Trade Transparency and Management Earnings Forecasts*†

Deniz Anginer
Simon Fraser University

Stephen Baginski
University of Georgia

Xue Han
San Francisco State University

Çelim Yıldızhan
Koç University

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Abstract. Employing a plausibly exogenous shock that increased the extent to which private information is revealed in debt markets – implementation of the Trade Reporting and Compliance Engine (TRACE) - we document a change in longer-run management earnings forecast policy. We find that managers decreased forecast frequency over the three-year period post-TRACE. The decline was greater for bad news management earnings forecasts when the pre-disclosure information environment was weaker and for firms that were closer to credit default. These findings, taken in conjunction with prior findings of increases in earnings forecasting activity in response to declines in the strength of the information environment, suggest that the inverse relation between changes in the strength of the information environment and management earnings forecasting activity is symmetric. The finding that managers are also willing to curtail management earnings forecasting when the informational benefits of forecasting are reduced is significant given the actual and perceived costs of doing so. Our results also suggest that prior research understates the informational benefits of TRACE, per se, because of the decline in management earnings forecasting pursuant to TRACE.

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† Deniz Anginer can be reached at deniz_anginer@sfu.ca. Stephen P. Baginski can be reached at baginski@uga.edu. Snow Xue Han can be reached at hanx@sfsu.edu. Çelim Yıldızhan can be reached at cyildizhan@ku.edu.tr.

1. Introduction

Management earnings forecasts are an important source of information for the securities markets (Beyer, Cohen, Lys, and Walther 2010). Accordingly, the investment public, regulators, and researchers have a substantial interest in understanding the factors that shape a firm's earnings forecast disclosure policy. Recently, research has focused on whether management reacts to a deterioration in its firm's information environment by substituting its own disclosure of private information, the idea being that the informational benefits of disclosing private information increase when the information environment becomes weaker.

Anantharaman and Zhang (2011) and Balakrishnan, Billings, Kelly, and Ljungqvist (2014) exploit the plausibly exogenous variation in analyst coverage provided by brokerage house mergers and closures and find evidence that managers respond to the reduction in coverage (a *decrease* in the strength of the firm's information environment) by *increasing the quantity* of earnings guidance. Baginski and Hinson (2016) exploit the plausibly exogenous reduction of industry-level information from competitor decisions to cease forecasting (a *decrease* in the strength of the industry information environment) and find evidence that previously non-forecasting peer firms *initiate* management earnings forecasting.[1], In summary, reductions in the strength of the pre-disclosure information environment increase the information benefits of management earnings forecasts and thus increase the likelihood that managers will publicly disclose their forecasts.

But what happens when there is an *increase* in the strength of the firm's information environment? If a manager currently provides earnings forecasts, investors view the disclosure policy as an implicit commitment to provide earnings forecasts in the future (Graham, Harvey, and Rajgopal 2005; Einhorn and Ziv 2008). Given that past earnings forecasting activity implies that

[1] In a similar vein, using an alternative identification strategy, Kim, Shroff, Vyas, and Wittenberg-Moerman (2018) find that managers are more likely to *issue earnings forecasts and forecast more frequently* when traded CDSs reference their firms (an indicator of information environment deterioration from lack of monitoring).

managers have systems in place to produce private information (i.e., they are likely currently privately informed), a reduction of forecast activity could be interpreted by investors as a signal of expected poor performance (Dye 1985; Houston, Lev, and Tucker 2010; Chen, Matsumoto, and Rajgopal 2011) or a lack of control over the operating environment (Trueman 1986). Because of the substantial potential costs of partially or fully curtailing current forecasting activity, it is not clear if the finding of increased management forecasting activity when the information environment weakens implies a decrease in management forecasting activity when the information environment strengthens. Therefore, we ask the question: "Does an *increase* in the strength of the information environment lead to a *reduction* in management earnings forecasting activity?"

To address our research question, we exploit a plausibly exogenous shock introduced by a regulatory change that increased the amount of private information reflected in bond prices – the implementation of the Trade Reporting and Compliance Engine (TRACE) – to examine whether management's earnings forecast activity decreased after the act was implemented. Almost all bond trading occurs over the counter. Before the implementation of TRACE, price transparency in the bond market was limited, with transaction details known only to the parties engaging in the trade. This resulted in a highly opaque trading environment relative to the equity market. Starting in 2002, the Financial Regulation Authority (FINRA), prompted by the SEC, started requiring dealers to collect and disclose information on bond transactions. Upon completion of the full TRACE implementation, FINRA was able to disseminate publicly transaction information on virtually all corporate bonds in real time. As one would expect, this regulatory change greatly improved the public information environment. Several papers have shown that the introduction of TRACE has led to improvements in liquidity and price discovery in the corporate bond market (Goldstein, Hotchkiss and Sirri 2007; Bessembinder and Maxwell 2008; Edwards, Harris, and Piwowar 2007). These results are consistent with the theoretical literature showing that transparency in financial market transactions increases the accuracy and speed with which information is priced (Madhavan 1995; Bloomfield and O'Hara 1999).[2]

We focus on the effects of the TRACE information environment shock on management earnings forecast policy, and thus our hypotheses are grounded in the theoretical information-related benefits of high-quality forecasts disclosed under that policy (i.e., greater liquidity, lower bid-ask spread, and a lower cost of capital). By *policy*, we mean a firm pre-commitment to high quality mandatory disclosure rules (e.g., Leuz and Verrecchia 2000) or an observable demonstration over time that a firm will provide credible voluntary disclosures to remove information asymmetry regardless of disclosure content (i.e., disclosure is not "selective"). Greater frequency of disclosures by managers over time is evidence that selective disclosure is less likely. Accordingly, our focus on *policy* requires us to measure management forecasting activity over a longer run.[3] Our focus on information *quality* requires the choice of a management forecast that is a (relatively) more precise measure of future valuation-related payoffs (e.g., future earnings, future free cash flows, or future dividends) because the ability to remove information asymmetry depends on signal precision (Kim and Verrecchia 1991). Accordingly, we examine management *earnings* forecasts rather than sales forecasts, cash forecasts, or qualitative forecasts (e.g., linguistic tone). Current earnings are superior predictors of future earnings, free cash flows, and dividends. Sales forecasts omit expenses, cash forecasts omit expected accruals, and linguistic tone is imprecisely related to future payoffs.

Consistent with the increased transparency of prices leading to lower information asymmetry, greater liquidity, and hence fewer benefits to a policy of issuing regular management earnings forecasts, we find a decrease in the number of voluntary management earnings forecasts

[2] Bessembinder and Maxwell (2008) describe TRACE as "… a major shock to this previously opaque market."

[3] We use a three-year period to measure the quantity of management earnings forecast disclosures instead of immediate short-run disclosure and replicate our results for a six-year period (untabulated) as well.

over a longer-run window pursuant to TRACE. We provide several supplementary analyses to support our main result. First, given that a bond investment has an asymmetric payoff (i.e., a limited potential upside and a significant loss from distress or default), bond prices are more likely to reflect private information about the downside risk of the firm. TRACE's increased bond price transparency places that unfavorable private information in the public domain, which in turn, decreases the benefits of bad news management earnings forecasts in particular. Thus, we predict that TRACE led to a greater decrease in bad relative to good news management forecasts, and our results support the prediction. Second, regardless of the motive for continuing to issue forecasts after TRACE, the post-TRACE forecasts should have less of an effect on security prices because of the relatively stronger pre-disclosure information environment after TRACE-induced price transparency (e.g., Kim and Verrecchia 1991). We find that short-term equity price reactions to management forecasts decreased after TRACE was fully implemented.

Finally, we test two cross-sectional predictions about when the substitution of TRACE transparency for management earnings forecasts should be greatest. First, when the public information environment is weak, which we proxy by low analyst following and low institutional ownership, the effects of TRACE-induced decreases in information asymmetry should be greater and thus lead to greater decreases in management earnings forecasts. Second, the impact of trade transparency should also depend on a firm's credit risk. As mentioned earlier, bond prices contain more information about the downside risk of a firm. The information coming from post-TRACE bond prices would therefore have a larger impact on firms that are close to the default boundary. We would thus expect firms that have greater credit risk to reduce management earnings forecasts relatively more in response to TRACE. Our evidence is consistent with both conjectures.

In summary, and with consideration of the prior management earnings forecasting literature which documents increases in earnings forecasting activity in response to deterioration in the information environment (e.g., Anantharaman and Zhang 2011; Balakrishnan et al. 2014; Baginski and Hinson 2016), we contribute by providing empirical evidence that the inverse relation between changes in the strength of the information environment and management earnings forecasting activity is symmetric. Plausibly exogenous shocks that decrease (increase) the strength of the information environment increase (decrease) the quantity of management earnings forecasts of affected firms. Given that we use a bond market-related change in the information environment, our results also add to the literature documenting the effects of bond market conditions on management earnings forecasts (e.g., Kim et al. 2018). Finally, we contribute to the literature on the longer-run capital market effects of TRACE by showing that the positive information effects of TRACE implementation might have been somewhat offset by firms' substitution out of management forecasts.

2. Hypotheses

We develop our hypotheses in the sub-sections that follow. Our primary hypothesis is about the effect of TRACE on management earnings forecast quantity (i.e., the substitution effect). The remainder of the hypotheses are about more nuanced relations that we expect to exist given the theory underlying the benefits of management forecasts and the nature of bond markets. One hypothesis conditions the substitution effect on the nature of management forecast news (i.e., a greater substitution effect for bad news). Another hypothesis is about a key condition underlying the substitution effect (i.e., the reduction in the benefits of issuing a management forecast). Finally, we have two cross-sectional hypotheses about firm-specific conditions that should enhance the substitution effect (i.e., weaker firm-specific information environment and greater credit risk).

2.1 The substitution effect

Prior to the introduction of TRACE, properly incentivized and monitored, value-maximizing managers set a disclosure policy to maximize net disclosure benefits, taking into account all markets in which the firm raises capital, the regulatory agencies and legal systems which could impose disclosure-related costs (e.g., litigation risk), the industry characteristics which could also impose disclosure-related costs (e.g., proprietary costs linked to competitive conditions), and the existence of alternative sources of information. We assume that managers are likely aware (both pre-TRACE and post-TRACE) that, over the longer term, investors and creditors are able to infer their forecast policy by regularly observing conditions that drive short-run manager incentives (e.g., corporate control contests, insider trades), the release and content of management earnings forecasts, and the outcomes of earnings disclosed in a relatively short period after forecast issuance (e.g., the actual earnings release). Thus, the "repeated game" that exists in the longer run enhances monitoring, establishes manager reputation, and leads to a credible disclosure policy (Stocken 2000).

King et al. (1990) identify the key benefit of a high-quality management earnings forecast policy to the firm and its shareholders as the removal of information asymmetry, accomplished by credibly adjusting market expectations either upward or downward (i.e., not suppressing disclosure in the face of management's bad news private information). The specific benefits are greater liquidity, lower bid-ask spread, and a lower cost of capital achieved via some combination of lower information asymmetry (Amihud and Mendelson 1986; Diamond and Verrecchia 1991; Easley and O'Hara 2004) and lower information risk (Barry and Brown 1985; Handa and Linn 1993; Coles, Loewenstein, and Suay 1995; Lambert, Luez, and Verrecchia 2007, 2012).[4]

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[4] Management earnings forecasts can also be motivated by incentives other than the goals of reducing information asymmetry or information risk. Other short-run, non-cost of capital-related, strategic, management forecast disclosure benefits include conveying good performance (Verrecchia 1983, Miller 2002), manipulating market beliefs around insider and compensation-related transactions (e.g., Noe 1999; Aboody and Kasznik 2000; Cheng, Luo, and Yue 2013),

The lack of reporting for bond transaction data in the pre-TRACE information environment caused private information underlying bond trades to remain private, yielding an information disadvantage to parties not involved in the trade. Removing such information disadvantage (i.e., removing information asymmetry) is a benefit of releasing a management forecast, and thus, in the pre-TRACE period, managers would issue a management forecast to remove the information disadvantage, assuming that the benefit of removing the information asymmetry exceeded the costs of disclosure.

The introduction of TRACE increased the public dissemination of private information, leading to an increase in the informational efficiency of bond prices. The bond price-reflection of private information would, in turn, reduce the benefits of alternative sources of information supplied by information intermediaries and managers. With respect to information intermediaries, Badoer and Demiroglu (20197) show that the introduction of TRACE has significantly reduced the market reaction to rating downgrades by credit rating agencies.

We argue that the better price discovery of private information could also act as a substitute for management earnings forecasts. The TRACE-mandated dissemination of transaction data is an alternative source of information that can reduce the key benefit of management forecasts: the removal of information asymmetry. In particular, we propose that while in the short run, management forecasts might act as compliments with respect to bond prices (i.e., further explain the information in bond prices), in the longer-run, once TRACE implementation is wide-spread and the bond prices reflect rich information about variables relevant to the credit market, management

avoiding legal liability when bad news is imminent (Skinner 1994, 1997), winning proxy contests (Baginski, Clinton, and McGuire 2014), and obtaining higher prices in takeovers (Brennan 1999). These disclosure acts are generally driven by period-specific conditions, most notably, the news that can be disclosed (i.e., good or bad news).

forecasts should take on more of a supplemental role and thus will be less needed. Accordingly, our primary hypothesis predicts the substitution effect:[5]

***H1**. An increase in bond trade transparency leads to a decrease in the number of management earnings forecasts.*

The tension surrounding this prediction is significant. Once a firm sets a disclosure policy that includes management earnings forecasts, the investment public expects the policy to continue and might penalize a reduction in guidance (Healy and Palepu 2001, Einhorn and Ziv 2008). Firms that provide disclosure develop a reputation of being informed, which in turn increases the manager's implicit commitment to future disclosure and the adverse price reaction of withholding future disclosure. In addition, as bond prices become more transparent and available to more market participants, there will be immediate feedback to creditors (both bondholders and other lenders such as banks). Furthermore, this improved transparency should also impact equity prices. Even-Tov (2017) finds that bond price reaction to earnings announcements has predictive power for post-announcement stock returns and that this predictive ability is driven largely by the bonds of non-investment grade firms, and specifically for bad news. Introduction of TRACE, thus, would improve not only the transparency of bond prices but would also impact the speed with which information gets incorporated in equity values. Potential private information revelation through more transparent prices would pressure managers to supply more information to avoid reputational damage or potential litigation. This could lead managers to maintain the supply of management earnings forecasts post-TRACE.

[5] Greater trade transparency also yields an indirect incentive for managers to disclose less information. If management believes that greater transparency will lead to better monitoring of their actions by the market and the board, then they may reduce the amount of information they disclose (Holmstrom 2005, Adams and Ferreira 2007).

A concurrent paper by Rickmann (2022) focuses on immediate short-run reactions to TRACE implementation during the implementation period and documents increases in shorter-run management forecast activity by firms as they are included in the set of firms required to implement. Rickmann (2022) does not design his study to evaluate the information substitution hypothesis as we do. Because we assess that hypothesis, our focus is on longer-run disclosure policy change after the full implementation of TRACE changed the information environment for the universe of traded bonds. Shorter-run forecasting behavior pursuant to a regulatory change does not imply longer-run forecasting behavior. For example, Baginski and Rakow (2012) document that forecasting behavior during the year immediately following the issuance of Regulation FD is not predictive of future years' behavior.

2.2 Greater substitution effect for bad news

As bondholders have fixed claims against a firm's assets, they face non-linear pay-offs with respect to changes in a firm's value. They have limited gains on the upside but can suffer significant loses on the downside if a firm defaults on its obligations.[6] Post-TRACE, bond prices will contain more information about the downside risk of a firm, thus rendering bad news forecasts less useful in removing information asymmetry. If the substitution hypothesis *H1* above is correct, then there should be fewer bad news management forecasts after TRACE implementation.

> ***H2.*** *An increase in trade transparency leads to a greater decline in the number of bad news voluntary management earnings forecasts compared to good news forecasts.*

[6] Consistent with this view, Easton, Monahan, and Vasvari (2009) and Shivakumar, Urcan, Vasvari, and Zhang (2011) show that bond prices react more to negative surprises than to positive earnings surprises from earnings announcements and management earnings forecasts, respectively.

2.3 Reduction of the benefits of issuing a management forecast

An increase in trade transparency increases the informativeness of bond prices. The substitution of the information in bond prices for the information in management forecasts yields our H1 prediction. However, some firms will continue to issue management earnings forecasts because the benefits lost from the substitution effect are not sufficiently large to change their forecast policy (i.e., forecast benefits continue to outweigh forecast costs in a post-TRACE environment). Accordingly, we expect the information content of (i.e., short-term market reactions to) management forecasts to decline after TRACE implementation.

> ***H3.*** *An increase in trade transparency reduces the short-term market reaction to remaining post-TRACE management earnings forecasts.*

2.4 Cross-sectional hypotheses: Conditional effects on the impact TRACE

The impact of trade transparency should depend on the larger information environment in which the firm operates. In particular, the marginal impact of TRACE on firm disclosures should be greater when there is less information available to investors. That is, when there is significant information already available to investors, the incremental value of information coming from bond prices should be less.

> ***H4.*** *Firms with a weaker information environment experience greater decreases in management earnings forecasts when trade transparency increases.*

The impact of trade transparency should also depend on credit risk of firms. As mentioned earlier, bond prices contain more information about the downside risk of a firm. The information coming from the bond market would therefore have a larger impact on firms that are close to the default boundary. We would thus expect firms that have greater credit risk to reduce management earnings forecasts relatively more in response to TRACE.

H5. *Firms with greater credit risk experience greater decreases in management earnings forecasts when trade transparency increases.*

3. Research Design and Empirical Results Related to Our Primary Substitution Hypothesis

Our sample includes all firms at the intersection of the Thomson Reuters First Call, CRSP, Compustat and IBES databases.[7] After merging, our final sample includes 98,791 firm-quarter observations for the period Q3-1998 to Q1-2008.

3.1 Tests of H1 (the substitution hypothesis)

We use a difference-in-difference (DID) regression approach to examine the impact of TRACE on management earnings forecasting behavior (H1). That is, we compare the changes in management earnings forecasting behavior from before TRACE implementation to after the full implementation of TRACE for firms that were affected by the introduction of TRACE to the same intertemporal changes for firms that were *not* affected by TRACE. Using the common nomenclature in DID analyses, we refer to the affected firms as *Treated* and the after-TRACE period as *Post.* The empirical model is as follows:

$$\begin{aligned} Disclosure_{i,t} = \alpha + \beta Treated_i \times Post_t + \boldsymbol{\theta}'_{i,t}\boldsymbol{X}_{i,t} + \varphi_1 Trend_t \\ + \varphi_2 Trend_t \times Treated_i + \gamma_i + \delta_t + \varepsilon_{i,t} \end{aligned} \quad (1)$$

$Disclosure_{i,t}$ measures management earnings forecast activity for firm *i* in quarter *t*. We use two measures of disclosure: i) a dummy variable that takes on a value of one if the firm has issued

[7] We obtain management forecast characteristics from First Call, stock price, return and volatility information from CRSP, firm financial information from Compustat, and analyst coverage, equity issuances and institutional ownership from IBES.

at least one annual or quarterly earnings forecast in a quarter (*Issue*), ii) the number of annual and quarterly earnings forecasts (*Count*) in a given quarter. As the number of forecasts can be highly skewed, we supplement the count measure with the log of one plus the number of forecasts, *log(1+Count)*.

The TRACE system was implemented in three phases. Phase 1 started with public dissemination of investment grade bond transactions data with issue sizes larger than $1 billion on July 1, 2002. Phase 2 began on March 3, 2003, with public dissemination of all investment grade bonds with issue size larger than $100 million. Phase 3 started on February 7, 2005, with the immediate dissemination of all bond transactions. Accordingly, we divide the sample period into three periods: 1) before dissemination (before July 2002), 2) first dissemination period (July 2002 to September 2004), and 3) complete dissemination (February 2005 onwards). We identify (from the Fixed Income Securities Database) firms that have bonds outstanding as of July 1, 2002 and designate them as *Treated* ($Treated_i$ = 1) because they are affected by the full implementation of TRACE. Since we have firm fixed effects, this variable is omitted in the regression as it does not vary over time. $Treated_i$ equals zero for control firms.

Consistent with our interest in management earnings forecast policy and the resulting need for longer-run measurement of forecast activity, we examine management forecast disclosure in the three years (i.e., 12 quarters) before TRACE implementation began (July 2002) compared to management earnings forecast disclosures in the three years after the full implementation (February 2005).[8] Accordingly, $Post_t$ is a dummy variable equal to one for the 12 quarters after the full

[8] Disclosure behavior tends to be “sticky” but there is little evidence on how long it takes to get that way after a shock to disclosure costs or benefits. Baginski and Rakow (2012) document that, after the passage of Regulation FD, one year of management forecast activity did not predict forecast activity in subsequent years, but two and three years of forecast activity had high predictive power for subsequent years’ activity. Accordingly, we use three years to capture longer-run management forecast policy for our main (tabulated) tests. However, we recognize that the three-year period includes one year pre-FD. The private disclosure existing pre-FD biases the results against our prediction (i.e., pre-TRACE

implementation of TRACE on February 7, 2005, and zero for the 12 prior when implementation began. The dummy variable is also omitted as we include quarter fixed effects.

An alternative research design approach would be to investigate disclosure change "within" the implementation period using a "staggered" DID, as in Rickmann (2022), and a control group of firms with bonds outstanding but not yet required to adhere to TRACE regulations. While this would be a strong research design, it is not amenable to an investigation of long-run disclosure policy for several reasons. First, the time frame between these implementation stages is noticeably short, causing few available quarters to measure management forecasts before the next TRACE stage is implemented. Second, the control group composition would change with each implementation phase, and it is unclear whether previously treated firms should be reclassified as control firms. Third, the assumption that other firms with actively traded bonds were unaffected by TRACE is questionable. Because the details of the TRACE program implementation were made public, it is reasonable to expect managers to incorporate future expected changes in dissemination rules in their current disclosure decisions.[9] For instance, transactions data on investment grade bonds with an initial issue size of at least $1 billion were initially disseminated on July 1, 2002. On March 3, 2003, transactions data on investment grade bonds with issue size greater than $100 million were disseminated publicly. We would expect managers whose firms have investment grade bonds with issue size greater than $100 million but less than $1 billion to anticipate that their firms' bonds trades will be disseminated

forecast activity would be biased downward by including the pre-FD period, making it more difficult to find a decrease post-TRACE). Nonetheless, we replicate our tests using a two-year window to capture forecast policy and find similar results (untabulated, available upon request). Our results are also robust to using a longer window length of twenty-four quarters.

[9] A series of papers including Sant'Anna and Zhao (2020), Baker, Larcker and Wang (2022) and Barrios (2021) document through simulations that the combination of staggered treatment timing and treatment effect heterogeneity, either across groups or over time, leads to biased TWFE (two-way fixed effects) DiD (difference in differences) estimates for the sample-average ATT in staggered DiD regressions and further show that biases from bad controls can arise even when the parallel-trends assumption holds.

publicly in the near future and possibly change their forecasting behavior.[10] Baginski (1987) and Han, Wild, and Ramesh (1989) document information transfer associated with management earnings forecasts, and Pownall and Waymire (1989) document that firms receiving information transfers are less likely to disclose management earnings forecasts. Moreover, it is possible that firms not yet subject to TRACE receive or expect to receive information transfers from the disclosures of firms subject to TRACE and alter their disclosure decisions to free-ride on disclosing firms. Or firms could herd in disclosure depending on the content of the news to be disclosed (e.g., Tse and Tucker 2010; Jorgenson and Kirschenheiter 2012). Disclosure changes by control firms related to receiving information transfers and herding would violate the DID research design requirement that control firms are unaffected by the treatment effect. Finally, it is also possible for a given firm to have bonds that qualify for dissemination at different stages making it difficult to ascertain the impact of trade transparency on disclosure.[11]

A disadvantage of our approach is that longer windows raise the possibility that other regulatory events or other phenomena affect treated firms differently than control firms. The treated firms have listed bonds and the control firms do not. During this period, credit default swaps (CDS) emerged, and bond markets became less efficient (Das, Kalimipalli, and Nayak 2014). As we mention earlier, Kim et al. (2017) document a positive association between management forecast frequency and having a CDS reference the firm. But this condition biases against our H1 prediction because some treated firms will be referenced by a CDS and this will lead to an *increase* in management earnings forecasting, contrary to our prediction of a decline in management earnings forecasting. Control firms will not be affected because they do not have listed bonds.

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[10] The requirement to disclose trades on bonds with issue size greater than $100 million was made public on January 31, 2003.

[11] For instance, a firm may have multiple bonds outstanding with some bonds with issue size greater than $1 billion and some with issue size less than a $1 billion.

$\boldsymbol{X}_{i,t}$ is a vector of firm-level controls selected because of their association with management earnings forecast disclosure and as will be seen shortly, association with the treatment condition. *Firm Size* is the natural log of total assets. *Leverage* is total liabilities divided by total assets. *Market-to-Book* is the market value of equity divided by the book value of total equity at the end of each quarter. *Return Volatility* is the standard deviation of a firm's daily stock returns during a given quarter measured as a percentage. *ROA* is return on assets, computed as income before extraordinary items divided by total assets. *Mid Z-score* is an indicator variable that takes the value of one if a firm's Altman (1968) Z-score falls within the middle quintile of the sample distribution in a given quarter, and zero otherwise.[12] *Analyst Following* is calculated as the natural logarithm of one plus the number of analysts following each firm. *Equity Issuance* is an indicator variable that equals one if a firm issues equity in a given quarter, and zero otherwise. *High Litigation Industry* is an indicator variable that equals one if a firm belongs to a high litigation industry, and zero otherwise. *Institutional Ownership* is the percentage of total shares outstanding held by institutional investors. All firm characteristics are calculated at the end of each quarter.[13] We also include firm (γ_i) and quarter (δ_t) fixed effects to control for all time invariant firm characteristics and quarterly macro factors affecting all firms.

We also include a time-trend variable ($Trend_t$) and its interaction with the treatment variable ($Trend_t \times Treated_i$) to capture potentially differential pre-event trends in the dependent variables for treated firms. If we do not control for pre-event trends, differential growth rates for the dependent

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[12] Altman (1968) Z-score is calculated as 1.2 × (current assets minus current liabilities, divided by total assets) + 1.4 × (retained earnings divided by total assets) + 3.3 × (earnings before interest and taxes divided by total assets) + 0.6 × (market value of equity divided by total liabilities) + 0.999 × (sales divided by total assets).

[13] Table 1 provides a detailed description of all the variables used in the analyses.

variable for the treated and untreated firms over time could lead to incorrect inferences.[14] For instance, management disclosure by untreated firms with no bonds outstanding could be increasing at a greater rate compared to treated firms over the sample period we study. Without controlling for time trends, we could mistakenly attribute the increase over time to the TRACE event.

The variable of interest is $Treated_i \times Post_t$. The coefficient on this variable, β, provides us the difference in disclosures by firms that have bonds traded (treated firms) compared to firms with no public bonds traded (control firms) after the introduction of TRACE. A negative coefficient on β indicates that firms with bonds that were affected by TRACE have seen a greater decline in management forecasts after TRACE compared to firms that were not affected, consistent with H1, the substitution hypothesis.

We provide summary statistics in Table 2, which we calculate by pooling quarterly observations over the sample period. Panel A presents statistics for the entire sample, including bond issuing companies that were affected by TRACE (treated firms), as well as non-bond-issuing companies that were not affected (control firms). Panel B and C present statistics for the treated and control group, respectively. Panel B also reports significance tests for differences in means for each firm characteristic between the control and treated samples. On average treated firms are larger, have higher leverage, lower valuations relative to book, higher institutional ownership and analyst following, and lower return volatility and a higher likelihood of bankruptcy (although the mean likelihood is low for both treated and control firms). Treated firms are also more likely to issue management forecasts (0.4206) compared to control firms (0.1590).[15]

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[14] Another approach to address pre-event trends is to do a placebo test by moving the event window forward and backwards in time. For robustness, we also do a placebo test later on in this section.

[15] Because of these significant differences, we employ propensity score matching and Heckman adjustment in tests described later.

We report results from the DID regression in Table 3, using pairs of columns for each dependent variable (*Issue*, *Count*, and *log(1+Count)*). The first of each column pair omits control variables and the second of each column pair includes control variables. In all specifications, the coefficient β on $Treated_i \times Post_t$ is negative and significant. After controls, treated firms see a 0.224 decline (column 4) in the number of forecasts they make after the introduction of TRACE. Given that the mean number of forecasts for the treated firms is 0.9083 with a standard deviation of 1.4596, the decline is economically significant. Overall, these results lend support to the hypothesis (*H1*) that trade transparency decreases the number of management earnings forecasts.

3.2 Placebo tests, propensity score matching, and Heckman correction

Table 2 shows several significant differences in firm characteristics between firms in the treated and control groups. It is important to note that since we have firm fixed effects and since we examine changes after an event, unobserved selection characteristics would need to affect management forecasts at the time of the event for pre-event selection to affect our results. Nonetheless, to address potential endogenous selection issues, we conduct three additional tests: i) we perform placebo tests by moving the event window forward and backward in time, ii) we create an alternative control group using propensity score matching, and finally iii) we use Heckman (1979) correction to control for endogenous selection.

For the placebo tests, we counterfactually move the event window forwards and backwards in time as if TRACE were implemented before and after the actual implementation date (i.e., we create several pseudo-events). We then run the same DID analyses surrounding these pseudo-event windows. We keep the treated and control sample constant when we do the placebo analyses. In the main DID analyses, we examine the impact of TRACE after full implementation and skip the partial implementation period, leaving us with an event window from Q3 2002 to Q1 2005. The first pseudo-event window (P1) starts 2 years before the TRACE implementation (Q3 2000) and ends right immediately after the start of the TRACE event window (Q1 2003). The second pseudo-event window (P2) starts 3 years before the start of the TRACE event window (Q3 1999) and ends right before the start of the TRACE event window (Q1 2002). The third pseudo-event window (P3) starts 2 years after the TRACE event (Q3 2004) and ends in Q1 2007. The fourth pseudo-event window (P4) starts 3 years after the start of the TRACE event window (Q3 2005) and ends in Q1 2008. This approach provides us with four pseudo-events, which have the same length as the actual TRACE implementation period.

The results from the placebo analyses are reported in Table 4. We only report the coefficient β on $Treated_i \times Post_t$ for brevity. The top panel with the header, *Before TRACE Launch*, presents results for the two pseudo-events (P1 and P2) that start before the actual event window. The bottom panel with the header, *After TRACE Launch*, presents the results for the two pseudo-events (P3 and P4) that start after the actual event window. The β coefficients on $Treated_i \times Post_t$ are all insignificant except for one case – in the second pseudo-event (P2) the likelihood of the issuance of an earnings forecast is higher for treated firms (i.e., β is positive), the opposite of our prediction and findings. These results suggest that our results are unlikely to be driven by selection issues or pre-event trends.

We also use propensity score matching (PSM) to match a control firm to each of the treated firms in our sample based on the firm-specific characteristics in Table 2.[16] Although the OLS regression model used in Table 3 controls for differences in various firm characteristics, the model assumes a linear relation between the explanatory variables and the dependent variable.

[16] The PSM method was first developed by Rosenbaum and Rubin (1983) and is commonly used in accounting and financial studies (see Tucker 2010 for a review).

In the first PSM step, we run a logistic regression where the dependent variable is a dummy that takes on a value of one if a given firm has bonds outstanding over the 1998 Q3 to 1999 Q2 period (the last quarter preceding the DID analysis).[17] Table 5 shows that firms with bonds outstanding are larger, have higher leverage, higher analyst following and lower equity volatility. Based on the pseudo-R^2 of 0.43, the set of dependent variables explain a significant variation in whether firms issue bonds.

In the second PSM step, we create a propensity score for each firm based on the coefficients obtained from the logistic regression estimated in Table 5. We then pair treated and control firms with the smallest score differences. Specifically, we use the nearest-neighbor matching method with and without replacement. When we use the matching procedure with replacement, we allow for the possibility that the same firm from the control group can be matched to more than one treatment firm. After the matching procedure, we end up with a corresponding control firm for each of the treated firms in our sample.

After the matching procedure, we re-estimate the DID analysis in equation (1) using the matched firms as our alternative control group. Table 6, columns (1) – (3) report results when using the matched sample with no replacements and columns (4) – (6) report results with replacements. We use firm and quarter fixed effects, time trend and its interaction with the treated dummy, and the same set of control variables. Again, we only report the estimated β coefficient for the interaction term on the variable $Treated_i \times Post_t$. We obtain similar results to those reported in Table 3. The economic significance is marginally higher when we use the PSM approach with replacement while it is marginally lower when we use the PSM approach without replacement.

[17] We use the Q3 2002 to Q1 2005 as the TRACE period. For the DID analyses the pre-event window is twelve quarters long and takes place from Q3 1999 to Q2 2002 while the post-event window is also twelve quarters and takes place from Q2 2005 to Q2 2008.

The third approach we use to address potential selection bias is Heckman's (1979) two-stage correction. While propensity score matching helps mitigate selection bias due to observable variables, the Heckman model addresses selection bias due to un-observable variables. In the first stage of the Heckman approach, we estimate the probability that a firm has issued bonds using a probit model. In the second stage, we add the inverse Mills ratio (*Lambda*) obtained in the first stage to the main regression specified in equation (1) as an additional explanatory variable. If the coefficient on the inverse Mills ratio is significantly different from zero, there is significant selection bias. The sign of the coefficient indicates the direction of the possible bias. In particular, a negative sign on the inverse Mills ratio coefficient indicates a downward bias in the treated coefficient.

In the first step probit regression, we include the same set of firm controls used in Table 3. Following Faulkender and Petersen (2005), we also add two potentially exogenous variables that drive firms' choice to issue bonds. These variables are i) a dummy variable that takes on a value of one if a firm is rated (*Rated*), ii) a set of dummy variables indicating the State in which a firm is head quartered. Not all firms that are rated have bonds outstanding, but it is significantly easier for firms that are already rated to issue bonds. Firms whose headquarters are closer to financial centers also find it easier to access capital markets to issue bonds. These exogenous variables are excluded in the second-stage model.

Column (1) of Table 7 reports the first stage probit results. The coefficient on the *Rated* variable is positive and highly significant. As expected, firms that are rated are more likely have bonds outstanding. The probit regression includes firm and quarter fixed effects and the same set of control variables in regression specified in equation (1). For brevity we do not report the coefficients on these control variables. In columns (2) to (4), we re-run the regression but include the inverse Mills ratio (*Lambda*) as an additional control. The coefficient on the Lambda variable is negative and significant, suggesting that estimates without a selection correction are biased downward. When we compare the coefficients on the *Treated* × *Post* variable in Table 7 and Table 3, we find that the coefficients are 20% to 50% smaller in absolute value (i.e., a smaller negative coefficient) but still highly significant. Overall, these additional tests show that our findings are robust to potential selection issues and are consistent with *H1*, that trade transparency reduces management earnings forecasts.

3.3 Test of H2: Greater reduction of bad news management disclosures post-TRACE

Bondholders face limited upside potential but can suffer severe loses on the downside when a firm defaults. As a result, bond prices reveal more information about the downside risk of a firm. H2 predicts that greater transparency in bond transactions will lead to a greater decline in management earnings forecasts that relate to the downside risk of a firm. That is, we expect the substitution effect to be stronger for bad news forecasts compared to good news forecasts after the introduction of TRACE.

In Table 8, we report results from DID regression (equation 1) for bad and good news forecasts, separately.[18] We include the same set of control variables, fixed effects, and trend interactions. The coefficient on the $Treated_i \times Post_t$ variable is negative and significant for the bad news sample. For the good news forecast sample on the other hand the coefficients are insignificant. These results (consistent with H2) suggest that the impact of TRACE on management earnings forecast disclosure mostly relates to bad news forecasts.

3.4 Tests of H3: The information content of management forecasts post-TRACE

The substitution effect hypothesis is based on the idea that post-TRACE management earnings forecasts are less useful in moving earnings expectations and thus less beneficial in

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[18] We use the approach in Kothari, Shu, and Wysocki (2009) to classify management forecasts into good/bad news forecasts. Good (bad) news management earnings forecasts are higher than (lower than) the most recent analyst consensus forecast. We use the mid-point of the range for range forecasts.

reducing information asymmetry (H3). We estimate the reduction in the informational value of forecasts by examining equity market reaction to forecasts when they are released.[19] Using the Fama and French (1993) 3-factor model, we calculate absolute cumulative abnormal equity returns (*CAR*) over a three-day window around management forecast disclosures. If there are multiple forecasts released in a given quarter, we compute the average absolute CAR across all releases for a given firm. We then re-estimate our baseline DID regression using cumulative abnormal returns as the dependent variable. The results from this regression are reported in the last column of Table 8 (column 7). Consistent with H3, we find that the introduction of TRACE leads to a significant decline (2.43%) in the absolute equity market reaction to management earnings forecasts for affected firms relative to control firms.[20]

4. Cross-sectional Tests: Conditional Effects on the Impact of TRACE

In this section, we test hypotheses *H4* and *H5*. We expect trade transparency to be a more effective substitute for management earnings forecast disclosure when a firm faces greater information asymmetry (*H4*). As information quality and quantity increases with greater institutional ownership and more analyst coverage, we use these two variables to proxy for information asymmetry. Since bond prices reveal more information about the downside risk of a firm, we expect the impact of trade transparency on disclosure to be greater for distressed firms (H5).

To evaluate these conjectures, we create three dummy variables - *High Institution Dummy*, *High Analyst Dummy*, and *Distress Dummy* - to proxy for firm information asymmetry and firm

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[19] Because we use the DID design with a control group comprised of firms without bonds, we test this hypothesis using equity market returns. However, bond and stock markets quickly react when private information becomes public (DeFond and Zhang 2014), and while one market might lead the other (e.g., Downing, Underwood, and Xing 2009), they are reasonably efficient.

[20] This decline is likely an upper bound to the decline in benefits for the management forecasts that were not issued because, presumably, the forecasts that were issued post-TRACE are those for which the benefits of forecasting continue to exceed disclosure costs.

distress. We set *High Institution Dummy*, *High Analyst Dummy*, and *Distress Dummy* to one for firms that have greater institutional ownership than the median firm, have more analysts following the firm than the median firm, and have an Altman (1968) Z-score in the bottom 25th percentile, respectively. We then interact the dummy variables with $Post_t, Treated_i$, and $Treated_i \times Post_t$ in a modified equation (1):

$$\begin{aligned} Disclosure_{i,t} = \alpha &+ \beta_1 Treated_i \times Post_t + \beta_2 Treated_i \times CRDummy_i \\ &+ \beta_3 Post_t \times CRDummy_i + \beta_4 Treated_i \times Post_t \times CRDummy_i \qquad (2) \\ &+ \boldsymbol{\theta}'_{i,t}\boldsymbol{X}_{i,t} + \varphi_1 Trend_t + \varphi_2 Trend_t \times Treated_i + \gamma_i + \delta_t + \varepsilon_{i,t} \end{aligned}$$

$CRDummy_i$ is either *High Institution Dummy*, *High Analyst Dummy* or *Distress Dummy* in separate regressions. All other variables are the same as in regression (1). We are interested in the β_4 coefficient on $Treated_i \times Post_t \times CRDummy_i$. This coefficient captures how the impact of TRACE on treated firms varies based on firm-level information asymmetry and distress risk.

We report the results in Table 9. In Panel A, we report interaction results for the *High Analyst Dummy* using *Issue*, *Count*, and *log(1+Count)* as dependent variables in columns (1), (2) and (3), respectively. Coefficient β_4 on the triple interaction term is positive and significant in columns (2) and (3). In Panel B, we report interaction results for the *High Institution Dummy*. The triple interaction coefficient is also positive and significant in all three specifications. Overall, these results are consistent with our *H4* conjecture that lower levels of information asymmetry reduce the impact of trade transparency on management earnings forecast frequency.

In Panel C, we report interaction results for the *Distress Dummy*. Consistent with *H5*, β_4 coefficient β_4 on the triple interaction term is negative and significant. That is, the decline in management disclosures after TRACE are greater for firms that have higher likelihood of default.

As bonds provide more information about downside risk, the substitution effect is stronger for firms that are closer to the default boundary.

5. Conclusion

Employing a plausibly exogenous shock that strengthened the information environment (TRACE), we document a change in longer-run management earnings forecast policy. Managers decreased forecast frequency over the three-year period post-TRACE, more so for bad news management earnings forecasts and more so when the pre-disclosure information environment was weaker, and firms were closer to credit default. These findings, taken in conjunction with prior findings of increases in earnings forecasting activity in response to declines in the strength of the information environment, suggest that the inverse relation between changes in the strength of the information environment and management earnings forecasting activity is symmetric. The finding that managers are willing to curtail management earnings forecasting when the informational benefits of forecasting are reduced is significant given the actual and perceived costs of doing so.

Our research goal is to assess the validity of the information substitution hypothesis, which is grounded in the long-run benefits to the firm of managers credibly conveying private information in a precise manner in order to correct market expectations. Accordingly, we design our tests to increase the power to reject the null. Specifically, theory speaks to the conditions under which voluntary disclosure can achieve informational benefits – a commitment to credible and non-selective disclosure, which we capture by longer-run behavior, and precise disclosure, which we capture by using management earnings forecasts rather than forecasts of earnings components. We do not design our tests to speak to other non-information-based disclosure incentives such as conveying good performance, manipulating market beliefs around insider and compensation-related transactions, avoiding legal liability when bad news is imminent, and manipulating market beliefs during contests for corporate control. These are acts of selective disclosure, driven by the news content (bad or good) that can be disclosed and short-run management incentives with little value to the firm and its shareholders. Because the repeated game disciplines management behavior in the long run, these short-run incentives are far less likely to explain longer-run management earnings forecasting behavior after full implementation of a regulation that changes the strength of the information environment market wide.

Aside from the implication of our results for understanding the incentives to provide management earnings forecasts, our results also have implications for understanding the effects of the TRACE regulation. As mentioned earlier, several papers find that TRACE improved the information environment. Our results imply that those studies might understate the benefits of TRACE, per se, by not controlling for the decrease in the strength of the information environment when managers curtail earnings forecasting.

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Table 1: Variable definitions

Variable	Definition
Analyst Following	Analyst coverage, calculated as the natural logarithm of one plus the number of analysts following each firm at the end of a given quarter.
Count	The number of EPS forecasts issued by a firm in a given quarter.
Equity Issuance	An indicator variable that equals one if a firm issues equity during a given quarter, and zero otherwise.
Firm Size	The natural log of total assets at the end of each quarter.
High Litigation Industry	An indicator variable that equals one if a firm belongs to a high litigation industry (SIC codes: 2833–2836, 3570–3577, 7370–7374, 3600–3674, 5200–5961, and 8731–8734), and zero otherwise.
Institutional Ownership	The percentage (%) of total shares outstanding held by institutional investors at the end of each quarter.
Issue	A binary variable that equals to one if a firm issues any management EPS forecasts during a given quarter, and zero otherwise
Leverage	Total liabilities divided by total assets at the end of each quarter.
Market-to-Book (M/B)	The market value of total assets divided by the book value of total assets at the end of each quarter.
Mid Z-score	An indicator variable that is set to one if a firm's Altman (1968) Z-score falls within the middle quintile of the sample distribution in a given quarter, and zero otherwise.
Post	An indicator variable that is set to one for the post-TRACE period (2005: Q2 – 2007: Q1), and zero otherwise. An indicator variable that is set to one for the post-TRACE period (2005: Q2 – 2007: Q1), and zero otherwise.
Return Volatility	The standard deviation of a firm's daily stock returns in a given quarter reported in percentage.
ROA	Return on assets, computed as income before extraordinary items divided by total assets for each firm each quarter
Treated	A dummy variable that is set to one if a company has any bonds outstanding prior to the implementation of TRACE, and zero otherwise.
Z-Score	Altman (1968) Z-score, which is calculated as 1.2 × (current assets minus current liabilities, divided by total assets) + 1.4 × (retained earnings divided by total assets) + 3.3 × (earnings before interest and taxes divided by total assets) + 0.6 × (market value of equity divided by total liabilities) + 0.999 × (sales divided by total assets).

Table 2: Summary Statistics

Variables	N	Mean	SD	Q1	Median	Q3	
Panel A: Full sample							
Post TRACE	98,791	0.3610	0.4803	0.0000	0.0000	1.0000	
Treat	98,791	0.1119	0.3153	0.0000	0.0000	0.0000	
Issue	98,791	0.1889	0.3909	0.0000	0.0000	0.0000	
Count	98,791	0.3617	0.9529	0.0000	0.0000	0.0000	
Firm Size	98,791	5.2368	2.0596	3.7598	5.0205	6.5003	
Market to Book	98,791	2.4580	3.5726	1.1064	1.5331	2.5324	
ROA	98,791	0.0002	0.1187	-0.0099	0.0232	0.0424	
Leverage	98,791	0.2168	0.2821	0.0077	0.1550	0.3429	
Institutional Ownership (%)	98,791	40.1569	31.4376	11.3800	34.7875	66.0057	
Analyst Following	98,791	1.0133	0.9690	0.0000	0.6931	1.7918	
Return Volatility	98,791	4.8637	3.3423	2.4938	3.8981	6.2239	
Equity Issuance (dummy)	98,791	0.0203	0.1409	0.0000	0.0000	0.0000	
High Litigation Industry (dummy)	98,791	0.3757	0.4843	0.0000	0.0000	1.0000	
Z-score	98,791	5.6769	12.1642	1.2117	2.4485	5.1812	
Mid Z-score (dummy)	98,791	0.2294	0.4205	0.0000	0.0000	0.0000	
Panel B: Treatment Sample							
Issue	11,056	0.4206	0.4937	0.0000	0.0000	1.0000	***
Count	11,056	0.9083	1.4596	0.0000	0.0000	1.0000	***
Firm Size	11,056	8.2320	1.5831	7.1998	8.3120	9.4186	***
Market to Book	11,056	1.8926	1.6748	1.1785	1.4888	2.0668	***
ROA	11,056	0.0349	0.0301	0.0225	0.0342	0.0476	***
Leverage	11,056	0.3264	0.1912	0.2038	0.3024	0.4156	***
Institutional Ownership (%)	11,056	67.5129	23.8991	52.6855	70.8910	83.8875	***
Analyst Following	11,056	2.0602	0.8548	1.6094	2.1972	2.7081	***
Return Volatility	11,056	2.6468	1.6396	1.6198	2.2334	3.1623	***
Equity Issuance (dummy)	11,056	0.0197	0.1390	0.0000	0.0000	0.0000	
High Litigation Industry (dummy)	11,056	0.2071	0.4053	0.0000	0.0000	0.0000	***
Z-score	11,056	2.6336	4.2458	1.1726	1.9241	3.0067	***
Mid Z-score (dummy)	11,056	0.3275	0.4693	0.0000	0.0000	1.0000	***

Table 2 (continued)

Panel C: Control Sample						
Issue	87,735	0.1590	0.3656	0.0000	0.0000	0.0000
Count	87,735	0.2928	0.8435	0.0000	0.0000	0.0000
Firm Size	87,735	4.8586	1.7839	3.6011	4.7620	5.9863
Market to Book	87,735	2.5292	3.7381	1.0917	1.5416	2.6142
ROA	87,735	-0.0041	0.1248	-0.0168	0.0208	0.0414
Leverage	87,735	0.2029	0.2886	0.0030	0.1222	0.3251
Institutional Ownership (%)	87,735	37.7096	30.5731	9.4889	30.0066	59.8616
Analyst Following	87,735	0.8814	0.8999	0.0000	0.6931	1.6094
Return Volatility	87,735	5.1431	3.3974	2.7134	4.2039	6.5780
Equity Issuance (dummy)	87,735	0.0203	0.1411	0.0000	0.0000	0.0000
High Litigation Industry (dummy)	87,735	0.3970	0.4893	0.0000	0.0000	1.0000
Z-score	87,735	6.0604	12.7682	1.2218	2.5649	5.6836
Mid Z-score (dummy)	87,735	0.2171	0.4122	0.0000	0.0000	0.0000

Table 2 reports descriptive statistics for firms included in the study. Summary statistics are calculated by pooling quarterly observations over the sample period (1999: Q3 - 2008: Q1). Panel A presents results for the entire sample, including bond issuing companies that are affected by TRACE (treatment sample), as well as non-bond-issuing companies unaffected (control sample). Panel B and C present statistics for the treatment and control group, respectively. Panel B also reports whether the means of each firm characteristic differs significantly between the treatment and control samples. ***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively.

Table 3: Impact of Bond Market Transparency on Firm Disclosure

	(1) Issue	(2) Issue	(3) Count	(4) Count	(5) log (1+ count)	(6) log (1+ count)
Treated × Post	-0.156***	-0.205***	-0.225***	-0.224***	-0.119***	-0.119***
	-4.64	-4.89	-3.03	-3.06	-2.93	-2.97
ROA		-0.094***		-0.157***		-0.092***
		-6.04		-5.22		-5.4
Firm Size		0.035***		0.073***		0.042***
		6.54		6.94		7.1
Leverage		-0.022*		-0.028		-0.018
		-1.9		-1.23		-1.37
M/B		-0.005***		-0.007***		-0.004***
		-8.46		-6.8		-7.13
Mid Z-score		0.012***		0.022**		0.014***
		2.82		2.49		2.8
Institutional Ownership		0.119***		0.247***		0.140***
		7.19		7.47		7.5
Return Volatility		0.003***		0.004***		0.003***
		6.32		4.69		5.17
Equity Issuance		-0.023***		-0.025		-0.015*
		-3.07		-2.64		-1.83
High Litigation Industry		0.007		0.007		0.008
		0.37		0.2		0.37
Analyst Following		0.083***		0.151***		0.086***
		19.33		18.07		18.47
Observations	98,791	98,791	98,791	98,791	98,791	98,791
Adjusted R-square	0.081	0.151	0.081	0.151	0.085	0.158
Time trend & Interaction	Yes	Yes	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

This table reports results from the regression specified in (1) in the text. All variables are defined in Table 1. In columns (1) and (2), we report regression results where the dependent variable is *Issue,* a dummy that takes on a value of one if a firm has issued forecasts in a given quarter. In column (3)-(6), we report regression results where the dependent number of management forecasts (*Count*) and the natural log of one plus this variable (*log(1+Count)*). All regressions control for time trend interactions, quarter and firm fixed effects. Standard errors are clustered at firm level. T-stats are reported below each estimated coefficient. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

Table 4: Placebo Tests

	(1)	(2)	(3)
	Issue	Count	Log (1+Count)
Before TRACE Launch			
P1 (Coefficient β_1 on *Treated* × *Post)*	0.056	0.118	0.039
Observations	88,800	88,800	88,800
Adjusted R-square	0.1673	0.1447	0.1752
P2 (Coefficient β_1 on *Treated* × *Post)*	0.117**	0.026	0.018
Observations	89,475	89,475	89,475
Adjusted R-square	0.1704	0.1098	0.1698
After TRACE Launch			
P3 (Coefficient β_1 on *Treated* × *Post)*	0.016	-0.030	-0.041
Observations	73,025	73,025	73,025
Adjusted R-square	0.1776	0.1822	0.1857
P4 (Coefficient β_1 on *Treated* × *Post)*	-0.021	-0.061	-0.031
Observations	69,593	69,593	69,593
Adjusted R-square	0.1792	0.1831	0.1872

This table reports placebo tests whereby we counterfactually move the event window forwards and backwards in time and run the regression specified in (1). The top panel with the header, *Before TRACE Launch*, presents results for the two pseudo-events (*P1* and *P2*) that are two and three years before the actual event window, respectively. The bottom panel with the header, *After TRACE Launc*h, presents the results for the two pseudo-events (*P3* and *P4*) that are two and three years after the actual event window, respectively. In column (1), we report regression results where the dependent variable is *Issue,* a dummy that takes on a value of one if a firm has issued forecasts in a given quarter. In column (2), we report regression results where the dependent number of management forecasts (*Count*), and in column (3) we report results where the dependent variable is the natural log of one plus the number of forecasts (*log(1+Count)*). All regressions control for firm characteristics in column (2) of Table 3, time trend interactions, quarter and firm fixed effects. Standard errors are clustered at the firm level and t-stats are reported below coefficient estimates. ***, **, or * indicates that the coefficient estimate is significant at the 1%, 5%, or 10%.

Table 5: Propensity Score Matching

Variables	**Coefficient (t-statistic)**
ROA	-1.515
	-1.02
Firm Size	0.535***
	9.12
Leverage	1.102***
	3.99
M/B	0.058*
	1.66
Mid Z-score	0.037
	0.29
Institutional Ownership	0.093
	0.32
Return Volatility	-0.264***
	-5.58
Equity Issuance	0.188
	0.84
High Litigation Industry	-0.162
	-1.00
Analyst Following	0.674***
	7.97
Observations	5394
Pseudo R-square	0.4329

This table presents the results from a first stage logit regression used in propensity score matching. The dependent variable is a dummy that takes on a value of one if a firm in our sample has bonds outstanding one year before our event window (1998: Q3 – 1999: Q2). The variables are described in Table 1. Robust t-stats are reported below coefficient estimates. ***, **, or * indicates that the coefficient estimate is significant at the 1%, 5%, or 10%.

Table 6: Impact of Bond Market Transparency on Firm Disclosure – Propensity Score Matching

Matching Methods	**PSM without replacement**			**PSM with replacement**		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Dependent Variable	**Issue**	**Count**	**Log (1+count)**	**Issue**	**Count**	**Log (1+count)**
Treated × Post	-0.222***	-0.288***	-0.197***	-0.182**	-0.312*	-0.176**
	-6.49	-2.77	-4.80	-2.24	-1.77	-2.01
Observations	22,112	22,112	22,112	14,738	14,738	14,738
Adjusted R-square	0.297	0.2085	0.2736	0.1399	0.1232	0.1337
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time trend & Interaction	Yes	Yes	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm Clustering	Yes	Yes	Yes	Yes	Yes	Yes

This table shows the results from regression specified in (1) in the text. The control group of firms are determined using propensity score matching (PSM). Based on the propensity score estimated from Table 5, we pair treatment and control firms with the smallest score differences. Specifically, we use nearest-neighbor matching without and with replacement. Column (1) – (3) report estimates obtained from matched sample with no replacements, while column (4) – (6) report estimates obtained from matched sample with replacements. In columns (1) and (4), we report regression results where the dependent variable is *Issue,* a dummy that takes on a value of one if a firm has issued forecasts in a given quarter. In column (2) and (5), we report regression results where the dependent number of management forecasts (*Count*), and in columns (3) and (6) we report results where the dependent variable is the natural log of one plus the number of forecasts (*log(1+Count)*). All regressions control for firm characteristics in column (2) of Table 3, time trend interactions, quarter and firm fixed effects. For brevity, we only report the estimated coefficient for the interaction term *Treated* Post TRACE*. Standard errors are clustered at the firm level and t-stats are reported below coefficient estimates. ***, **, or * indicates that the coefficient estimate is significant at the 1%, 5%, or 10%.

Table 7: Impact of Bond Market Transparency on Firm Disclosure – Heckman Selection Model

Test	First Stage Heckman Selection (Probit)	Heckman Selection Model Outcome Equation		
Dependent Variable	**Treat**	**Issue**	**Count**	**Log (1+count)**
Rated	0.916***			
	43.09			
Treated × Post		-0.108***	-0.150**	-0.102***
		-2.96	-2.35	-2.87
Lambda		-1.125***	-1.509***	-1.008***
		-19.11	-19.00	-19.23
Observations	82,613	82,613		
Adjusted R-Square	0.315	0.171	0.175	0.178
STATE fixed effect	Yes		n/a	
Firm controls	Yes		Yes	
Quarter fixed effect	Yes		Yes	
Time trend & Interaction	No		Yes	
Firm fixed effects	No		Yes	

This table shows the results from the two-step Heckman selection model described in section 4.3 of the text. In the first step, we run a probit regression where the dependent variable is a dummy set to one for firms that have bonds outstanding in a given quarter. The results are reported in column (1). The probit regression includes all the firm controls used in the regression reported in column (2) of Table 3 and includes two potentially exogenous variables. These variables are i) a dummy variable that takes on a value of one if a firm is rated (*Rated*), ii) a set of dummy variables indicating the *State* in which a firm is head quartered. In the second step, we compute the inverse mill's ratio (*Lambda*) using the coefficients calculated from the first step probit regression and include it as an additional covariate in the regression model specified in equation (1). The results from these second stage regressions are reported in columns (2) to (4). In column (2), we report regression results where the dependent variable is *Issue*, a dummy that takes on a value of one if a firm has issued forecasts in a given quarter. In column (3), we report regression results where the dependent number of management forecasts (*Count*), and in columns (4) we report results where the dependent variable is the natural log of one plus the number of forecasts (*log(1+Count)*). All regressions control for firm characteristics in column (2) of Table 3, time trend interactions, quarter and firm fixed effects. For brevity, we only report the estimated coefficient for the interaction term *Treated* Post* and on *Lambda*. Standard errors are clustered at the firm level and t-stats are reported below coefficient estimates. ***, **, or * indicates that the coefficient estimate is significant at the 1%, 5%, or 10%.

Table 8: Impact of TRACE on the Content of Management Disclosures

	Bad News			Good News			Market Reaction
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Issue	Count	Log (1+ count)	Issue	Count	Log (1+ count)	|CAR (-1, +1)|
Treated × Post	-0.125***	-0.167***	-0.158***	0.012	0.005	0.004	-2.433***
	-3.71	-2.85	-3.17	0.6	0.27	0.27	-3.71
ROA	-0.068***	-0.094***	-0.058***	0.006	0.006	0.004	-1.465
	-4.75	-4.03	-4.29	0.93	0.99	0.99	-0.38
Firm Size	0.031***	0.052***	0.300***	0.011***	0.009***	0.006***	0.401
	6.08	6.19	6.21	4.92	4.48	4.48	1.38
Leverage	-0.019*	-0.022	-0.014	0.007	0.005	0.003	-1.473*
	-1.77	-1.19	-1.38	1.47	1.15	1.15	-1.92
M/B	-0.002***	-0.003***	-0.002***	0.000	0.000	0.000	-0.092
	-4.56	-3.76	-4.04	-0.66	-0.6	-0.6	-0.69
Mid Z-score	0.012***	0.018***	0.011***	0.004*	0.003*	0.002*	-0.233
	2.83	2.64	2.74	1.84	1.75	1.75	-1.4
Institutional Ownership	0.138***	0.229***	0.133***	0.243***	0.030***	0.021***	0.144
	8.44	8.5	8.57	3.82	5.31	5.30	0.21
Return Volatility	0.004***	0.005***	0.003***	0.000	0.000	0.000	2.941***
	7.07	6.39	6.68	0.33	-0.88	-0.89	22.42
Equity Issuance	-0.021***	-0.022*	-0.015**	-0.002	-0.002	-0.001	-0.209
	-2.88	-1.82	-2.17	-0.45	-0.43	-0.43	-0.54
High Litigation Industry	-0.001	-0.012	-0.005	0.008	0.006	0.004	-2.386**
	-0.09	-0.42	-0.33	1.02	0.93	0.93	-2.41
Analyst Following	0.076***	0.115***	0.068***	0.011***	0.008***	0.006***	1.328***
	18.13	16.91	17.47	6.06	5.03	5.04	7.47
Observations	98,791	98,791	98,791	98,791	98,791	98,791	18,593
Adjusted R-square	0.1502	0.1499	0.1539	0.0212	0.0294	0.0294	0.3025
Time trend & Interaction	Yes			Yes			Yes
Quarter fixed effects	Yes			Yes			Yes
Firm fixed effect	Yes			Yes			Yes
Firm Clustering	Yes			Yes			Yes

This table reports results from the regression specified in (1) in the text. The dependent variables proxy for the content of management forecasts. We classify management forecasts as good (bad) news, if management quarterly earnings forecast is higher (lower) than the most recent analyst consensus forecast. If a range forecast is provided, we use the mid-point of the range to estimate the difference between the mid-point forecast and the analyst consensus forecast. We run the regression specified in (1) and use issuance and number of bad and good news forecasts as our dependent variables. In the left panel under the heading *Bad News*, we report the results using the bad news forecasts as the dependent variable. In the panel to the right under the heading *Good News*, we report the results using the good news forecasts as the dependent variable. We use the absolute value of cumulative abnormal returns (|*CAR*|) computed over a three-day window using a 3-factor model to measure short-term market reaction to management forecasts. In columns (1) and (4), we report regression results where the dependent variable is *Issue,* a dummy that takes on a value of one if a firm has issued forecasts in a given quarter. In column (2) and (5), we report regression results where the dependent number of management forecasts (*Count*), and in columns (3) and (6) we report results where the dependent variable is the natural log of one plus the number of forecasts (*log(1+Count)*). In column (7) the dependent variable is |*CAR*|. All regressions control for firm characteristics in column (2) of Table 3, time trend interactions, quarter and firm fixed effects. Standard errors are clustered at the firm level and t-stats are reported below coefficient estimates. ***, **, or * indicates that the coefficient estimate is significant at the 1%, 5%, or 10%.

Table 9: Cross Sectional Variation in Trace Impact

Panel A: High Analyst Coverage	Issue	Count	Log (1+ Count)
High Analyst Dummy × Treated × Post	0.066	0.341***	0.124**
	1.10	3.27	2.34
Treated × Post	-0.255***	-0.557***	-0.286***
	-3.90	-4.60	-4.77
High Analyst Dummy × Treated	0.014	-0.127**	-0.027
	0.43	-1.96	-0.85
High Analyst Dummy × Post	0.097***	0.273***	0.120***
	10.79	12.28	12.42
Observations	98,791	98,791	98,791
Adjusted R-Square	0.1393	0.1429	0.1496

Panel B: High Institutional Ownership	Issue	Count	Log (1+ Count)
High Institution Dummy × Treated × Post	0.075**	0.278***	0.108***
	1.97	2.8	2.61
Treated × Post	-0.253***	-0.497***	-0.270***
	-5.71	-4.00	-5.30
High Institution Dummy × Treated × Post	-0.021	-0.096	-0.040
	-0.53	-0.89	-0.96
High Institution Dummy × Post	1.010***	0.286***	0.126***
	11.03	12.53	12.70
Observations	98,791	98,791	98,791
Adjusted R-Square	0.1525	0.1549	0.1621

Panel C: High Distress Firms	Issue	Count	Log (1+ Count)
Distress Dummy × Treated × Post	-0.152**	-0.273***	-0.202***
	-2.43	-2.71	-2.97
Treated × Post	-0.153***	-0.239***	-0.117***
	-4.59	-4.02	-2.86
Distress Dummy × Treated	-0.015	-0.023	-0.014
	-0.44	-0.39	-0.35
Distress Dummy × Post	-0.035***	-0.054***	-0.036***
	-3.04	-2.98	-2.91
Observations	98,791	98,791	98,791
Adjusted R-Square	0.1597	0.1616	0.1688

This table reports results from the regression specified in (2) in the text. Panel A reports results for the triple interaction regression examining how analyst coverage affects the impact of TRACE on firm disclosure. Panels B and C similarly show results for the triple interaction regression examining the how institutional ownership and firm distress affect the impact of TRACE on management forecasts. Variables are defined in Table 1. High Analyst Dummy is a dummy variable that equals one for firms that have more analysts following the firm than the average firm in our sample period, zero otherwise. High Institution Dummy is a dummy variable set to one for firms that have higher institutional ownership than the average firm in our sample period, zero otherwise. Distress Dummy is dummy variable set to one for firms whose Altman (1968) Z-score is in the bottom 25% of firms in our sample, and zero otherwise. *Issue* is a dummy that takes on a value of one if a firm has issued forecasts in a given quarter. *Count* is the number of management forecasts made by a firm in a given quarter, and *log(1+Count)* is the natural log of one plus the number of forecasts. All regressions control for firm characteristics in column (2) of Table 3, time trend interactions, quarter and firm fixed effects. For brevity, we only report coefficients on the interaction variables that are of interest. Standard errors are clustered at the firm level and t-stats are reported below coefficient estimates. ***, **, or * indicates that the coefficient estimate is significant at the 1%, 5%, or 10%.