors in which a researcher fails to identify a violation using *Estimated Slack* but a violation has occurred (based on *True Slack*). Panel B reports violations (i.e., negative slack) and *Violation %* using four different computations for slack. *True Slack* is based on true realizations and true thresholds reported by firms. *True Realization & Estimated Threshold* uses true realizations reported by firms and estimated thresholds from Dealscan. *Estimated Realization & True Threshold* uses true thresholds reported by firms and estimated realizations based on Compustat data and standardized covenant definitions from Demerjian and Owens (2016). *Estimated Slack* uses estimated realizations that are based on Compustat data and standardized covenant definitions from Demerjian and Owens (2016) and estimated thresholds that are based on the initial covenant thresholds specified in Dealscan. Panel C shows the percentage of observations where *Difference %* is greater than 10 or 25 percent for total covenant slack, thresholds, and realizations. *Difference %* is the absolute value of the difference between the true and estimated measure scaled by the true measure.

Table 4: Determinants of Measurement Error

	(1)	(2)	(3)	(4)	(5)	(6)
	Large Slack Error			Type I Error		
Log(Assets)	-0.019*		-0.038***	-0.010		-0.034***
	(-1.802)		*(-2.776)*	*(-0.908)*		*(-2.829)*
Leverage	0.020		0.045	0.114		0.128
	(0.302)		*(0.705)*	*(1.327)*		*(1.497)*
Current Ratio	-0.026*		-0.032**	-0.010		-0.016
	(-1.849)		*(-2.197)*	*(-0.809)*		*(-1.320)*
Tangibility	0.085		-0.060	0.031		-0.033
	(0.909)		*(-0.642)*	*(0.385)*		*(-0.419)*
Loss	0.090***		0.069***	0.117***		0.093***
	(5.343)		*(4.322)*	*(5.298)*		*(4.749)*
S&P Rated	-0.023		-0.016	-0.032		-0.004
	(-0.654)		*(-0.464)*	*(-1.070)*		*(-0.116)*
CFO Volatility	-0.002		-0.001	-0.001		-0.001
	(-1.330)		*(-0.859)*	*(-1.630)*		*(-0.762)*
Sales Growth	0.022		0.013	0.038		0.028
	(0.992)		*(0.583)*	*(1.430)*		*(1.077)*
Zscore	-0.019		-0.030**	-0.015		-0.032**
	(-1.206)		*(-2.071)*	*(-0.799)*		*(-2.389)*
Debt-to-EBITDA	0.022***		0.017***	0.050***		0.045***
	(5.557)		*(4.831)*	*(5.146)*		*(4.779)*
Interest Coverage	-0.001		-0.001**	-0.001**		-0.001*
	(-1.625)		*(-2.042)*	*(-2.412)*		*(-1.830)*
N_Covenants		0.091***	0.076***		0.100***	0.100***
		(6.214)	*(4.982)*		*(5.400)*	*(6.416)*
Log (Maturity)		0.027	0.006		0.052	-0.002
		(0.924)	*(0.210)*		*(1.453)*	*(-0.075)*
Log (Loan Amount)		-0.074	-0.229***		-0.072	-0.213***
		(-1.238)	*(-2.839)*		*(-1.232)*	*(-3.470)*
Relationship Lender		-0.028	-0.007		-0.030	-0.036*
		(-1.299)	*(-0.271)*		*(-1.297)*	*(-1.816)*
Observations	7,410	9,010	7,353	7,410	9,010	7,353
Adjusted R^2	0.175	0.168	0.234	0.323	0.242	0.418
Fixed Effects	Industry & Year	Industry, Year & Lender	Industry, Year & Lender	Industry & Year	Industry, Year & Lender	Industry, Year & Lender

This table reports the results of Large Slack and Type I errors on potential borrower- and loan-level determinants. *Large Slack Error* is an indicator variable equal to one if at least one covenant type incurs a *Difference %* greater than 25 percent associated with covenant slack for a given firm-quarter, and zero otherwise. *Type I Error* is an indicator equal to one if, for a given firm-quarter, there is at least one covenant type for which estimated slack is negative while true slack is not, and zero otherwise. All other variables are defined in Appendix A. *,**, and *** indicate significance at the 10%, 5%, and 1% levels, respectively, using a two-tailed *t*-test. *T*-stats are reported below coefficient estimates. Standard errors are clustered at the borrower level.

Table 5: Covenant Violations and Regression Discontinuity Designs

Panel A – Reexamination of Chava and Roberts (2008)

	1	2	3	4	5
EBind	-0.003***				
	(-4.276)				
EBind2		-0.005***			
		(-3.288)			
Bind			-0.007*	-0.002	0.001
			(-1.708)	*(-0.568)*	*(0.229)*
Macro q	0.001***	0.001***	0.001***	0.001***	0.001***
	(14.757)	*(13.385)*	*(7.298)*	*(7.343)*	*(4.960)*
Cash Flow	0.009***	0.010***	0.009*	0.008	0.001
	(5.167)	*(4.644)*	*(1.795)*	*(1.601)*	*(0.157)*
Log (Assets)	-0.005***	-0.007***	-0.006**	-0.006*	-0.006
	(-5.170)	*(-5.989)*	*(-2.042)*	*(-1.892)*	*(-1.600)*
ESlack	0.001***	0.001***			
	(2.834)	*(3.415)*			
TSlack			-0.001	0.007	0.010
			(-0.950)	*(1.644)*	*(1.594)*
Observations	51,167	34,842	6,065	5,928	4,181
Adjusted R^2	0.407	0.417	0.533	0.540	0.562
Sample	Full	Compliance	True Slack	True Slack	True Slack
Bandwidth	None	None	None	1.000	0.500
Fixed Effects	Firm & Year-Quarter	Firm & Year-Quarter	Firm & Year-Quarter	Firm & Year-Quarter	Firm & Year-Quarter

Panel B – Analysis of Investment Around the Covenant Threshold when Including Debt-to-EBITDA Covenants

	ESlack				TSlack			
	Obs	% Obs	Investment %	Inv % Change	Obs	% Obs	Investment %	Inv % Change
Slack Greater than 1 (Bin 1)	3,340	6.5%	6.86%		104	1.7%	4.33%	
Slack of 0.5 to 1 (Bin 2)	11,094	21.7%	6.64%	-0.22%	1,699	28.0%	6.19%	1.86%
Slack of 0.25 to 0.5 (Bin 3)	9,630	18.8%	5.59%	-1.05%	2,385	39.3%	5.17%	-1.03%
Slack of 0 to 0.25 (Bin 4)	9,598	18.8%	5.09%	-0.49%	1,820	30.0%	4.35%	-0.82%
Slack less than 0 to -0.25 (Bin 5)	6,355	12.4%	5.18%	0.09%	35	0.6%	3.94%	-0.40%
Slack of -0.25 to -0.5 (Bin 6)	3,816	7.5%	5.38%	0.19%	12	0.2%	3.23%	-0.71%
Slack of -0.5 to -1 (Bin 7)	3,525	6.9%	4.87%	-0.51%	10	0.2%	3.13%	-0.10%
Slack less than -1 (Bin 8)	3,809	7.4%	4.82%	-0.04%	0	0.0%	0.00%	N/A
Total / Average	**51,167**	**100%**	**5.63%**		**6,065**	**100.0%**	**5.18%**	
Positive Slack	33,662	65.8%	5.92%		6,001	98.9%	5.20%	
Negative Slack	17,505	34.2%	5.08%		64	1.1%	3.65%	
Difference (Positive - Negative)			-0.84%				-1.55%	
True Slack Sample - EBIND False Positive Percentage Rate							**96.3%**	

Panel A presents the results of our reexamination of Chava and Roberts (2008) when including debt-to-EBITDA covenants following Ferreira et al. (2018). The dependent variable is *Investment*, which is the quarterly capital expenditures scaled by beginning-of-period net property, plant, and equipment. *EBind* (*Bind*) is an indicator variable equal to one if Estimated (True) Slack is negative and zero otherwise. If a borrower is subject to more than one covenant type, then we use the minimum slack value among the relevant covenant types. *ESlack* (*TSlack*) is the minimum *Estimated (True) Slack %* value across all covenant types that a borrower is subject to for a given firm-quarter, where *Estimated (True) Slack %* is the difference between the estimated (true) realization and estimated (true) threshold scaled by the estimated (true) threshold. For covenants with a minimum (maximum) threshold, the difference is the realization less the threshold (threshold less the realization). *,**, and *** indicate significance at the 10%, 5%, and 1% levels, respectively, using a two-tailed *t*-test. For Panel A, *t*-statistics are shown below coefficient estimates, and standard errors are clustered at the borrower level. Panel B reports the number of observations and average Investment (i.e., *Investment %*) for four different bins above the covenant threshold (Bins 1–4) and four different bins below (Bins 5–8). The *Inv % Change* column shows the percentage change in investment compared to the bin above. At the end of Panel B, we show the percentage of *EBIND* observations within the True Slack Sample that are not true covenant violations (i.e., Type I errors).

Table 6: Covenant Violations and Lender Forbearance

Panel A – Reexamination of Bird et al. (2022a)

	Enforcement				
	(1)	(2)	(3)	(4)	(5)
Negative Estimated Slack	9.117***	10.993***	7.028***		
	(0.008)	*(0.011)*	*(0.015)*		
Negative Estimated Slack2				94.172***	
				(0.005)	
Negative True Slack					94.370***
					(0.019)
Observations	134,938	83,953	13,022	83,953	13,022
R^2	0.127	0.229	0.367	0.763	0.637
Sample	Full Sample	Compliance	True Slack	Compliance	True Slack
Sample Period	2000-2016	2000-2016	2000-2016	2000-2016	2000-2016
Fixed Effects	Industry x Year-Quarter	Industry x Year-Quarter	Industry x Year-Quarter	Industry x Year-Quarter	Industry x Year-Quarter

This panel reports our reexamination of Bird et al. (2022a). Column (1) reports our reproduction of the regression by Bird et al. (2022a) reported in their Table 2, Column (1), but using our sample period (2000–2016). Column (2) reports our reproduction of the regression in Bird et al. (2022a) when using our Compliance Sample. Column (3) reports our reproduction of the regression in Bird et al. (2022a) when using our True Slack Sample. Column (4) reports the regression of *Enforcement* on *Negative Estimated Slack2* when using our Compliance Sample. Column (5) reports the regression of *Enforcement* on *Negative True Slack* using our True Slack Sample. *Enforcement* is an indicator equal to one if a borrowing firm qualitatively discloses a violation in its SEC filing for a given firm-quarter, and zero otherwise (Nini et al. 2012). *Negative Estimated Slack* is an indicator variable equal to one if estimated slack is negative, and zero otherwise. To calculate *Negative Estimated Slack*, we follow Bird et al. (2022a) and rely on covenant definitions in Demerjian and Owens (2016). *Negative Estimated Slack2* is an indicator variable equal to one if estimated slack is negative and the firm does not affirmatively state that it complies with all covenants, and zero otherwise. *Negative True Slack* is an indicator variable equal to one if true slack is negative. *,**, and *** indicate significance at the 10%, 5%, and 1% levels, respectively, using a two-tailed *t*-test. Following Bird et al. (2022a), we cluster standard errors at the borrower and lender levels and report them below coefficient estimates.

Panel B – Reexamination of Bird et al. (2022b)

	Enforcement				
	(1)	(2)	(3)	(4)	(5)
Negative Estimated Slack * STLender	0.035***	0.053***	0.051*		
	(0.007)	*(0.010)*	*(0.026)*		
Negative Estimated Slack2 * STLender				-0.005	
				(0.005)	
Negative True Slack * STLender					0.031
					(0.120)
Controls	No	No	No	No	No
N	36,112	23,272	3,284	23,272	3,284
Adjusted R^2	0.091	0.183	0.227	0.731	0.464
Sample	Full	Compliance	True Slack	Compliance	True Slack
Quarter Fixed Effect	Yes	Yes	Yes	Yes	Yes
Industry-Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes

This panel reports the results of our reexamination of Bird et al. (2022b). Column (1) reports our reproduction of the result from Bird et al. (2022b) reported in their Table 2, Column (2), but using our Full Sample. Column (2) reports results when using our Compliance Sample. Column (3) reports our reproduction of the result in Bird et al. (2022b) when using our True Slack Sample. Column (4) reports results when replacing *Negative Estimated Slack* with *Negative Estimated Slack2* and using our Compliance Sample. Column (5) reports the results when replacing *Negative Estimated Slack* with *Negative True Slack* when using our True Slack Sample. *Enforcement* is an indicator equal to one if a borrowing firm discloses a violation in its SEC filing for a given firm-quarter, and zero otherwise (Nini et al. 2012). *Negative Estimated Slack* is an indicator variable equal to one if estimated slack is negative, and zero otherwise. For calculating *Negative Estimated Slack*, we follow Bird et al. (2022a) and Bird et al. (2022b) and rely on covenant definitions in Demerjian and Owens (2016). *Negative Estimated Slack2* is an indicator variable equal to one if estimated slack is negative and the firm does not affirmatively state that it complies with all covenants, and zero otherwise. *Negative True Slack* is an indicator variable equal to one if true slack is negative. *STLender* is an indicator that equals one if the *Lender EPS Surprise* equals zero or one cent, and zero otherwise, where *Lender EPS Surprise* is the realized EPS (from I/B/E/S) minus the median analyst EPS forecast (from I/B/E/S). *,**, and *** indicate significance at the 10%, 5%, and 1% levels, respectively, using a two-tailed *t*-test. Following Bird et al. (2022b), we cluster standard errors at the borrower and lender levels and report them below coefficient estimates.

Table 7: Reexamination of the Determinants of Covenant Slack

	1	2	3
	Estimated Tightness		True Tightness
Log (Relation (Duration))	-0.016***	-0.012	-0.024**
	(-3.807)	*(-0.926)*	*(-2.271)*
Log (Loan Amount)	-0.001	-0.011	-0.007
	(-0.190)	*(-1.035)*	*(-0.587)*
Log (Maturity)	-0.001	-0.015	0.019
	(-0.083)	*(-0.851)*	*(0.793)*
Log (Lenders)	-0.001	0.015	-0.005
	(-0.290)	*(1.264)*	*(-0.474)*
Log (Assets)	-0.008*	0.001	0.003
	(-1.790)	*(0.141)*	*(0.345)*
Leverage	0.043***	0.135***	0.098**
	(3.092)	*(2.848)*	*(2.169)*
Tangibility	-0.058***	-0.150***	-0.028
	(-3.487)	*(-4.088)*	*(-0.717)*
Current Ratio	-0.012***	-0.009	-0.010
	(-3.860)	*(-1.004)*	*(-1.007)*
Log (Interest Coverage Ratio)	-0.040***	-0.014	-0.035***
	(-14.046)	*(-1.405)*	*(-3.598)*
Rating	0.014***	0.022***	0.013**
	(8.362)	*(4.167)*	*(2.510)*
Not Rated	0.155***	0.262***	0.159**
	(7.562)	*(4.392)*	*(2.588)*
S&P 500	-0.004	0.023	-0.002
	(-0.371)	*(0.903)*	*(-0.076)*
Observations	4,633	552	552
Adjusted R^2	0.234	0.245	0.263
Sample	Full	True Slack	True Slack
Industry Effects	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes
Loan Purpose Effects	Yes	Yes	Yes
Loan Type Effects	Yes	Yes	Yes

This table reports our reexamination of the result from Prilmeier (2017). In Column (1), we report results of regressing *Estimated Tightness* on Relation Duration and a set of controls when using our Full Sample. Column (2) reports the results of regressing *Estimated Tightness* on Relation Duration and a set of controls when using our True Slack Sample. Column (3) reports the results of regressing *True Tightness* on Relation Duration and a set of controls when using our True Slack Sample. All variables are defined in Appendix A. *,**, and *** indicate significance at the 10%, 5%, and 1% levels, respectively, using a two-tailed *t*-test. *T*-stats are reported below coefficient estimates. Standard errors are clustered at the borrower level.

Table 8: Responses to Covenant Violations

	Return Window = **(-1, +1)**		**DV** = Renegotiation	
	1	2	3	4
Estimated Violation	0.002		-0.005	
	(1.043)		*(-1.569)*	
True Violation		**-0.039*****		**0.020****
		(-5.032)		***(2.048)***
Leverage	-0.004	-0.003	0.004	0.000
	(-0.866)	*(-0.648)*	*(0.556)*	*(0.069)*
MB	-0.000	-0.000	0.000	0.000
	(-0.510)	*(-0.500)*	*(0.033)*	*(0.134)*
ROA	0.101***	0.090***	-0.030	-0.020
	(4.721)	*(4.215)*	*(-1.176)*	*(-0.789)*
WhitedWu	0.011	0.018	-0.023	-0.030**
	(1.017)	*(1.639)*	*(-1.596)*	*(-2.075)*
N	7,454	7,454	5,951	5,951
Adjusted R^2	0.008	0.011	0.022	0.022
Fixed Effects	Industry & Year	Industry & Year	Industry & Year	Industry & Year

This table compares responses to *Estimated Violations* against responses to *True Violations*. For this analysis, *True Violation* is an indicator variable equal to one if true slack is negative for at least one covenant type for a given firm-quarter, and zero otherwise. *Estimated Violation* is an indicator variable equal to one if estimated slack is negative for at least one covenant type for a given firm-quarter, and zero otherwise. Columns (1) and (2) report results when examining the stock market response for a three-day estimation period around a firm's fiscal quarter reporting date. Columns (3) and (4) report results when examining the occurrence of future renegotiations associated with a covenant violation. *Renegotiation* is an indicator equal to one if a renegotiation occurs in quarter t+1, and zero otherwise. All other variables are defined in Appendix A. *,**, and *** indicate significance at the 10%, 5%, and 1% levels, respectively, using a two-tailed *t*-test. *T*-stats are reported below coefficient estimates.

Table 9: Proposed Adjustments to Reduce Measurement Error

Panel A – Violations using Alternative Measures of EBITDA

EBITDA defined as OIBDPQ

Covenant	N	True Violation %	Estimated Violation %	Difference	% Improvement over Baseline
DBEBD	6,011	0.7%	22.6%	21.9%	-
ICVR	3,589	0.8%	10.3%	9.5%	-
FCVR	1,708	2.6%	34.7%	32.1%	-
SDBEBD	335	0.9%	57.3%	56.4%	-

Adjusted EBITDA 1: OIBDP + Stock Comp

Covenant	N	True Violation %	Estimated Violation %	Difference	% Improvement over Baseline
DBEBD	6,011	0.7%	20.4%	19.7%	10.0%
ICVR	3,589	0.8%	9.3%	8.5%	10.0%
FCVR	1,708	2.6%	33.3%	30.7%	4.4%
SDBEBD	335	0.9%	54.9%	54.0%	4.2%

Adjusted EBITDA 2: OIBDP +Stock Comp + Pension Expense

Covenant	N	True Violation %	Estimated Violation %	Difference	% Improvement over Baseline
DBEBD	6,011	0.7%	18.3%	17.6%	19.4%
ICVR	3,589	0.8%	8.7%	7.9%	17.1%
FCVR	1,708	2.6%	31.0%	28.3%	11.7%
SDBEBD	335	0.9%	53.4%	52.5%	6.9%

Panel B – Type I and Type II Errors using Alternative Measures of EBITDA

TYPE I AND TYPE II ERRORS

					Adjusted EBITDA 1: OIBDP + Stock Comp					
		Baseline			Type I Error Analysis			Type II Error Analysis		
Covenant	**N**	**Total Errors**	**Type I Error %**	**Type II Error %**	**Type I Error %**	**Type I Change**	**% Improve**	**Type II Error %**	**Type II Change**	**% Improve**
DBEBD	6011	22.18%	22.03%	0.15%	19.83%	-2.20%	9.90%	0.13%	-0.02%	0.1%
ICVR	3589	9.97%	9.72%	0.25%	8.78%	-0.95%	9.50%	0.25%	0.00%	0.0%
FCVR	1708	32.90%	32.49%	0.41%	31.09%	-1.41%	4.27%	0.41%	0.00%	0.0%
SDBEBD	335	56.42%	56.42%	0.00%	54.03%	-2.39%	4.23%	0.00%	0.00%	0.0%
					Adjusted EBITDA 2: OIBDP + Stock Comp + Pension Expense					
		Baseline			Type I Error Analysis			Type II Error Analysis		
Covenant	**N**	**Total Errors**	**Type I Error %**	**Type II Error %**	**Type I Error %**	**Type I Change**	**% Improve**	**Type II Error %**	**Type II Change**	**% Improve**
DBEBD	6011	22.18%	22.03%	0.15%	17.77%	-4.26%	19.20%	0.13%	-0.02%	0.1%
ICVR	3589	9.97%	9.72%	0.25%	8.11%	-1.62%	16.20%	0.25%	0.00%	0.0%
FCVR	1708	32.90%	32.49%	0.41%	28.75%	-3.75%	11.39%	0.41%	0.00%	0.0%
SDBEBD	335	56.42%	56.42%	0.00%	52.54%	-3.88%	6.88%	0.00%	0.00%	0.0%

Panel A reports the frequency of true and estimated violations for four EBITDA-based covenant types and investigates how violation frequencies change when modifying the proxy for contractual EBITDA. We restrict the analysis to a constant sample. *True Violation %* is proportion of observations for which *True Slack* is negative. *Estimated Violation %* is the proportion of observations for which estimated slack is negative, where the underly calculations for estimated realizations are modified for the proxy for contractual EBITDA. The top panel uses OIBDP based on Demerjian and Owens (2016) as the proxy for contractual EBITDA. The middle (lower) panel use OIBDP plus stock compensation (stock compensation and pension expense) as the proxy for contractual EBITDA. *% Improvement over Baseline* shows the percentage improvement in violations over the baseline proxy for contractual EBITDA - OIBDP. Panel B reports the percentage of observations that are misclassified as violations (Type 1 errors) or non-violations (Type II errors) for each covenant type and how these frequencies change using two adjusted proxies for contractual EBITDA. We restrict the analysis to a constant sample. The Baseline measures contractual EBITDA using OIBDP following Demerjian and Owens (2016). The adjusted proxies for contractual EBITDA use panel use OIBDP plus stock compensation (stock compensation and pension expense) % Improve shows the percentage improvement in Type I or Type II errors over baseline proxy of OIBDP.

Internet Appendix

This Internet Appendix provides supplemental materials for the manuscript "Measurement Error When Estimating Covenant Violations"

Summary of Supplemental Information and Examples

Summary of Additional Analysis

Appendix 1 – Additional Information about Sample Selection

1. Compiling initial firm-quarter sample

Our firm-quarter sample covers fiscal quarters ending from January 1, 2000, through December 31, 2016. Because we are interested in assessing measurement and compliance associated with financial covenants, we begin by extracting a set of loan packages (or deals) from Dealscan will deal dates outstanding between January 1, 2000, and December 31, 2016.[33] We then remove deals without covenant information reported in Dealscan and deals associated with borrowers that do not have a "GVKEY" match using the Robert's linking table available on WRDS.

Next, we match this loan sample to quarterly accounting information from Compustat. To do this, we match loans to fiscal quarters ending between January 1, 2000, and December 31, 2016, only keeping fiscal quarters for which we have loans that are reported as outstanding based on the loan's beginning and end dates that are reported in Dealscan. This yields a sample of 128,722 unique firm-quarters, covering 6,244 unique firms. We then remove firm-quarters without disclosed violation data, where information on disclosed violations is compiled following Nini et al. (2012).[34] Requiring violation data subjects the sample to the same sample restrictions imposed by Nini et al. (2012), such as removing borrowers that are not domiciled in the U.S. (Compustat FIC = "USA"), removing borrowers within the financial industry (SIC = 6000-6999) and removing firm-quarter observations with missing information about total assets (Compustat = ATQ), total sales (Compustat = SALEQ), common shares outstanding (Compustat = CSHOQ), closing share price (Compustat = PRCCQ) or the exact calendar quarter (Compustat = DATACQTR). Additionally, we implicitly require that each fiscal quarter has an available Central Index Key (CIK) that we use to match accounting information from Compustat with a corresponding quarterly filing on the SEC's EDGAR website. After imposing these restrictions, we are left with a sample of 93,092 unique firm-quarters, covering 4,493 unique firms.

2. Identifying firm-quarters that disclose true covenant thresholds and realizations

After constructing our sample of firm-quarters with an outstanding loan and covenant information from Dealscan, information about whether the firm disclosed that it violated a covenant during the quarter, and a match to the firm's quarterly filing on EDGAR, we then want to identify whether these firm-quarters disclose true covenant thresholds and realizations. This requires us to search the linked SEC filings found on the EDGAR website. To ease the burden of manually reviewing over 90,000 periodic filings, we created a text-searching algorithm to identify periodic filings that disclose the relevant information. In creating this text-searching algorithm, we sought to be conservative, meaning that our process generates a lot of false positives (relative to false negatives), which then require manual review.

To create a text-searching algorithm for identifying disclosure of true covenant thresholds and realizations, we follow Nini et al. (2012) and first select a random sample of 1,000 10-K filings

[33] We consider a loan or deal to be outstanding on a given date if the date meets the following two criteria: 1) the date is equal to or after the facility start date reported in Dealscan and 2) the date is equal to or before the facility start date reported in Dealscan.

[34] We thank Greg Nini for sharing covenant violation data with us that covers fiscal quarters ending through 2016.

(within the 93,092 filings) for manual review.[35] We manually review these 1,000 filings and identify: 1) whether the filing reports the covenant thresholds that the firm is subject to as of the end of the fiscal quarter (i.e., true covenant thresholds), and 2) whether the filing reports the covenant realizations for such covenants as of the end of the fiscal quarter (i.e., true covenant realizations). Of the 1,000 10-K filings we reviewed, 139 report information about both true covenant thresholds and true covenant realizations.

While manually reviewing the 1,000 we compiled a list of relevant terms that would be useful for identifying filings that report both true covenant thresholds and realizations. After working through several iterations, we determined the best text-search algorithm for identifying filings that report both true covenant thresholds and realizations is the following. First, if the filing contains any of the following words or phrases: "covenant", "leverage ratio", "interest coverage", or "net worth", then our algorithm extracts the sentence containing the aforementioned word or phrase the sentence immediately before and after the sentence containing the flagged word or phrase. The algorithm then searches within the three-sentence grouping for any of the following words or phrases: "ratio equal", "ratio at", "ratio of", "ratio for", "ratio was", "ratios were", "actual ratio", "worth was", "EBITDA was", "capitalization was", "summary", "covenant level", and "comparison". If one of the aforementioned words or phrases is identified in the three-sentence grouping, then the search algorithm flags the filing and extracts the three-sentence grouping.

This text-search algorithm finds approximately 91% (127 out of 139) of the occurrences within our sample of 1,000 10-Ks in which the filing reports both true covenant thresholds and realizations. However, the algorithm also generates a large number of false positives. For example, within our 1,000 10-Ks the algorithm generates 245 false positives. Due to the large number of false positives, when we apply our text-search algorithm to our sample of 93,092 filings, we must manually review the three-sentence groupings associated with each "hit" and at times also manually review the underlying filings associated with the "hit" to determine whether the filing actually reports information about covenant thresholds and realizations.

3. Collecting information about true covenant thresholds and realizations from SEC Filings

Once we confirm that a firm-quarter reports both true covenant thresholds and realizations (either from our manual review of 1,000 10-Ks or from our text-search algorithm), we manually review all periodic filings within our sample for that firm.[36] We then collect the following information from firm-quarters that disclose the relevant covenant information: 1) covenant type or types, 2) required covenant thresholds[37], 3) actual covenant realizations, and 4) the scale associated with the covenant threshold and realization.[38]

[35] 10-K filings are annual reports and are filed in connection with a firm's fiscal year end (fourth fiscal quarter for a given year). 10-K filings, on average, contain more information than 10-Q (quarterly) filings. Consistent with this, Nini et al. (2012) find that 10-K filings have a higher incidence of reported covenant violations relative to 10-Q filings.
[36] The requirement by itself led us to manually review over 25,000 periodic filings (10-Qs or 10-Ks).
[37] In a few rare cases, we observe that a firm is subject to more than one of the same covenant type at the same time (e.g., interest coverage ratio). Thus, the firm may be subject to two different thresholds for the same covenant type, yet the realizations for both covenants are the same. In these cases, we collect information for the threshold that is more likely to be violated. For covenant types with a minimum (maximum) threshold, this means we keep the higher (lower) required threshold.
[38] The scale generally is relevant only for non-ratio covenants such as net worth covenants that could be reported in thousands, millions, etc.

Appendix 2 – Compliance Sample

1. Overview of Covenant Compliance Disclosure

To qualify and be included in our True Slack Sample a periodic filing must disclose both true covenant thresholds and realizations. These requirements reduce the size of our True Slack Sample to approximately 10% of the 90,000 periodic filings for 2000 through 2016 that constitute the Broad. However, because a periodic filing fails to report true covenant thresholds and realizations, this does not mean that filing fails to report or disclose the firm's covenant compliance status. Many periodic filings may report only true covenant thresholds or realizations or neither, but also explicitly report their compliance status with covenants in a qualitative format. For example, in its annual filing for the fiscal period ending on December 31, 2007, Lithia Motors does not report information about its required covenant thresholds or covenant realizations, but does state "*At December 31, 2007, we were in compliance with all of the financial and restrictive covenants*."[39] Thus even without reporting information about true covenant thresholds and realizations, Lithia Motors is disclosing its compliance (as of the end of the fiscal period) with all applicable financial covenants. Thus, for many firm-quarters compliance status is known and reported, even if the firm does not report its true covenant thresholds and realizations. This disclosure compliance status may be useful in settings where a researcher is interested in the covenant compliance status of a firm, but true thresholds and realizations are not available.

2. Identifying Covenant Compliance Disclosure

To identify firm-quarters (or filings) that disclose covenant compliance status requires a review of a firm's periodic filing. Because manually reviewing over 90,000 periodic filings for disclosures regarding covenant compliance is a nontrivial task, we develop a text-based methodology for classifying whether a given fiscal quarter qualifies as a "Compliance Discloser". To do this we manually reviewed over 3,000 periodic filings from our broad sample of over 90,000 periodic filings. During our manual review, we determined whether the filing disclosed its covenant compliance status and identified keywords and phrases that were commonly used to report covenant compliance status. In creating the text-search algorithm to identify whether a filing disclosed its covenant compliance status, we sought precision, by minimizing both false positives and false negatives.[40] After several iterations, we determined that the best text-search algorithm identifies sentences that contain one or more of the following words/phrases from both sets of wordlists:

- **Wordlist 1**: "covenant*", "credit agreement", "ratio*", "facility"
- **Wordlist 2**: "in compliance", "compliant", "met", "no event[s] of default"

This text-search algorithm correctly classified approximately 91% of the more than 3,000 periodic filings from our manually reviewed sample as disclosing (or not disclosing) covenant compliance status. We then applied this text-search algorithm to our "broad sample" of over 93,000 firm-

[39] See Lithia Motors 10-K Filing for Fiscal 2007.
[40] This process differs from our text-based approach to identifying periodic filings that disclose "true" covenant information as in our search for "true" covenant information we were less concerned about false positives.

quarters and classified these firm-quarters as "Compliance Disclosers" or "Non-Disclosers". The results indicate that 61,303 firm-quarters (approximately 66% of the Broad Sample) qualify as "Compliance Disclosers". Within these 61,303 firm-quarters, 55,801 explicitly report compliance with financial covenants and 5,502 firm-quarters qualitatively disclose lack of compliance (e.g., Nini et al. 2012).[41]

[41] By construction, our True Slack Sample is strict subset of the Compliance Sample.

Appendix 3 – Measurement Error Example

To provide clarity on the measurement issues that arise when using GAAP numbers from Compustat to determine covenant realizations and data from Dealscan to determine covenant thresholds, we provide a specific example from Ruby Tuesday, Inc. In its 10-Q filing for the quarter ending December 2, 2008, Ruby Tuesday, Inc. reports that its required debt-to-EBITDA ratio (called a Maximum funded debt ratio) for the period is 4.5, while its true ratio at the end of the quarter is 4.22 suggesting Ruby Tuesday's is compliant with its covenant. However, using Compustat data and standardized covenant definitions from Demerjian and Owens (2016) to estimate Ruby Tuesday's debt-to-EBITDA ratio for the quarter ending December 2, 2008, returns a ratio of 5.96. The difference between the true debt-to-EBITDA realization (4.22) and the estimated realization (5.96) arises because the contractual definitions of debt and EBITDA, the two components of the debt-to-EBITDA ratio, include numerous non-GAAP numbers.[42] For example, the EBITDA calculation used in Ruby Tuesday's debt-to-EBITDA covenant definition (called EBITDAR) includes many items not incorporated in Compustat's OIBDP measure such as goodwill impairments, equity in losses of subsidiaries and dead site write-offs (see excerpt from Ruby Tuesday's 10-Q filing below). Often a researcher is unable to calculate true covenant realizations, such as Ruby Tuesday's debt-to-EBITDA ratio of 4.22, using data available on Compustat because certain components of covenant definitions are not collected by data providers, and may not even be reported as a separate line item in a firm's periodic filings.

An additional measurement issue researchers face when estimating covenant slack and violations using traditional methods is the use of covenant thresholds from Dealscan, which may differ from the true covenant threshold. A primary reason for the difference between the threshold reported in Dealscan and the true threshold is that Dealscan often fails to detect adjustments to covenant thresholds after contract origination that occur due to contractual amendments and pre-planned adjustments (e.g., Roberts (2015); Li et al. (2016)). For example, the original credit agreement between Ruby Tuesday and its lenders, dated February 28, 2007, reports that the required maximum debt-to-EBITDA ratio (Adjusted Total Debt to EBITDAR) for all future fiscal periods is 3.25.[43] By relying on the initial required threshold of 3.25, the researcher would conclude that Ruby Tuesday violated its covenant even if able to correctly calculate the true covenant realization of 4.22 for the quarter ending December 2, 2008. To correctly identify the true threshold of 4.5, a researcher would have to either: 1) collect data on covenant realizations and thresholds from periodic filings (which is what we do) or 2) review Ruby Tuesday's material contracts and find its amended credit agreement that reports changes to its required covenant thresholds.[44] Thus, we avoid both sources of measurement error (realization measurement error and threshold measurement error) by hand-collecting information about covenant realizations and required thresholds from a borrower's periodic filings which reflect adjustments from contractual amendments.[45]

[42] See Ruby Tuesday's 10-Q for the period ending December 2, 2008.
[43] See the original credit agreement.
[44] Ruby Tuesdays Inc. reports an amendment to their credit agreement on May 21, 2008 that increased the required maximum threshold for its debt-to-EBITDA covenant from 3.25 to 4.5 for the quarter ending on December 2, 2008 (See the amendment).
[45] Dealscan updated its structure and content in August 2021. One of the updates allows researchers to more easily connect contractual amendments with the original debt contract. Unfortunately, Dealscan rarely contains updated covenant information for these contractual amendments. For example, Dealscan reports that Ruby Tuesday's contract

Excerpt from Ruby Tuesday 10-Q Filing for the period ending December 2, 2008:

Our maximum funded debt covenant is an Adjusted Total Debt to Consolidated EBITDAR ratio. Adjusted Total Debt, as defined in our covenants, includes items both on-balance sheet (debt and capital lease obligations) and off-balance sheet (such as the present value of leases, letters of credit and guarantees). Consolidated EBITDAR is consolidated net income (for the Company and its majority-owned subsidiaries) plus interest charges, income tax, depreciation, amortization, rent and other non-cash charges. Among other charges, we have reflected share-based compensation, asset impairment and bad debt expense, as non-cash. Until the end of the quarter ending March 3, 2009, we can add back the costs (up to $10.0 million) incurred in connection with the closing of restaurants recorded in accordance with GAAP.

Consolidated EBITDAR and Adjusted Total Debt are not presentations made in accordance with GAAP, and, as such, should not be considered a measure of financial performance or condition, liquidity or profitability. They also should not be considered alternatives to GAAP-based net income or balance sheet amounts or operating cash flows or indicators of the amount of free cash flow available for discretionary use by management, as Consolidated EBITDAR does not consider certain cash requirements such as interest payments, tax payments or debt service requirements and Adjusted Total Debt includes certain off-balance sheet items. Further, because not all companies use identical calculations, amounts reflected by RTI as Consolidated EBITDAR or Adjusted Total Debt may not be comparable to similarly titled measures of other companies. We believe that the information shown below is relevant as it presents the amounts used to calculate covenants which are provided to our lenders. Non-compliance with our debt covenants could result in the requirement to immediately repay all amounts outstanding under such agreements.

The following is a reconciliation of net income, which is a GAAP measure of our operating results, to Consolidated EBITDAR as defined in our bank covenants (in thousands):

	Twelve Months Ended December 2, 2008
Net (loss)	$ (11,496)
Interest expense	36,707
Benefit for income taxes	(34,989)
Depreciation	84,567
Amortization of intangibles	788
Rent expense	46,966
Share-based compensation expense	11,943
Goodwill impairment	18,957
Asset impairments	38,851
Equity in losses of subsidiaries	1,155
Bad debt expense	2,602
Dead site write-offs	2,773
Amortization of debt issuance costs	1,342
Non-cash accruals	955
Other	652
Consolidated EBITDAR	$ 201,773

Our covenant requirements and actual ratios for the fiscal quarter ended December 2, 2008 are as follows:

	Covenant Requirements	Actual Ratios
Maximum funded debt ratio (1)	4.50x	4.22x

was amended on May 21, 2008; however, Dealscan does not report any changes to covenant types or thresholds in connection with this contractual amendment.

Appendix 4 – Compustat Structure and Common EBITDA Adjustment

This appendix provides an example of a standard Compustat Income Statement (left panel) and common EBITDA adjustments from debt contracts. The figure below maps each adjustment to a Compustat line item and states whether that line item is included (above) or excluded (below) Compustat item *OIBDP*, which is the commonly used proxy for contractual EBITDA (e.g., Demerjian and Owens 2016).

Standard Annual Compustat Income Statemenet

Sales	SALE		
Operating Expenses	XOPR		
Cost of Goods Sold		COGS	
Selling, General and Administrative Expenses		XSGA	
Research and Development Expense			XRD
Staff Expense			XLR
Pension Expense**			XPR
Rental Expense			XRENT
Advertising Expense			XAD
Operating Income Before Depreciation	**OIBDP**		
Depreciation and Amortization - Total		DP	
Operating Income After Depreciation	OIADP		
Interest and Related Expense		XINT	
Nonoperating Income (Expense) - Total		NOPI	
Special Items		SPI	
Pretax Income	PI		
Income Taxes - Total		TXT	
Minority Interest - Income Account		MII	
Income Before Extraordinary Items	IB		
Components of Special Items			
Acquisition/Merger Pretax		AQP	
Gain/Loss on Sale of Assets Pretax		GLP	
Impairment of Goodwill Pretax		GDWLIP	
Settlement (Litigation/Insurance) Pretax		SETP	
Restructuring Costs Pretax		RCP	
Write-downs Pretax		WDP	
Extinguishment of Debt Pretax		DTEP	
In-Process Research & Development Pretax		RDIP	
Supplemental Items			
Stock Compensation Expense**		STKCO	
Equity in Earnings - Unconsolidated Subsidiaries		ESUB	

Standard EBITDA Adjustments from Debt Contracts

Item	Compustat Item	Below or above OIBDP
Extraordinary, unusual, or nonrecurring items	Special Items	Below
Asset sales or dispositions	Special Items	Below
Asset write-downs	Special Items	Below
Restructuring charges	Special Items	Below
Non-operating income	NOPI	Below
Equity method earnings	ESUB	Below
Adjustments related to insurance	Special Items	Below
Non-cash compensation**	**STKCO / XPR**	**Above**

Figure IF1

Reported Violations and True Covenant Information Disclosure

This figure examines the timing of true covenant information disclosures in relation to reported violations. Specifically, the figure reports the proportion of observations in the Full Sample (See Table 1 in the manuscript) that disclose true covenant information in the fiscal quarters surrounding a reported covenant violation (e.g., Nini et al. 2012).

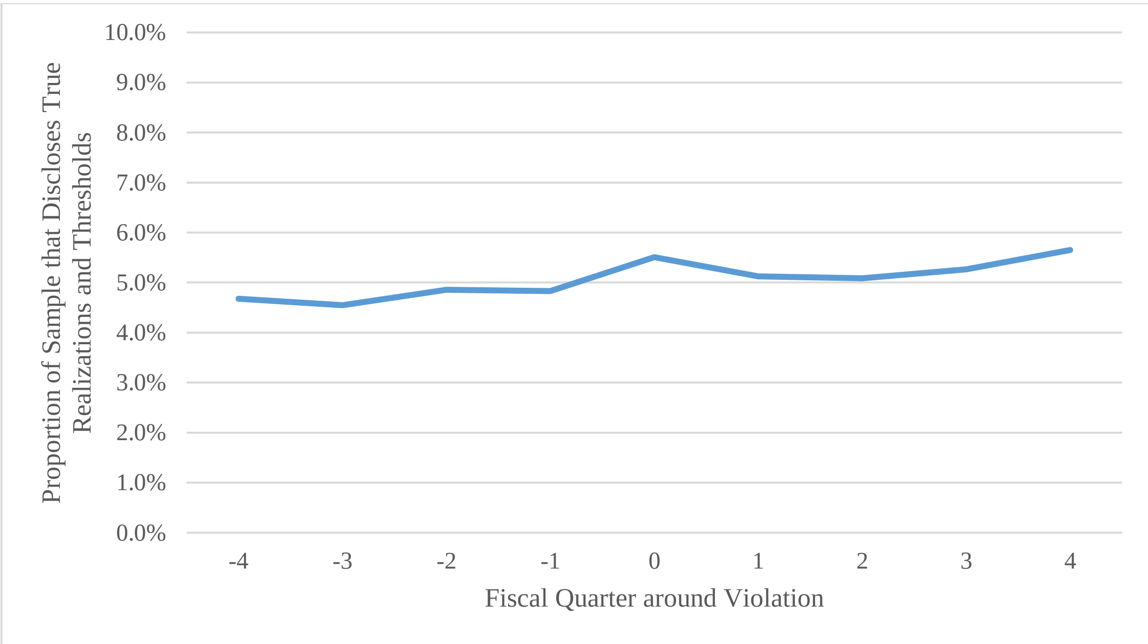


Figure IF2

Reported Violations and Disclosure of True Covenant Information by Firm

This figure reports the percent of unique firms in the True Slack Sample that 1) never report a violation in our sample period (2000-2016); 2) report at least one violation and begin disclosing true covenant information at least two quarters before the reported violation; 3) report at least one violation and begin disclosing true covenant information within one quarter of the violation.

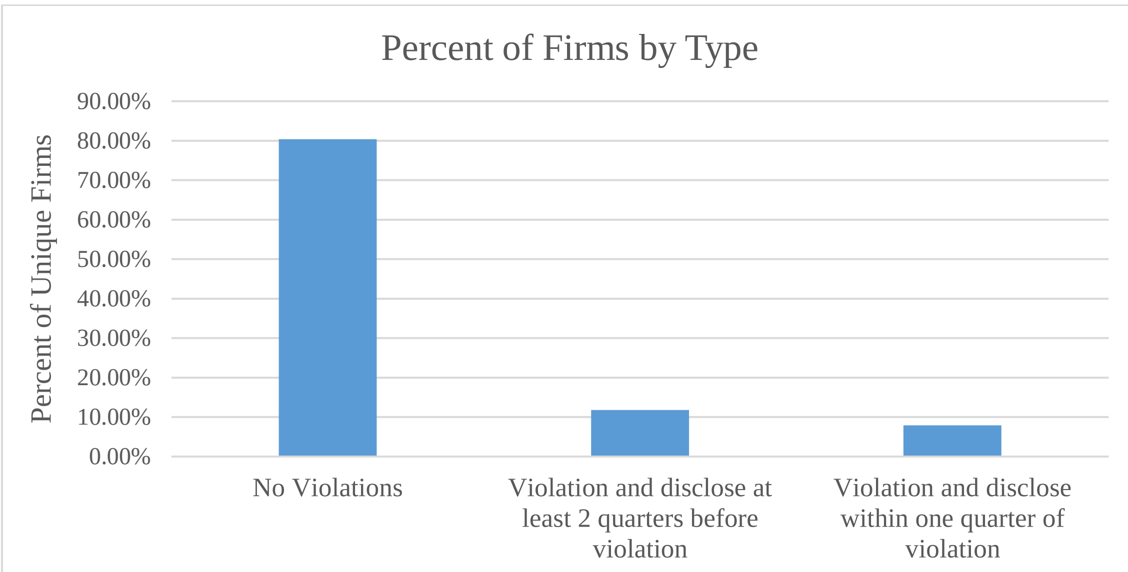
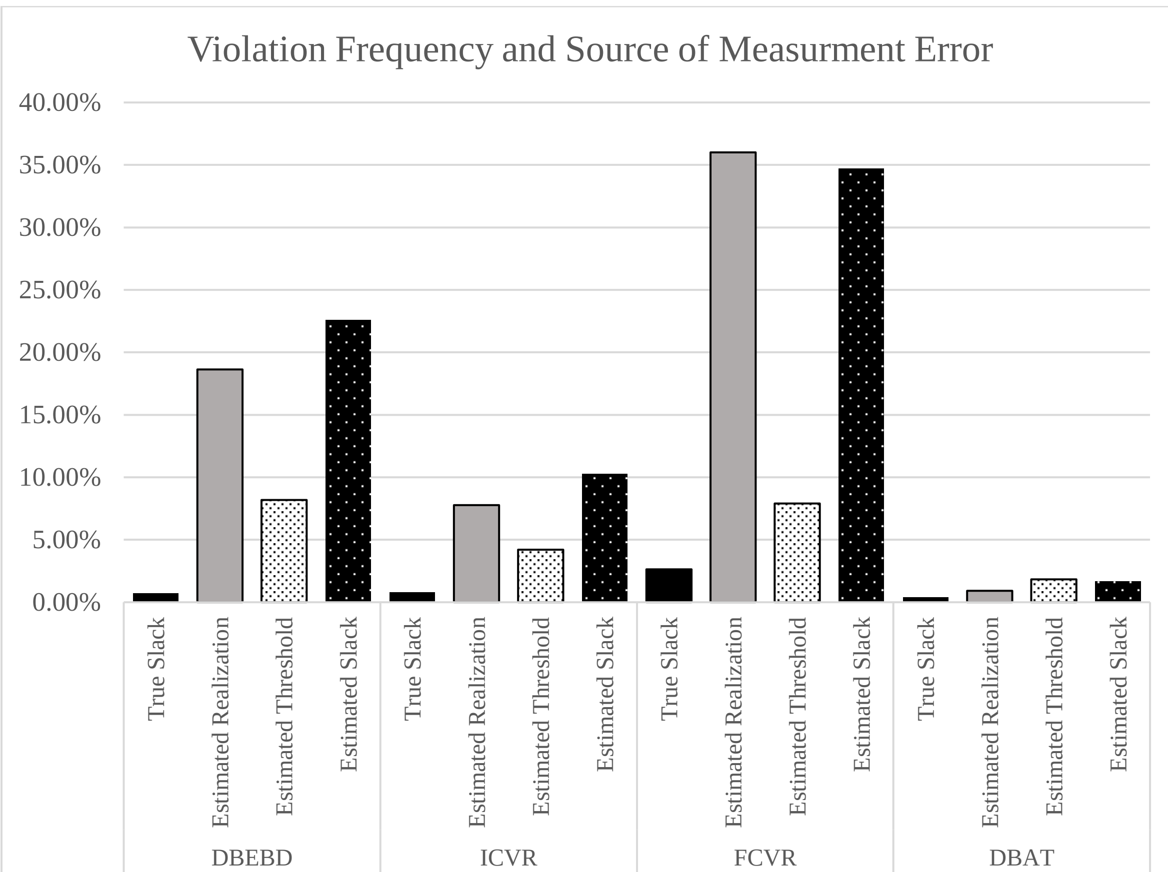


Figure IF3

Source of Measurement Error in Estimated Slack

This figure compares the frequency of violations, by covenant type, across four unique slack measures: 1) True Slack, 2) a slack measure that incorporates estimated realizations and true thresholds (Estimated Realization), 3) a slack measure that incorporates true realization and estimated thresholds (Estimated Threshold), and 4) Estimated Slack. Panel A reports results for debt-to-EBITDA ratios (DBEBD), interest coverage ratios (ICVR), fixed charge coverage ratios (FCVR) and leverage ratios (DBAT). Panel B reports results for senior debt-to-EBITDA ratios (SDBEBD), tangible net worth covenants (TNW), current ratios (CRTO) and net worth covenants (NW). The sample used in this analysis is the True Slack Sample from Table 1 in the manuscript. All variables are defined in Appendix A of the manuscript.

Panel A – Four Most Common Covenant Types



Panel B – Other Four Covenant Types

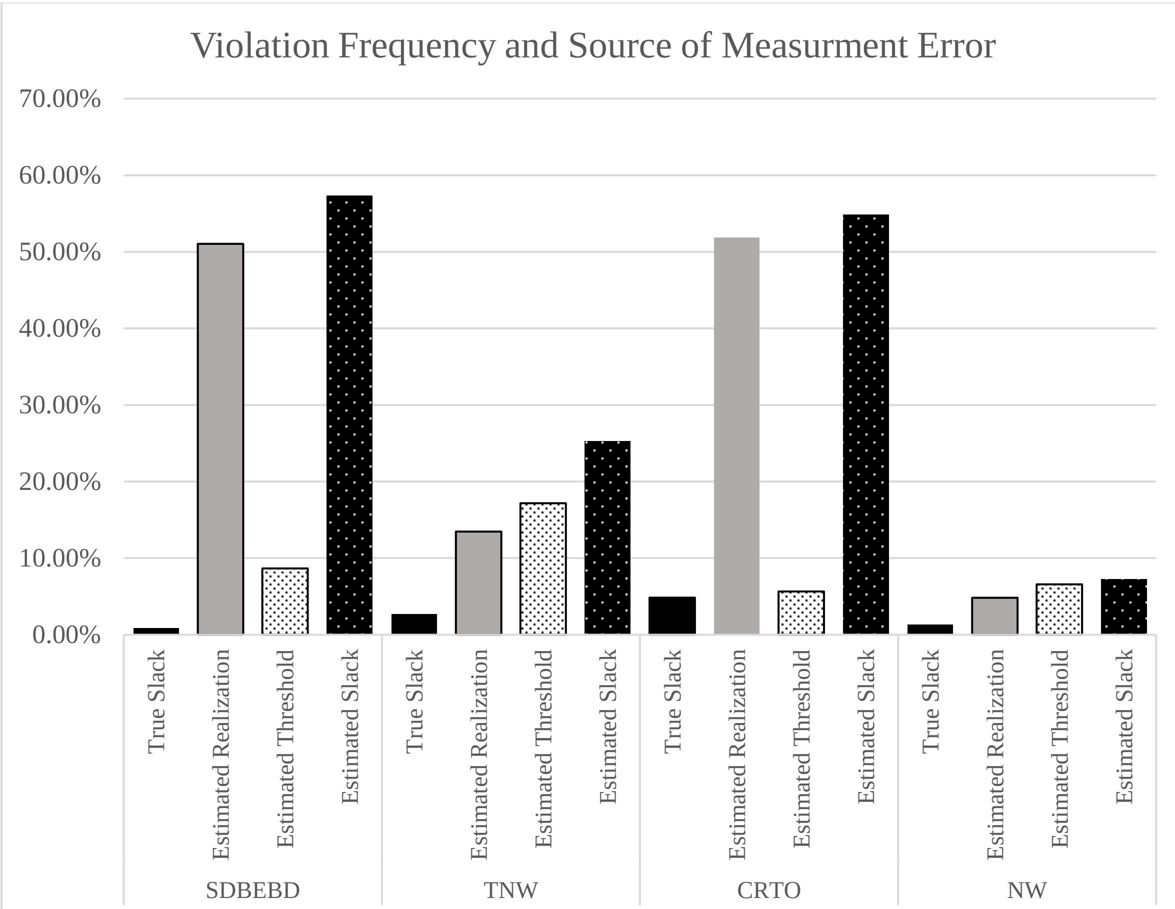
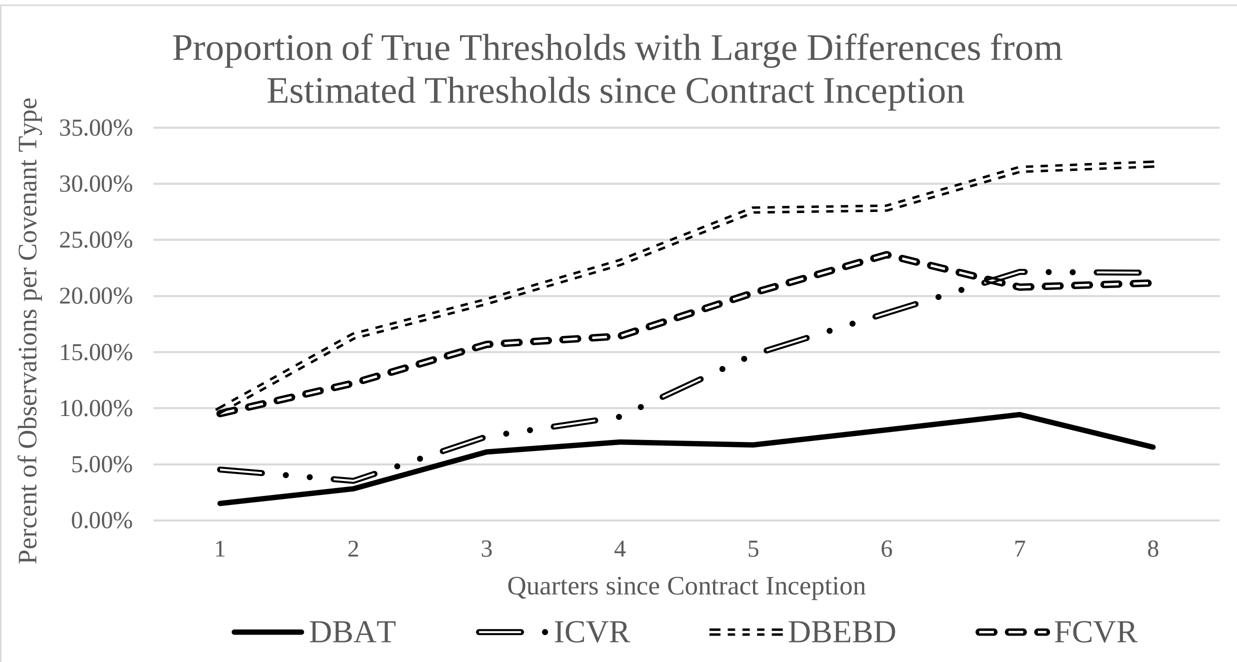


Figure IF4

Threshold Differences since Contract Inception

This figure reports the proportion of observations per covenant type by quarter since contract inception for which there is a large difference between the true threshold and estimated threshold. We identify a threshold difference as being large if the absolute value of the difference between the true threshold and the estimated threshold, scaled by the true threshold is equal to or greater than 0.25. Panel A reports results for debt-to-EBITDA ratios (DBEBD), interest coverage ratios (ICVR), fixed charge coverage ratios (FCVR) and leverage ratios (DBAT). Panel B reports results for senior debt-to-EBITDA ratios (SDBEBD), tangible net worth covenants (TNW), current ratios (CRTO) and net worth covenants (NW).

Panel A – Four Most Common Covenant Types



Panel B – Other Four Covenant Types

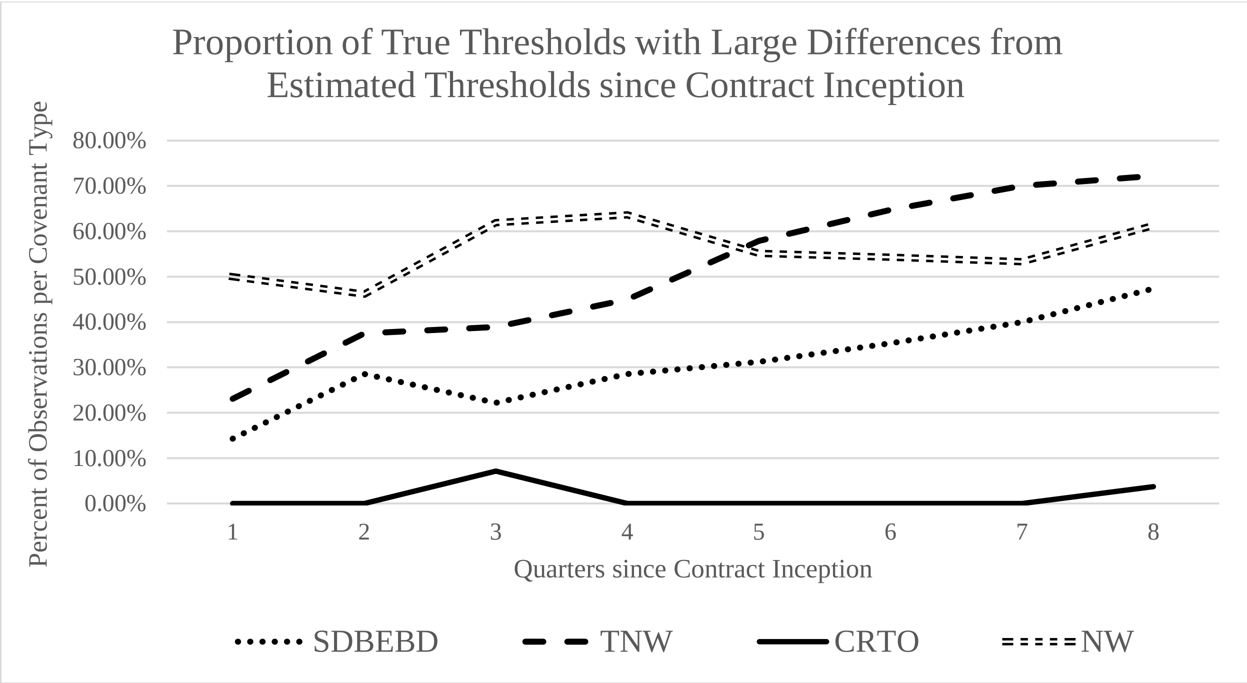
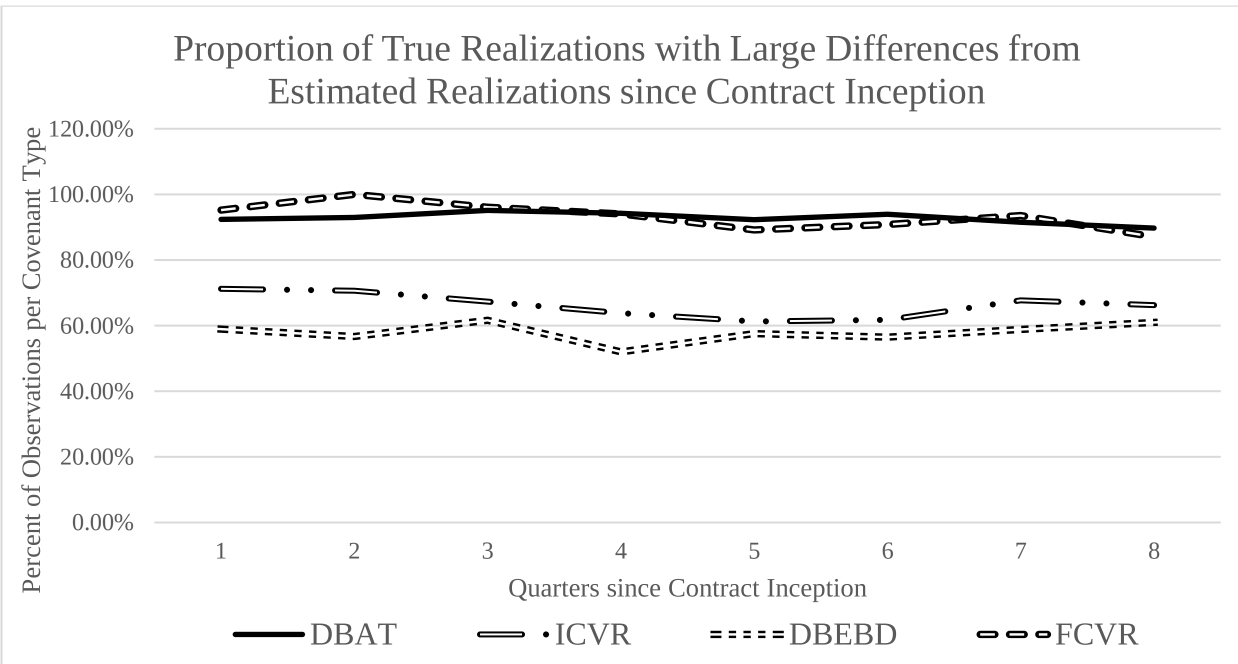


Figure IF5

Realization Differences since Contract Inception

This figure reports the proportion of observations per covenant type by quarter since contract inception for which there is a large difference between the true realization and estimated realization. We identify a realization difference as being large if the absolute value of the difference between the true realization and the estimated realization, scaled by the true realization is equal to or greater than 0.25. Panel A reports results for debt-to-EBITDA ratios (DBEBD), interest coverage ratios (ICVR), fixed charge coverage ratios (FCVR) and leverage ratios (DBAT). Panel B reports results for senior debt-to-EBITDA ratios (SDBEBD), tangible net worth covenants (TNW), current ratios (CRTO) and net worth covenants (NW).

Panel A – Four Most Common Covenant Types



Panel B – Other Four Covenant Types

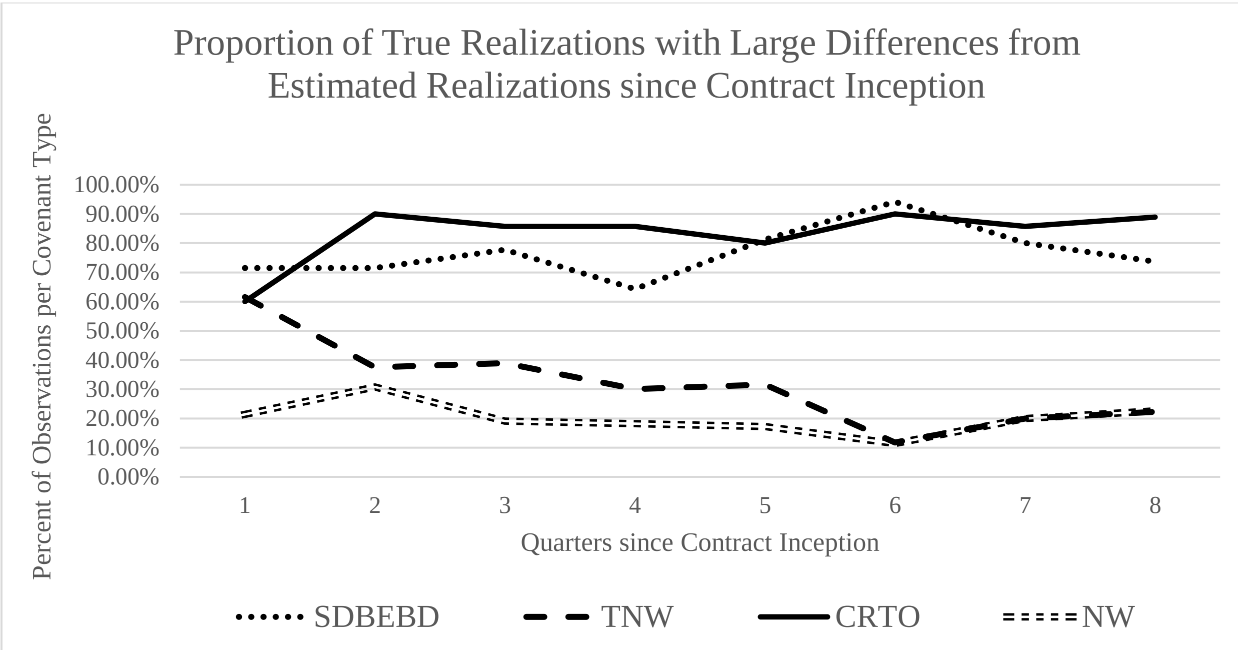


Figure IF6

Percent of Large Realization Error Observations Where True Realization Exceeds Estimated Realization

This figure reports the percent of Large Realization Error observations by covenant types, where True Realization is greater than Estimated Realization. Large Realization Error is an indicator equal to one for a given firm-quarter-covenant if the absolute value of the difference between the true and estimated realization scaled by the true realization exceeds 25%.

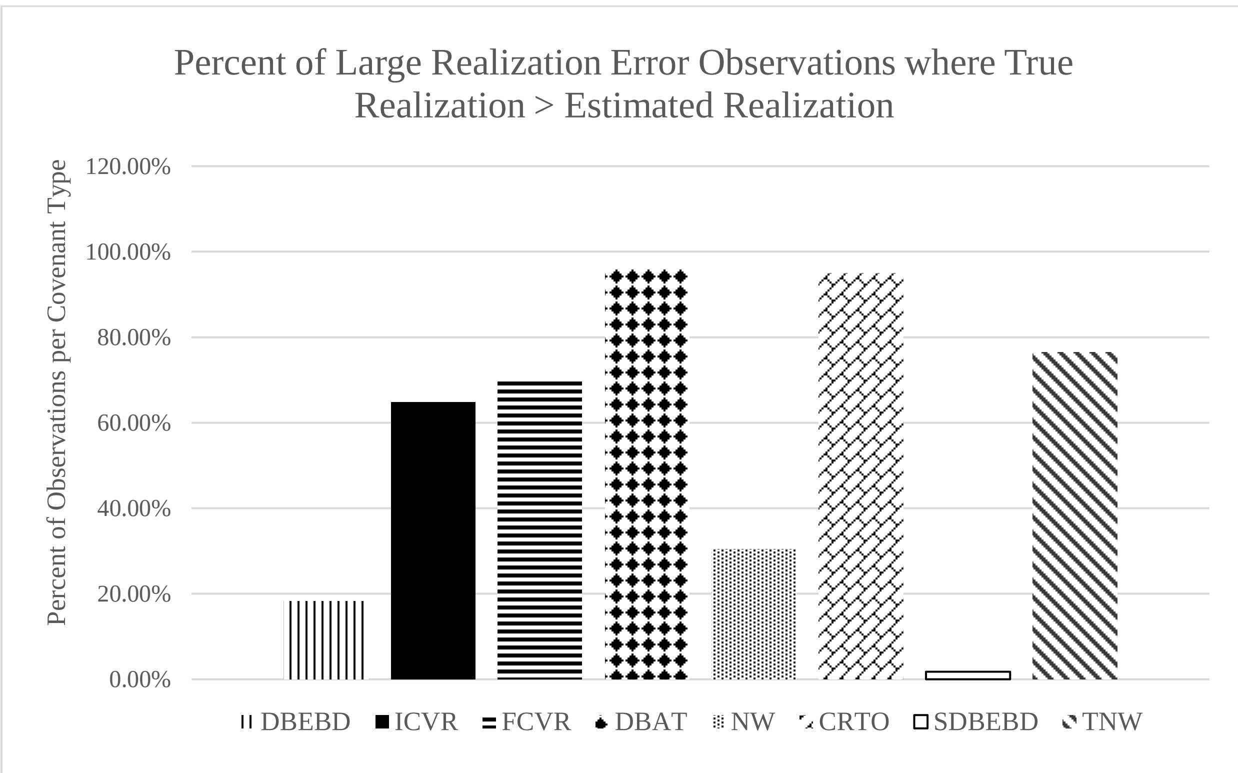


Figure IF7

Percent of Large Threshold Error Observations Where True Threshold Exceeds Estimated Threshold

This figure reports the percent of Large Threshold Error observations by covenant types, where True Threshold is greater than Estimated Threshold. Large Threshold Error is an indicator equal to one for a given firm-quarter-covenant if the absolute value of the difference between the true and estimated threshold scaled by the true threshold exceeds 25%.

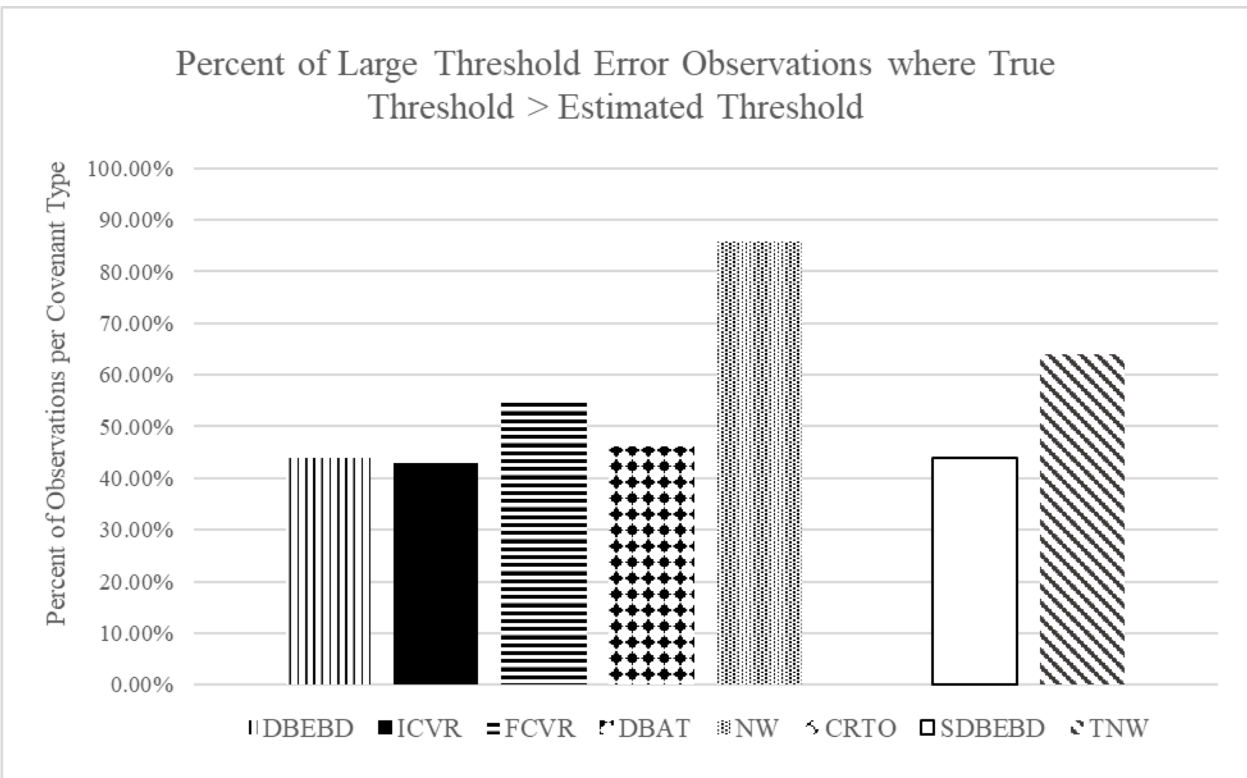


Table IA1

Measurement Error Decomposition – Violation Frequency

This table reports violations (i.e., negative slack) and *Violation %* for eight covenant types using different computations for slack to determine the source of measurement error in estimated slack. *Violation %* is the proportion of observations per covenant type for which slack (using one of four measures) is negative, indicating a violation. *Overestimation %* is calculated as the difference between *Violation %* based on an estimated measure of slack and *Violation %* based on true slack, scaled by the *Violation %* based on true slack.

Realization			Threshold					
			True Threshold			**Estimated Threshold**		
True Realization			*True Slack*			*True Realization & Estimated Threshold*		
Covenant	**N**		**Violations**	**Violation %**	**Overestimation %**	**Violations**	**Violation %**	**Overestimation %**
DBEBD	6,011		42	0.70%	-	492	8.18%	1071%
ICVR	3,589		29	0.81%	-	151	4.21%	421%
FCVR	1,708		45	2.63%	-	135	7.90%	200%
DBAT	1,855		7	0.38%	-	34	1.83%	386%
NW	456		6	1.32%	-	30	6.58%	400%
CRTO	372		18	4.84%	-	21	5.65%	17%
SDBEBD	335		3	0.90%	-	29	8.66%	867%
TNW	297		8	2.69%	-	51	17.17%	538%
Total	14,623		158	1.08%		943	6.45%	497%
Estimated Realization			*Estimated Realization & True Threshold*			*Estimated Slack*		
Covenant	**N**		**Violations**	**Violation %**	**Overestimation %**	**Violations**	**Violation %**	**Overestimation %**
DBEBD	6,011		1,120	18.63%	2567%	1,357	22.58%	3131%
ICVR	3,589		279	7.77%	862%	369	10.28%	1172%
FCVR	1,708		615	36.01%	1267%	593	34.72%	1218%
DBAT	1,855		17	0.92%	143%	31	1.67%	343%
NW	456		22	4.82%	267%	33	7.24%	450%
CRTO	372		193	51.88%	972%	204	54.84%	1033%
SDBEBD	335		171	51.04%	5600%	192	57.31%	6300%
TNW	297		40	13.47%	400%	75	25.25%	838%
Total	14,623		2,457	16.80%	1455%	2,854	19.52%	1706%

Table IA2

Measurement Error Decomposition – Frequency of Type I and Type II Errors

This table reports the number and percentage of observations, per covenant type, that are misclassified as violations (Type 1 errors) or non-violations (Type II errors) relative to True Slack. Type I errors are errors in which a violation is identified using and estimated slack measure, but when no violation has actually occurred (based on true slack). Type II errors are errors in which an estimated slack measure fails to identify a violation, but where a violation has occurred (based on true slack).

			Threshold							
			True Threshold				Estimated Threshold			
Realization			*True Slack*				*True Realization & Estimated Threshold*			
True Realization	**Covenant**	**N**	**Type I**	**Type I %**	**Type II**	**Type II %**	**Type I**	**Type I %**	**Type II**	**Type II %**
	DBEBD	6,011	No Errors				458	7.62%	8	0.13%
	ICVR	3,589					126	3.51%	4	0.11%
	FCVR	1,708					95	5.56%	5	0.29%
	DBAT	1,855					29	1.56%	2	0.11%
	NW	456					26	5.70%	2	0.44%
	CRTO	372					3	0.81%	0	0.00%
	SDBEBD	335					26	7.76%	0	0.00%
	TNW	297					44	14.81%	1	0.34%
			Estimated Realization & True Threshold				*Estimated Slack*			
Estimated Realization	**Covenant**	**N**	**Type I**	**Type I %**	**Type II**	**Type II %**	**Type I**	**Type I %**	**Type II**	**Type II %**
	DBEBD	6,011	1,083	18.02%	5	0.08%	1,324	22.03%	9	0.15%
	ICVR	3,589	262	7.30%	12	0.33%	349	9.72%	9	0.25%
	FCVR	1,708	575	33.67%	5	0.29%	555	32.49%	7	0.41%
	DBAT	1,855	15	0.81%	5	0.27%	29	1.56%	5	0.27%
	NW	456	16	3.51%	0	0.00%	29	6.36%	2	0.44%
	CRTO	372	177	47.58%	2	0.54%	187	50.27%	1	0.27%
	SDBEBD	335	168	50.15%	0	0.00%	189	56.42%	0	0.00%
	TNW	297	34	11.45%	2	0.67%	69	23.23%	2	0.67%

Table IA3

Determinants of Large Slack and Type I Errors at Firm-Quarter-Covenant Level

This table reports regressions of Large Slack Error (Panel A) and Type I Error (Panel B) on indicators for covenant type. Additional firm and loan level controls are suppressed for brevity. The unit of analysis for these regressions is the firm-quarter-covenant level.

Panel A – Large Slack Error

	1	2	3	4	5	6	7	8
	Large Slack Error							
DBEBD	-0.095***							
	(-4.054)							
ICVR		-0.117***						
		(-4.283)						
FCVR			0.237***					
			(6.571)					
DBAT				0.241***				
				(5.316)				
NW					0.089			
					(1.099)			
CRTO						0.163***		
						(3.207)		
SDBEBD							0.038	
							(0.867)	
TNW								-0.015
								(-0.490)
Observations	11,877	11,877	11,877	11,877	11,877	11,877	11,877	11,877
Adjusted R^2	0.223	0.225	0.235	0.229	0.215	0.217	0.215	0.215
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender

Panel B – Type I Error

	1	2	3	4	5	6	7	8
	Type I Error							
DBEBD	0.036** *(2.034)*							
ICVR		-0.104*** *(-7.205)*						
FCVR			0.143*** *(4.036)*					
DBAT				-0.123*** *(-3.970)*				
NW					-0.076* *(-1.757)*			
CRTO						0.122 *(1.029)*		
SDBEBD							0.281*** *(4.828)*	
TNW								0.034*** *(2.602)*
Observations	11,877	11,877	11,877	11,877	11,877	11,877	11,877	11,877
Adjusted R^2	0.354	0.364	0.364	0.358	0.353	0.354	0.362	0.353
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender

Table IA4

Determinants of Large Realization and Threshold Error at Firm-Quarter Level

This table reports determinant models for both large realization and large threshold errors, estimated at the firm-quarter level. For each firm-quarter observation, large realization error (large threshold error) equals one if at least one of the underlying required covenant types experiences a large realization (large threshold) error. All variables are defined in Appendix A in the manuscript.

	1	2	3	4	5	6
	Large Realization Error			Large Threshold Error		
Log(Assets)	-0.024		-0.041**	-0.049***		-0.059***
	(-1.485)		*(-2.134)*	*(-4.121)*		*(-4.129)*
Leverage	-0.183**		-0.258***	-0.094		-0.087
	(-2.506)		*(-3.948)*	*(-1.599)*		*(-1.611)*
Current Ratio	-0.016		-0.014	-0.030**		-0.026*
	(-0.864)		*(-0.783)*	*(-2.326)*		*(-1.957)*
Tangibility	0.102		-0.049	-0.035		-0.043
	(0.735)		*(-0.340)*	*(-0.381)*		*(-0.439)*
Loss	0.091***		0.072***	0.055***		0.040***
	(3.562)		*(3.135)*	*(3.030)*		*(2.592)*
S&P Rated	-0.010		0.028	0.069*		0.046
	(-0.230)		*(0.611)*	*(1.858)*		*(1.214)*
CFO Volatility	0.000		0.000	-0.001		-0.001
	(0.195)		*(0.139)*	*(-1.142)*		*(-0.866)*
Sales Growth	0.040		0.030	-0.003		-0.021
	(1.455)		*(1.067)*	*(-0.150)*		*(-0.968)*
Zscore	-0.023		-0.051***	-0.045***		-0.050***
	(-1.438)		*(-3.146)*	*(-2.752)*		*(-3.732)*
Debt-to-EBITDA	0.026***		0.021***	0.015***		0.014***
	(4.849)		*(4.271)*	*(3.430)*		*(3.435)*
Interest Coverage	0.001		0.001	0.001		0.000
	(1.576)		*(1.458)*	*(1.085)*		*(0.512)*
N_Covenants		0.081***	0.057**		0.102***	0.085***
		(3.553)	*(2.370)*		*(5.655)*	*(4.331)*
Maturity		-0.097**	-0.139***		0.093***	0.075**
		(-2.127)	*(-2.970)*		*(2.683)*	*(2.101)*
Loan Amount		0.030	-0.010		-0.100**	-0.255***
		(0.460)	*(-0.124)*		*(-2.025)*	*(-3.243)*
Relationship Lender		-0.033	-0.030		-0.042*	-0.008
		(-1.219)	*(-0.996)*		*(-1.737)*	*(-0.303)*
Observations	7,410	9,010	7,353	7,410	9,010	7,353
Adjusted R^2	0.184	0.226	0.279	0.152	0.232	0.279
Fixed Effects	Industry & Year	Industry, Year & Lender	Industry, Year & Lender	Industry & Year	Industry, Year & Lender	Industry, Year & Lender

Table IA5

Determinants of Large Realization and Threshold Error at Firm-Quarter-Covenant Level

This table reports regressions of Large Realization Error (Panel A) and Large Threshold Error (Panel B) on indicators for covenant type. Additional firm and loan-level controls are suppressed for brevity. The unit of analysis for these regressions is the firm-quarter-covenant level.

Panel A – Large Realization Error

	1	2	3	4	5	6	7	8
	Large Realization Error							
DBEBD	-0.145***							
	(-5.370)							
ICVR		-0.094***						
		(-3.023)						
FCVR			0.356***					
			(8.712)					
DBAT				0.349***				
				(4.812)				
NW					-0.361***			
					(-6.513)			
CRTO						0.052		
						(0.358)		
SDBEBD							0.279***	
							(4.671)	
TNW								-0.042**
								(-2.087)
Observations	11,877	11,877	11,877	11,877	11,877	11,877	11,877	11,877
Adjusted R^2	0.231	0.219	0.256	0.241	0.224	0.213	0.219	0.219
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender

Panel B – Large Threshold Error

	1	2	3	4	5	6	7	8
	Large Threshold Error							
DBEBD	0.008 *(0.462)*							
ICVR		-0.013 *(-0.776)*						
FCVR			-0.019 *(-0.589)*					
DBAT				-0.063* *(-1.802)*				
NW					0.196** *(2.337)*			
CRTO						-0.079 *(-1.603)*		
SDBEBD							0.082 *(1.423)*	
TNW								0.022 *(1.585)*
Observations	11,877	11,877	11,877	11,877	11,877	11,877	11,877	11,877
Adjusted R^2	0.211	0.211	0.211	0.213	0.218	0.212	0.212	0.215
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender	Industry, Year & Lender

Table IA6

Correlation of Large Measurement Errors

This table reports the correlations for Large Slack Error, Large Realization Error, and Large Threshold Error. Each Large Error measure is defined as an indicator equal to one if the absolute value between the true measure and estimated measure scaled by the true measures exceeds 25%. Because this correlation table is reported at the firm-quarter level, each Large Error measure is set to one if the underlying Large Error measure for any covenant type for the given firm-quarter is equal to one.

		1	2
1	Large Slack Error		
2	Large Realization Error	0.46***	
3	Large Threshold Error	0.21***	0.02**

Table IA7

Violation Frequency when using Lagged True Thresholds

This table compares the frequency of violations, by covenant type, using three unique measures to capture violations. *True Violations* are violations identified by comparing the True Realization_t to the True Threshold_t. *True Violations (Lagged)* are violations identified by comparing True Realization_t to the True Threshold_{t-1}. *Estimated Violations* are violations identified by comparing Estimated Realization_t to the Estimated Threshold_t. *True Violations (Lagged)* are intended to capture both True Violations and likely violations that are avoided because the covenant thresholds is preemptively adjusted to avoid the violation. When comparing *True Violations (Lagged)* to *Estimated Violations*, we see that *Estimated Violations* still grossly overstate the occurrence of a violation (on average by a magnitude greater than tenfold).

		True Violations		*True Violations (Lagged)*		*Estimated Violations*			
Covenant	**N**	**Violations**	**Violation %**	**Violations**	**Violation %**	**Violations**	**Violation %**	**Overestimation %**	**Overestimation % (Lagged)**
DBEBD	5,305	27	0.5%	85	1.6%	1,160	21.9%	4196.3%	1264.7%
ICVR	3,183	16	0.5%	27	0.8%	315	9.9%	1868.8%	1066.7%
FCVR	1,455	26	1.8%	38	2.6%	477	32.8%	1734.6%	1155.3%
DBAT	1,695	3	0.2%	8	0.5%	29	1.7%	866.7%	262.5%
NW	386	5	1.3%	8	2.1%	19	4.9%	280.0%	137.5%
CRTO	323	11	3.4%	12	3.7%	177	54.8%	1509.1%	1375.0%
SDBEBD	291	2	0.7%	5	1.7%	166	57.0%	8200.0%	3220.0%
TNW	254	3	1.2%	6	2.4%	61	24.0%	1933.3%	916.7%
Totals	12,892	93	0.7%	189	1.5%	2,404	18.6%	2484.9%	1172.0%

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