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Bond Market Transparency and Stock Price Crash Risk: Evidence from a Natural Experiment

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ABSTRACT: Utilizing the Trade Reporting and Compliance Engine (TRACE) setting as an exogenous shock to bond market transparency, we find that improved bond market transparency leads to lower crash risk in the stock market, consistent with increased information spillover from the bond market into the stock market. Results from the Path analysis suggest that bond market transparency affects stock price crash risk not only directly, but also indirectly through its effects on management guidance, analyst forecasts, and media reports. We also find that the mitigation effect of bond market transparency on stock price crash risk is more pronounced for firms with higher default risk bonds, lower institutional stock ownership, and more opaque financial reporting. Overall, our findings suggest that increased bond market transparency following TRACE generates a positive externality in reducing crash risk in the stock market.

JEL Classifications: D83; G14; G24.

Keywords: bond market transparency; information spillover; bad news dissemination; stock price crash risk.

I. INTRODUCTION

Market transparency plays a crucial role in facilitating price discovery in financial markets, and improving transparency at the firm and market levels has been one of the key areas addressed by post-crisis financial regulatory reforms (Duffie 2018). Although corporate bonds have long been a principal source of external financing for U.S. firms, they were primarily traded in an opaque environment in which the prices and volumes of

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completed trades were not made publicly available prior to the introduction of the Trade Reporting and Compliance Engine (TRACE) in July 2002 (Bessembinder and Maxwell 2008).[1] The implementation of TRACE constitutes a major shock to the transparency of the bond market and has been shown to affect liquidity, transaction costs, and price dispersion in the bond market (e.g., Bessembinder, Maxwell, and Venkataraman 2006; Edwards, Harris, and Piwowar 2007; Goldstein, Hotchkiss, and Sirri 2007). Particularly, consistent with the role of market transparency in facilitating information aggregation and revelation, Badoer and Demiroglu (2019) find that bond prices become more informative about default risk following TRACE. In this study, we take a novel approach to provide fresh insights into the consequences of bond market transparency by exploring its spillover effects on downside risk in the stock market.

The intermarket information spillover has been utilized by fund managers to enhance their investment strategies.[2] When a firm's securities are traded on multiple financial markets, the information spillover can significantly affect the pricing of securities (Asriyan, Fuchs, and Green 2017; Even-Tov 2017). To the extent that the TRACE implementation enhances bond market transparency in general and bond price efficiency in particular, with accelerated bad news revelation in the bond market, bad news can potentially flow from the bond market into the stock market. However, existing evidence in the literature on whether the bond market leads the stock market in revealing information is inconclusive. On the one hand, prior studies show that the bond market can lead the stock market in revealing bad news (Bittlingmayer and Moser 2014; DeFond and Zhang 2014; Even-Tov 2017). This is possible for at least two reasons: (1) bond investors care more about bad news due to their asymmetric payoff functions (DeFond and Zhang 2014)[3]; and (2) the bond market is dominated by institutional investors who are more sophisticated in information acquisition and trading (De Franco, Vasvari, and Wittenberg-Moerman 2009; Ronen and Zhou 2013). On the other hand, arguing that the bond market is less liquid relative to the stock market, prior studies also show that the information spillover, if any, is from the stock market to the bond market (Kwan 1996; Hotchkiss and Ronen 2002). An important reason for the inconclusive evidence in the literature is that prior studies rely mainly on the lead-lag relation between bond returns and stock returns to draw their conclusions, making it difficult to derive a causal inference on the direction of intermarket information spillover.[4] Taking advantage of the TRACE implementation setting, we use a difference-in-differences (DiD) research design to identify the causal effect of bond market transparency on information spillover across markets.

Our study specifically focuses on the spillover effects of bond market transparency on stock price crash risk because stock price crashes, as shown by prior studies, are caused primarily by bad news hoarding and accumulation (Chen, Hong, and Stein 2001; Jin and Myers 2006; Hutton, Marcus, and Tehranian 2009; Kim and Zhang 2016; Hong, Kim, and Welker 2017). When the amount of hidden bad news accumulated over time reaches a tipping point, it tends to be released to the market all at once, resulting in a large-scale, unanticipated decline in stock price, or simply a stock price crash. If improved bond market transparency subsequent to the TRACE implementation facilitates the revelation of bad news in the bond market, with bad news spillover from the bond market into the stock market, we expect to find lower crash risk in the stock market.

Using a sample of 30,725 firm-year observations over the 1999–2008 period, we find significant evidence that increased bond market transparency following TRACE leads to lower stock price crash risk. This finding is robust to alternative measures of stock price crash risk. The results are consistent with our expectation that increased bond market transparency following TRACE facilitates the flow of bad news from the bond market to the stock market, thereby reducing crash risk in the stock market.

We then directly examine the sources of increased information spillover from the bond market into the stock market. We find that the bond market response to negative earnings surprises in the window (-10, -2) prior to the earnings announcement date is stronger in the post-TRACE period, suggesting that bond market transparency accelerates the bad news revelation in the bond market. Moreover, using the lead-lag relation between monthly bond returns and stock returns to capture the intermarket information spillover effect, we find that the lead-lag relation becomes stronger following TRACE. Therefore, both accelerated bad news revelation in the bond market and stronger lead-lag relation between bond and stock returns contribute to the increased information spillover from the bond market to the stock market.

[1] The TRACE implementation requires bond dealers to report all trades in publicly issued corporate bonds to the National Association of Security Dealers (NASD), which, in turn, publicly disseminates such transaction data.

[2] For example, the asset-management company Robeco adds credit momentum spillover to the momentum factor list in its stock selection models (Blonk, van der Grient, and de Groot 2017).

[3] Specifically, bond investors may pay more attention to bad news collection, interpret bad news more negatively, and respond to bad news more quickly than stock investors (DeFond and Zhang 2014).

[4] For example, the correlation between bond returns in period t and stock returns in period $t+1$ is positive if both of them are positively correlated with stock returns in period t or period $t-1$. This is possible given the serial correlations in stock returns (e.g., Jegadeesh and Titman 1993).

We further perform the Path analysis to investigate the specific mechanisms through which bond market transparency affects stock price crash risk. Using management guidance, analyst forecasts, and media reports as potential mediating variables, we find a significant effect of bond market transparency on these mediating variables, as well as a significant effect of each mediating variable on stock price crash risk. These findings are consistent with the indirect information spillover from the bond market to the stock market. Moreover, we find that bond market transparency has a significant effect on stock price crash risk, consistent with direct information spillover across the two markets.[5] Therefore, the results from the Path analysis suggest that bond market transparency affects stock price crash risk not only directly, but also indirectly through its effects on the mediating variables.

The final series of tests in our study evaluates the robustness of the main findings and conducts cross-sectional analyses. Specifically, we find that our main findings are robust to each phase of TRACE implementation and alternative sample periods. Furthermore, cross-sectional analyses, which show a more pronounced effect for firms with higher default risk bonds, less sophisticated stock investors (i.e., lower institutional stock ownership), and opaque information environments, reinforce our inference that TRACE influences the downside risk of the stock market via the spillover of bad news from the bond market to the stock market.

Our study makes the following contributions to the existing literature. First, we extend prior studies on the consequences of improved bond market transparency. Utilizing the TRACE setting, prior studies have investigated the impact of bond market transparency on the efficiency of the bond market, such as bond price dispersion and trading (Asquith, Covert, and Pathak 2013), bond liquidity (Bessembinder et al. 2006; Edwards et al. 2007; Goldstein et al. 2007), and informational efficiency of bond prices (Badoer and Demiroglu 2019). We expand this literature by examining the spillover effect of bond market transparency on the stock market. A recent study by Rickmann (2022) finds that firms issue more guidance following TRACE because bond market transparency increases investors' access to market information and, thus, limits managers' incentives to withhold information. In contrast to Rickmann (2022), which focuses on the impact of TRACE on corporate disclosure, we utilize the TRACE setting to provide causal evidence on bad news information spillover from the bond market to the stock market. Our findings suggest that the TRACE implementation generates a positive externality in reducing crash risk in the stock market.

Second, our study contributes to the information spillover literature. We provide causal evidence on bad news spillover from the bond market into the stock market. So far, existing evidence is inconclusive with respect to whether and, if so, how the bond market leads the stock market in revealing negative information. Several recent studies find that bond returns lead stock returns in revealing bad news (DeFond and Zhang 2014; Even-Tov 2017). Other studies, however, show that the intermarket information spillover, if any, is from the stock market to the bond market (Kwan 1996; Gebhardt, Hvidkjaer, and Swaminathan 2005). Moreover, prior studies in this area rely mainly on the lead-lag relations between bond and stock returns to infer the existence and direction of information spillover across markets, making it difficult to draw a convincing conclusion. Taking advantage of the TRACE setting, our study provides plausibly causal evidence on bad news spillover from the bond market into the stock market.[6]

Moreover, our study adds to prior studies that investigate the existence of information spillover from other debt markets to the stock market. Bushman, Smith, and Wittenberg-Moerman (2010) document evidence that institutional investors who are involved in syndicated loans exploit confidential syndicate information via trading in the stock market, resulting in accelerated information arrival in stock prices. Qiu and Yu (2012) find that there is a greater information flow from the credit default swaps (CDS) market to the stock market ahead of major credit events. Gao, Kim, Tsang, and Wu (2020) find that financial fraud information is first discovered in the CDS market and then flows into the stock market. Our study complements these studies by providing evidence on information spillover from the bond market into the stock market. In addition, differing from Bushman et al. (2010), which documents information flow through a *private* channel (i.e., institutional investors transmitting their private information gained in syndicated loans to the stock market), our study provides evidence on information spillover from the bond market into the stock market through a *public* channel (i.e., stock investors learning from publicly available prices and transactions in the bond market).

The paper proceeds as follows. Section II reviews the related literature and develops the testable hypothesis. In Section III, we describe the sample and research design. Section IV presents the empirical results. Section V concludes the paper.

[5] That is, the information conveyed through the trading activity of institutional investors in the bond market is transferred to the knowledge set and, therefore, the trading activity of investors in the stock market.

[6] Our paper provides causal evidence to support the direction of information spillover from the bond market to the stock market in the case of bad news. However, such evidence does not preclude the information spillover from the stock market to the bond market for alternative information types (e.g., good news). Therefore, the direction of the information spillover across the stock and bond market remains inconclusive and calls for future research.

II. BACKGROUND, LITERATURE REVIEW, AND HYPOTHESIS DEVELOPMENT

The Impact of TRACE on the Bond Market

Corporate bonds are traded in decentralized markets intermediated by dealers; a small fraction of bonds are traded on the New York Stock Exchange (NYSE), and a substantial fraction of bond trades are carried out over the counter (OTC) (Hite and Warga 1997; Edwards et al. 2007). Prior to the introduction of the TRACE system, bond investors had limited access to both pre-trade information and post-trade information and relied mainly on quotations from dealers and price estimates from data vendors to estimate the value of their bond holdings.

In 1998, the U.S. Securities and Exchange Commission (SEC) called upon the National Association of Securities Dealers (NASD) to take three steps to provide more transparent information to bond investors. In response, the NASD prepared and proposed TRACE Rule 6210, which, after a number of amendments, was approved by the SEC in January 2001.

Following this TRACE Rule, starting from July 1, 2002, all members of the NASD have been required by the SEC to report their transaction information on all eligible corporate debt securities to TRACE. However, the public reporting of transaction information to bond investors was implemented in multiple phases to address concerns regarding the potential negative impact that public transaction reporting could have on smaller, less actively traded securities.

More specifically, Phase I began on July 1, 2002, and required the public reporting of all transactions in investment-grade bonds with an original issue size of $1 billion or more, as well as 50 actively traded noninvestment-grade securities that were transferred to TRACE from the Fixed Income Pricing System (FIPS). Phase II was implemented on March 3, 2003, and expanded public transaction reporting to include all bonds with an original issue size of at least $100 million and rated A3/A− or higher. An additional 120 BBB-rated bonds with issue sizes of less than $1 billion were added as part of Phase II on April 14, 2003. Phase III of TRACE was implemented in two stages. Phase IIIa became effective on October 1, 2004, and initiated transaction reporting for 9,558 new bonds rated BBB− or higher. Phase IIIb became effective on February 7, 2005, and initiated transaction reporting for 3,016 new bonds rated BB+ or lower. According to the NASD, TRACE covered around 99 percent of all eligible corporate debt securities transactions after the Phase IIIb implementation. Appendix A summarizes the effective dates and bonds affected in each phase.

The public reporting for corporate bond trades through the TRACE system comprises a major shock to the transparency of the bond market, which creates an ideal setting to examine the impact of transaction transparency on the functional efficiency of the bond market. Based on the TRACE setting, Bessembinder et al. (2006) find that trade execution costs fall by about 50 percent for those bonds whose transactions are covered by TRACE. Goldstein et al. (2007) find that spreads on newly transparent bonds decline relative to bonds that experience no transparency changes. Moreover, Asquith et al. (2013) provide empirical evidence that the TRACE implementation causes a significant decrease in price dispersion for all bonds. Collectively, these studies provide supportive evidence that the implementation of TRACE generally increases the functional efficiency of the bond market, which may further increase the informational efficiency of bond pricing.

Moreover, theoretical work suggests that information can be transmitted and aggregated more rapidly and completely in transparent markets than in opaque markets (Madhavan 1995; Pagano and Röell 1996; Bloomfield and O'Hara 1999). Based on this insight, recent studies provide direct evidence that bond market transparency increases the efficiency of bond pricing following TRACE. Chen and Lu (2017) find that the TRACE implementation leads to a shorter bond return drift following bond analyst reports or credit rating revisions, suggesting less delay in incorporating information into bond prices. Badoer and Demiroglu (2019) find that the stock market reaction to credit rating downgrades decreases following TRACE, suggesting that bond prices are more informative about default risk when the bond market is more transparent in the post-TRACE period.

The Role of the Bond Market in Revealing Bad News

The bond market is more responsive to bad news due to the asymmetric payoff functions of bondholders (Easton, Monahan, and Vasvari 2009; DeFond and Zhang 2014). The payoffs of bondholders can be replicated by taking a long position in the issuing firm's assets and a short position in a call option on those assets (Black and Scholes 1973; Merton 1974). Whereas good news has a limited impact on bondholders' payoffs, bad news may imply an increase in the likelihood that bondholders will have an economic loss, especially when the call option is close to being out of money.[7] Therefore, bondholders care more about downside risk or bad news than upside potential or good news (Basu 1997;

[7] When the call option is out of money, bad news becomes irrelevant to stockholders, whereas bondholders are responsive to both good news and bad news.

Kothari, Ramanna, and Skinner 2010). Consistent with this view, prior studies find that the bond market's response to bad news is stronger and timelier than to good news. For example, Easton et al. (2009) show that both the bond trading and bond price reactions to earnings announcements are larger when earnings convey bad news. DeFond and Zhang (2014) find that bond price quotes impound negative earnings news in a timelier fashion in comparison to positive earnings news.

Moreover, the asymmetric responsiveness to bad news versus good news suggests that bondholders are more conservative than stockholders (Basu 1997; DeFond and Zhang 2014). Specifically, bond investors may pay more attention to bad news collection, interpret bad news more negatively, and respond to bad news more quickly than stock investors (DeFond and Zhang 2014). As a result, the bond market may lead the stock market in revealing bad news.[8] Prior studies on bond analysts also provide supportive evidence that bond investors are more responsive to bad news than stock investors. For example, De Franco et al. (2009) find that bond analysts issue more negative reports than stock analysts and provide more information about low-credit-quality bonds. Johnston, Markov, and Ramnath (2009) find that firms with higher leverage or higher default risk receive more debt analyst coverage, while losing stock analysts.

Another important reason that the bond market leads the stock market in revealing bad news is that, compared to the stock market, the bond market is heavily dominated by institutional investors who are more sophisticated in the acquisition and processing of information and may even possess more private information (De Franco et al. 2009; Wei and Zhou 2016; Even-Tov 2017). In particular, Ronen and Zhou (2013) show that retail trades account for only 1.8 percent of the volume in the corporate bond market.

Collectively, the bond market can be more efficient and timelier in revealing bad news, compared to the stock market. The negative information reflected in bond prices and transactions may be incremental over and beyond that reflected in stock prices and is likely to flow from the bond market to the stock market. Empirical evidence on the bond market leading the stock market has been documented in the literature. Bittlingmayer and Moser (2014) find that a large, abnormal price decline in a firm's most liquid bonds over a month is followed by an average abnormal stock price decline of 1.42 percent. DeFond and Zhang (2014) find that the bond market impounds bad news on a timelier basis than the stock market during the earnings announcement period. Even-Tov (2017) shows that the bond price reaction to earnings announcements has the predictive power for post-announcement stock returns, especially for noninvestment-grade bonds.

Although the growing body of literature provides evidence on information spillover from the bond market into the stock market, there is also evidence suggesting that the information spillover, if any, is in the opposite direction. The primary reason for the stock market leading the bond market is that the bond market is typically less liquid. Market illiquidity increases the cost of trading and, consequently, hinders bond investors from trading upon their private information. As a result, the speed of bad news incorporation into bond prices may slow down, deterring the bond market from leading the stock market in revealing bad news. Consistent with these arguments, Kwan (1996) finds that lagged stock returns have explanatory power for current bond yield changes, but lagged bond yield changes are unrelated to current stock returns. Gebhardt et al. (2005) show that stock returns over a six-month period predict bond returns over the following six months. Moreover, Downing, Underwood, and Xing (2009) conclude that the corporate bond market is less informationally efficient than the stock market, notwithstanding the improvement in bond market transparency following TRACE.

Bad News Revelation and Stock Price Crash Risk

Crash risk represents higher moments of stock return distribution—that is, extreme negative returns, which can be highly detrimental to investors' personal wealth (Kim, H. Li, and S. Li 2014; Habib, Hasan, and Jiang 2018). It is well established in the crash risk literature that bad news accumulation is a key determinant of stock price crash risk (Chen et al. 2001; Jin and Myers 2006; Hutton et al. 2009; Kim, Li, and Zhang 2011a). Specifically, bad news accumulation can be caused either by managers' incentives to withhold bad news or by the inefficiency of financial markets in incorporating bad news. However, the amount of bad news that can be withheld and stockpiled is limited. When the accumulated negative information reaches a tipping point, it comes out all at once, resulting in a large-scale, unanticipated decline in stock price or simply a stock price crash.

One stream of the crash risk literature investigates the relation between managerial bad news withholding and stock price crash risk (Jin and Myers 2006; Hutton et al. 2009; Kim et al. 2011a; Hong et al. 2017). Jin and Myers (2006) develop a model in which managerial incentives to withhold bad news, combined with information opacity, lead to stock

[8] Compared to the bond market, the stock market typically responds to bad news in a more sluggish manner, relative to good news. For example, Bernard and Thomas (1989) show empirical evidence that the post-earnings announcement drift is stronger for negative than for positive earnings surprises.

price crashes. Hutton et al. (2009) find that a firm's financial reporting opacity is positively related to its stock price crash risk. Following Jin and Myers (2006) and Hutton et al. (2009), a large body of research has documented ample evidence suggesting that managerial incentives for bad new hoarding lead to stock price crash risk (Kim et al. 2011a; Kim, Li, and Zhang 2011b; Kim and Zhang 2014; Kim, Wang, and Zhang 2016; Kim, Li, Lu, and Yu 2016; Hong et al. 2017).

Another stream of research investigates the efficiency or speediness of bad news incorporation into stock prices and resultant stock price crash risk. So far, there are relatively few studies in this area. Chen et al. (2001) test a model in which bad news is not fully revealed in stock prices due to heterogeneity in investors' opinions and short sale constraints; when the accumulated negative information comes out all at once, it leads to stock price crashes. Using the SEC Regulation SHO pilot program as an exogenous shock to short-sale constraints, Deng, Gao, and Kim (2020) find a significant decrease in stock price crash risk for firms randomly selected into the SHO program.

Testable Hypothesis

Increased bond market transparency following TRACE improves bond price informativeness by accelerating the incorporation of bad news into bond prices (Chen and Lu 2017; Badoer and Demiroglu 2019). Based on the findings of prior studies that the bond market can lead the stock market in revealing bad news (Ronen and Zhou 2013; Bittlingmayer and Moser 2014; DeFond and Zhang 2014; Even-Tov 2017), we expect that the TRACE implementation facilitates bad news spillover from the bond market into the stock market.

The stock price crash literature suggests that stock price crashes are caused mainly by bad news withholding (Chen et al. 2001; Jin and Myers 2006; Hutton et al. 2009; Kim and Zhang 2016). To the extent that the TRACE implementation facilitates bad news revelation in the bond market, with information spillover from the bond market into the stock market, bad news is less likely to be accumulated until it reaches a tipping point. This prevents the occurrence of a large-scale, unexpected decline in stock prices, and thus reduces stock price crash risk.

Nevertheless, it is also possible that the implementation of TRACE may not exert a significant influence over stock price crash risk. First of all, as discussed earlier, the bond market is typically less liquid relative to the stock market, which may prevent the bond market from leading the stock market in revealing bad news. So far, empirical evidence on whether the bond market leads the stock market is inconclusive. Second, existing evidence on the bond market leading the stock market is either restricted to earnings announcement periods (DeFond and Zhang 2014; Even-Tov 2017) or applicable only to high-yield bonds (Bittlingmayer and Moser 2014). Third, even if the introduction of TRACE generally improves the informational efficiency of the bond market, the extent of such an improvement may not be significant enough to generate a spillover effect into the stock market.

Based on the above discussions, whether the TRACE implementation eventually leads to lower stock price crash risk is ultimately an empirical question. We propose our hypothesis in alternative form as follows: *All else equal, the implementation of TRACE in the bond market leads to lower crash risk in the stock market.*

III. RESEARCH DESIGN

Sample and Data

We start our sample construction with the intersection of Compustat and CRSP for the period 1999 to 2008, which is three years before the first implementation of TRACE in 2002 and three years after its last implementation in 2005. We first extract all the firm-year observations from Compustat and CRSP. We then require each observation in our sample to have both total assets and a book value of equity greater than zero and a year-end share price greater than $1. We identify 53,055 firm-years (or 9,572 firms) that meet the above requirements. After further requiring the necessary data from Compustat and CRSP to construct the measures of stock price crash risk and control variables, we obtain a sample of 35,874 firm-years (or 6,483 firms).

Our sample includes treatment firms, which have outstanding bonds and therefore are affected by TRACE, and control firms, which are not affected by TRACE. We use the linking table provided by WRDS Bond Database to match Compustat/CRSP with bond transaction data from the TRACE Enhanced database.[9] In order to determine the affected year of each treatment firm, we obtain the list of bonds in each phase from the Financial Industry Regulatory Authority (FINRA). For firms that are affected by more than one phase, we use the earliest phase year as their affected year.[10]

[9] The WRDS Bond Database includes a unique and essential mapping table that links all bond and stock issues for every firm and at each point time using information in the TRACE and CRSP databases. The mapping uses the trading symbol of the issuing company to link the corporate bond identifiers (CUSIP_ID) to the stock identifier (PERMNO).

[10] In robustness tests, our main findings still hold if we use the latest phase year as the affected year or if we drop the firms that are affected by multiple phases.

Moreover, we exclude firms with bonds that reach maturity before the first TRACE implementation year (i.e., before 2002) and firms with bonds that are issued after the last TRACE implementation year (i.e., after 2005). For firms with bonds that reach maturity during the TRACE implementation period (i.e., from 2002 to 2005), we only include their pre-maturity firm-year observations. We obtain bond characteristics data, including issue dates and maturity dates, from the Fixed Investment Securities Database and bond ratings from S&P Credit Ratings.

Finally, we obtain a sample of 30,725 firm-year observations (or 4,903 firms), among which there are 9,061 firm-year observations (or 1,233 firms) in the treatment group and 21,664 firm-year observations (or 3,670 firms) in the control group.

Main Variables

Stock Price Crash Measures

We use three measures to capture firm-specific stock price crash risk. Following Hutton et al. (2009), Kim et al. (2011a, 2011b), and Kim and Zhang (2016), our first measure of crash risk, *Crash*, is an indicator variable that equals 1 if there is at least one crash week for firm i in year t and 0 otherwise. To identify crash weeks, we first calculate firm-specific weekly returns ($W_{i,\tau}$) for each firm-year observation in our sample. $W_{i,\tau}$ is defined as the natural logarithm of 1 plus the residual return ($\varepsilon_{i,\tau}$)—i.e., $W_{i,\tau} = \ln(1 + \varepsilon_{i,\tau})$—and $\varepsilon_{i,\tau}$ is estimated from the following model:

$$r_{i,\tau} = \alpha_i + \beta_{1,i} r_{m,\tau-2} + \beta_{2,i} r_{m,\tau-1} + \beta_{3,i} r_{m,\tau} + \beta_{4,i} r_{m,\tau+1} + \beta_{5,i} r_{m,\tau+2} + \varepsilon_{i,\tau},$$

where $r_{i,\tau}$ is the raw stock return for firm i in week τ of year t, and $r_{m,\tau}$ is the CRSP value-weighted market index return in week τ of year t. As in Kim et al. (2011a), we include the lead and lag terms of market returns to allow for nonsynchronous trading (Dimson 1979).

We estimate the above model for firm i in year t using its weekly returns in the 12-month period ending three months after the firm's fiscal year end, requiring at least 26 weekly returns in year t. We define crash weeks for firm i in year t as those weeks in which firm-specific weekly returns ($W_{i,\tau}$) are 3.2 standard deviations below the mean firm-specific weekly returns in year t.

The second measure of stock price crash risk is the negative conditional skewness of firm-specific weekly returns, *Ncskew*, as used in Chen et al. (2001) and Kim et al. (2011a). Specifically, *Ncskew* is the negative of the third moment of firm-specific weekly returns in year t, divided by the cubed standard deviation of the firm-specific weekly returns. That is,

$$Ncskew_{i,\tau} = -\left[n(n-1)^{3/2} \sum W_{i,\tau}^3\right] / \left[\left(n-1\right)\left(n-2\right)\left(\sum W_{i,\tau}^2\right)^{3/2}\right],$$

where $W_{i,\tau}$ is the firm-specific weekly return for firm i in week τ of year t, and n is the number of firm-specific weekly returns in year t. A higher *Ncskew* value indicates a higher stock price crash risk.

Our third measure of stock price crash risk, *Duvol*, is the asymmetric volatility of negative versus positive returns (Chen et al. 2001). We classify all the weeks for firm i in year t into "down"/"up" weeks—i.e., the weeks with firm-specific weekly returns below/above the annual mean of weekly returns—and calculate standard deviations for the "down" weeks and "up" weeks separately. *Duvol* is calculated as the natural logarithm of the standard deviation of weekly stock returns *for the* "down" weeks over the standard deviation of weekly stock returns for the "up" weeks. Specifically, *Duvol* for each firm i in year t is calculated as:

$$Duvol_{i,\tau} = log\left\{\left[(n_u - 1)\sum_{DOWN} W_{i,\tau}^2\right] / \left[(n_d - 1)\sum_{UP} W_{i,\tau}^2\right]\right\},$$

where n_u is the number of "up" weeks and n_d is the number of "down" weeks. A higher *Duvol* value indicates a higher stock price crash risk.

Control Variables

Following prior studies (Chen et al. 2001; Hutton et al. 2009; Kim et al. 2011a, 2011b; Callen and Fang 2015), we include a series of control variables in our regressions to isolate the effect of bond market transparency on stock price crash risk. Specifically, we control for several stock-level variables, *Dturn*, *Sigma*, and *Return*, where *Dturn* is the detrended average monthly share turnover, measured as the average monthly turnover ratio in a fiscal year minus that of the previous fiscal year; *Sigma* is the standard deviation of firm-specific weekly returns, a measure of return volatility; and *Return* is the average firm-specific weekly returns. Moreover, firm size (*Size*), leverage (*LEV*), market-to-book ratio (*MB*), return on assets (*ROA*), and absolute abnormal accruals (*ABACC*) are included to control for the effects of other firm-level characteristics on crash risk. Appendix B provides detailed definitions of all the variables used in our regression analysis.

Regression Model

Although most prior studies rely on the traditional staggered DiD design—i.e., the two-way fixed effects (TWFE) model—to estimate the treatment effects in the staggered regulation settings like the TRACE setting in our study, recent advances in econometric theory show that the traditional staggered DiD design often provides biased estimates of the treatment effects (Barrios 2021; Callaway and Sant'Anna 2021; Sun and Abraham 2021; Baker, Larcker, and Wang 2022). In addition to the parallel-trends and no-anticipation assumptions for the DiD research design in general,[11] the validity of the traditional staggered DiD design also relies on the assumption of homogenous treatment effects over time and across cohorts (Goodman-Bacon 2021; Baker et al. 2022). Specifically, the estimates obtained through the TWFE model are variance-weighted averages of many different "2×2" DiDs across cohorts, in some of which already-treated units act as the control group for later-treated units. On the one hand, if the treatment effects vary across cohorts, the estimates from the TWFE model are biased because the weight assigned by the OLS regression on each cohort is generally different from the corresponding sample share. On the other hand, if the treatment effects vary over time, the estimates from the TWFE model are biased because pre-treatment trends can arise for later-treated units, and the post-treatment effects of later-treated units are contaminated when the outcome changes of already-treated units are subtracted from those of later-treated units.[12]

To circumvent the problems with the traditional staggered DiD design, several recent econometric studies propose different ways to adjust the TWFE model. In our study, we closely follow the approach developed by Sun and Abraham (2021), which first estimates the cohort-specific relative-time treatment effects, allowing for treatment-effect heterogeneity, and then aggregates them to obtain the *average treatment effect on the treated* (ATT). Specifically, as shown below, Sun and Abraham (2021) propose an adjusted TWFE model (with y_{it} being the outcome of interest and φ_i and λ_t being the unit and time fixed effects) that estimates the full set of cohort-specific relative-time treatment effects using an interacted specification that is saturated in relative-time indicators D_{it}^l and cohort indicators $\lessdot\{E_i = g\}$, where E_i is the time when unit i receives the treatment and g is the cohort of treatment. And they impose restrictions on the treatment group ($g \notin C$)—that is, always-treated units are dropped, and the only units that can be used as effective controls are those that are never-treated or last-treated (when the last-treated are used as controls, they are never used as treated units). Lastly, based on the estimates of $\beta_{g,l}$, they calculate the effect of each relative-time period ("relative-time period effect") as the weighted average of the effects of the particular relative-time period across all cohorts (e.g., weighting by each cohort's sample shares).

$$y_{it} = \varphi_i + \lambda_t + \sum_{g \notin C} \sum_{l \neq -1} \beta_{g,l} \left(\lessdot\{E_i = g\} \cdot D_{it}^l \right) + \epsilon_{it.}$$

In the TRACE setting, each phase of TRACE represents one cohort of treatment, and relative-time periods we use in our study are years relative to the treatment event. We follow Sun and Abraham (2021) and estimate the following regression model by including the full set of phase $\times$ relative-year indicators.

$$Crash_{i,t+1} = \alpha + \sum_{g} \sum_{l \neq -1} \beta_{g,l} \left(\lessdot\{E_i = g\} \cdot Year_{it}^l \right) + \gamma Controls_{i,t} + \varphi_i + \lambda_t + \epsilon_{it.} \tag{1}$$

[11] The parallel-trends assumption requires that the trends of the treatment group and the control group are parallel in the absence of the treatment event, whereas the no-anticipation assumption requires that the treatment effect does not exist before the treatment event.

[12] For example, the treatment units in cohort I may act as the control group for the treatment units in cohort II. When the treatment effects of cohort I are time-varying and the outcome changes of cohort I are subtracted from the outcome changes of cohort II, the pre-treatment estimates of cohort II can be biased. It may result in either significant pre-treatment estimates when pre-trends actually do not exist or insignificant pre-treatment estimates when pre-trends exist. As a result, the observed pre-trends would lead to incorrect inferences on the validity of the parallel trends and no-anticipation assumptions. Similarly, the post-treatment estimates of cohort II can also be biased when the time-varying outcome changes of cohort I are subtracted from the outcome changes of cohort II.

TABLE 1

Summary Statistics

	n	Mean	Std. Dev.	p25	p50	p75
$Crash_{t+1}$	30,725	0.189	0.392	0.000	0.000	0.000
$Ncskew_{t+1}$	30,725	−0.048	0.801	−0.485	−0.081	0.348
$Duvol_{t+1}$	30,725	−0.038	0.362	−0.275	−0.050	0.189
$TRACE_t$	30,725	0.135	0.342	0.000	0.000	0.000
$Dturn_t$	30,725	0.007	0.112	−0.023	0.002	0.034
$Sigma_t$	30,725	0.062	0.034	0.037	0.054	0.078
$Return_t$	30,725	−0.249	0.317	−0.300	−0.141	−0.068
$Size_t$	30,725	6.054	2.140	4.460	5.981	7.521
MB_t	30,725	3.097	4.511	1.218	1.960	3.353
LEV_t	30,725	0.197	0.190	0.013	0.159	0.323
ROA_t	30,725	0.016	0.123	−0.012	0.037	0.086
$ABACC_t$	30,725	0.084	0.105	0.024	0.055	0.106

Table 1 presents the summary statistics of the main variables.
The definitions of the variables are reported in Appendix B.

In this model, the dependent variable is one of the three crash measures: the likelihood of crash occurrence (*Crash*), negative conditional skewness of weekly returns (*Ncskew*), and down-to-up volatility (*Duvol*).[13] Cohort indicators, $\lessdot\{E_i = g\}$, are a set of indicators that equal 1 if a firm is covered by a specific phase (I, II, IIIa, or IIIb) of TRACE and 0 otherwise. We use never-treated firms as the control group, so all firms affected by each of the four phases of TRACE are included in the treatment group. Relative-time indictors, $Year^{l}_{it}$, are a set of indicators that equal 1 if a given year is l years relative to the year when the firm is covered by TRACE and 0 otherwise. In addition, $Year^{-1}$ for each cohort is omitted from the regressions.

After obtaining the "relative-time period effect," we further calculate the average of "relative-time period effect" over the post-treatment window as the single overall measure of ATT (i.e., TRACE effect).[14] We expect to find that the TRACE effect is significantly negative if the TRACE implementation leads to lower crash risk in the stock market.

One critical assumption underlying the DiD design is that the treatment effect is reasonably random in a way that it would not bias toward the results observed. In the TRACE setting, since the phases of TRACE are assigned by initial issue size and credit rating of bonds, firms may possibly choose a specific phase to be affected by manipulating the issue size of bonds. However, in our sample, we find that 699 out of 1,233 bonds are not subject to manipulation because these bonds were issued before the earliest announcement date of TRACE (i.e., January 2001). For the remaining bonds, we plot the frequency of their issue size surrounding the cutoff thresholds of each phase and find no significant bunching surrounding the thresholds, suggesting that the treatment effect is reasonably random in the TRACE setting.

IV. EMPIRICAL RESULTS

Summary Statistics

Table 1 presents descriptive statistics of the main variables in our regressions. The mean (median) values of our crash risk measures are 0.189 (0.00) for *Crash*, −0.048 (−0.081) for *Ncskew*, and −0.038 (−0.050) for *Duvol*. These statistics of the crash measures are generally consistent with those of prior studies (e.g., Kim et al. 2011a; Kim and Zhang 2016). The mean of *TRACE* is 0.135, suggesting that about 13.5 percent of firm-year observations are covered by

[13] We find similar results if we measure crash risk in the year of the TRACE implementation (i.e., year t) or we remove the observations with crash risk measured in the treatment year from our sample.

[14] Sun and Abraham (2021) provide a Stata package (eventstudyinteract) that estimates the "relative-time period effect" and the single overall treatment effect.

TRACE in our sample. Moreover, in the year before crash risk is measured, an average firm in our sample has a *Dturn* of 0.007, a *Sigma* of 0.062, a weekly return (*Return*) of −0.249, a *Size* (measured by total book assets in log-transformation) of 6.054, an *MB* of 3.097, a *LEV* of 0.197, a *ROA* of 0.016, and an *ABACC* of 0.084. These summary statistics are largely consistent with those reported in prior studies.

The Effect of Bond Market Transparency on Stock Price Crash Risk

To test the effect of bond market transparency on a firm's stock price crash risk, we estimate the DiD regression as specified in regression model (1). We run a linear probability model when the dependent variable is *Crash* and an OLS model when the dependent variables are *Ncskew* and *Duvol*.

We report the estimation results for the effects of each relative-time period in Table 2, Panel A.[15] As shown, across all three columns, the effects of pre-treatment periods (i.e., $Year^{-5}$ to $Year^{-2}$) are all insignificant, suggesting that there is no significant difference in the crash risk between treatment and control firms prior to the TRACE implementation. The insignificant difference between treatment and control groups validates the parallel trend assumption underlying the DiD research design. In contrast, the effects of post-treatment periods (i.e., $Year^{1}$ to $Year^{5}$) are negative and significant in all three columns, except for the $Year^{5}$ effect in column (1), suggesting that stock price crash risk decreases following TRACE. The results are robust to alternative measures of stock price crash risk. Overall, these findings provide strong support to our hypothesis that increased bond market transparency following TRACE leads to lower stock price crash risk.

We further calculate the average of relative-time period effect over the post-treatment window as the single overall effect of TRACE and report the results in Table 2, Panel B. In column (1), the TRACE effect (i.e., the overall effect of TRACE on crash risk) is −0.051, which is significant at the 1 percent level (t = −2.79). In column (2), we continue to find that the TRACE effect is negative and highly significant (−0.133, t = −4.13). Similarly, in column (3), the overall effect of TRACE is also negative and significant (−0.056, t = −3.77). These findings in all three columns, taken together, show that the TRACE implementation has an overall negative effect on stock price cash risk.

Our findings are economically significant as well. Take column (1) of Panel B as an example: the TRACE effect indicates that, relative to control firms, an average decrease in crash risk for treatment firms is 0.051 following TRACE, which amounts to about 27 percent (= 0.051/0.189) of the mean of *Crash* in our sample. With respect to the control variables, we find that the divergence of opinion among investors (*Dturn*) is positively related to stock price crash risk, consistent with Chen et al. (2001). Consistent with prior studies (Kim et al. 2011a; Callen and Fang 2015), we find that *Size* and *ROA* are positively related to stock price crashes.

Bond Market Transparency and Bad News Spillover across Markets

Our baseline findings are consistent with increased information spillover from the bond market into the stock market following TRACE. To provide further support to these findings, we examine the sources of increased information spillover from the bond market into the stock market. On the one hand, increased bond market transparency following TRACE may accelerate bad news revelation in bond prices, because information can be transmitted and aggregated more rapidly and completely in transparent markets (after TRACE) than in opaque markets (before TRACE). On the other hand, the lead-lag relation between bond returns and stock returns can also become stronger following TRACE, to the extent that the implementation of TRACE improves the informativeness of bond prices (Chen and Lu 2017; Badoer and Demiroglu 2019).[16]

Following DeFond and Zhang (2014), we use bond market response to the negative earnings surprise (*SUE*) during the pre-earnings announcement window (−10, −2)—i.e., *Bond CAR (−10, −2)*—to capture bad news revelation in the bond market. A larger bond market response during the pre-earnings announcement window can be viewed as an indication of stronger and timelier bad news revelation in the bond market. Moreover, following Bittlingmayer and Moser (2014), we use the correlation between bond returns in month t (*Bond* $MRET_t$) and stock returns in month t+1 (*Stock* $MRET_{t+1}$) to measure the lead-lag relation between bond returns and stock returns. A larger correlation suggests a

[15] We include the full set of indicators (i.e., phase × relative-year indicators) with $Year^{-1}$ omitted in our regressions. In this panel, we only report the effects of $Year^{-5}$ to $Year^{-2}$ and $Year^{0}$ to $Year^{5}$ for brevity.

[16] For example, if we use the mean (μ) and standard deviation (σ) of market response to represent the information content (i.e., the amount of information) and noise (i.e., the accuracy of information) of bond prices, the bond market response to an event may have a larger μ or smaller σ following TRACE, both of which may lead to a stronger lead-lag relation between bond returns and stock returns. Moreover, note that even if the lead-lag relation between bond returns and stock returns does not change following TRACE, with accelerated bad news revelation in the bond market, the amount of information spillover from the bond market into the stock market may still increase, leading to lower stock price crash risk.

TABLE 2

The Effect of TRACE on Stock Price Crash Risk

Panel A: Dynamic Analysis on the Effect of TRACE on Crash Risk

	(1) $Crash_{t+1}$	(2) $Ncskew_{t+1}$	(3) $Duvol_{t+1}$
$Year^{-5}$ effect	0.044	0.020	0.008
	(1.59)	(0.36)	(0.32)
$Year^{-4}$ effect	0.005	−0.002	0.011
	(0.20)	(−0.04)	(0.54)
$Year^{-3}$ effect	−0.024	−0.041	−0.013
	(−1.22)	(−1.11)	(−0.75)
$Year^{-2}$ effect	−0.004	−0.020	0.003
	(−0.20)	(−0.55)	(0.20)
$Year^{0}$ effect	−0.030	−0.058	−0.017
	(−1.61)	(−1.51)	(−0.99)
$Year^{1}$ effect	−0.034*	−0.150***	−0.063***
	(−1.73)	(−3.75)	(−3.53)
$Year^{2}$ effect	−0.075***	−0.194***	−0.079***
	(−3.83)	(−4.95)	(−4.46)
$Year^{3}$ effect	−0.045**	−0.136***	−0.054***
	(−2.19)	(−3.49)	(−3.04)
$Year^{4}$ effect	−0.076***	−0.195***	−0.086***
	(−3.66)	(−4.68)	(−4.52)
$Year^{5}$ effect	−0.033	−0.170***	−0.079***
	(−0.86)	(−2.78)	(−2.70)
$Dturn_t$	0.046**	0.200***	0.072***
	(2.00)	(4.42)	(3.61)
$Sigma_t$	−0.746**	−1.635**	−0.615**
	(−2.50)	(−2.57)	(−2.17)
Ret_t	−0.032	−0.059	−0.013
	(−1.23)	(−0.99)	(−0.47)
$Size_t$	0.071***	0.212***	0.099***
	(13.94)	(20.08)	(20.85)
MB_t	−0.000	−0.001	−0.000
	(−0.61)	(−0.40)	(−0.11)
LEV_t	0.070**	−0.020	−0.003
	(2.31)	(−0.32)	(−0.12)
ROA_t	0.174***	0.350***	0.172***
	(5.26)	(5.15)	(5.66)

(continued on next page)

stronger information spillover across these two markets. We focus on the bad news subsample in our analysis, where bad news is defined as negative *SUE* or negative *Bond MRET*, respectively.[17] We then define an indicator variable, *TRACE*, which equals 1 if a firm is covered by any phase of TRACE in year t and 0 otherwise,[18] and estimate the

[17] We remove firms covered by Phase I from our analysis because their bond trading data are available only during the post-TRACE period. Starting from July 2002, FINRA collected transaction data for both disseminated bonds and nondisseminated bonds, though the transaction data of nondisseminated bonds were not publicly disseminated immediately. In March 2010, FINRA released a "historical" TRACE dataset, which includes both disseminated and nondisseminated transaction records.

[18] We define *TRACE* in this section to differentiate its effect from the overall effect of TRACE (i.e., TRACE effect), which is calculated from the estimated coefficients on the full set of indicators (i.e., phase × relative-year indicators), in our baseline regressions. Essentially, the analyses in this section are not based on DiD regressions, as the variable of interest is in the interaction term.

TABLE 2 (continued)

	(1) $Crash_{t+1}$	(2) $Ncskew_{t+1}$	(3) $Duvol_{t+1}$
$ABACC_t$	−0.014	−0.056	−0.037
	(−0.56)	(−1.10)	(−1.64)
Firm and Year FE	Yes	Yes	Yes
R^2	0.204	0.237	0.238
Observations	30,725	30,725	30,725

Panel B: Overall Effect of TRACE on Crash Risk

	(1) $Crash_{t+1}$	(2) $Ncskew_{t+1}$	(3) $Duvol_{t+1}$
TRACE effect	−0.051***	−0.133***	−0.056***
	(−2.79)	(−4.13)	(−3.77)
Control Variables	Yes	Yes	Yes
Firm and Year FE	Yes	Yes	Yes
R^2	0.204	0.237	0.238
Observations	30,725	30,725	30,725

***, **, * Indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.
Table 2 reports the regression results estimating the effect of TRACE on stock price crash risk. Panel A reports the results of dynamic analysis on the effect of TRACE on crash risk. The relative-year effect (e.g., $Year^{-5}$ effect) is calculated as the weighted average of the effects of the particular relative-year across all cohorts. Panel B reports the overall effect of TRACE on crash risk. TRACE effect is calculated as the average of each relative-year effect over the post-treatment window. In both panels, the dependent variables are *Crash* in column (1), *Ncskew* in column (2), and *Duvol* in column (3). Estimated constants are untabulated. In parentheses below the coefficient estimates, we report t-statistics based on robust standard errors adjusted for firm-level clustering.
All variables are defined in Appendix B.

regressions with the interaction term $TRACE \times SUE$ or $TRACE \times Bond\ MRET$ included in the regression model and report the regression results in Table 3.[19]

The regression results for bad news revelation in the bond market are reported in Table 3, Panel A. The dependent variables are *Bond CAR (−10, −2)* in both columns. Industry and year fixed effects are included in column (1), and industry and year-quarter fixed effects are included in column (2). As shown in Panel A, we find that the coefficients of $TRACE \times SUE$ are positive and significant in both columns, suggesting that bond market response to bad news during the (−10, −2) window is incrementally stronger following TRACE. In other words, bad news is revealed in the bond market more quickly and strongly in the post-TRACE period than in the pre-TRACE period.[20]

Table 3, Panel B reports the regression results for the lead-lag relation between bond returns and stock returns. The dependent variables are *Stock MRET* (in period t+1) in both columns. We include industry and year fixed effects in column (1) and industry and year-month fixed effects in column (2). As shown, the coefficients of the interaction term $TRACE \times Bond\ MRET$ are positive and significant in both columns, suggesting that the lead-lag relation between bond returns and stock returns becomes stronger in the post-TRACE period.

Moreover, if both bad news revelation in the bond market and the lead-lag relation between bond returns and stock returns become stronger following TRACE, bad news should eventually be revealed more quickly and strongly in the stock market. We directly test this conjecture. We use stock market response to negative earnings surprise (*SUE*) during the window (−10, −2)—i.e., *Stock CAR (−10, −2)*—to capture bad news revelation in the stock market. The regression results are reported in Table 3, Panel C. As shown, we find that the interaction term $TRACE \times SUE$ has positive and significant coefficients in both columns. This finding suggests that bad news is indeed revealed more quickly and strongly in the stock market following TRACE.

[19] The sample size varies in the three panels because: (1) the observations are at the firm-quarter level in Panels A and C and firm-month level in Panel B; and (2) additional data (i.e., bond trading data during the pre-earnings announcement window) are required in Panel A.
[20] In untabulated results, we find that there is no significant difference in bond market response to good news during post- versus pre-TRACE periods, consistent with bondholders' asymmetric responsiveness to bad news versus good news (DeFond and Zhang 2014).

TABLE 3

The Effect of TRACE on Bad News Spillover

Panel A: Bad News Revelation in the Bond Market

	(1)	(2)
	Bond CAR (−10, −2)	***Bond CAR (−10, −2)***
SUE_t	0.016	0.011
	(0.69)	(0.47)
$TRACE_t$	0.004**	0.005***
	(2.41)	(2.69)
$TRACE_t \times SUE_t$	0.052*	0.061**
	(1.75)	(2.06)
Industry FE	Yes	Yes
Year FE	Yes	No
Year-quarter FE	No	Yes
R^2	0.048	0.066
Observations	2,748	2,748

Panel B: Bad News Spillover from the Bond Market into the Stock Market

	(1)	(2)
	Stock $MRET_{t+1}$	***Stock $MRET_{t+1}$***
Bond $MRET_t$	0.015	0.031
	(0.18)	(0.37)
$TRACE_t$	−0.002	−0.002
	(−0.67)	(−0.58)
$TRACE_t \times$ *Bond* $MRET_t$	0.206**	0.195*
	(2.07)	(1.91)
Industry FE	Yes	Yes
Year FE	Yes	No
Year-month FE	No	Yes
R^2	0.012	0.026
Observations	12,302	12,302

Panel C: Bad News Revelation in the Stock Market

	(1)	(2)
	Stock CAR (−10, −2)	***Stock CAR (−10, −2)***
SUE_t	0.036*	0.036*
	(1.75)	(1.79)
$TRACE_t$	0.002	0.001
	(0.39)	(0.14)
$TRACE_t \times SUE_t$	0.061**	0.061**
	(2.05)	(2.03)
Industry FE	Yes	Yes
Year FE	Yes	No
Year-quarter FE	No	Yes
R^2	0.015	0.019
Observations	10,147	10,147

***, **, * Indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.

Table 3 reports the regression results estimating the effect of TRACE on bad news spillover across the bond and stock markets. Panel A reports the regression results on bad news revelation in the bond market. Panel B reports the regression results on bad news spillover from the bond market into the stock market. Panel C reports the regression results on bad news revelation in the stock market. The dependent variables are *Bond CAR (−10, −2)* in Panel A, stock market monthly return (*MRET*) in Panel B, and *Stock CAR (−10, −2)* in Panel C. In parentheses below the coefficient estimates, we report t-statistics based on robust standard errors adjusted for firm-level clustering.

The Mechanisms of Bad News Spillover

Our analysis thus far shows the increased information spillover from the bond market into the stock market following TRACE. We further investigate the specific mechanisms through which information spillover affects stock price crash risk. We expect that bad news revealed in the bond market may flow into the stock market in two ways: (1) *directly*, i.e., stock investors learning from information that is conveyed through the trading activity of investors in the bond market; and (2) *indirectly*, i.e., information spillover through corporate disclosure or other information intermediaries.[21] To empirically capture these direct and indirect information flows, we perform the Path analysis. The Path analysis decomposes the correlation between the source variable (i.e., *TRACE*) and an outcome variable (i.e., crash risk) into their direct path and their indirect paths through mediating variables (Wright 1934; Landsman, Maydew, and Thornock 2012). A direct path includes only one path coefficient, whereas an indirect path includes a path coefficient between the source variable and the mediating variable, as well as a path coefficient between the mediating variable and the outcome variable. The total magnitude of the indirect path is the product of these two path coefficients. The Path analysis therefore enables us to identify the mediating variables, or the specific mechanisms, through which bond market transparency affects stock price crash risk.

More specifically, we examine whether management guidance, analyst forecasts, and media reports work as the mediating variables between bond market transparency and stock price crash risk. On the one hand, the incremental bad news revelation in the bond market following TRACE may influence the information acquisition, interpretation, and dissemination activities of managers and other information intermediaries, such as financial analysts and the media (Batta, Qiu, and Yu 2016; Rickmann 2022). On the other hand, with increased bad news acquisition, interpretation, and dissemination by managers, analysts, and the media, the extent of bad news hoarding and accumulation in stock prices can be reduced, eventually leading to lower stock price crash risk.

To conduct empirical analysis, we measure negative management guidance (*Management Guidance*) as the number of bad news forecasts issued by managers during year *t*.[22] Similarly, we measure negative analyst forecasts (*Analyst Forecasts*) as the number of bad news forecasts issued by analysts during year *t* and negative media reports (*Media Reports*) as the number of earnings-related bad news reported by the media during year *t*. In all three measures, bad news is defined as events with negative market response (CAR) in a three-day window surrounding the event date. We then perform the Path analysis in the main sample by estimating a structural equation model (SEM), which includes three regressions of each mediating variable (i.e., *Management Guidance*, *Analyst Forecasts*, or *Media Reports*) on *TRACE* and a regression of crash risk on *TRACE* and the mediating variables with all the control variables included.

Table 4 reports the estimation results from the Path analysis. The outcome variables are *Crash*, *Ncskew*, and *Duvol* in Panels A, B, and C, respectively. The indirect effect of TRACE on crash risk is the product of the effect of *TRACE* on the mediating variable and the effect of the mediating variable on stock price crash risk. The significance of the indirect effect is estimated by using the Sobel (1982) test statistics.

In Panel A, we find that the indirect effect of *TRACE* on stock price crash risk through each mediating variable is negative and significant in all three columns. Take column (1) as an example: the effect of *TRACE* on *Management Guidance* is 0.111 and the effect of *Management Guidance* on *Crash* is −0.016. Both effects are significant and give rise to a significant, indirect effect of *TRACE* on stock price crash risk (coef. = −0.002, t = −2.32). Moreover, we find a significant direct effect of *TRACE* on stock price crash risk. Results in Panels B and C are similar, where the indirect effects of TRACE on stock price crash risk are significant in all six columns, and the direct effects are also significant.

The results from the Path analysis in Table 4, taken together, suggest that TRACE affects stock price crash risk directly, consistent with direct information spillover from the bond market into the stock market.[23] Moreover, the results also suggest that TRACE affects stock price crash risk indirectly through its effects on mediating variables, such as management guidance, analyst forecasts, and media reports.

Robustness Tests

As an alternative way to address the treatment effect heterogeneity problem, following Baker et al. (2022), we examine the effect of each cohort of treatment (i.e., each phase of TRACE) on crash risk separately using clean control groups. We construct four separate samples for each phase of TRACE (i.e., I, II, IIIa, and IIIb) in a six-year window

[21] Managers and other information intermediaries may not only learn from the bond market, but also change their reporting incentives following TRACE. For example, managers may issue more earnings forecasts due to the increased litigation risk following TRACE (Rickmann 2022).

[22] We measure only the bad news of management guidance because we focus on bad news spillover from the bond market into the stock market in our study.

[23] In untabulated tests, we directly control for variables capturing firms' public information environment, such as management forecasts, analyst coverage, and media reports, in our baseline regressions and continue to find a significant effect of TRACE on stock price crash risk.

TABLE 4

Results of Path Analysis

Panel A: Crash Risk Measured as *Crash*

	(1) *Management Guidance*	(2) *Analyst Forecasts*	(3) *Media Reports*
Direct Path			
*p[*TRACE effect, $Crash_{t+1}$*]*	−0.046**	−0.046**	−0.046**
	(−2.56)	(−2.56)	(−2.56)
Mediated path			
(*a*) *p[*TRACE effect, *Mediating Variable*$_t$*]*	0.111***	0.289***	0.372***
	(5.19)	(3.16)	(5.10)
(*b*) *p[Mediating Variable*$_t$, $Crash_{t+1}$*]*	−0.016***	−0.004**	−0.003**
	(−2.59)	(−2.34)	(−2.36)
Indirect effect (a × b)	−0.002**	−0.001*	−0.001**
	(−2.32)	(−1.88)	(−2.14)
Observations	30,725	30,725	30,725

Panel B: Crash Risk Measured as *Ncskew*

	(1) *Management Guidance*	(2) *Analyst Forecasts*	(3) *Media Reports*
Direct Path			
*p[*TRACE effect, $Ncskew_{t+1}$*]*	−0.124***	−0.124***	−0.124***
	(−3.86)	(−3.86)	(−3.86)
Mediated path			
(*a*) *p[*TRACE effect, *Mediating Variable*$_t$*]*	0.111***	0.289***	0.372***
	(5.19)	(3.16)	(5.10)
(*b*) *p[Mediating Variable*$_t$, $Ncskew_{t+1}$*]*	−0.025**	−0.007**	−0.011***
	(−2.03)	(−2.11)	(−3.59)
Indirect effect (a × b)	−0.003*	−0.002*	−0.004***
	(−1.89)	(−1.75)	(−2.94)
Observations	30,725	30,725	30,725

Panel C: Crash Risk Measured as *Duvol*

	(1) *Management Guidance*	(2) *Analyst Forecasts*	(3) *Media Reports*
Direct Path			
*p[*TRACE effect, $Duvol_{t+1}$*]*	−0.052***	−0.052***	−0.052***
	(−3.48)	(−3.48)	(−3.48)
Mediated path			
(*a*) *p[*TRACE effect, *Mediating Variable*$_t$*]*	0.111***	0.289***	0.372***
	(5.19)	(3.16)	(5.10)
(*b*) *p[Mediating Variable*$_t$, $Duvol_{t+1}$*]*	−0.011**	−0.003**	−0.005***
	(−2.07)	(−2.42)	(−3.85)
Indirect effect (a × b)	−0.001*	−0.001*	−0.002***
	(−1.92)	(−1.92)	(−3.07)
Observations	30,725	30,725	30,725

***, **, * Indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.
Table 4 reports the regression results for the Path analysis, which estimates the direct effect of TRACE on stock price crash risk, as well as the indirect effects of TRACE on stock price crash risk through the mediating variables (i.e., *Management Guidance*, *Analyst Forecasts*, or *Media Reports*). The outcome variables are *Crash* in Panel A, *Ncskew* in Panel B, and *Duvol* in Panel C. The mediating variables are *Management Guidance* in column (1), *Analyst Forecasts* in column (2), and *Media Reports* in column (3) in all the three panels. In parentheses below the coefficient estimates, we report t-statistics based on robust standard errors adjusted for firm-level clustering.
All variables are defined in Appendix B.

TABLE 5

Robustness Tests

Panel A: Effects of Each Phase of TRACE

	(1) $Crash_{t+1}$	(2) $Ncskew_{t+1}$	(3) $Duvol_{t+1}$
$Treat_i \times Post_t$ in Phase I Sample	−0.056*	−0.169***	−0.079***
	(−1.73)	(−2.88)	(−2.71)
$Treat_i \times Post_t$ in Phase II Sample	−0.068***	−0.130***	−0.064***
	(−3.29)	(−3.18)	(−3.28)
$Treat_i \times Post_t$ in Phase IIIa Sample	−0.020	−0.117***	−0.052***
	(−1.13)	(−3.08)	(−3.08)
$Treat_i \times Post_t$ in Phase IIIb Sample	−0.044**	−0.142***	−0.056***
	(−2.02)	(−3.33)	(−2.97)

Panel B: Alternative Sample Period

	(1) $Crash_{t+1}$	(2) $Ncskew_{t+1}$	(3) $Duvol_{t+1}$
TRACE effect	−0.086***	−0.167***	−0.072***
	(−4.42)	(−4.66)	(−4.31)
Control Variables	Yes	Yes	Yes
Firm and Year FE	Yes	Yes	Yes
R^2	0.266	0.282	0.282
Observations	18,656	18,656	18,656

***, **, * Indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.
Table 5 reports the regression results for robustness tests. In both panels, the dependent variables are *Crash* in column (1), *Ncskew* in column (2), and *Duvol* in column (3). Panel A reports the regression results for the sample of each phase of TRACE. We construct four separate samples for each phase of TRACE (i.e., I, II, IIIa, IIIb) in a six-year window surrounding the implementation year of each phase. In each phase sample, *Treat* equals 1 if a firm is a treatment firm and 0 otherwise, and *Post* equals 1 if a year is in the post-TRACE period and 0 otherwise. Panel B reports the regression results for an alternative sample period, which is one year before the first TRACE implementation in 2002 and one year after the last TRACE implementation in 2005. In parentheses below the coefficient estimates, we report t-statistics based on robust standard errors adjusted for firm-level clustering.
All variables are defined in Appendix B.

surrounding the implementation year of each phase. In each phase sample, we only include firms that are affected by the specific phase as the treatment group and never-treated firms as the control group. We define two indicator variables in each phase sample: *Treat*, which equals 1 if a firm is a treatment firm and 0 otherwise; and *Post*, which equals 1 if a year is in the post-TRACE period and 0 otherwise. We then regress one of the crash measures on *Treat* × *Post* and control variables, as well as firm and year fixed effects in each phase sample. We report the coefficients of *Treat* × *Post* in each of the four phase samples, respectively, in Table 5, Panel A. As shown, in all four samples, the coefficients of *Treat* × *Post* are negative across all three columns. The coefficients, except for one of them, are also statistically significant. Overall, these findings show that each phase of TRACE has a significant effect on reducing stock price crash risk. The results also alleviate the concerns that our main findings are driven by certain characteristics of bonds affected by one particular phase of TRACE or by any confounding factors that coincide with one specific phase of TRACE.

We also check the robustness of main findings to alternative sample periods. Our sample period is 1999–2008, which is three years before the first TRACE implementation in 2002 and three years after the last TRACE implementation in 2005. Although this three-year wedge is arbitrary, it is a trade-off between sample size and the chances of introducing confounding events into our sample period. As a robustness check, we reconstruct our sample by including one year before and after the TRACE implementation period (i.e., 2001–2006) and re-estimate our regression model (1).[24]

[24] One advantage of using this shorter sample period is that it is during the post-Regulation Fair Disclosure (Reg FD) period and does not overlap with the 2007–2008 financial crisis period.

TABLE 6

The Effect of Bond Default Risk on TRACE-Crash Risk Relation

	(1) $Crash_{t+1}$	(2) $Ncskew_{t+1}$	(3) $Duvol_{t+1}$
(*a*) TRACE effect for *Low_Rating* firms	−0.110***	−0.209***	−0.085***
	(−3.38)	(−4.12)	(−3.67)
(*b*) TRACE effect for *High_Rating* firms	−0.021	−0.072*	−0.034
	(−0.91)	(−1.66)	(−1.63)
Difference (a − b)	−0.089**	−0.137**	−0.051*
	(−2.26)	(−2.13)	(−1.69)
Control Variables	Yes	Yes	Yes
Firm and Year FE	Yes	Yes	Yes
R^2	0.204	0.238	0.239
Observations	29,790	29,790	29,790

***, **, * Indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.
Table 6 reports the regression results estimating the effect of bond default risk on the relation between TRACE and crash risk. The dependent variables are *Crash* in column (1), *Ncskew* in column (2), and *Duvol* in column (3). *Low_Rating* (*High_Rating*) is an indicator variable that equals 1 if the credit rating of a bond is below (equal or above) BBB− in the year before the TRACE coverage and 0 otherwise. In parentheses below the coefficient estimates, we report t-statistics based on robust standard errors adjusted for firm-level clustering.
All variables are defined in Appendix B.

Table 5, Panel B presents the estimated results. As shown, we continue to find negative and significant effects of the TRACE implementation on stock price crash risk across all three columns.

Cross-Sectional Variations in the Relation between Bond Market Transparency and Stock Price Crash Risk

In this section, we investigate the cross-sectional variations in the relation between bond market transparency and stock price crash risk. As discussed earlier, the effect of bond market transparency on reducing stock price crash risk depends on: (1) the extent to which the bond market leads the stock market in revealing bad news; and (2) the role of

TABLE 7

The Effect of Institutional Stock Ownership on TRACE-Crash Risk Relation

	(1) $Crash_{t+1}$	(2) $Ncskew_{t+1}$	(3) $Duvol_{t+1}$
(*a*) TRACE effect for *Low_InstOwn* firms	−0.085**	−0.231***	−0.112***
	(−2.49)	(−3.79)	(−3.70)
(*b*) TRACE effect for *High_InstOwn* firms	−0.045**	−0.102***	−0.040**
	(−2.40)	(−2.83)	(−2.37)
Difference (a − b)	−0.040	−0.129*	−0.072**
	(−1.07)	(−1.89)	(−2.17)
Control Variables	Yes	Yes	Yes
Firm and Year FE	Yes	Yes	Yes
R^2	0.204	0.237	0.238
Observations	29,953	29,953	29,953

***, **, * Indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.
Table 7 reports the regression results estimating the effect of institutional ownership on the relation between TRACE and crash risk. The dependent variables are *Crash* in column (1), *Ncskew* in column (2), and *Duvol* in column (3). *Low_InstOwn* (*High_InstOwn*) is an indicator variable that equals 1 if institutional stock ownership of a firm is lower (higher) than the sample median in the year before the TRACE coverage and 0 otherwise. In parentheses below the coefficient estimates, we report t-statistics based on robust standard errors adjusted for firm-level clustering.
All variables are defined in Appendix B.

TABLE 8

The Effect of Financial Reporting Opacity on TRACE-Crash Risk Relation

Panel A: Lower Reporting Comparability

	(1) $Crash_{t+1}$	(2) $Ncskew_{t+1}$	(3) $Duvol_{t+1}$
(*a*) TRACE effect for *Low_Comp* firms	−0.105***	−0.220***	−0.100***
	(−3.34)	(−4.25)	(−4.11)
(*b*) TRACE effect for *High_Comp* firms	−0.021	−0.097*	−0.041*
	(−0.74)	(−1.87)	(−1.70)
Difference (a − b)	−0.083**	−0.123*	−0.058*
	(−2.01)	(−1.73)	(−1.75)
Control Variables	Yes	Yes	Yes
Firm and Year FE	Yes	Yes	Yes
R^2	0.205	0.240	0.242
Observations	27,321	27,321	27,321

Panel B: Higher Abnormal Accruals

	(1) $Crash_{t+1}$	(2) $Ncskew_{t+1}$	(3) $Duvol_{t+1}$
(*a*) TRACE effect for *High_ABACC* firms	−0.096***	−0.255***	−0.109***
	(−3.03)	(−4.75)	(−4.54)
(*b*) TRACE effect for *Low_ABACC* firms	−0.015	−0.069*	−0.029
	(−0.73)	(−1.84)	(−1.64)
Difference (a − b)	−0.081**	−0.186***	−0.080***
	(−2.25)	(−2.98)	(−2.79)
Control Variables	Yes	Yes	Yes
Firm and Year FE	Yes	Yes	Yes
R^2	0.204	0.237	0.238
Observations	29,953	29,953	29,953

***, **, * Indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.
Table 8 reports the regression results estimating the effect of financial reporting opacity on the relation between TRACE and crash risk. The dependent variables are *Crash* in column (1), *Ncskew* in column (2), and *Duvol* in column (3), in each panel. Financial reporting opacity is measured as lower financial statement comparability (*Low_Comp*) in Panel A and higher abnormal accruals (*High_ABACC*) in Panel B. In parentheses below the coefficient estimates, we report t-statistics based on robust standard errors adjusted for firm-level clustering.
All variables are defined in Appendix B.

bad news dissemination in reducing stock price crash risk. The former, as suggested by prior studies, is related to bond default risk and stock investor sophistication (Easton et al. 2009; Even-Tov 2017), and the latter is related to financial reporting opacity at the firm level (e.g., Jin and Myers 2006; Hong et al. 2017). We therefore expect to find that the effect of bond market transparency on stock price crash risk varies with bond default risk, stock investor sophistication, and financial reporting opacity.

We use bonds' credit ratings to measure their default risk following prior studies (e.g., Even-Tov 2017) and define *Low_Rating* (*High_Rating*) as an indicator variable, which equals 1 if the credit rating of a firm's bond is below (equal or above) BBB− in the year *before* the TRACE coverage and 0 otherwise. We then replace the full set of indicators (i.e., phase × relative-year indicators) in model (1) with the full set of indicators interacted with *Low_Rating* and the full set of indicators interacted with *High_Rating* and re-estimate the regressions. In this way, we create the full set of indicators for the two subsamples of treatment firms (i.e., treatment firms with *Low_Rating* and treatment firms with *High_Rating*), respectively, and the never-treated firms act as the control group for the two subsamples of treatment firms. Based on the estimates for these two sets of indicators, we calculate the overall effect of TRACE for the subsamples of firms with *Low_Rating* and *High_Rating*, respectively. The results are reported in Table 6. As shown, the differences in the TRACE effect between firms with *Low_Rating* and those with *High_Rating* are negative and significant across all three columns. These findings suggest that the effect of TRACE on reducing stock price crash risk is more pronounced among firms with higher default risk bonds.

To capture stock investor sophistication, we follow Even-Tov (2017) and define *Low_InstOwn* (*High_InstOwn*) as an indicator variable, which equals 1 if institutional stock ownership of a firm is lower (higher) than the sample median in the year *before* the TRACE coverage and 0 otherwise. Institutional ownership is measured as the number of common shares held by institutional investors at the end of year divided by the number of common shares outstanding. We then replace the full set of indicators (i.e., phase × relative-year indicators) in model (1) with the full set of indicators interacted with *Low_InstOwn* and the full set of indicators interacted with *High_InstOwn* and re-estimate the regressions. We further calculate the overall effect of TRACE, separately, for the subsamples of firms with *Low_InstOwn* and *High_InstOwn*. The results are presented in Table 7. As shown, the differences in the TRACE effect between firms with *Low_InstOwn* and those with *High_InstOwn* are negative across all three columns and statistically significant in columns (2) and (3). Overall, these results suggest that the effect of TRACE on reducing stock price crash risk is stronger for firms with lower institutional stock ownership.

Following prior studies (e.g., Hutton et al. 2009; Kim, Li, Lu, and Yu 2016), we use financial reporting comparability and absolute abnormal accruals to capture firms' financial reporting opacity. Specifically, we define *Low_Comp* (*High_Comp*) as an indicator variable, which equals 1 if a firm's financial reporting comparability is lower (higher) than the sample median in the year *before* the TRACE coverage and 0 otherwise. We measure financial reporting comparability as the average of the comparability between a firm and its industry peers during the 16 previous quarters (De Franco, Kothari, and Verdi 2011). Similarly, we define *High_ABACC* (*Low_ABACC*) as an indicator variable, which equals 1 if a firm's absolute value of abnormal accruals is higher (lower) than the sample median in the year *before* the TRACE coverage and 0 otherwise. We then replace the full set of indicators (i.e., phase × relative-year indicators) in model (1) with the full set of indicators interacted with the low financial opacity indicator and the full set of indicators interacted with the high financial opacity indicator and re-estimate the regressions. We further calculate the overall effect of TRACE, separately, for the subsamples of high-opacity and low-opacity firms. The results are presented in Table 8, Panels A and B. In Panel A, we find that the differences in the TRACE effect between firms with *Low_Comp* and firms with *High_Comp* are negative and significant in all three columns. Panel B shows consistent results when we use *High_ABACC* to capture firms' reporting opacity. Collectively, these findings suggest that the effect of TRACE on reducing stock price crash risk is more pronounced for firms with more opaque financial reporting.

V. CONCLUSION

Our study investigates whether the TRACE implementation accelerates bad news revelation in the bond market and eventually contributes to reducing crash risk in the stock market. Using a difference-in-differences research design, we find robust evidence that increased bond market transparency following TRACE leads to lower stock price crash risk. We further find that both bad news revelation in the bond market and bad news spillover from the bond market into the stock market become stronger following TRACE. Further evidence suggests that bad news revealed in the bond market spills over to the stock market not only directly (i.e., stock investors learning from the bond market), but also indirectly (i.e., through management guidance, analyst forecasts, and media reports). Our main findings are robust to each phase of TRACE implementation and alternative sample periods. Finally, we find that the mitigation effect of bond market transparency on stock price crash risk is stronger for firms with higher default risk bonds, lower institutional stock ownership, and more opaque financial reporting.

Our study highlights the role of the bond market in revealing bad news information to public investors. We find that improved bond market transparency following the TRACE implementation facilitates bad news spillover from the bond market to the stock market. Our study also shows that the TRACE implementation, representing a regulatory change in the bond market, generates a positive externality in reducing crash risk in the stock market.

Future research may further investigate the impact of improved bad news revelation in the bond market on other participants, such as short sellers in the stock market. Short sellers are specialized in trading on bad news, and they profit from their information advantage over other investors. It is interesting to study whether improved bad news revelation in the bond market following TRACE narrows the information advantage that short sellers have over other investors. Moreover, to the extent that improved transparency in the bond market facilitates information dissemination to creditors in general (e.g., Badoer and Demiroglu 2019), another avenue for future research is to investigate whether and how improved bond market transparency affects the role or usefulness of accounting information in debt contracting. Lastly, whereas prior research primarily focuses on the feedback effects of information contained in stock prices on corporate financing and investment decisions (Bond, Edmans, and Goldstein 2012), the implementation of TRACE provides an ideal setting to examine the feedback effects of bond prices on managerial real decisions.

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APPENDIX A

Phases of TRACE Implementation

This table presents the effective dates of the TRACE implementation. The effective dates and descriptions of each implementation phase are collected from FINRA's TRACE Fact Book (https://www.finra.org/sites/default/files/2019-08/2018-trace-fact-book.pdf).

Phase	Date	Bonds Affected
I	July 1, 2002	Investment-grade debt securities having an initial issue size of $1 billion or greater.
TRACE50	July 1, 2002	Fifty noninvestment-grade (high-yield) securities disseminated under FIPS that were transferred to TRACE.[a]
II	March 3, 2003	All investment-grade, TRACE-eligible securities of at least $100 million par value (original issue size) or greater, rated A3/A− or higher. On April 14, 2003, 120 investment-grade, TRACE-eligible securities rated Baa/BBB are included.
IIIa	October 1, 2004	All bonds that are not qualified for delayed dissemination; usually they are bonds with rating of BBB− or higher.
IIIb	February 7, 2005	All public transactions subject to delayed dissemination; usually they are bonds with rating of BB+ or lower.

[a] The TRACE50 list was updated on July 13, 2003, October 15, 2003, January 15, 2004, April 14, 2004, and July 14, 2004 (Asquith et al. 2013).

APPENDIX B

Variable Definitions

Variables	Definition
Crash	An indicator variable that equals 1 if a firm experienced one or more crash week(s) in a year and 0 otherwise. A crash week is a week in which a firm-specific weekly return falls 3.2 standard deviations below the mean of firm-specific weekly returns over a fiscal year. Standard deviations of 3.2 generate a frequency of 0.1 percent in the normal distribution.
Ncskew	The negative conditional return skewness, which is calculated by taking the negative of the third moment of firm-specific weekly returns for each sample year divided by the standard deviation of firm-specific weekly returns raised to the third power.
Duvol	The down-to-up volatility, which is calculated as the natural logarithm of the standard deviation of weekly stock returns during the weeks in which they are lower than their annual mean ("down" weeks) over the standard deviation of weekly stock returns during the weeks in which they are higher than their annual mean ("up" weeks).

(continued on next page)

APPENDIX B (continued)

Variables	Definition
Dturn	The average monthly turnover ratio in a fiscal year minus that of the previous fiscal year.
Sigma	The standard deviation of firm-specific weekly returns over a fiscal year.
Return	The mean of firm-specific weekly returns over a fiscal year.
Size	The logarithm of a firm's total assets.
MB	The market-to-book ratio.
LEV	The ratio of long-term debts over total assets.
ROA	The ratio of income before extraordinary items divided by total assets.
ABACC	The absolute value of discretionary accruals in a fiscal year. The discretionary accruals are estimated from a modified Jones model, following Dechow, Sloan, and Sweeney (1995).
Bond CAR $(-10, -2)$	Bond market response to the negative earnings surprise (SUE) during the pre-earnings announcement window $(-10, -2)$.
Stock CAR $(-10, -2)$	Stock market response to the negative earnings surprise (SUE) during the pre-earnings announcement window $(-10, -2)$.
Bond MRET	Monthly bond returns.
Stock MRET	Monthly stock returns.
SUE	Quarterly standardized earnings surprises.
TRACE	An indicator variable that equals 1 if the bond of a firm is covered by TRACE in a given year and 0 otherwise. Data source: FINRA.
Management Guidance	The number of bad news forecasts issued by managers during the year, where bad news is defined based on the sign of market response (*CAR*) in a three-day window surrounding the event date. Data source: I/B/E/S Guidance.
Analyst Forecasts	The number of bad news forecasts issued by analysts during the year, where bad news is defined based on the sign of market response (*CAR*) in a three-day window surrounding the event date. Data source: I/B/E/S.
Media Reports	The number of earnings-related bad news reported by media during the year, where bad news is defined based on the sign of market response (*CAR*) in a three-day window surrounding the event date. Data source: Capital IQ.
Low_Rating (*High_Rating*)	An indicator variable that equals 1 if the credit rating of a firm's bond is below (equal or above) BBB− in the year before the TRACE coverage and 0 otherwise.
High_InstOwn (*Low_InstOwn*)	An indicator variable that equals 1 if institutional ownership of a firm is higher (lower) than the sample median in the year before the TRACE coverage and 0 otherwise, where institutional ownership is measured as the number of common shares held by institutional investors at the end of the year divided by the number of common shares outstanding. Data source: Thomson Reuters.
Low_Comp (*High_Comp*)	An indicator variable that equals 1 if financial reporting comparability of a firm is lower (higher) than the sample median in the year before the TRACE coverage and 0 otherwise, where financial reporting comparability is measured as the average of the comparability between the firm and its industry peers in the previous 16 quarters, following De Franco et al. (2011).
High_ABACC (*Low_ABACC*)	An indicator variable that equals 1 if the absolute value of discretionary accruals of a firm is higher (lower) than the sample median in the year before the TRACE coverage and 0 otherwise.