Journal of Accounting and Economics 75 (2023) 101536

Contents lists available at ScienceDirect

Journal of Accounting and Economics

journal homepage: www.journals.elsevier.com/journal-of-accounting-and-economics

The effect of bond market transparency on bank loan contracting☆

Mahfuz Chy*, Hoyoun Kyung

Trulaske College of Business, University of Missouri, Columbia, MO 65211, USA

ARTICLE INFO

Article history:
Received 11 September 2020
Received in revised form 15 July 2022
Accepted 28 July 2022
Available online 7 August 2022

JEL classification:
D82
D86
G14
G21
M40
M41

Keywords:
Debt contracting
Covenant intensity
Market transparency
TRACE
Information asymmetry
Secondary bond market

ABSTRACT

We find that bond issuers receive bank loans with 12% fewer covenants when the secondary corporate bond market becomes more transparent. The treatment effect is more pronounced when bond trades are more informative, when stock prices are less informative, and when the likelihood of future debt-equity agency conflicts is higher. The evidence suggests that bond prices reflect forward-looking information that mitigates banks' information risk in debt contracting. As such, banks impose fewer contractual restrictions on bond issuers when bond transactions become publicly observable. We find consistent results using a hand-collected dataset of negative covenants. Treatment firms are also less likely to subsequently renegotiate borrowing terms. Finally, we find corroborating evidence from new primary bond issues. Taken together, our findings suggest that public bond market frictions affect private debt contract design.

1. Introduction

Information asymmetry between borrowers and lenders is a key determinant of debt contract design (see, e.g., Armstrong et al., 2010; Bushman et al., 2017; Roberts and Sufi, 2009). Prior studies often associate information asymmetry with borrower characteristics, such as financial reporting quality (e.g., Bharath et al., 2008; Costello and Wittenberg-Moerman, 2011) or underlying uncertainty in firm fundamentals (e.g., Chen et al., 2016; Demerjian, 2017). In this study, we focus on information asymmetry that arises from the financial markets. Specifically, we examine whether greater transparency in the secondary

☆ For their helpful comments, we thank Peter Demerjian (reviewer), Richard Frankel, David Godsell, Joao Granja, Zhujin Guo, Michele Hanlon (editor), Christoph Herpfer (discussant), Jared Jennings, Sajid Kamal, Inder Khurana, Seil Kim, Inmoo Lee, Ningzhong Li, Xiumin Martin, Elaine Mauldin, Raynolde Pereira, Fellipe Raymundo (discussant), Valentina Salotti (discussant), Barbara Su, Musa Subasi (discussant), Oktay Urcan, Dushyant Vyas, Ivy Wang, Regina Wittenberg-Moerman, Byongwook Yun, conference participants at the 2020 FARS Midyear Meeting, 2020 MFA Annual Meeting, 2020 AAA Annual meeting, 2020 FMA Annual meeting, and workshop participants at Washington University in St. Louis, the University of Illinois at Urbana-Champaign, the Korea University - KAIST Joint Workshop, and the University of Missouri.

* Corresponding author.
E-mail addresses: chymr@missouri.edu (M. Chy), kyungh@missouri.edu (H. Kyung).

https://doi.org/10.1016/j.jacceco.2022.101536
0165-4101/

corporate bond market affects the use of debt covenants in private lending agreements. Following prior studies (Bloomfield and O'Hara, 1999; Madhavan, 1995, 1996; Pagano and Röell, 1996), we define market-level transparency as transparency that enables market participants to observe information about the trading process (e.g., prices, yields, volumes, date and time, security identification) at the security- and issuer-level for all transactions and securities in the market.

Studying the effect of bond market transparency improvements on debt contract design is important for at least two reasons. First, prior studies document that bank screening and monitoring benefit other stakeholders, including bond investors (Datta et al., 1999; Diamond, 1984; Fama, 1985; Ma et al., 2019). However, the literature has broadly ignored the role of the bond market in general, and its role in shaping bank loan contracts in particular. Second, whereas firm-level transparency is often an endogenous outcome based on a trade-off between the costs (e.g., proprietary costs) and benefits (e.g., favorable loan contract terms) of a firm's disclosure choices, market-level transparency is removed from individual firms' cost-benefit analyses. As such, inferences in prior studies that relate individual firms' incentives for transparency to debt contract design may not extend to the effect of market-level transparency on debt contracting.

Prior literature shows that the secondary bond market prices aggregate and disseminate *forward-looking* information that anticipates borrowers' default risk, changes in economic fundamentals, risk-taking, and moral hazard problems. For example, Han and Zhou (2014) document that bond spreads predict corporate defaults. Badoer and Demiroglu (2019) show that bond spreads better anticipate rating downgrades when bond market transparency improves. Bittlingmayer and Moser (2014) find that the bond market incorporates value-relevant negative information before the stock market does (see also DeFond and Zhang, 2013; Wei and Zhou, 2016). Several other studies suggest that informed trading in the bond market anticipates material events (e.g., M&As, class action lawsuits) that can impact a firm's ability and/or willingness to service debt obligations (e.g., Billings et al., 2012; Kedia and Zhou, 2014).

We argue that forward-looking information reflected in bond prices mitigates banks' information risk and facilitates loan contracting in two mutually corroborating ways (see Section 3.2 for more details). First, information in bond prices enables better verification of borrower-provided information. Specifically, the forward-looking information that banks obtain from borrowers may not be directly verifiable (Demerjian et al., 2020). As such, forward-looking information reflected in bond prices allows banks to better verify borrowers' private information, mitigating uncertainty about possible outcomes of borrowers' future performance (e.g., Demerjian et al., 2020; Hirst et al., 1999). Further, to the extent that an independent market signal reduces managers' incentives to make inflated projections (Hu, 2019; Rickmann, 2022), a more transparent bond market may also enhance the credibility of information that borrowers furnish. Second, information in bond prices plays a confirmatory role. Banks have imperfect information about their own screening ability (Murfin, 2012). An independent signal that aggregates bond market participants' private information can help reduce uncertainty about banks' screening ability. For example, if banks have doubts about their initial screening conclusions, forward-looking information reflected in bond prices may allow banks to validate their initial assessments.

The above discussions suggest that bond prices reflect forward-looking information that can mitigate banks' information risk. Prior literature shows that a reduction in information risk reduces the usefulness of debt covenants in private lending agreements (Chy et al., 2021; Demerjian, 2017; Dessein, 2005; Gârleanu and Zwiebel, 2009; Smith and Warner, 1979). Therefore, we hypothesize that improvements in secondary bond market transparency that enable banks to observe bond transactions reduce the usefulness of loan covenants.

Alternatively, the negative effect of enhanced bond market transparency on the usefulness of covenants may not be borne out empirically. Prior studies suggest that banks have an informational advantage and possess superior information-processing ability relative to other capital providers (Black, 1975; Diamond, 1984, 1991; Fama, 1985). Further, public and private debts differ in terms of payoff functions and lender incentives (see, e.g., Asquith et al., 1994; Denis and Mihov, 2003; Lin et al., 2013). Thus, the forward-looking information that bond prices convey may not be incrementally useful to banks when screening potential borrowers and designing loan contracts. As such, whether bond market transparency improvements affect private loan contract design is an empirical issue.

We exploit the staggered implementation of the Trade Reporting and Compliance Engine (TRACE) to capture enhancements in bond market transparency. TRACE is an automated system that assembles and disseminates all corporate bond transactions in the secondary dealer-oriented over-the-counter (OTC) bond market. For several decades prior to the implementation of TRACE, corporate bonds were traded in an opaque environment (Bessembinder and Maxwell, 2008). Specifically, the price and size of each completed trade were known only to the parties directly involved in the trade. In contrast, the TRACE platform publicly disseminates post-transaction details of each trade (e.g., date and time of the transaction, security identification, price, and size of the trade), thus significantly improving transparency in the OTC bond market (Asquith et al., 2019; Bessembinder and Maxwell, 2008). Further, under the TRACE system, dissemination of post-transaction details applied to outstanding bonds already trading in the OTC bond market before the implementation of TRACE, and hence was outside the control of an individual issuing entity. As such, bond market transparency enhancements under TRACE are plausibly exogenous to the individual firm-level transparency as well as to the private loan-level contract design choices.

Our empirical analyses compare covenant intensity (i.e., the number of covenants) for loans issued to treatment firms and loans issued to control firms, before and after the implementation of TRACE. The treatment group consists of borrowing firms that undergo transparency enhancements under the TRACE system. The control group of firms have no outstanding public bonds reported in the TRACE system but have S&P domestic long-term issuer credit ratings. We require the control group to have credit ratings (similar to the treatment group) because firms with and without credit ratings differ in financing and investment (Faulkender and Petersen, 2006; Harford and Uysal, 2014). TRACE was implemented in four phases and each

phase provides a separate group of treatment firms treated in a different period. As such, the use of TRACE as our empirical setting allows us to adopt a generalized difference-in-differences (DiD) research design. We include issuer firm rating fixed effects to compare loan covenants for treatment and control firms within the same rating category. We also include industry fixed effects to absorb industry heterogeneities and year-quarter fixed effects to account for secular time trends.

Our main finding is that treatment firms receive loans with 12% fewer covenants ($t\text{-}statistic = -4.52$) than control firms following the implementation of TRACE. The negative treatment effect manifests for both financial and non-financial covenants (sweeps). Supporting the parallel trend assumption underlying the DiD estimation, we show that the treatment and control groups do not have differential loan covenant trends prior to the implementation of TRACE. Our inferences also hold when we limit the sample to loans issued to treatment firms only.[1] Thus, our results are not sensitive to the choice of a control sample. The key inference is robust to the inclusion of borrower fixed effects, alternative estimation approaches, separate tests for different rating groups, tests addressing treatment effect biases in generalized DiD specifications (Baker et al., 2022; Goodman-Bacon, 2021), and tests addressing alternative explanations (see Sections 6 and 7).

In cross-sectional tests, we find that the negative effect of TRACE on loan covenant intensity is more pronounced when bond trading is more informative (measured by aggregate trade size and price impact), strengthening our argument that information in bond prices reduces banks' information risk. The treatment effect is also stronger for borrowers with higher stock market information asymmetry (measured by bid-ask spread) and uncertainty (measured by stock price dispersion and analyst forecast dispersion), suggesting that the incremental informational role of bond prices is more pronounced when information asymmetry and uncertainty as reflected in the stock prices are higher. Finally, consistent with prior evidence that information asymmetry exacerbates debt-equity agency conflicts, the treatment effect is stronger when the likelihood of future debt-equity agency conflicts, as measured by growth options and loan maturity, is higher (Barnea et al., 1980; Brockman et al., 2010; Jensen and Meckling, 1976; Myers, 1977).

In additional analyses, we find that our results for financial covenants are primarily driven by performance covenants, consistent with the notion that performance covenants are more sensitive to information risk and are used complementarily to sweeps (Christensen and Nikolaev, 2012). We also document a decrease in the likelihood of future renegotiations following the implementation of TRACE, suggesting that the initial contract better incorporates forward-looking information when bond prices become observable. To overcome the deficiency of DealScan as a source of negative covenant data, we hand-collect covenants from original loan contracts in EDGAR. Using the hand-collected data, we find consistent evidence that the TRACE implementation leads to a significant decrease in the use of negative covenants. In our final set of analyses, we find corroborating evidence from new primary bond issues. Specifically, we find that new bonds come with a lower cost of borrowing, better credit rating, and fewer covenants, consistent with our argument that bond market transparency enhancements reduce information risk.

Our study makes the following contributions. First, we provide novel evidence that exogenous enhancements in secondary bond market transparency reduce contractual restrictions in private lending agreements, thus extending the debt contracting literature (see, e.g., Armstrong et al., 2010; Roberts and Sufi, 2009). Whereas prior research has primarily focused on the role of banks in facilitating public debt contracting (e.g., Datta et al., 1999; Ma et al., 2019), our findings show that transparency improvements in the secondary bond market can affect private loan contract design, implying a mutual reliance between private and public debt market participants.

Second, our evidence on the role of forward-looking information reflected in bond prices extends prior studies documenting the decision usefulness of information embedded in security prices (e.g., Bond et al., 2012; Chen et al., 2007; Jayaraman and Wu, 2019). Our study also speaks to the policy debate about the consequences of secondary bond market transparency (Asquith et al., 2019). Bond market transparency is a policy variable that is determined by capital market regulations. Our evidence suggests that the effect of improved bond market transparency goes beyond the SEC's policy agenda, extending to private loan contracting.

Finally, our study is relevant in interpreting prior mixed evidence on the effect of credit default swap (CDS) trading on debt contracting. Amiram et al. (2017) find that the initiation of CDS trading increases loan spreads and the share of loans retained by lead arrangers, implying that credit protection afforded by the CDS market exacerbates moral hazard in bank monitoring. In contrast, Shan et al. (2019) suggest that CDS protection enhances debt contracting efficiency by reducing contractual restrictions. Because the CDS market disseminates credit risk information and, at the same time, affords banks credit risk protection, separating the two effects is difficult, which may explain prior mixed evidence on the effect of CDS trading initiation on debt contracting. In this study, we use an empirical setting that isolates the effect of market-level transparency enhancements and show that enhanced market-level transparency reduces contractual restrictions in private lending agreements.

2. Institutional background

2.1. The implementation of TRACE

TRACE is an automated system that assembles and disseminates all corporate bond transactions in the U.S. secondary OTC bond market.[2] Prior to the implementation of TRACE, post-transaction details were reported only to the parties

[1] Because the implementation of TRACE was staggered in multiple phases, we can estimate the treatment effect without having a separate control group.

[2] See Bessembinder et al. (2006), Bessembinder and Maxwell (2008), Bessembinder et al. (2020), Biais and Green (2019), and Asquith et al. (2019) for institutional background on the secondary corporate bond market and the implementation of the TRACE platform.

directly involved in each trade and thus the OTC corporate bond market was more opaque (Bessembinder and Maxwell, 2008). Portfolio managers often relied on vendor-provided matrix price information in order to value their bond portfolio holdings (Bessembinder and Maxwell, 2008).[3] In exchanges, which mainly catered to individual investors, a small subset of bonds was traded odd-lot (Edwards et al., 2007). Nunn et al. (1986) find significant differences between exchange-traded odd-lot prices and vendor-provided round-lot matrix prices. Similarly, there was significant price dispersion between different bond market platforms (e.g., exchange vs. OTC) and even among dealers within the same platform (Goldstein and Hotchkiss, 2012).

In an attempt to enhance bond market transparency, the Securities and Exchange Commission (SEC) approved the TRACE Rule, which was prepared and proposed by the National Association of Securities Dealers (NASD).[4] On July 1, 2002, the NASD commenced the implementation of TRACE, mandating that all brokers and dealers registered with the SEC report transactions that they facilitate for all TRACE-eligible corporate bond securities.[5] Due to the concerns that enhanced transparency could potentially harm the liquidity of smaller and less actively traded bonds, the NASD implemented TRACE in staggered phases (Phases I, II, IIIA, and IIIB) where earlier (later) phases targeted more (less) actively traded bonds (Bessembinder and Maxwell, 2008). When all phases had been implemented, the TRACE platform provided post-transaction details for 99% of corporate bond transactions. See Appendix A for a summary of the timeline and details of each phase. In sum, with TRACE implementation, the secondary corporate bond market became substantially more transparent, publicly disseminating post-transaction details, such as date and time, security identification, price, and size of each trade (Asquith et al., 2019; Edwards et al., 2007).

2.2. Overview of the secondary bond market trading activity

Compared to stock trading in the secondary equity market, bond market trades occur infrequently. According to Bessembinder et al. (2020), only 1.5% of bonds are traded every day and 39% are traded on 5 or fewer days in a year. However, Bessembinder et al. (2020) note that investment-grade (high-yield) bonds with issue sizes greater than $1 billion are traded, on average, 179 (150) days in a year. Further, whereas the median trade size is $3000–$5,000 for stocks (O'Hara et al., 2014), the average round-lot trade size in the bond market is $1.2 million (Bessembinder et al., 2020). Ronen and Zhou (2013) remark that "the relatively lower trading frequencies observed in the bond market (as compared to equity markets) are not necessarily indicative of inefficiency. Infrequent but large trades may reflect investors waiting for relevant and material information, and then rapidly acting on it in large blocks."

If a firm has multiple bonds, a few of these bonds attract most of the trades in any given period, with the rest seeing less active trading (Ronen and Zhou, 2013). Even after bonds settle into institutional investor portfolios, the secondary market remains active although it may be thinner (Edwards et al., 2007). Dick-Nielsen et al. (2012) provide evidence that there are usually at least some bonds that are actively traded for each issuer at any given point in time. Overall, prior studies suggest that even though some individual bond issues out of a firm's multiple bonds trade infrequently, the actively traded bonds are likely to reflect *firm-level* fundamentals and credit risks at any given point in time. See Section 5.1 for descriptive statistics on firm-level bond trading activity.

3. Hypothesis development

3.1. The role of information in debt contracting

Theory suggests that when there is information asymmetry between borrowers and lenders, a borrowing firm that is "good" type avoids being pooled with the "bad" types by agreeing to relinquish more control rights *ex-ante*. This is because the "good" type borrowing firm knows that the *ex-post* realization of the contractible signal(s) will be favorable to it, and hence it maintains control *ex-post*. As such, in equilibrium, the adverse selection argument dictates that creditor control rights––hence, covenant restrictions––are increasing in information asymmetry (Dessein, 2005; Gârleanu and Zwiebel, 2009; Roberts, 2015). Relatedly, information asymmetry also exacerbates moral hazard concerns (e.g., risk-shifting, underinvestment, aggressive shareholder payout) (Jensen and Meckling, 1976; Myers, 1977). Thus, when information asymmetry is higher, lenders have greater demand for debt covenants that limit the potential for moral hazards (Bradley and Roberts, 2015; Smith and Warner, 1979). Overall, debt contracting theories predict that information risk increases the demand for debt covenants. Using various settings, empirical studies find corroborating evidence supporting this theoretical prediction (Armstrong et al., 2010; Chy et al., 2021; Costello and Wittenberg-Moerman, 2011; Hollander and Verriest, 2016).

[3] Matrix prices were each vendor's price estimates based on quotations for securities with similar characteristics such as credit quality, maturity, and coupon rate (Chen et al., 2020).

[4] The NASD changed its name to the Financial Industry Regulatory Agency (FINRA) in 2007.

[5] A TRACE-eligible security is any U.S. dollar-denominated debt security that is depository-eligible and registered with the SEC, or issued pursuant to Section 4(2) of the Securities Act of 1933 and purchased or sold pursuant to Rule 144a (Asquith et al., 2019).

3.2. Forward-looking information in bond prices and loan contracting

Prior literature suggests that bond prices aggregate and disseminate forward-looking information that anticipates borrower default risk and changes in economic fundamentals (e.g., Badoer and Demiroglu, 2019; Han and Zhou, 2014). For example, Han and Zhou (2014) document that bond spreads predict corporate defaults even after controlling for other predictors of corporate defaults. Badoer and Demiroglu (2019) show that bond spreads in the post-TRACE bond market anticipate rating downgrades such that the downgrades themselves provide no additional information. Several studies also show that the secondary corporate bond market predicts borrowers' operating performance, which can determine borrowers' ability to make principal and interest payments (see, e.g., Bittlingmayer and Moser, 2014; DeFond and Zhang, 2013; Even-Tov, 2017; Wei and Zhou, 2016). In the same vein, other studies find that bond prices anticipate borrowers' risk-taking and moral hazard problems (see, e.g., Brockman et al., 2010; Chen et al., 2020), pending takeovers (Kedia and Zhou 2014), additional indebtedness (Klein and Zur 2011), class action lawsuits (Billings et al., 2012), and other material events (see, e.g., Furfine and Rosen, 2011; Vallascas and Hagendorff, 2011).

Based on the evidence in prior studies, we argue that forward-looking information reflected in bond prices can facilitate loan contracting through two complementary roles: verification of borrower-provided forward-looking information and confirmation of banks' own initial screenings. First, forward-looking information that banks gather from public and private sources includes expected future sales, expenses, budgets, and payoffs from investments (Demerjian et al., 2020). Demerjian et al. (2020) remark, "unlike historical financial statements, *forward-looking* information cannot be verified by lenders or external auditors" (emphasis added). Importantly, borrowers are required to make projections into an uncertain future when furnishing forward-looking information, which, in turn, introduces uncertainty into lenders' evaluation of borrowers' creditworthiness (Demerjian et al., 2020; Hirst et al., 1999). Therefore, forward-looking information in bond prices can mitigate lenders' information risk about borrowers' future performance by allowing banks to better verify borrower-provided private information.

Second, information in bond prices is also likely to play a confirmatory role. Prior evidence suggests that banks have imperfect information about their own screening ability (Murfin, 2012). Because bond prices aggregate many sophisticated investors' independent assessments (e.g., Badoer and Demiroglu, 2019; Chen and Lu, 2019; De Franco et al., 2014), access to the bond market signal can reduce banks' uncertainty about their initial screening assessments. As an example, the market's aggregated perspective reflected in bond spreads can complement banks' own subjective assessments of borrowers' default risk. Similarly, bond market reactions to earnings announcements can help banks evaluate the market assessment of earnings predictability and stochastic discount factors (e.g., Even-Tov, 2017). Bond market reactions to M&A announcements can help banks better assess to what extent the acquisition reduces future risk (by way of diversification) or destroys value (e.g., Chen et al., 2020).

Our discussions in Sections 3.1 and 3.2 collectively suggest that bond prices in the secondary corporate bond market reflect forward-looking information that mitigates banks' information risk. A large literature shows that a reduction in information risk reduces the usefulness of debt covenants (Chy et al., 2021; Demerjian, 2017; Dessein, 2005; Smith and Warner, 1979). Therefore, improvements in bond market transparency that enable banks (and other market participants) to observe bond prices reduce the need for debt covenants in private lending agreements. We propose the following hypothesis (stated in the alternative form):

H1. *Enhanced transparency in the secondary corporate bond market leads to a decrease in the use of debt covenants in private lending agreements*.

However, our prediction is not without tension because public and private debts are characterized by different payoff functions and lender incentives. Specifically, prior studies suggest that banks and public debtholders differ in terms of collateral requirements (Lin, 2016), the preferred mode of financial distress resolution (Asquith et al., 1994; Chemmanur and Fulgheri, 1994), and ownership structures (Lin et al., 2013). Further, borrowing firms that access public and private debt markets differ in terms of creditworthiness and reporting quality (Bharath et al., 2008; Denis and Mihov, 2003). Because the information in bond prices is primarily shaped by bondholders' payoff functions and incentives, information relevant to the secondary bond market may not be incrementally useful to banks in private loan contracting.

4. Data and estimation procedures

4.1. Sample selection

Our sample selection begins with retrieving quarterly financial data items from the Compustat database for the period from 1997:Q1 to 2006:Q4.[6] We remove firm-quarters with missing or negative sales or book assets, firms in financial and utility industries, and firms with missing industry information. We merge the Compustat financial data with analyst coverage data from I/B/E/S and institutional investor data from Thomson Reuters 13F.[7] We label the resulting dataset as *CS Financial* dataset, and this comprises the potential candidates for both the treatment and control firms. To identify the

[6] The sample period includes five years prior to the introduction of the TRACE system in 2002 and five years in the post-TRACE period. However, untabulated analyses show that our inferences are not sensitive to shortening or lengthening the sample period in either direction.

[7] We set missing analyst coverage and institutional ownership to zero.

treatment group, we use TRACE Bond Trades data for the period from 2002:Q3 to 2006:Q4 and follow Dick-Nielsen's (2009) procedures to clean data errors in the TRACE system. Firms with at least one outstanding public bond reported in the TRACE system during the sample period comprise the treatment group. Our control group of firms have no outstanding public bonds reported in the TRACE database but have S&P domestic long-term issuer credit ratings prior to 2002. We require the control group of firms to have credit ratings (similar to the treatment group) because firms with and without credit ratings differ in several important dimensions such as financing choices, financial constraints, information asymmetry, growth opportunities, and size (Barraza et al., 2015; Faulkender and Petersen, 2006; Harford and Uysal, 2014). The treatment and control groups of firms are then merged with the *CS Financial* quarterly dataset described above. Next, we construct firm-quarter level independent and control variables (see, Variable Construction in Section 4.2 below), and label the resulting firm-quarter level dataset *Treat and Control*. In the final step, we use the Loan Pricing Corporation's DealScan database to obtain loan-level data. To merge DealScan loans with the Compustat identifier *gvkey*, we use an updated version of the DealScan-Compustat link from Chava and Roberts (2008).[8] The DealScan database is organized at both the facility level and package level. Following prior studies (e.g., Demerjian, 2017; Hong et al., 2016), we measure covenants at the package level. To be included in the sample, a package must have at least one financial covenant or sweep as defined by Bradley and Roberts (2015). We also require loan-level control variables to be available. We merge the *package-level* DealScan data with the *firm-quarter* level *Treat and Control* dataset, which yields 2,638 loan packages in the final sample.

4.2. Variable Construction

4.2.1. Covenant variables

We measure covenant intensity by the total number of covenants, *TOTCOV*, which is the sum of all financial covenants and sweeps in a loan package. Because *TOTCOV* is right-skewed, we use the logarithm of *TOTCOV* as the main dependent variable, labeled *logTOTCOV*. We also separately examine financial and sweep covenants. We construct *logFINCOV* and *logSWEEP* by taking the logarithm of one plus the total number of financial covenants and sweeps, respectively.

Financial covenants, which include performance and capital covenants, require the maintenance of certain accounting ratios/numbers throughout the contractual period. Performance covenants are income statement-based covenants that serve as trip wires to transfer control rights to banks, thus addressing contractual incompleteness that arises due to the non-contractible nature of all future states (Aghion and Bolton, 1992; Christensen and Nikolaev, 2012). Capital covenants are balance sheet-based covenants that are used to align debtholder-equityholder interests (Christensen and Nikolaev, 2012).

Sweeps are covenants that require a borrower to pay down part or all of the outstanding loan balance if certain actions are undertaken (e.g., asset sales, debt or equity issuances). As such, sweeps mitigate debt-equity agency conflicts by restricting managerial actions that can potentially lead to debtholder wealth expropriation (De Franco et al., 2014; Smith and Warner, 1979). DealScan records five types of sweeps: asset sweeps, debt sweeps, equity sweeps, excess cash flow sweeps, and insurance proceeds sweeps. We include all five types of sweeps when counting the number of covenants for *SWEEP* and *TOTCOV*.[9, 10] In addition to the asset, debt, and cash flow sweeps that address classic agency conflicts (e.g., asset substitution, claim dilution, and additional indebtedness), we also include the equity sweeps in our *SWEEP* measure for the following two reasons. First, equity sweeps require borrowers to reduce indebtedness using the proceeds of equity issuance, thus limiting borrower riskiness. Second, equity issuance could encourage earnings management (Cohen and Zarowin, 2010; Kothari et al., 2016) and borrower risk-taking (Carlson et al., 2006; Lyandres et al., 2008). Equity sweeps can potentially limit the agency conflict related to earnings management and risk-taking.

4.2.2. Independent and control variables

The key independent variable is *TRACE*, which equals zero for a treatment firm before any of its bonds is included in the TRACE platform, and equals one when (and after) at least one of its bonds is included for dissemination in the TRACE platform. *TRACE* equals zero for control firms throughout the sample period. We also control for several loan-level and firm-level characteristics. Loan-level control variables include the logarithm of the loan facility amount (*LOANAMOUNT*), the logarithm of loan maturity in years (*MATURITY*), an indicator variable for the existence of performance pricing provisions (*PPP*), an indicator variable for the existence of collateral requirements (*SECURED*), and the total of all-in-drawn and all-in-undrawn spreads (*SPREAD*). Firm-level control variables include total accruals (*ACCRUALS*), firm age (*AGE*), size (*SIZE*), market-to-book asset ratio (*MARKET-TO-BOOK*), book leverage (*LEVERAGE*), institutional stock ownership (*INST*), analyst coverage (*ANALYST*), Altman's Z-score (*ALTz*), operating earnings (*EBITDA*), and shareholder payout (*PAYOUT*). See Appendix B for variable definitions.[11]

[8] The Dealscan-Compustat link we use covers bank loans issued between April 1982 and August 2017.

[9] It is worth noting that we exclude dividend restrictions from sweeps. However, our key inference is not sensitive to the inclusion of dividend restrictions in the main measure of covenants (see Section 6.2).

[10] Although capital expenditure covenants are classified as financial covenants in Dealscan, we categorize capital expenditure covenants under asset sweeps because they function more like a restrictive covenant, limiting borrower investments and thus reducing asset substitutions.

[11] When multiple loan facilities exist in a package, we use loan spread, maturity, and loan amount for the facility with the largest size within each package.

4.3. Regression specification

We exploit the implementation of TRACE as our research setting to capture exogenous enhancements in bond market transparency. The staggered implementation of TRACE allows us to adopt a generalized difference-in-differences research design (e.g., Bertrand and Mullainathan, 2003; Chy and Hope, 2021). Specifically, each of the TRACE phases−−Phase I, Phase II, Phase IIIA, and Phase IIIB−−provides a unique treatment sample that is treated at a different point in time. The generalized DiD design augments the typical DiD design with group and time fixed effects, thus allowing for separate group-specific intercepts as opposed to just two intercepts for the treatment and control firms in a typical DiD design (Angrist and Pischke, 2009).

We note that while Bertrand and Mullainathan (2003) employ firm-year panel data and use firm fixed effects to absorb firm-specific time-invariant heterogeneities, prior studies (e.g., Campello and Gao, 2017) recognize that in loan-level analysis such as ours, there are not enough within-firm variations to include borrower-firm fixed effects.[12] As a result, following prior studies (e.g., Campello and Gao, 2017; Carrizosa and Ryan, 2017; Ma et al., 2019), we instead include industry fixed effects, which absorb all time-invariant industry-level heterogeneities. We also include firm rating fixed effects because the SEC phased in the implementation of TRACE based on credit ratings, with worse-rated bonds generally included in the later phases. Credit ratings capture distress risk, information asymmetry, and moral hazard, all of which are likely to affect the use of debt covenants in lending agreements. The inclusion of rating fixed effects, which allows us to compare treatment and control firms within the same credit rating category, mitigates the concern that our findings may be driven by differences in credit ratings.

We estimate the following specification:

$$COVENANTS_{it} = \beta_1 TRACE_{it} + X_{it}\beta_2 + Firm\ Rating_i + Ind_i + YrQtr_t + \varepsilon_{it} \quad (1)$$

Here, *COVENANTS* is the measure of package-level covenant intensity (e.g., *logTOTCOV*). X_{it} represents the matrix of all time-varying controls and β_2 represents the column vector of coefficients associated with the control variables. $Firm\ Rating_i$ represents borrower rating fixed effects based on the S&P domestic long-term issuer credit ratings in incremental steps (e.g., there are separate fixed effects for BBB+, BBB, BBB-). Ind_i represents Fama-French 48 industry fixed effects. $YrQtr_t$ represents Year-Quarter fixed effects to control for secular time trends. $\varepsilon_{it} \sim N(0,\ \Sigma_g)$ is the error term, and allows for within-group arbitrary correlation. We cluster standard errors at the firm level in all regressions.

The key independent variable is *TRACE,* and β_1 is the coefficient of interest that captures the effect of enhanced bond market transparency on loan covenant intensity. Intuitively, each loan-package observation has a baseline regression intercept depending on the firm's industry and/or rating class categories.[13] Thus, the DiD coefficient β_1 represents a shift in the baseline intercept for the treatment group relative to the control group following the implementation of TRACE.

The unit of observation is the loan package. Note that the key independent variable *TRACE* and all firm-level control variables vary at the firm-quarter level. We use Ordinary Least Squares (OLS) regression in our main analyses. Because we use rating, industry, and year-quarter fixed effects, the use of negative binomial or Poisson regression may lead to inconsistent coefficient estimates due to the incidental parameter problem (Lancaster, 2000). Nonetheless, in robustness analyses, we show that our inferences are not sensitive to the use of a negative binomial regression.

5. Empirical findings

5.1. Descriptive statistics

Table 1, *Panel A*, provides summary statistics for the variables used in our empirical analyses. The mean number of covenants in loan packages is 5.03, with 2.80 financial covenants and 2.23 sweep covenants. The average loan maturity is 3.61 years, and the average spread is 183.64 bps. These statistics and other loan-level variables (e.g., *PPP*, *SECURED*) are comparable with prior studies (e.g., Bradley and Roberts, 2015). Of the 2,638 loan packages, 19% are issued to treatment firms after these firms are included in the TRACE system. Descriptive statistics for firm-level controls are also consistent with prior studies (e.g., Faulkender and Petersen, 2006).

Because treated firms access both the public and private debt markets, whereas control firms only access the private debt market, the two groups may differ in their use of debt covenants even prior to the implementation of TRACE. Table 1, *Panels B* and *C*, report the mean values of the loan- and firm-level variables for treated and control firms during the four quarters preceding the implementation of TRACE. Although covenant intensity is lower for the treated group than the control group, the difference is not statistically significant at conventional levels (p-value = 0.21). However, some loan-level and firm-level variables exhibit statistically significant differences in means between the two groups. To address the concern that differences

[12] Nonetheless, we conduct a robustness test using borrower-firm fixed effect as well (see Section 6.2).

[13] In an untabulated analysis, we instead assume that treatment and control firms have different baseline intercepts within industry and/or rating categories. We do so by including in the regression an indicator variable that equals one for treatment firms and zero for control firms. Our inferences are qualitatively similar.

between the treated and control groups drive our results, we incorporate loan- and firm-level control variables in our main model and conduct several additional tests in Section 6.1.

As we discuss in Section 2.2, prior studies imply that bond market trading is likely to convey information at the *firm level* even though individual bond issues may trade infrequently (e.g., Dick-Nielsen et al., 2012). To provide a more complete picture of bond trading at the firm level, we aggregate all bond-level trading activity at the firm-quarter level for all TRACE-reported trades for 2002:Q3–2006:Q4 (that is, the treatment period in our sample). *Panel D* of Table 1 shows that on average, a firm sees 488 quarterly transactions across all of its outstanding bonds. This translates to approximately 8 transactions per firm per trading day (488/63 trading days in a quarter). We also categorize the trading frequency across the four phases of

Table 1
Sample descriptive statistics.

Panel A: Summary Statistics—Full Sample

Variables	N	Mean	Std. Dev.	Median
TOTCOV	2,638	5.03	3.54	4.00
FINCOV	2,638	2.80	1.84	2.00
SWEEP	2,638	2.23	2.43	1.00
FACILITY AMOUNT ($ Mil)	2,638	484.41	681.32	250.00
YEARS TO MATURITY	2,638	3.61	1.80	4.00
SPREAD	2,638	183.64	126.87	157.50
SECURED	2,638	0.53	0.50	1.00
PPP	2,638	0.78	0.41	1.00
TRACE	2,638	0.19	0.39	0.00
AGE	2,638	2.73	0.81	2.83
SIZE	2,638	6.13	1.38	6.08
MARKET-TO-BOOK	2,638	1.31	0.81	1.10
LEVERAGE	2,638	0.38	0.20	0.35
INST	2,638	0.51	0.34	0.61
ANALYST	2,638	3.11	10.38	0.00
ALTz	2,638	1.62	1.47	1.34
EBITDA	2,638	0.03	0.02	0.03
PAYOUT	2,638	0.02	0.03	0.00
ACCRUALS	2,638	−0.04	0.07	−0.03

Panel B: Loan-level Variables During Four Quarters Preceding TRACE Implementation

Variable	Treated Mean (1)	Control Mean (2)	Difference: (1)–(2)	P-value
TOTCOV	5.03	5.44	−0.41	0.21
FINCOV	2.98	3.21	−0.23	0.17
SWEEP	2.06	2.23	−0.18	0.42
FACILITY AMOUNT ($ Mil)	543.58	401.68	141.90	0.21
YEARS TO MATURITY	3.04	3.36	−0.32	0.05
LOAN SPREAD	176.04	234.09	−58.06	<0.01
SECURED	0.49	0.59	−0.11	0.02
PPP	0.81	0.73	0.08	0.03

Panel C: Firm-level Variables During Four Quarters Preceding TRACE Implementation

Variable	Treated Mean (1)	Control Mean (2)	Difference: (1)–(2)	P-value
AGE	2.65	2.67	−0.02	0.83
SIZE	6.11	6.06	0.05	0.70
MARKET-TO-BOOK	1.35	1.21	0.14	0.09
LEVERAGE	0.42	0.34	0.08	<0.01
INST	0.51	0.32	0.19	<0.01
ANALYST	3.70	2.92	0.78	0.09
ALTz	1.60	1.58	0.02	0.89
EBITDA	0.03	0.03	0.00	0.17
PAYOUT	0.02	0.01	0.01	0.02
ACCRUALS	−0.03	−0.05	0.02	<0.01

Panel D: Bond Trade Frequency at Issuer-Quarter Level (2002:Q3–2006:Q4)

Phases	Mean (Median)
All Phases	488 (60)
Phase I	1404 (342)
Phase II	241 (57)
Phase IIIA	110 (36)
Phase IIIB	75 (26)

The table reports descriptive statistics. The empirical analysis is conducted at the loan-package level. The sample period is 1997:Q1–2006:Q4. See Appendix B for variable definitions.

TRACE implementation. Consistent with the implementation of TRACE (whereby more actively traded bonds entered TRACE earlier), we see a monotonic decrease in trading frequency.

5.2. Main results

Table 2 reports our main results. Column (1) reports results for the baseline model of our difference-in-differences regression in Equation (1), using *logTOTCOV* as the dependent variable and with the right-hand-side of the regression including our variable of interest (*TRACE*), firm rating fixed effects, industry fixed effects, and year-quarter fixed effects, but without any time-varying controls. We find a statistically and economically significant decrease in loan covenants for the treatment firms relative to the control firms following the implementation of TRACE. The estimated coefficient on *TRACE* is −0.1470 (*t-statistic* = -4.44), suggesting that on average, loan contracts include approximately 14% ($=(\exp^{-0.1470}-1)\times 100$) fewer covenants for treatment firms relative to control firms following the implementation of TRACE.

Column (2) includes time-varying loan-level and firm-level control variables. The estimated coefficient on *TRACE* is −0.1322 (*t-statistic* = −4.52), suggesting that on average, loan contracts include approximately 12% ($=(\exp^{-0.1322}-1)\times 100$) fewer covenants for treatment firms relative to control firms following the implementation of TRACE. The economic magnitude of the coefficient estimate on *TRACE* in Column (2) is not materially different from the estimate in Column (1). This is reassuring because it suggests that our treatment effect estimate is robust to the inclusion of control variables, which mitigates concerns about omitted variable biases. The signs of estimated coefficients on control variables are consistent with prior studies (e.g., Bradley and Roberts, 2015; Demerjian, 2017). For example, loans with higher spread and performance pricing provisions as well as loans requiring collaterals are associated with greater covenant intensity. Further, borrowers with a smaller size, higher leverage, higher institutional stock ownership, stronger financial performance, and lower dividend payouts have more covenants.

Table 2
The effect of bond market transparency enhancements on bank loan covenants.

VARIABLES	(1)	(2)	(3)	(4)
	logTOTCOV	logTOTCOV	logFINCOV	logSWEEP
TRACE	*−0.1470***	*−0.1322***	*−0.0569***	*−0.1384****
	(−4.44)	*(−4.52)*	*(−2.40)*	*(−3.44)*
LOANAMOUNT		0.0088	−0.0352**	0.0531**
		(0.55)	(−2.40)	(2.37)
MATURITY		0.0209	0.0383	0.0462
		(0.75)	(1.47)	(1.19)
SPREAD		0.0016***	0.0008***	0.0021***
		(9.57)	(4.85)	(9.19)
SECURED		0.1875***	0.0568*	0.2760***
		(5.29)	(1.81)	(5.72)
PPP		0.2550***	0.2628***	0.2097***
		(8.09)	(8.50)	(4.87)
AGE		0.0492***	0.0287**	0.0509**
		(3.28)	(2.01)	(2.48)
SIZE		−0.0581***	−0.0425***	−0.0362*
		(−4.10)	(−3.38)	(−1.73)
MARKET-TO-BOOK		0.0052	−0.0375**	0.0941***
		(0.23)	(−2.06)	(2.92)
LEVERAGE		0.1255	0.0236	0.1378
		(1.45)	(0.30)	(1.17)
INST		0.0858**	0.0772**	0.0705
		(2.49)	(2.41)	(1.48)
ANALYST		0.0003	−0.0003	0.0006
		(0.31)	(−0.36)	(0.39)
ALTz		0.0103	0.0246*	−0.0476**
		(0.74)	(1.91)	(−2.38)
EBITDA		1.2240**	1.1380*	0.3444
		(2.19)	(1.96)	(0.42)
PAYOUT		−1.0278***	−1.0342***	−0.5167
		(−3.18)	(−4.15)	(−1.15)
ACCRUALS		0.2394	−0.0575	0.3479
		(1.45)	(−0.39)	(1.53)
Observations	2,638	2,638	2,638	2,638
Adjusted R-squared	0.432	0.521	0.353	0.462
Firm Rating, Ind, & Year-Qtr FE	Yes	Yes	Yes	Yes

This table reports the effect of bond market transparency enhancements on loan covenant intensity. *logTOTCOV* is the natural log of the total number of financial and sweep covenants in the loan package. *logFINCOV (logSWEEP)* is the natural log of one plus the number of financial (sweep) covenants. *TRACE* equals one when (and after) an issuer has at least one outstanding bond included for dissemination in the TRACE system, and zero otherwise. The analysis is conducted at the loan-package level. The sample period is 1997:Q1−2006:Q4. Test statistics (two-sided) based on robust standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. See Appendix B for variable definitions.

In Columns (3) and (4), we separately estimate regressions for *logFINCOV* and *logSWEEP*, respectively. The estimated coefficients on *TRACE* are −0.0569 (*t-statistic* = −2.40) in Column (3) and −0.1384 (*t-statistic* = −3.44) in Column (4), suggesting that loan contracts include approximately 6% fewer financial covenants and 13% fewer non-financial sweep covenants following the implementation of TRACE. Thus, transparency enhancements in the secondary bond market affect both financial and non-financial covenants.

The magnitude of the treatment effect is economically meaningful when compared to those reported in prior studies (e.g., Chy et al., 2021; Costello and Wittenberg-Moerman, 2011; Demerjian, 2017; Hollander and Verriest, 2016). For example, Demerjian (2017) reports one standard deviation increase in his proxy for uncertainty is associated with a 3.1% increase in financial covenant intensity. Chy et al. (2021) report 9% more financial covenants for their treatment group of borrowers following an increase in auditor litigation risk. Costello and Wittenberg-Moerman (2011) report that firms with material internal control weaknesses have 22% fewer financial covenants in their loan contracts than firms without material internal control weaknesses. Thus, our estimated economic magnitude falls within the range of estimated effects reported in prior studies.

Table 3
Cross-sectional variations in the treatment effect.

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	logTOTCOV	logTOTCOV	logTOTCOV	logTOTCOV	logTOTCOV	logTOTCOV	logTOTCOV
TRACE × High AGGREGATE TRADE	*−0.1488*** (−2.60)*						
TRACE × High PRICE IMPACT		*−0.1204** (−2.13)*					
TRACE × High STOCK DISP			*−0.0938** (−2.15)*				
TRACE × High ANALYST DISP				*−0.0879** (−1.99)*			
TRACE × High BID-ASK					*−0.0911* (−1.83)*		
TRACE × High MARKET-TO-BOOK						*−0.0985** (−2.12)*	
TRACE × High MATURITY							*−0.0941** (−1.98)*
TRACE	−0.0278 (−0.57)	−0.0474 (−0.96)	−0.0916*** (−2.66)	−0.0936*** (−2.60)	−0.0651 (−1.41)	−0.0769* (−1.86)	−0.0912** (−2.57)
High AGGREGATE TRADE	0.1005** (2.22)						
High PRICE IMPACT		0.0747* (1.66)					
High STOCK DISP			0.0409* (1.77)				
High ANALYST DISP				−0.0037 (−0.15)			
High BID-ASK					0.0588** (2.12)		
High MARKET-TO-BOOK						0.0609** (2.30)	
High MATURITY							0.0617** (2.40)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2638	2,638	2,638	2,638	2,638	2,638	2,638
Adjusted R-squared	0.521	0.521	0.521	0.517	0.533	0.522	0.522
Firm Rating, Ind, & Year-Qtr FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table reports results from cross-sectional tests. *logTOTCOV* is the natural log of the total number of financial and sweep covenants in the loan package. *TRACE* equals one when (and after) an issuer has at least one outstanding bond included for dissemination in the TRACE system, and zero otherwise. *High AGGREGATE TRADE* equals one if the firm-level aggregate trade size in a quarter is higher than the median TRACE phase-quarter aggregate trade size, and zero otherwise. *High PRICE IMPACT* equals one if the firm-level aggregate *PRICE IMPACT* in a quarter is greater than the median TRACE phase-quarter aggregate *PRICE IMPACT*, and zero otherwise. *High STOCK DISP* equals one if the borrowing firm's stock price dispersion is above the industry median, and zero otherwise. *High ANALYST DISP* equals one if the borrowing firm's analyst earnings forecast dispersion is above the industry median, and zero otherwise. *High BID-ASK* equals one if the borrowing firm's stock price bid-ask spread is above the industry median, and zero otherwise. *High MARKET-TO-BOOK* equals one if the borrowing firm's *MARKET-TO-BOOK* is above the industry median, and zero otherwise. *High MATURITY* equals one if the loan's maturity is above the industry median, and zero otherwise. Control variables are included in each column but omitted from the table for brevity. The analysis is conducted at the loan-package level. The sample period is 1997:Q1−2006:Q4. Test statistics (two-sided) based on robust standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. See Appendix B for variable definitions.

5.3. Cross-sectional tests

We conduct several cross-sectional tests to sharpen the study's inferences. These cross-sectional tests are broadly divided into three categories. In the first category, we focus on the informativeness of bond prices. If our main findings are indeed driven by a reduction in lenders' information risk following the TRACE implementation, the effect is likely to be greater when borrowers' bond prices are more informative. We construct two indicator variables to capture bond price informativeness. *High AGGREGATE TRADE* equals one if the firm-level aggregate trade size in a quarter is higher than the median TRACE phase-quarter aggregate trade size, and zero otherwise. *High PRICE IMPACT* equals one if a firm's quarterly aggregate *PRICE IMPACT* is higher than the median TRACE phase-quarter aggregate *PRICE IMPACT*, and zero otherwise.[14] Table 3 reports the results. Columns (1) and (2) show that the treatment effect is stronger when bond trade size is greater and price impact is higher, respectively. These findings suggest that the effect of TRACE implementation on loan contracting is likely to be due to information contained in bond prices.

In the second category, we focus on the informational role of the bond market relative to the stock market. When borrowers' information asymmetry and uncertainty as reflected in the stock prices are higher, the incremental informational role of the bond prices is likely to be more pronounced. We construct three indicator variables to capture the informational role of stock prices. *High STOCK DISP* equals one if the borrowing firm's quarterly stock price dispersion is greater than the industry median, and zero otherwise. *High ANALYST DISP* equals one if the borrowing firm's equity market analyst earnings forecast dispersion is greater than the industry median, and zero otherwise. The intuition is that when prices are more volatile or analyst forecast dispersion is greater, the uncertainty about firm fundamentals is likely to be higher. *High BID-ASK* equals one if the borrowing firm's equity market bid-ask spread is greater than the industry median, and zero otherwise. When the bid-ask spread is higher, speculators are less likely to trade because trading is costlier. Consequently, a greater bid-ask spread reduces the informativeness of stock prices. In Columns (3) to (5) of Table 3, respectively, we find that the treatment effect is stronger when stock price dispersion, analyst forecast dispersion, and bid-ask spread are higher.

In the third category of tests, we focus on two firm- and loan-level variables that capture the potential for future debt-equity agency conflicts. Prior studies suggest that the potential for debt-equity agency conflicts is greater when information asymmetry is higher (e.g., Krishnaswami et al., 1999; Petacchi, 2015). Thus, we expect TRACE implementation to have a more pronounced effect on the use of debt covenants when the likelihood of future debt-equity agency conflicts is higher. The first proxy for debt-equity agency conflict is the market-to-book assets ratio, which captures the availability of growth options. Firms with higher growth options are more likely to engage in future risk-shifting actions and more prone to the debt-overhang problem (Billett et al., 2007; Bradley and Roberts, 2015). *High MARKET-TO-BOOK* is an indicator variable that equals one if a borrowing firm's market-to-book ratio is above the industry median, and zero otherwise. The second proxy is loan maturity. Longer maturity leads to greater future uncertainty and moral hazard (Barnea et al., 1980; Brockman et al., 2010). *High MATURITY* is an indicator variable that equals one if a loan's maturity is greater than the industry median, and zero otherwise. Columns (6) and (7) of Table 3 show that the treatment effect is more pronounced for firms that have higher growth options and for loans with a longer maturity, respectively.

6. Robustness tests

6.1. Treatment vs. control sample differences

Firms in our treatment sample borrow from both the public and the private debt markets whereas our control firms only borrow from the private debt market. The difference between the treated and control firms' debt market choices may reflect systematic differences between the two groups, which could affect our inferences (see, e.g., Bharath et al., 2008; Demerjian, 2019; Denis and Mihov, 2003). To address this concern, we conduct the following additional analyses.

6.1.1. Parallel trends and dynamic treatment effects

An important concern with the DiD estimation is that loan covenant trends for treatment and control groups could be different even before the implementation of TRACE, meaning that the parallel trend assumption underlying a DiD estimation is not satisfied. We follow prior studies (e.g., Armstrong et al., 2012; Bertrand and Mullainathan, 2003; Chy et al., 2021) to provide suggestive evidence that the assumption is likely to be satisfied. We construct four indicator variables to capture the differential trends in the use of covenants between the treatment and control firms prior to the implementation of TRACE. $TRACE^{(t=-4)}$, $TRACE^{(t=-3)}$, $TRACE^{(t=-2)}$, and $TRACE^{(t=-1)}$ are indicator variables that equal one if a treatment firm will enter the TRACE platform four, three, two, and one quarter(s) after the observation quarter, respectively, and zero otherwise. Following prior studies (e.g., Armstrong et al., 2012; Bertrand and Mullainathan, 2003), we also construct four indicator variables to capture the post-treatment dynamic effects of TRACE implementation. $TRACE^{(t=1)}$, $TRACE^{(t=2)}$, and $TRACE^{(t=3)}$ are indicator variables that equal one if a treatment firm is in its first, second, and third quarter after entering the TRACE platform, respectively, and zero otherwise. $TRACE^{(t>4)}$ equals one for a treatment firm in the fourth quarter or later after

[14] For this measure, *PRICE IMPACT* equals zero if a trade does not cause any change in price, one if the last price changes, two if either the low or high price changes, three if both the last and low prices or last and high prices change, four if both the high and low prices change, and five if all prices change.

entering the TRACE platform, and zero otherwise. Note that for the control firms, the four indicator variables capturing pre-treatment trends and the four capturing post-treatment effects all remain zero throughout the sample period.

In Column (1) of Table 4, we regress the dependent variable *logTOTCOV* on the four pre-treatment and four post-treatment indicator variables, as well on the time-varying control variables and fixed effects. None of the coefficient estimates on the four pre-treatment indicator variables, $TRACE^{(t=-4)}$, $TRACE^{(t=-3)}$, $TRACE^{(t=-2)}$, and $TRACE^{(t=-1)}$, are statistically significant. Consistent with the main results, the estimated coefficients on post-treatment indicator variables are all negative and statistically significant. This evidence suggests that the treatment effect we document manifests only after a firm enters the TRACE platform, and there are no statistically significant differences between the treatment and control firms' loan covenant intensity prior to the implementation of TRACE.

To mitigate the concern that the inference from the parallel trend test in Column (1) of Table 4 may be distorted by the inclusion of post-treatment indicator variables ($TRACE^{(t=1)}$, $TRACE^{(t=2)}$, $TRACE^{(t=3)}$, and $TRACE^{(t>4)}$) in the regression, we conduct an alternative test of pre-treatment trends in Column (2) of Table 4. Specifically, we exclude the post-treatment observations (and hence also the post-treatment indicator variables) to remove the post-treatment effects of TRACE implementation. Similarly, we also limit the sample window for the control group to the pre-TRACE period (i.e., 1997:Q1 - 2002:Q2). Because there are no loan-package observations that receive treatment in this sample, the tests of pre-trends cannot be influenced by the post-treatment effects of TRACE implementation. Column (2) shows that the coefficient estimates on the four pre-treatment indicator variables are not statistically significant, suggesting that covenant intensity trends in the pre-TRACE periods are not significantly different between the treatment and control firms.

6.1.2. Results based on treatment sample only

Because the implementation of TRACE was staggered in multiple phases, treatment firms that have not yet been treated at time *t* can serve as the control group for firms receiving treatment at time *t*. This feature of our research setting allows us to estimate the effect of TRACE implementation on covenant intensity without using a separate control sample. As Column (1) of *Panel A*, Table 5 shows, our key inference from Table 2 continues to hold. Because our key inference holds in the treatment-group-only sample (i.e., without using a separate control group of firms), the choice of a particular control group is unlikely to drive our main results.

Table 4
Test of a parallel trend.

VARIABLES	(1)	(2)
	logTotCOV	logTotCOV
$TRACE^{(t=-4)}$	0.0004	−0.0150
	(0.01)	(−0.19)
$TRACE^{(t=-3)}$	0.0047	0.0425
	(0.07)	(0.58)
$TRACE^{(t=-2)}$	−0.0441	−0.0405
	(−0.84)	(−0.56)
$TRACE^{(t=-1)}$	−0.0239	−0.0188
	(−0.33)	(−0.20)
$TRACE^{(t=1)}$	−0.2091***	
	(−3.01)	
$TRACE^{(t=2)}$	−0.1455**	
	(−2.19)	
$TRACE^{(t=3)}$	−0.0952*	
	(−1.89)	
$TRACE^{(t>4)}$	−0.1527***	
	(−3.81)	
Observations	2,638	1,634
Adjusted R-squared	0.534	0.464
Controls	Yes	Yes
Firm Rating, Ind, & Year-Qtr FE	Yes	Yes

This table reports the results of parallel trend tests. $TRACE^{(t=-4)}$, $TRACE^{(t=-3)}$, $TRACE^{(t=-2)}$, and $TRACE^{(t=-1)}$ are indicator variables that equal one if the treatment firm will enter the TRACE platform in four, three, two, and one quarter(s) from the observation quarter, respectively, and zero otherwise. $TRACE^{(t=1)}$, $TRACE^{(t=2)}$, and $TRACE^{(t=3)}$ are indicator variables that equal one if the treatment firm is in its first, second, and third quarter after entering the TRACE platform, respectively, and zero otherwise. $TRACE^{(t>4)}$ equals one if the treatment firm is in the fourth or a later quarter after entering the TRACE platform, and zero otherwise. *logTOTCOV* is the natural log of the total number of financial and sweep covenants in the loan package. The analysis is conducted at the loan-package level. The sample period is 1997:Q1−2006:Q4 in Column (1). Column (2) excludes post-TRACE treatment and control loan packages from the sample. Test statistics (two-sided) based on robust standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. See Appendix B for variable definitions.

6.1.3. Borrower fixed effects

The choice of whether to access the public or private debt market could reflect time-invariant differences between the treated and control firms that industry and rating fixed effects may not adequately capture. As such, in Column (2) of *Panel A*, Table 5, we control for borrower fixed effects and we find that our key inference continues to hold.[15] Because we only exploit within-firm variations under the borrower fixed effects specification, this specification allows us to mitigate the concern that time-invariant differences between the treatment and control groups drive our results.[16]

6.1.4. Propensity score matching

We also use the same control variables as in our main regression as observable firm characteristics to implement 1:2 propensity score matching with replacement in the 2nd quarter of 2002 (the quarter before TRACE implementation). The differences between the treated and control firms in terms of the matched characteristics and the loan characteristics after matching are not statistically significant at conventional levels (untabulated). In *Panel A* of Table 5, Column (3), we find that our main inference holds using the matched sample.[17]

We recognize that each robustness test in this section has its own limitations. For example, time-varying changes affect firms' capital structures and the types of debt markets they access. Controlling for time-invariant changes by means of borrower fixed effects does not account for any unobservable time-varying changes influencing firms' access to public and

Table 5
Robustness tests.

Panel A: Alternative Measures, Samples, and Specifications

VARIABLES	(1)	(2)	(3)	(4)	(5)
	logTOTCOV	logTOTCOV	logTOTCOV	TOTCOV	logCOVALT
TRACE	*−0.1142***	*−0.0758***	*−0.1041***	*−0.2166***	*−0.1305***
	(−2.98)	*(−2.14)*	*(−3.57)*	*(−5.06)*	*(−3.73)*
Control Variables	Yes	Yes	Yes	No	Yes
Observations	1,540	2,270	3,097	2,638	2,638
Adj. (pseudo) R-sq	0.550	0.620	0.450	0.140	0.553
Firm Rating FE	Yes	Yes	Yes	No	Yes
Ind FE	Yes	No	Yes	Yes	Yes
Year-Qtr FE	Yes	Yes	Yes	Yes	Yes
Borrower FE	No	Yes	No	No	No

Panel B: Phase-specific Effects

VARIABLES	(1)	(2)	(3)	(4)	(5)
	logTOTCOV	logTOTCOV	logTOTCOV	logTOTCOV	logTOTCOV
TRACE I	*−0.269***				*−0.257***
	(-5.137)				*(-5.891)*
TRACE II		*−0.153***			*−0.141***
		(−3.174)			*(−3.095)*
TRACE IIIA			*−0.118**		*−0.108**
			(−1.854)		*(−1.962)*
TRACE IIIB				*−0.048*	*−0.019*
				(−0.834)	*(−0.389)*
Observations	1,425	1,585	1,406	1,516	2,638
Adjusted R-squared	0.539	0.482	0.429	0.428	0.493
Control Variables	Yes	Yes	Yes	Yes	Yes
Firm Rating, Ind, & Year-Qtr FE	Yes	Yes	Yes	Yes	Yes

This table reports a battery of robustness tests. *logTOTCOV* is the natural log of the total number of financial and sweep covenants in the loan package. *TOTCOV* is the total number of financial and sweep covenants in the loan package. *logCOVALT* is the natural log of *COVALT*, which equals the total of financial covenants, sweep covenants, and an indicator for dividend restrictions in the loan package. In *Panel A*, *TRACE* equals one when (and after) an issuer has at least one outstanding bond included for dissemination in the TRACE system, and zero otherwise. Column (1) reports results based on treatment firms only. Column (2) controls for borrower fixed effects. Column (3) reports results based on a propensity-score matched sample. Column (4) employs negative binomial regression using *TOTCOV* as the dependent variable. Column (5) uses an alternative definition of covenant intensity, *logCOVALT*, as the dependent variable. In *Panel B*, *TRACE I*, *TRACE II*, *TRACE IIIA*, and *TRACE IIIB* are indicator variables that equal one when (and after) an issuer has at least one outstanding bond included for dissemination in Phase I, II, IIIA, and IIIB of the TRACE platform, respectively, and zero otherwise. Control variables are included in all columns of both panels but omitted from the table for brevity. The analysis is conducted at the loan-package level. The sample period is 1997:Q1−2006:Q4. Test statistics (two-sided) based on robust standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. See Appendix B for variable definitions.

[15] We lose 368 (14% of the full sample) loan package observations because some firms have no within-firm variations in the sample.

[16] In untabulated analyses, we examine the robustness of other results (e.g., cross-sectional analyses in Table 3 and parallel trend tests in Table 4) to the inclusion of borrower fixed effects. The tenor of the results is similar to those reported in the study. However, because there are fewer within-borrower firm variations to exploit, the economic magnitudes and statistical significance are weaker.

[17] To include rating fixed effects in the regression, we assign firms without a rating to a "not rated" category.

private debt markets (Demerjian, 2019). Similarly, although propensity score matching is a useful technique, it only allows us to match on observable characteristics. As such, it cannot fully address concerns about sample selection that may arise from unobservable differences between the treatment and control groups. However, although each robustness test in this section has its own limitations, we believe that the collective evidence from all the tests mitigates the concern that pre-existing differences between the treatment and control groups drive our results.

6.2. Other robustness tests

6.2.1. Alternative estimation approach: negative binomial

As discussed in Section 4.3, we use the OLS regression in our main analyses. To examine the sensitivity of our inference to the choice of estimation function, we also use a negative binomial regression with the total number of covenants (*TOTCOV*) as the dependent variable. In Table 5, *Panel A*, Column (4), we find consistent evidence that covenant intensity significantly decreases following the implementation of TRACE.[18]

6.2.2. Alternative measure of covenant intensity

Our main measure of covenant intensity does not include dividend restrictions.[19] However, prior studies suggest that dividend restrictions can address dividend payout and underinvestment problems (Myers, 1977; Smith and Warner, 1979). As a robustness check, in Table 5, *Panel A*, Column (5), we alternatively define our covenant intensity measure by including dividend restrictions. Specifically, we define *logCOVALT* as the sum of financial covenants, sweeps, and an indicator variable for dividend restrictions. Using this alternative measure, we continue to observe a negative effect of TRACE implementation on covenant intensity. Thus, our key inference is not affected by the inclusion of dividend restrictions in covenant measures.

6.2.3. Treatment effect biases in generalized difference-in-differences specifications

Recent studies suggest that the use of earlier-treated firms as a control sample for later-treated firms (and vice-versa) in generalized DiD specifications may introduce biases in treatment effect estimates (Athey and Imbens, 2022; Baker et al., 2022; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021). Several features of our research design, however, mitigate concerns about such biases. For instance, biases in generalized DiD specifications are greater when there are more variations in treatment timing across cohorts (Baker et al., 2022). The implementation of TRACE was completed in four phases within a short three-year period between July 2002 and February 2005. As such, our inferences are less likely to be affected by biases due to variations in treatment timing across cohorts. Further, as Baker et al. (2022) show, generalized DiD treatment effect biases are more severe when all firms are eventually treated and no *never-treated* control firms exist in the sample. In our study, we use firms with S&P credit ratings as never-treated control firms, mitigating concerns about biases resulting from the absence of never-treated control firms. Finally, biases in generalized DiD specifications are higher if long-run treatment effects persist (Goodman-Bacon, 2021). We use a short window for the post-treatment period (2002:Q2−2006:Q4) in our setting, which mitigates concerns about long-run effects contaminating the treatment effect.

We conduct three additional tests to further mitigate concerns about treatment effect biases in our generalized DiD setting. First, to remove long-run effects, we restrict the sample window for treatment firms from event-quarter −4 to event-quarter +4. Similarly, for the control group, we limit the sample period from four quarters before the Phase I implementation until four quarters after the Phase IIIB implementation. Untabulated analysis shows that the key inference holds, implying that dynamic long-run effects do not contaminate our treatment effect estimate. Second, following Baker et al.'s (2022) recommendation, we estimate the effect for each phase separately in Table 5, *Panel B*. Specifically, we compare the covenant intensity of loans issued to firms in each of Phases I, II, IIIA, and IIIB with loans issued to never-treated control firms.[20] In contrast to Baker et al.'s (2022) simulation results, we do not observe phase-specific treatment effects with signs opposite to that of the aggregate treatment effect (see Table 2) for the whole sample, mitigating concerns about biases in the treatment effect estimate.[21] Third, as recommended by Baker et al. (2022), we conduct stacked regression analyses. Specifically, we construct stacks 1, 2, 3, and 4 that contain the treated loan-package observations for Phase I, II, IIIA, and IIIB, respectively. As a clean control sample, we include in each stack the sample of never-treated loan-package observations. Untabulated analyses

[18] The negative binomial estimation does not converge with rating fixed effects, so we drop rating fixed effects from the regression.

[19] We exclude dividend restrictions from our main measure of covenant intensity for two reasons. First, equity issuance and shareholder payout are sensitive to information asymmetry (see, e.g., Miller and Rock, 1985; Hail et al., 2014). Consistent with this argument, Chy (2022) finds that firms increase equity issuance and reduce shareholder payouts following the implementation of TRACE. Thus, because payout restrictions already become less binding after TRACE implementation, borrowers may not find it optimal to bargain for a reduction in dividend restrictions. Second, Bradley and Roberts (2015) suggest that there is very little loan-level variation in the incidence of dividend covenants in loan contracts. As a result, adding an indicator variable that proxies for the incidence of loan-level dividend restrictions to the main measure of loan covenants may reduce the precision of our treatment effect estimate.

[20] Firms with multiple bonds in different phases are considered to be treated in the earliest phase.

[21] Because the earlier phases of TRACE include bonds that are more actively traded (hence, more transactions reported in the TRACE system), the gradual attenuation of the treatment effect from earlier to later phases shown in Table 5, *Panel B* is consistent with our key argument that information reflected in bond prices reduces banks' information risk in loan contracting.

show that the key inference remains unaffected. Overall, the above analyses collectively suggest that our key inference is unlikely to be affected by generalized DiD treatment effect biases.

7. Additional analyses

7.1. Separate tests by rating groups

In Table 6, instead of using issuer rating fixed effects, we run separate regressions for different rating groups to provide additional insights. In Columns (1) and (2), we find that TRACE loads negatively for both investment and non-investment grade issuers. In an untabulated test, we find that the coefficient estimates for the two groups are not statistically significantly different. To further explore the potential heterogeneous effects among rating categories, we categorize the ratings into four broad categories: A and Above (Upper Echelon of Investment Grade); BBB (Lower Echelon of Investment Grade); BB (Upper Echelon of Non-investment Grade); and B and below (Lower Echelon of Non-investment Grade). These broad rating categories include all incremental ratings. As reported in Column (3), we do not find any statistically or economically significant effect for the upper echelon of the investment-grade group. The evidence is consistent with the notion that the upper echelon of investment-grade issuers presents low default risk and has low information asymmetry. In Column (4), we observe a strong negative effect for the lower echelon of the investment-grade issuers. Although these issuers are investment grade, they bear a higher risk of being relegated to non-investment-grade ratings, which will significantly affect their credit risk and financing, investment, and cash flow uncertainties. Further, these firms may engage in opportunistic activities to maintain their investment-grade ratings (e.g., Brown et al., 2015; Jung et al., 2013; Kisgen, 2006), which increases agency conflicts. As such, with improved bond market transparency in the post-TRACE regime, banks may be able to mitigate information asymmetry about these firms by observing bond prices. Column (5) shows the results for the upper echelon of non-investment-grade issuers, and Column (6) shows the results for the lower echelon of non-investment-grade issuers. We find that both groups experience a reduction in debt covenants following the implementation of TRACE. Overall, the evidence from these rating-level regressions is consistent with the prediction that banks rely more on bond price signals when information asymmetry and agency conflicts are likely to be more pronounced.

7.2. Financial covenants: performance vs. capital covenants

Financial covenants can be of two types: performance covenants and capital covenants. Prior studies (see, e.g., Christensen and Nikolaev, 2012) suggest that performance covenants are more sensitive to information risk than capital covenants. As such, if TRACE implementation reduces information risk for lenders, then we expect the effect on financial covenants to be more pronounced for performance covenants. Table 7 reports that the effect of TRACE is concentrated among performance covenants (Column (1)) and is absent for capital covenants (Column (2)). We also note that the evidence in Table 7 and our results on sweep covenants in Table 2 are consistent with Christensen and Nikolaev's (2012) intuition that performance covenants (but not capital covenants) and sweep covenants are used complementarily in loan contracts.

7.3. Negative covenants: hand-collected data from original loan contracts

Although DealScan is a sufficient source of data on financial covenants, its coverage of non-financial negative covenants is less comprehensive. As such, to corroborate our main findings from negative covenants, we hand-collect negative covenant

Table 6
Separate tests by rating groups.

VARIABLES	Investment vs. Non-investment Grade		Separate Tests for Different Rating Groups			
	Investment	Non-investment	A and above	BBB	BB	B and below
	(1)	(2)	(3)	(4)	(5)	(6)
	logTOTCOV	logTOTCOV	logTOTCOV	logTOTCOV	logTOTCOV	logTOTCOV
TRACE	*−0.0934***	*−0.1312***	*−0.0146*	*−0.1249***	*−0.1141**	*−0.1305***
	(−2.66)	*(−3.14)*	*(−0.25)*	*(−2.81)*	*(−1.92)*	*(−2.11)*
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,071	1,567	332	739	780	787
Adjusted R-squared	0.334	0.317	0.460	0.311	0.312	0.320
Ind FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Qtr FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports separate results for different rating types. *logTOTCOV* is the natural log of the total number of financial and sweep covenants in the loan package. *TRACE* equals one when (and after) an issuer has at least one outstanding bond included for dissemination in the TRACE system, and zero otherwise. Control variables are included in each column but omitted from the table for brevity. The analysis is conducted at the loan-package level. The sample period is 1997:Q1−2006:Q4. Test statistics (two-sided) based on robust standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. See Appendix B for variable definitions.

data from Nini et al.'s (2009) EDGAR private loan contract files.[22] We carefully read through the loan contract files to hand collect both affirmative and negative covenants. When contracts do not classify covenants into affirmative and negative categories, we do so manually. To enrich the analysis, we further classify the negative covenants into 12 sub-categories: 1) indebtedness; 2) investments, loans, and advances; 3) transactions with affiliates; 4) mergers, acquisitions, and consolidations; 5) sales of assets; 6) liens; 7) negative pledges; 8) employee benefits related; 9) changes in business and accounting; 10) amendment and modification; 11) subsidiary related; 12) others. We construct indicator variables for each of these 12 negative covenant categories. For example, *Indebtedness* is an indicator variable that equals one if a loan contract contains at least one negative covenant restricting borrower indebtedness, and zero otherwise.

Table 8, *Panel A*, presents descriptive statistics for these hand-collected negative covenants.[23] On average, a loan contract contains 7.6 negative covenant sub-categories. Table 8, *Panel B*, presents the results based on the EDGAR negative covenant

Table 7
Financial Covenants—Performance vs. Capital Covenants.

VARIABLES	(1)	(2)
	logPERFCOV	logCAPCOV
TRACE	*−0.0898***	*−0.0054*
	(−3.01)	*(−0.16)*
LOANAMOUNT	−0.0007	−0.0147
	(−0.05)	(−0.81)
MATURITY	0.1139***	−0.0432
	(4.53)	(−1.39)
SPREAD	0.0007***	0.0000
	(5.13)	(0.00)
SECURED	0.1261***	−0.0252
	(4.71)	(−0.66)
PPP	0.2705***	0.1300***
	(9.89)	(4.21)
AGE	−0.0010	0.0138
	(−0.07)	(0.69)
SIZE	−0.0556***	−0.0483***
	(−4.20)	(−2.80)
MARKET-TO-BOOK	−0.0303	−0.1535***
	(−1.37)	(−5.02)
LEVERAGE	0.1434*	0.0595
	(1.84)	(0.59)
INST	0.0638**	0.0956**
	(2.00)	(2.23)
ANALYST	0.0001	−0.0004
	(0.07)	(−0.35)
ALTz	0.0022	0.1146***
	(0.14)	(5.60)
EBITDA	2.0831***	0.1889
	(3.70)	(0.27)
PAYOUT	−0.9657***	−1.0889***
	(−3.41)	(−3.18)
ACCRUALS	−0.0000	0.1809
	(−0.00)	(1.02)
Observations	2,638	2,638
Adjusted R-squared	0.376	0.207
Firm Rating, Ind, & Year-Qtr FE	Yes	Yes

The table reports results for two types of financial covenants, income statement-based performance covenants and balance sheet-based capital covenants. *logPERFCOV (logCAPCOV)* is the natural log of one plus the number of performance (capital) covenants in the loan package. *TRACE* equals one when (and after) an issuer has at least one outstanding bond included for dissemination in the TRACE system, and zero otherwise. The analysis is conducted at the loan-package level. The sample period is 1997:Q1−2006:Q4. Test statistics (two-sided) based on robust standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. See Appendix B for variable definitions.

[22] We are grateful to the reviewer for suggesting this analysis. We also thank Professor Amir Sufi for making original loan contracts publicly available on his website: https://amirsufi.net/chronology.html.

[23] We note that (i) Nini et al.'s (2009) sample period is 1996−2005, whereas our sample period is 1997−2006. As such, our analysis based on EDGAR's negative covenants does not include data for 2006, (ii) Nini et al. (2009) match 40% of loan contracts in DealScan to loan contracts from the SEC's EDGAR. Consistent with Nini et al. (2009), 41.62% of the 2638 DealScan loan contracts included in our main sample are successfully matched to actual loan contracts to yield a sample of 1098 original loan agreements, and (iii) Because DealScan has good coverage of financial and dividend covenants and Nini et al. (2009) hand-collect capital expenditure covenants, we do not collect these covenants to reduce the cost of manual data collection. As such, our negative covenant intensity variable does not include dividend and capital expenditure covenants.

Table 8
Negative covenants—tests based on hand-collected data.

Panel A: Descriptive Statistics

Variables	N	Mean	Std. Dev.	Median
Total Negative Covenants	1,098	7.60	2.58	8
Indebtedness	1,098	0.85	0.36	1
Investments, Loans, and Advances	1,098	0.65	0.48	1
Transactions with Affiliates	1,098	0.78	0.42	1
Mergers, Acquisitions, Consolidations	1,098	0.75	0.43	1
Sales of Assets	1,098	0.84	0.37	1
Liens	1,098	0.88	0.33	1
Negative Pledges	1,098	0.28	0.45	0
Employee Benefits Related	1,098	0.34	0.47	0
Changes in Business and Accounting	1,098	0.85	0.36	1
Amendment and Modification	1,098	0.34	0.48	0
Subsidiary Related	1,098	0.44	0.50	0
Others	1,098	0.60	0.49	1

Panel B: The Effect of Bond Market Transparency on Affirmative and Negative Covenants

VARIABLES	(1)	(2)
	logNEGCOV	logAFFCOV
TRACE	*−0.0941****	*−0.0102*
	(−3.18)	*(−0.30)*
LOANAMOUNT	0.0065	−0.0007
	(0.44)	(−0.04)
MATURITY	0.0784***	0.0638**
	(2.94)	(2.04)
SPREAD	0.0002	0.0002
	(0.96)	(0.80)
SECURED	0.1070***	0.1143***
	(3.56)	(3.53)
PPP	−0.0015	0.0184
	(-0.06)	(0.55)
AGE	−0.0090	0.0112
	(−0.64)	(0.57)
SIZE	−0.0682***	−0.0695***
	(−4.75)	(−4.32)
MARKET-TO-BOOK	−0.0094	−0.0325
	(−0.46)	(−1.26)
LEVERAGE	0.0700	−0.0292
	(0.78)	(−0.28)
INST	0.0077	0.0827*
	(0.24)	(1.89)
ANALYST	0.0006	−0.0015
	(0.68)	(−1.28)
ALTz	0.0137	0.0319*
	(0.93)	(1.81)
EBITDA	0.5625	0.0992
	(1.21)	(0.16)
PAYOUT	−0.1419	0.0258
	(−0.39)	(0.06)
ACCRUALS	−0.3538**	−0.3216*
	(−2.34)	(−1.77)
Observations	1,098	1,098
Adjusted R-squared	0.489	0.295
Firm Rating, Ind, Year-Qtr FE	Yes	Yes

Panel C: Separate Tests for Different Types of Negative Covenants

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Indebtedness	Investments, Loans, and Advances	Transactions with Affiliates	Changes in Business and Accounting	Amendment and Modification	Subsidiary Related
TRACE	*−0.0733**	*−0.0886***	*−0.1065***	*−0.1239****	*−0.0731**	*−0.0931**
	(−1.88)	*(−2.26)*	*(−2.36)*	*(−3.10)*	*(−1.79)*	*(−1.73)*
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,098	1,098	1,098	1,098	1,098	1,098
Adj. R-squared	0.386	0.487	0.297	0.205	0.296	0.177
Firm Rating, Ind, Year-Qtr FE	Yes	Yes	Yes	Yes	Yes	Yes

VARIABLES	(7)	(8)	(9)	(10)	(11)	(12)
	Mergers, Acquisitions, Consolidations	Sales of Assets	Liens	Negative Pledges	Employee Benefits Related	Others
TRACE	*−0.0008*	*−0.0145*	*−0.0198*	*−0.0617*	*0.0080*	*0.0181*
	(−0.02)	*(−0.54)*	*(−0.57)*	*(−1.34)*	*(0.17)*	*(0.32)*
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,098	1,098	1,098	1,098	1,098	1,098
Adj. R-squared	0.173	0.133	0.070	0.129	0.156	0.250
Firm Rating, Ind, Year-Qtr FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports results based on hand-collected covenant data from original loan contracts. *logNEGCOV* is the natural log of one plus the number of negative covenants in the loan package from hand-collected data. *logAFFCOV* is the natural log of one plus the number of affirmative covenants in the loan package from hand-collected data. *TRACE* equals one when (and after) an issuer has at least one outstanding bond included for dissemination in the TRACE system, and zero otherwise. *Panel A* reports descriptive statistics for hand-collected negative covenants separately by type. *Panel B* reports regression results for negative and affirmative covenant intensity in Columns (1) and (2), respectively. *Panel C* reports regression results separately for each of the negative covenant categories. *Indebtedness* equals one if the loan contract contains at least one negative covenant restricting borrower indebtedness, and zero otherwise. *Investments, Loans, and Advances* equals one if the loan contract contains at least one negative covenant restricting borrower investment and/or loan and advances, and zero otherwise. *Transactions with Affiliates* equals one if the loan contract limits transactions with affiliates, and zero otherwise. *Mergers, Acquisitions, Consolidations* equals one if the loan contract limits a borrower's M&A-related activities, and zero otherwise. *Sale of Assets* equals one if the loan contract restricts a borrower's disposal of assets, and zero otherwise. *Liens* equals one if the loan contract contains at least one negative covenant related to a lien, and zero otherwise. *Negative Pledges* equals one if the loan contract contains at least one negative covenant related to a negative pledge, and zero otherwise. *Employee Benefits Related* equals one if the loan contract contains at least one negative covenant related to employee benefit plans, and zero otherwise. *Changes in Business and Accounting* equals one if the loan contract restricts the borrower's changes in business and/or accounting practices, and zero otherwise. *Amendment and Modification* equals one if the loan contract restricts any amendment and modification of certain documents and/or agreements, and zero otherwise. *Subsidiary Related* equals one if the loan contract contains at least one negative covenant related to the borrower's subsidiaries, and zero otherwise. *Others* equals one for inclusion of any other remaining negative covenant, and zero otherwise. Control variables are included in *Panel C* but omitted from the table for brevity. The analysis is conducted at the loan-package level. The sample period is 1997:Q1−2005:Q4. Test statistics (two-sided) based on standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. See Appendix B for variable definitions.

data. In Column (1), *logNEGCOV* is the logarithm of one plus the total number of negative covenant sub-categories. We find strong evidence that the implementation of TRACE leads to a statistically significant reduction in negative covenant intensity. Column (2) examines whether affirmative covenants react to bond market transparency enhancements. Affirmative covenants tend to use boilerplate wording (e.g., compliance with the law, maintenance of insurance, property, and records) and are likely to be less sensitive to changes in information risk. Consistent with this intuition, Column (2) shows that TRACE does not affect the use of affirmative covenants in debt contracts. *Panel C* presents results for each negative covenant sub-category. We find that the likelihood of including negative covenants in the categories of indebtedness (Column 1), investments, loans, and advances (Column 2), related party transactions (Column 3), changes in business and accounting methods (Column 4), amendments and modifications (Column 5), and subsidiary related (Column 6) decreases following the implementation of TRACE. We do not observe any statistically significant effect for the other six categories.

7.4. Renegotiations

If a more transparent bond market helps the contracting parties to better incorporate forward-looking information *ex-ante* in the initial contract, we expect a reduction in renegotiations that accommodate *ex-post* changes in borrowers' fundamentals. We construct a loan-level indicator variable, *AMEND*, that equals one if a loan is amended at least once following inception, and zero otherwise. Table 9, Column (1), shows that loans initiated after TRACE implementation are less likely to be amended than loans initiated before TRACE. We also examine whether TRACE implementation affects the likelihood of covenant amendments. We construct an indicator variable *COV AMEND* which equals one if the deal amendment description contains the words “covenant,” “definition,” “provision,” “ratio,” “permit,” “allow,” “waive,” “capex,” “sweep,” or “prepayment,” and zero otherwise.[24] Column (2) shows that loans initiated after TRACE implementation are less likely to have subsequent covenant amendments than loans initiated before TRACE implementation. Column (3) uses an indicator variable *SPREAD AMEND* that equals one if the loan's initial spread was subsequently renegotiated, and zero otherwise. Column (3) shows that loans initiated after TRACE implementation are less likely to have their spreads amended than loans initiated before TRACE implementation. Overall, we find that the implementation of TRACE reduces the likelihood of loan renegotiations. These findings further reinforce our argument that forward-looking information in bond prices reduces banks' information risk.

[24] Christensen and Nikolaev (2012) identify covenant amendments where the amendment description contains the words “covenant,” “definition,” or “provision.” We observe that, like these three words, the other words we include also describe covenant amendments. Therefore, we include these additional words in our definition of *COV AMEND*.

Table 9
The effect of bond market transparency enhancements on renegotiations.

VARIABLES	(1)	(2)	(3)
	AMEND	COV AMEND	SPREAD AMEND
TRACE	*−0.0612***	*−0.0597***	*−0.0438**
	(−1.97)	*(−2.00)*	*(−1.64)*
LOANAMOUNT	0.0005	−0.0110	−0.0089
	(0.04)	(−0.89)	(−0.80)
MATURITY	0.1323***	0.1069***	0.1024***
	(5.80)	(4.75)	(5.62)
SPREAD	−0.0001	−0.0001	0.0001
	(-0.44)	(-0.46)	(0.53)
SECURED	0.1063***	0.1098***	0.0508**
	(3.49)	(3.66)	(2.03)
PPP	0.0201	0.0207	−0.0272
	(0.79)	(0.84)	(−1.29)
AGE	−0.0146	−0.0120	−0.0049
	(−1.05)	(−0.88)	(−0.42)
SIZE	0.0220*	0.0258**	0.0226**
	(1.73)	(2.17)	(2.24)
MARKET-TO-BOOK	−0.0195	−0.0358*	−0.0013
	(−0.93)	(−1.86)	(−0.08)
LEVERAGE	0.0397	0.0562	0.0476
	(0.53)	(0.77)	(0.76)
INST	0.0773**	0.0949***	0.0494*
	(2.38)	(3.07)	(1.83)
ANALYST	−0.0018*	−0.0010	−0.0010
	(−1.82)	(−1.01)	(−1.33)
ALTz	0.0034	0.0087	−0.0033
	(0.25)	(0.65)	(−0.31)
EBITDA	−1.1369**	−0.8043	−1.2500***
	(−2.30)	(−1.64)	(−2.97)
PAYOUT	−0.1030	−0.0205	0.2366
	(−0.33)	(−0.07)	(0.85)
ACCRUALS	−0.0331	−0.0488	−0.0171
	(−0.20)	(−0.30)	(−0.12)
Observations	2,638	2,638	2,638
Adjusted R-squared	0.123	0.119	0.065
Firm Rating, Ind, & Year-Qtr FE	Yes	Yes	Yes

This table estimates the effect of bond market transparency enhancements on renegotiations. *AMEND* equals one if the loan is amended at least once following inception, and zero otherwise. *COV AMEND* equals one if the amendment description indicates that at least one loan covenant was amended following inception, and zero otherwise. *SPREAD AMEND* equals one if the initial spread was subsequently amended, and zero otherwise. *TRACE* equals one when (and after) an issuer has at least one outstanding bond included for dissemination in the TRACE system, and zero otherwise. The analysis is conducted at the loan package level. Test statistics (two-sided) based on firm-level clustered standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. See Appendix B for variable definitions.

7.5. Evidence from new bond issues

In our final set of analyses, we examine how bond market transparency enhancements affect bond contracting for new bond issues, using data from Mergent's Fixed Income Securities Database (FISD). After applying the same sample filters as used in the loan contract tests in the primary analysis, we obtain a treatment sample comprising 1,822 new bond issues for the sample period 1997:Q1–2006:Q4. Table 10 reports the results. *YIELDSPREAD* is the yield spread over treasury for new bond issues at the time of the offering. *logBONDRATING* is the logarithm of the numerically converted bond rating at the time of the offering. A lower rating implies better credit quality.[25] *logBONDCOV* is the logarithm of one plus the number of bond covenants at offering.[26] We include *ISSUESIZE* (logarithm of the offering amount), *MATURITY* (logarithm of years left to maturity), and *COUPON* (logarithm of the coupon rate) as bond-level controls. Column (1) shows that new bond issues are statistically significantly less costly following the implementation of TRACE. In Column (2), we find that new bond issues come with better credit ratings following the implementation of TRACE. Finally, Column (3) shows that bond market transparency also reduces the use of bond covenants. The evidence that bond market transparency enhancements affect bond contracting in the same direction that they affect private debt contracting corroborates this study's central argument.

[25] We drop issuer rating fixed effects from Column (2) because there are substantially fewer variations in bond issue ratings within the same issuer-level credit rating category.

[26] Bond covenant data are not available for some bond issues, and so there is a reduced number of observations for the bond covenant test in Column (3) of Table 10.

Table 10
The effect of bond market transparency enhancements on new bond issue terms.

VARIABLES	(1)	(2)	(3)
	YIELDSPREAD	logBONDRATING	logBONDCOV
TRACE	*−31.4463***	*−0.0993****	*−0.1063***
	(−2.35)	*(−3.29)*	*(−2.29)*
ISSUESIZE	−9.1754	−0.0166	0.0048
	(−1.21)	(−0.75)	(0.15)
MATURITY	−24.8899***	−0.0045	−0.0075
	(−2.94)	(−0.16)	(−0.23)
COUPON	77.4930***	0.0454***	0.0974***
	(9.41)	(3.08)	(3.38)
AGE	10.3084	−0.0724***	0.0373
	(1.42)	(−4.17)	(1.31)
SIZE	−5.6120	−0.0977***	−0.0765***
	(−0.98)	(−8.24)	(−3.52)
MARKET-TO-BOOK	−35.8213***	−0.0491	−0.1043**
	(−3.19)	(−1.48)	(−2.10)
LEVERAGE	87.5276*	0.4819***	0.0947
	(1.84)	(3.43)	(0.59)
INST	−99.3965***	0.0672	−0.1265*
	(−4.94)	(1.28)	(−1.65)
ANALYST	0.0785	0.0002	−0.0002
	(0.24)	(0.23)	(−0.14)
ALTz	10.3960	−0.0569**	0.0461*
	(1.45)	(−2.06)	(1.73)
EBITDA	−406.3210	−1.2176**	1.7312
	(−1.47)	(−2.32)	(1.20)
PAYOUT	14.8800	−1.4143***	0.4036
	(0.11)	(−3.49)	(0.72)
ACCRUALS	−18.9540	0.2398	−0.1010
	(−0.23)	(1.43)	(−0.41)
Observations	1,822	1,822	1,229
Adjusted R-squared	0.651	0.693	0.546
Firm Rating FE	Yes	No	Yes
Ind & Year-Qtr FE	Yes	Yes	Yes

This table estimates the effect of bond market transparency enhancements on new primary bond terms. *YIELDSPREAD* is the offering yield spread over treasury in bps. *logBONDRATING* is the logarithm of the numerically converted bond rating, where a lower rating implies better creditworthiness. *logBONDCOV* is the logarithm of one plus the number of bond covenants. *TRACE* equals one when (and after) an issuer has at least one outstanding bond included for dissemination in the TRACE system, and zero otherwise. The analysis is conducted at the bond issue level. Test statistics (two-sided) based on robust standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. See Appendix B for variable definitions.

8. Concluding remarks

We show that secondary bond market transparency enhancements significantly reduce the use of covenants in bank loan contracts. Further, the reduction in covenant intensity is more pronounced when bond trading is more informative, when the stock market is less informative, and when the likelihood of debt-equity agency conflicts is higher. We strengthen our inferences by conducting a battery of robustness tests that rule out alternative explanations. Finally, we provide corroborating evidence from primary bond issues. Taken together, our findings provide novel evidence to the debt contracting literature that market-level transparency influences private loan contract design. Whereas prior research has primarily focused on the role of banks in facilitating public debt contracting (e.g., Datta et al., 1999; Ma et al., 2019), our findings suggest that the secondary bond market can affect private loan contract design, implying a mutual reliance between private and public debt market participants.

Appendix A. TRACE Timeline

Phases	Date	Coverage
Phase I	Jul 1, 2002	(i) Bond issues with an initial issue size of at least $1 billion and an investment-grade credit rating; (ii) non-investment grade Fixed Income Pricing System (FIPS 50) securities. By the end of 2002, around 520 corporate bonds had their transactions reported in the TRACE system.
Phase II	Mar 3, 2003	(i) Bond issues with issue size from $100 million to $1 billion and a credit rating of A- or higher; (ii) 120 investment-grade bonds rated BBB. With the Phase II implementation, the number of corporate bonds having their post-trade information reported in the TRACE system increased to approximately 4650.

(*continued*)

Phases	Date	Coverage
Phase IIIA	October 1, 2004	All bonds except for Rule 144A bonds and Phase IIIB bonds as defined below. In Phase IIIA, a further 9558 bonds rated BBB- or better were included in the TRACE system.
Phase IIIB	February 7, 2005	Transactions over $1 million in infrequently-traded BB or lower rated bond issues. In Phase IIIB, 3016 bonds were added to the TRACE system.

Appendix B. Variable Definitions

Variables	Definitions
ACCRUALS	Total accruals, defined as income before extraordinary items *(ibq)* minus operating cash flows *(oancfy)*, scaled by the book value of assets
AGE	Log of one plus the number of years the firm has been included in the Compustat database
ALTz	Altman Z-score
AMEND	An indicator variable that equals one if the loan is amended at least once following inception, and zero otherwise
ANALYST	Number of equity analysts issuing forecasts in a firm-quarter
COUPON	Log of bond coupon rate
COV AMEND	An indicator variable that equals one if the deal amendment description contains the words "covenant," "definition," "provision," "ratio," "permit," "allow," "waive," "capex," "sweep," or "prepayment," and zero otherwise
EBITDA	Operating income before depreciation, scaled by book assets
FINCOV	Total number of financial covenants in the loan package at contract inception
High ANALYST DISP	An indicator variable that equals one if the borrowing firm's analyst earnings forecast dispersion is above the industry median, and zero otherwise
High BID-ASK	An indicator variable that equals one if the borrowing firm's stock price bid-ask spread is above the industry median, and zero otherwise.
High MATURITY	An indicator variable that equals one if the loan's maturity is above the industry median, and zero otherwise
High MARKET-TO-BOOK	An indicator variable that equals one if the borrowing firm's MARKET-TO-BOOK is above the sample median, and zero otherwise
High PRICE IMPACT	An indicator variable that equals one if the firm-level aggregate *PRICE IMPACT* in a quarter is greater than the median TRACE phase-quarter aggregate *PRICE IMPACT*, and zero otherwise, where *PRICE IMPACT* equals zero if the trade does not cause any change in price, one if the last price changes, two if either the low or high price changes, three if both the last and low prices or last and high prices change, four if both the high and low prices change, and five if all prices change
High STOCK DISP	An indicator variable that equals one if the borrowing firm's stock price dispersion is above the industry median, and zero otherwise
High AGGREGATE TRADE	An indicator variable that equals one if the firm-level aggregate trade size in a quarter is higher than the median TRACE phase-quarter aggregate trade size, and zero otherwise
INST	Institutional stock ownership percentage
ISSUESIZE	Log of bond issue size
LEVERAGE	Book leverage, defined as the sum of current *(dlcq)* and long-term liabilities *(dlttq)*, scaled by book assets
LOANAMOUNT	Log of the largest facility amount in the loan package
logBONDCOV	Log of one plus the number of bond covenants
logBONDRATING	Log of the numerically converted bond rating, where a lower rating indicates better creditworthiness
logCAPCOV	Log of one plus the number of capital covenants in the loan package
logFINCOV	Log of one plus the number of financial covenants in the loan package
logPERFCOV	Log of one plus the number of performance covenants in the loan package
logSWEEP	Log of one plus the number of sweep covenants in the loan package
logTOTCOV	Log of total financial and sweep covenants in the loan package
MATURITY	Log of loan (bond) maturity in years at loan (bond) contract inception
MARKET-TO-BOOK	Market value of equity and debt, scaled by the book value of assets. The market value of assets is calculated as current liabilities *(dlcq)* plus long-term liabilities *(dlttq)* plus market capitalization *(prccq* $\times$ *cshoq)* plus preferred stock value *(pstkq)* minus deferred taxes and investment tax credit *(txditcq)*
PAYOUT	Total shareholder payout in cash dividends *(dvy)* and share repurchases *(prstkcy)*, scaled by book assets
PPP	An indicator variable that equals one if the loan package includes performance-contingent pricing provisions, and zero otherwise
SPREAD AMEND	An indicator variable that equals one if the loan's initial spread was subsequently renegotiated, and zero otherwise
SECURED	An indicator variable that equals one if the loan package includes a collateral requirement, and zero otherwise
SIZE	Log of book assets
SWEEP	Total number of prepayment/sweep covenants, defined as the sum of asset, debt, equity, cash flow, and insurance proceeds sweeps
TOTCOV	Total number of covenants, defined as the sum of financial covenants and sweeps (general covenants) in the loan package
TRACE	An indicator variable that equals one when (and after) an issuer has at least one outstanding bond included for dissemination in the TRACE system, and zero otherwise
$TRACE^{(t=-\tau)}$	An indicator variable that equals one if the treatment firm enters the TRACE platform in τ quarter(s) from the observation quarter, and zero otherwise

(*continued on next page*)

(*continued*)

Variables	Definitions
TRACE $^{(t=\tau)}$	An indicator variable that equals one if the treatment firm is in its τ-*th* quarter after entering the TRACE platform, and zero otherwise
TRACE $^{(t>\tau)}$	An indicator variable that equals one if the treatment firm is in the τ-*th* or a later quarter after entering the TRACE platform, and zero otherwise
TRACE I	An indicator variable that equals when (and after) an issuer has at least one outstanding bond included for dissemination in Phase I of the TRACE platform, and zero otherwise
TRACE II	An indicator variable that equals one when (and after) an issuer has at least one outstanding bond included for dissemination in Phase II of the TRACE platform, and zero otherwise
TRACE IIIA	An indicator variable that equals one when (and after) an issuer has at least one outstanding bond included for dissemination in Phase IIIA of the TRACE platform, and zero otherwise
TRACE IIIB	An indicator variable that equals one when (and after) an issuer has at least one outstanding bond included for dissemination in Phase IIIB of the TRACE platform, and zero otherwise
YIELDSPREAD	New bond offering yield spread over treasury in bps

References

Aghion, P., Bolton, P., 1992. An incomplete contracts approach to financial contracting. Rev. Econ. Stud. 59 (3), 473–494.
Amiram, D., Beaver, W.H., Landsman, W.R., Zhao, J., 2017. The effects of credit default swap trading on information asymmetry in syndicated loans. J. Financ. Econ. 126 (2), 364–382.
Angrist, J.D., Pischke, J.-S., 2009. Mostly Harmless Econometrics: an Empiricist's Companion. Princeton University Press, New Jersey.
Armstrong, C., Balakrishnan, K., Cohen, D., 2012. Corporate governance and the information environment: evidence from state antitakeover laws. J. Account. Econ. 53, 185–204.
Armstrong, C., Guay, W., Weber, J., 2010. The role of information and financial reporting in corporate governance and debt contracting. J. Account. Econ. 50 (2–3), 179–234.
Asquith, P., Covert, T., Pathak, P., 2019. The Effects of Mandatory Transparency in Financial Market Design: Evidence from the Corporate Bond Market. Work. Pap.
Asquith, P., Gertner, R., Scharfstein, D., 1994. Anatomy of financial distress: an examination of junk-bond issuers. Q. J. Econ. 109 (3), 625–658.
Athey, S., Imbens, G.W., 2022. Design-based analysis in difference-in-differences settings with staggered adoption. J. Econom. 226 (1), 62–79.
Badoer, D., Demiroglu, C., 2019. The relevance of credit ratings in transparent bond markets. Rev. Financ. Stud. 32 (1), 42–74.
Baker, A.C., Larcker, D.F., Wang, C.C.Y., 2022. How much should we trust staggered difference-in-differences estimates? J. Financ. Econ. 144 (2), 370–395.
Barnea, A., Haugen, R., Senbet, L., 1980. A rationale for debt maturity structure and call provisions in the agency theoretic framework. J. Finance 35 (5), 1223–1234.
Barraza, J.S., Lee, W., Yeager, T., 2015. Financial crisis and the supply of corporate credit. Work. Pap.
Bertrand, M., Mullainathan, S., 2003. Enjoying the quiet life? Corporate governance and managerial preferences. J. Polit. Econ. 111 (5), 1043–1075.
Bessembinder, H., Maxwell, W., 2008. Transparency and the corporate bond market. J. Econ. Lit. 22 (2), 217–234.
Bessembinder, H., Maxwell, W., Venkataraman, K., 2006. Market transparency, liquidity externalities, and institutional trading costs in corporate bonds. J. Financ. Econ. 82 (2), 251–288.
Bessembinder, H., Spatt, C., Venkataraman, K., 2020. A survey of the microstructure of fixed-income markets. J. Financ. Quant. Anal. 55 (1), 1–45.
Bharath, S.T., Sunder, J., Sunder, S.V., 2008. Accounting quality and debt contracting. Account. Rev. 83 (1), 1–28.
Biais, B., Green, R., 2019. The microstructure of the bond market in the 20th century. Rev. Econ. Dynam. 33, 250–271.
Billett, M., King, T.-H.D., Mauer, D., 2007. Growth opportunities and the choice of leverage, debt maturity, and covenants. J. Finance 62 (2), 697–730.
Billings, M.B., Klein, A., Zur, E., 2012. Shareholder class action suits and the bond market. Work. Pap.
Bittlingmayer, G., Moser, S.M., 2014. What does the corporate bond market know? Financ. Rev. 49 (1), 1–19.
Black, F., 1975. Fact and fantasy in the use of options. Financ. Anal. J. 31 (4), 36–41.
Bloomfield, R., O'Hara, M., 1999. Market transparency: who wins and who loses? Rev. Financ. Stud. 12 (1), 5–35.
Bond, P., Edmans, A., Goldstein, I., 2012. The real effects of financial markets. Annu. Rev. Financ. Econ. 4, 339–360.
Bradley, M., Roberts, M., 2015. The structure and pricing of corporate debt covenants. Q. J. Finance 5 (2), 1550001.
Brockman, P., Martin, X., Unlu, E., 2010. Executive compensation and the maturity structure of corporate debt. J. Finance 65 (3), 1123–1161.
Brown, K., Chen, V.Y.S., Kim, M., 2015. Earnings management through real activities choices of firms near the investment-speculative grade borderline. J. Account. Publ. Pol. 34 (1), 74–94.
Bushman, R.M., Williams, C.D., Wittenberg-Moerman, R., 2017. The informational role of the media in private lending. J. Account. Res. 55 (1), 115–152.
Callaway, B., Sant'Anna, P.H.C., 2021. Difference-in-differences with multiple time periods. J. Econom. 225 (2), 200–230.
Campello, M., Gao, J., 2017. Customer concentration and loan contract terms. J. Financ. Econ. 123, 108–136.
Carlson, M., Fisher, A., Giammarino, R., 2006. Corporate investment and asset price dynamics: implications for SEO event studies and long-run performance. J. Finance 61 (3), 1009–1034.
Carrizosa, R., Ryan, S.G., 2017. Borrower private information covenants and loan contract monitoring. J. Account. Econ. 64 (2–3), 313–339.
Chava, S., Roberts, M., 2008. How does financing impact investment? The role of debt covenants. J. Finance 63 (5), 2085–2121.
Chemmanur, T.J., Fulgheri, P., 1994. Reputation, renegotiation, and the choice between bank loans and publicly traded debt. Rev. Financ. Stud. 7 (3), 475–506.
Chen, Q., Goldstein, I., Jiang, W., 2007. Price informativeness and investment sensitivity to stock price. Rev. Financ. Stud. 20 (3), 619–650.
Chen, P.F., He, S., Ma, Z., Stice, D., 2016. The information role of audit opinions in debt contracting. J. Account. Econ. 61 (1), 121–144.
Chen, J., Lu, R., 2019. Does public disclosure crowd out private information production? SSRN Electron. J.
Chen, F., Ramaya, K., Wu, W., 2020. The wealth effects of merger and acquisition announcements on bondholders: new evidence from the over-the-counter market. J. Econ. Bus. 107, 105862.
Christensen, H.B., Nikolaev, V.V., 2012. Capital versus performance covenants in debt contracts. J. Account. Res. 50 (1), 75–116.
Chy, M., 2022. Do Bond Market Frictions Impede Equity Financing? Work. Pap.
Chy, M., De Franco, G., Su, B., 2021. The effect of auditor litigation risk on clients' access to bank debt: evidence from a quasi-experiment. J. Account. Econ. 71 (1), 101354.
Chy, M., Hope, O.-K., 2021. Real effects of auditor conservatism. Rev. Account. Stud. 26 (2), 730–771.
Cohen, D.A., Zarowin, P., 2010. Accrual-based and real earnings management activities around seasoned equity offerings. J. Account. Econ. 50 (1), 2–19.

Costello, A.M., Wittenberg-Moerman, R., 2011. The impact of financial reporting quality on debt contracting: evidence from internal control weakness reports. J. Account. Res. 49 (1), 97–136.
Datta, S., Iskandar-Datta, M., Patel, A., 1999. Bank monitoring and the pricing of corporate public debt. J. Financ. Econ. 51 (3), 435–449.
DeFond, M.L., Zhang, J., 2013. The timeliness of the bond market reaction to bad earnings news. Contemp. Account. Res. 31 (3), 911–936.
De Franco, G., Vasvari, F., Vyas, D., Wittenberg-Moerman, R., 2014. Debt analysts' view of debt-equity conflicts of interest. Account. Rev. 89 (2), 571–604.
Demerjian, P., 2017. Uncertainty and debt covenants. Rev. Account. Stud. 22 (3), 1156–1197.
Demerjian, P., 2019. How do lenders monitor? A discussion of Shan, Tang, and Winton (2019). J. Account. Econ. 68 (2–3), 101245.
Demerjian, P., Donovan, J.B., Jennings, J., 2020. Assessing the accuracy of forward-looking information in debt contract negotiations: management forecast accuracy and private loans. J. Manag. Account. Res. 32 (1), 79–102.
Denis, D.J., Mihov, V.T., 2003. The choice among bank debt, non-bank private debt, and public debt: evidence from new corporate borrowings. J. Financ. Econ. 70 (1), 3–28.
Dessein, W., 2005. Information and control in ventures and alliances. J. Finance 60 (5), 2513–2549.
Diamond, D., 1984. Financial intermediation and delegated monitoring. Rev. Econ. Stud. 51 (3), 393–414.
Diamond, D., 1991. Monitoring and reputation: the choice between bank loans and directly placed debt. J. Polit. Econ. 99 (4), 689–721.
Dick-Nielsen, J., 2009. Liquidity biases in TRACE. J. Fixed Income 19, 43–55.
Dick-Nielsen, J., Feldhütter, P., Lando, D., 2012. Corporate bond liquidity before and after the onset of the subprime crisis. J. Financ. Econ. 103, 471–492.
Edwards, A., Harris, L., Piwowar, M., 2007. Corporate bond market transparency and transaction costs. J. Finance 62 (3), 1421–1451.
Even-Tov, O., 2017. When does the bond price reaction to earnings announcements predict future stock returns? J. Account. Econ. 64 (1), 167–182.
Fama, E.F., 1985. What's different about banks? J. Monetary Econ. 15 (1), 29–39.
Faulkender, M., Petersen, M.A., 2006. Does the source of capital affect capital structure? Rev. Financ. Stud. 19 (1), 45–79.
Furfine, C.H., Rosen, R.J., 2011. Mergers increase default risk. J. Corp. Finance 17 (4), 832–849.
Gârleanu, N., Zwiebel, J., 2009. Design and renegotiation of debt covenants. Rev. Financ. Stud. 22 (2), 749–781.
Goldstein, M.A., Hotchkiss, E.S., 2012. Dealer Behavior and the Trading of Newly Issued Corporate Bonds (Working paper).
Goodman-Bacon, A., 2021. Difference-in-differences with variation in treatment timing. J. Econom. 225 (2), 254–277.
Hail, L., Tahoun, A., Wang, C., 2014. Dividend payouts and information shocks. J. Account. Res. 52 (2), 403–456.
Han, S., Zhou, X., 2014. Informed bond trading, corporate yield spreads, and corporate default prediction. Manag. Sci. 60 (3), 675–694.
Harford, J., Uysal, V., 2014. Bond market access and investment. J. Financ. Econ. 112 (2), 147–163.
Hirst, D.E., Koonce, L., Miller, J., 1999. The joint effect of management's prior forecast accuracy and the form of its financial forecasts on investor judgment. J. Account. Res. 37, 101–124.
Hollander, S., Verriest, A., 2016. Bridging the gap: the design of bank loan contracts and distance. J. Financ. Econ. 119 (2), 399–419.
Hong, H.A., Hung, M., Zhang, J., 2016. The use of debt covenants worldwide: institutional determinants and implications on financial reporting. Contemp. Account. Res. 33 (2), 644–681.
Hu, D., 2019. Does the public availability of market participants' trading data affect firm disclosure? Evidence from short sellers. Work. Pap.
Jayaraman, S., Wu, J.S., 2019. Is silence golden? Real effects of mandatory disclosure. Rev. Financ. Stud. 32 (6), 2225–2259.
Jensen, M.C., Meckling, W.H., 1976. Theory of the firm: managerial behavior, agency costs and ownership structure. J. Financ. Econ. 3 (4), 305–360.
Jung, B., Soderstrom, N., Yang, Y.S., 2013. Earnings smoothing activities of firms to manage credit ratings. Contemp. Account. Res. 30 (2), 645–676.
Kedia, S., Zhou, X., 2014. Informed trading around acquisitions: evidence from corporate bonds. J. Financ. Mark. 18, 182–205.
Kisgen, D.J., 2006. Credit ratings and capital structure. J. Finance 61 (3), 1035–1072.
Klein, A., Zur, E., 2011. The impact of hedge fund activism on the target firm's existing bondholders. Rev. Financ. Stud. 24 (5), 1735–1771.
Kothari, S.P., Mizik, N., Roychowdhury, S., 2016. Managing for the moment: the role of earnings management via real activities versus accruals in SEO valuation. Account. Rev. 91 (2), 559–586.
Krishnaswami, S., Spindt, P.A., Subramaniam, V., 1999. Information asymmetry, monitoring, and the placement structure of corporate debt. J. Financ. Econ. 51 (3), 407–434.
Lancaster, T., 2000. The incidental parameter problem since 1948. J. Econom. 16 (1), 391–413.
Lin, L., 2016. Collateral and the choice between bank debt and public debt. Manag. Sci. 62 (1), 111–127.
Lin, C., Ma, Y., Malatesta, P., Xuan, Y., 2013. Corporate ownership structure and the choice between bank debt and public debt. J. Financ. Econ. 109 (2), 517–534.
Lyandres, E., Sun, L., Zhang, L., 2008. The new issues puzzle: testing the investment-based explanation. Rev. Financ. Stud. 21 (6), 2825–2855.
Ma, Z., Stice, D., Williams, C., 2019. The effect of bank monitoring on public bond terms. J. Financ. Econ. 133 (2), 379–396.
Madhavan, A., 1995. Consolidation, fragmentation, and the disclosure of trading information. Rev. Financ. Stud. 8 (3), 579–603.
Madhavan, A., 1996. Security prices and market transparency. J. Financ. Intermediation 5 (3), 255–283.
Miller, M.H., Rock, K., 1985. Dividend policy under asymmetric information. J. Finance 40 (4), 1031–1051.
Murfin, J., 2012. The supply-side determinants of loan contract strictness. J. Finance 67 (5), 1565–1601.
Myers, S., 1977. Determinants of corporate borrowing. J. Financ. Econ. 5 (2), 147–175.
Nini, G., Smith, D.C., Sufi, A., 2009. Creditor control rights and firm investment policy. J. Financ. Econ. 92 (3), 400–420.
Nunn, K., Hill, J., Schneeweis, T., 1986. Corporate bond price data sources and return/risk measurement. J. Financ. Quant. Anal. 21 (2), 197–208.
O'Hara, M., Yao, C., Ye, M., 2014. What's not there: odd lots and market data. J. Finance 69 (5), 2199–2236.
Pagano, M., Röell, A., 1996. Transparency and liquidity: a comparison of auction and dealer markets with informed trading. J. Finance 51 (2), 579–611.
Petacchi, R., 2015. Information asymmetry and capital structure: evidence from Regulation FD. J. Account. Econ. 59 (2–3), 143–162.
Rickmann, G., 2022. The effect of market transparency on corporate disclosure: evidence from the observability of bond prices and trading. Account. Rev. 97 (4), 371–397.
Roberts, M., 2015. The role of dynamic renegotiation and asymmetric information in financial contracting. J. Financ. Econ. 116 (1), 61–81.
Roberts, M., Sufi, A., 2009. Financial contracting: a survey of empirical research and future directions. Annu. Rev. Financ. Econ. 1 (1), 207–226.
Ronen, T., Zhou, X., 2013. Trade and information in the corporate bond market. J. Financ. Mark. 16, 61–103.
Shan, C., Tang, D.Y., Winton, A., 2019. Do banks still monitor when there is a market for credit protection? J. Account. Econ. 68 (2–3), 1–24.
Smith, C., Warner, J., 1979. On financial contracting: an analysis of bond covenants. J. Financ. Econ. 7 (2), 117–161.
Sun, L., Abraham, S., 2021. Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. J. Econom. 225 (2), 175–199.
Vallascas, F., Hagendorff, J., 2011. The impact of European bank mergers on bidder default risk. J. Bank. Finance 35 (4), 902–915.
Wei, J., Zhou, X., 2016. Informed trading in corporate bonds prior to earnings announcements. Financ. Manag. 45 (3), 641–674.