

Evidence on the role of banks in borrowers' disclosure

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Abstract: Banks that actively monitor borrowers acquire private information that gives them an information advantage relative to competing potential lender banks and dispersed bondholders and shareholders, who are relatively more reliant on public disclosures to obtain information about the firm. Banks benefit from this information advantage and, therefore, prefer less public dissemination of information that is available to them privately. I test this theory by examining changes in firms' disclosures around covenant violations, which shift the control rights to creditors, who can then use their enhanced bargaining power to impose their preferences on various firm policies, including disclosure policies. Consistent with this theory, I find that managers reduce the extent to which they publicly disclose their private information after covenant violations.

Keywords: disclosure; banks; covenant violation; control rights; information monopoly.

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1. Introduction

This study examines whether banks' incentive to maintain their information advantage over competing capital providers influences their borrowers' disclosure choices. Banks that actively monitor borrowers over the course of their relationship acquire private information, which gives them an information advantage relative to competing potential lender banks and other outside investors such as dispersed bondholders and shareholders, who are relatively more reliant on public disclosures to obtain information about the firm.¹ Banks have incentives to maintain or enhance this information advantage since it allows them to capture a greater share of the gains generated by their monitoring effort. This benefit of the information advantage can accrue to banks through multiple mechanisms including reduced competition from outside banks (Sharpe, 1990; Rajan, 1992), enhanced ability to protect their claims if borrowers' credit quality deteriorates (Daniels and Triantis, 1995), or reduced variability in borrowers' profits (Perotti and Von Thadden, 2005).² Prior empirical studies have shown that the rents earned by banks through their information advantage are economically significant.³ Consequently, banks will prefer less public dissemination of information that is available to them privately.

Unlike banks, outside investors rely more on public disclosures to obtain information about the firm and, therefore, are likely to prefer greater public dissemination of information. Consequently, a firm's optimal disclosure policy should reflect the trade-off between these two competing demands for disclosure and should be a function of the relative bargaining power of these two types of capital providers, and of the extent to which its managers' interests are aligned

¹ Henceforth, I will use the term "outside investors" to refer to capital providers that primarily rely on public disclosures to obtain information about the firm, and "inside investors" to refer to capital providers that have access to private information about the firm.

² See Section 2 for a detailed explanation of these mechanisms.

³ See Peterson and Rajan (1994), Degryse and Van Cayseele (2000), and Schenone (2009) for empirical evidence on extraction of rents by banks from their informationally captured borrowers.

with each capital provider. Our understanding, however, of the economic significance of the effect of these conflicting preferences on firms' disclosure choices is limited since prior empirical literature on the determinants of disclosure has largely ignored the effect of banks' preference for reduced disclosure. This study aims to fill this gap by examining whether banks' incentive to maintain their relative information advantage plays a significant role in influencing borrowers' disclosure choices.

I focus on changes in firms' disclosure choices around violations of covenants in credit agreements to examine this research question for two reasons. First, following a covenant violation, managers are more likely to rationally choose policies, including disclosure policies, that reflect a greater consideration for their banks' preferences. Covenant violations are associated with a transfer of control rights from shareholders to banks who can then use the threat to demand immediate re-payment of the outstanding principal and interest to wield significant influence over firm policies that fall within or outside the scope of the credit agreement (Aghion and Bolton, 1992; Dewatripont and Tirole, 1994; Baird and Rasmussen, 2006; Chava and Roberts, 2008; Roberts and Sufi, 2009; Nini, Smith, and Sufi, 2011). Banks can exercise this influence both directly by explicitly negotiating with managers about firm policies and indirectly by exercising greater control over the welfare of the managers.⁴ The indirect effect arises because banks' enhanced bargaining power after a covenant violation gives them an ability to impose large personal costs on managers through termination of employment or by influencing their compensation (Gilson, 1989, 1990; Gilson and Vetsuypens 1993; and Nini, Smith and Sufi, 2011). Anticipating the greater influence of banks on their welfare after covenant

⁴ The exercise of direct influence, as noted by Nini et al. (2011), is likely to be unobservable and "behind-the-scenes" in nature since banks may be reluctant to appear excessively controlling of borrowers' management due to concerns regarding lender liability.

violation, managers better internalize the banks' future payoffs and give more weight to banks' preferences.

The second advantage of this setting is that the discontinuity in banks' bargaining power at the covenant threshold enables more precise identification of the causal relation between banks' preferences and borrowers' disclosure choices by allowing me to adopt a quasi-regression discontinuity research design similar to the one used by Roberts and Sufi (2009) and Nini, Smith, and Sufi (2011). This design helps ensure that the effect of covenant violation is identified primarily through discontinuous changes in disclosure occurring at the covenant threshold, and which can therefore be more appropriately attributed to a discontinuous shift in banks' bargaining power at the covenant threshold. All of my findings are robust to the implementation of this discontinuity approach, mitigating concerns that my inferences may be confounded by other determinants of disclosure that change around covenant violations.

Using multiple measures of the precision with which firms' disclosures convey information about their cash flows, I find strong evidence of a decline in the extent to which managers communicate their private information to outside investors after a covenant violation. My main inferences are based on an analysis of the incidence of management earnings forecasts, which provide an appealing setting for examining this research question because management forecasts are a key discretionary channel through which managers communicate their private information (Beyer, Cohen, Lys, and Walther, 2010).⁵

⁵ I do not use measures of earnings informativeness for two reasons. First, financial covenants in credit agreements often involve GAAP earnings, and therefore untimely and unreliable earnings can limit the effectiveness of covenants to act as a device for timely transfer of control rights. Thus, on one hand poor earnings informativeness can benefit the banks by enhancing their information advantage; on the other hand, it can also hurt them by limiting the protective role of covenants. Hence, theoretically the preferences of banks regarding informativeness of accounting earnings are ambiguous. Second, prior literature has provided evidence that firms engage in earnings management to avoid the violation of covenants (Sweeney, 1994; DeFond and Jiambalvo, 1994; Dichev and

I find that managers are significantly less likely to issue a forecast following a covenant violation. Additionally, consistent with a greater increase in bargaining power of banks when covenant violations are more severe, I find that the reduction in the likelihood of forecast issuance is greater following violations that persist for longer periods and thus reflect a more significant decline in credit quality. I do not, however, find any evidence of changes in the informativeness of forecasts for firms that continue to issue them after violations. This finding suggests that firms cater to banks' preferences for opacity by omitting the forecasts altogether rather than by issuing less informative forecasts. Finally, I find that some firms resume issuing forecasts once they are no longer in violation of covenants and their bargaining power with their banks improves. These findings are robust to the inclusion of controls for firm performance, uncertainty and other determinants of disclosure that may change around covenant violations.

Several patterns in the cross-sectional variation in the effect of covenant violation further indicate that the above findings reflect the effect of banks' preferences on firms' disclosure choices. First, the decline in the likelihood of forecast issuance is greater for firms whose banks enjoy greater bargaining power relative to outside investors as measured by the extent of firms' reliance on bank debt to finance their assets. Second, the decline is not significant for firms that obtain a large portion of their total debt from outside creditors who are primarily reliant on public disclosures for information about borrowers, and therefore do not benefit from a decline in disclosure. Third, the decline manifests only for firms reliant on relationship lenders, lenders who acquire significant private information through repeated interactions during the lending relationship, and consequently gain the most from a decline in disclosure. Fourth, the decline attenuates with firms' growth opportunities, suggesting that managers' propensity to cater to

Skinner, 2002). This makes it difficult to detect deterioration in earnings quality following covenant violation for firms that manage earnings in anticipation of a covenant violation.

banks' preference for reduced disclosure is tempered by a concern for increased reliance on a single source of capital to finance future growth opportunities (Houston and James, 1996). Finally, the decline is significant only for firms in industries where accounting earnings are timely indicators of firm performance, which is where the omission of earnings forecasts can give banks a meaningful information advantage.

To mitigate concerns that firms may offset the effect of the omission of earnings forecasts by disclosing more through other channels, I also examine the effect of covenant violations on the informativeness of two additional disclosure channels: Management Discussion and Analysis section (MD&A) of 10-K and 10-Q filings and earnings press releases. I find evidence of a decline in the informativeness of both of these disclosure channels. Following a violation, managers make fewer numeric disclosures in MD&As and are less likely to disclose information on cash flows and capital expenditures in earnings press releases. Finally, to ensure that reduced disclosure indeed results in less information to outside investors, I also examine the effect of covenant violations on analyst forecast errors and the adverse selection component of the bid-ask spread in stock prices. Consistent with my hypothesis, I find strong evidence of an increase in analyst forecast errors and the adverse selection component of the bid-ask spreads after covenant violations.

Throughout the analyses, I consider the possibility that my findings may be caused by the omission or use of imperfect empirical proxies for some other determinants of firms' disclosure choices that change around covenant violations, such as performance, uncertainty or litigation risk. Several features of my research design and findings mitigate this concern. First, identification of the effect of covenant violation primarily through discontinuous changes in disclosure at the covenant threshold helps ensure that the changes in disclosure can be attributed

to the discontinuous change in banks' bargaining power, even if violation is correlated with some omitted variable, provided that this omitted variable is not discontinuous at the covenant threshold.⁶ Second, none of the likely omitted correlated variables provide an obvious explanation for the cross-sectional variation in the effect of covenant violation. For example, it is not clear why the effect of an increase in uncertainty or an increase in litigation risk around covenant violations would be systematically greater for firms that rely more on bank debt or for firms that rely on relationship banking, which are the firms that exhibit a decline in disclosure after a covenant violation. Finally, my findings related to earnings press releases further mitigate these concerns. Unlike a management forecast, disclosure of a cash flow statement or capital expenditure for a prior period is not forward-looking in nature, and thus the decision on its disclosure is unlikely to be driven either by concerns regarding future litigation related to unattained forecasts, or by managers' inability to make forecasts under heightened uncertainty. Collectively, my findings appear to be most consistent with the manifestation of banks' preference for reduced disclosure by their borrowers.

This study contributes to the prior literature on disclosure by providing evidence suggesting that banks' incentive to enhance their share of the surplus from the lending relationship by reducing disclosure to other competing capital providers can exercise a significant influence on borrowers' disclosure choices. To the best of my knowledge, this is the first study to provide evidence suggesting that banks can use their bargaining power to impose

⁶ One potential concern is that the reduced disclosure may reflect a discontinuous decrease in firms' reliance on equity capital following covenant violations. Firms that do not anticipate raising equity capital may reduce costly disclosure. I do not believe my inferences are confounded by this alternative explanation for two reasons. First, Roberts and Sufi (2009) find that firms' reliance on equity capital relative to debt capital increases after covenant violations. Second, my findings hold on a subsample of firms that continue to raise equity capital after covenant violations.

their preference for opacity on borrowers.⁷ This study also contributes to the stream of literature that examines the effect of banks' information monopolies on borrowers' choice of financial contracts (e.g., Sharpe, 1990; Rajan, 1992; Houston and James, 1996). These studies typically consider a firm's information environment to be exogenous and beyond banks' influence, an assumption that my findings indicate is unlikely to be descriptive of all bank-borrower relationships.

Finally, this study is related to the prior literature that finds a negative association between disclosure and the presence of institutional equity investors with concentrated holdings (e.g., Ajinkya, Bhojraj, and Sengupta, 2005). These studies note that equity investors with concentrated holdings may prefer less disclosure of information that they can obtain privately through their better access to firms' managers. My study complements this stream of literature in two ways. First, it shows that such a preference for opacity is also exhibited by another class of capital providers – banks. Second, this study suggests that the negative association between disclosure and the presence of investors with access to inside information can be causal, i.e., such investors can influence firms' disclosure choices to enhance opacity.

The remainder of the paper is organized as follows. Section 2 discusses theories that provide rationales for banks' preference for less public disclosure by borrowers, and for greater manifestation of these preferences around covenant violations. Section 3 describes my research

⁷ Note that the evidence provided in this study is distinct from two indirectly related studies: Erkens, Subramanyam, and Zhang (2011) and Tan (2011). Erkens et al. (2011) examine the representation of the lender on the board as an alternative governance mechanism that reduces reliance on conservative accounting in debt contracting as a mechanism for alleviating debtholder-shareholder conflicts. In contrast, my study focuses on the effect of banks' incentive to suppress disclosure to other competing capital providers. Consistent with this difference in focus, I examine non-contractible disclosures to ensure that my inferences are not confounded by the contracting role of GAAP accounting numbers. Tan (2011) also focuses on contractible disclosures and documents an increase in conservative accounting after covenant violations. Another key difference between my study and Tan (2011) emanates from my focus on a specific type of creditor: banks that have access to private information and thus are unlikely to be reliant on public disclosures to obtain information about their borrowers. I find that my findings are primarily driven by firms that exhibit a substantial reliance on banks for financing.

design. Section 4 describes the main empirical findings. Section 5 reports the additional analysis and robustness checks. Section 6 concludes.

2. Theory and hypothesis development

2.1 Banks' preference for less public disclosure by borrowers

Prior literature on financial intermediation has highlighted the unique role played by banks which gives them preference for less public disclosure by their borrowers. In particular, banks' disclosure preferences stem from their unique ability and incentives to cost efficiently produce private information about their borrowers through repeated interactions during the lending relationship.⁸ This ability enables banks to provide financing to firms that may not be able to obtain it directly from outside investors due to moral hazard and adverse selection problems (e.g., Leland and Pyle, 1977; Diamond, 1984, 1991; Ramakrishnan and Thakor, 1984; Fama, 1985). Unlike the incumbent bank, outside banks, dispersed shareholders and bondholders are more reliant on public disclosures to obtain information about the firm, and therefore are likely to be less informed than the incumbent bank. This relative information advantage allows incumbent banks to capture a greater share of the surplus generated from their relationship with the borrowers in several ways discussed below, and they will therefore prefer less public disclosure of information that is available to them privately.

First, inside banks' information advantage allows them to better protect their rents from competing outside banks (e.g., Sharpe, 1990; Rajan, 1992). Specifically, if not so well-informed competing outside banks, which cannot distinguish between the incumbent bank's borrowers of varying credit quality, were to offer rates that reflected the average credit quality of borrowers, only below average firms would switch lenders, and outside banks would lose money on

⁸ For empirical evidence on the production of private information by banks, see, for instance, James (1987), Lummer and McConnell (1989), James and Wier (1990), Best and Zhang (1993), and Norden and weber (2010).

average. Thus, to protect themselves against this so-called “winner’s-curse”, outside banks would have to charge higher interest rates. This, in turn, lets the incumbent bank charge a higher interest rate to above average firms and earn informational rents.

Second, the information advantage allows banks to better protect their claims in the event their borrowers’ credit quality deteriorates. A bank that has privately observed early warning signs can renegotiate favorable lending terms (such as additional collateral) from the borrower before the firms’ other creditors observe it (Daniels and Triantis, 1995).

Finally, Perotti and Von Thadden (2005) provide another rationale for banks’ preference for less public disclosure. They argue that opaque firms reveal less about their competitive strength to competitors. This harms strong firms as their competitors will be more aggressive, forcing these firms to reduce their output. However, opacity benefits weak firms, which will face less competition and can retain more market share and profitability. As a result, under opaque competition borrowers’ expected profits are lower but less variable. Consequently banks prefer less disclosure because they do not gain from higher profits but suffer from higher risk.

There are several reasons why firms may not be able to eliminate the effect of banks’ information monopolies by relying more on non-bank sources of capital or by relying on multiple banks. First, raising capital directly from public equity or debt markets may not be feasible for firms characterized by significant agency problems that can only be resolved through monitored capital. Second, loss of monitoring effectiveness and increased transaction costs can render multiple banking relationships an unattractive strategy to overcome information monopolies for some borrowers. For example, if some banks free ride on others’ efforts, too little monitoring can result. Alternatively, duplication in the costs of sharing information with banks can make multiple banking relationships prohibitively expensive for some borrowers.

Unlike banks, outside investors are likely to prefer greater public dissemination of information. Consequently, a firm's optimal disclosure policy should reflect the trade-off between these two competing demands for disclosure and should be a function of the relative bargaining power of these two kinds of capital providers and of the extent to which managers' interests are aligned with these two capital providers. As described in the following section, I identify the effect of banks' preferences by examining changes in firms' disclosure choices around covenant violations, which are associated with an increase in the bank's bargaining power and therefore switch to a disclosure regime that places greater weight on banks' preferences.

2.2 Covenant violation and disclosure

To examine the effect of banks' preferences on borrowers' disclosure choices, I focus on changes in firms' disclosures around violations of covenants in their credit agreements. Following a covenant violation, managers are more likely to rationally choose policies, including disclosure policies, which reflect greater consideration of banks' preferences. This shift in firms' policies in favor of banks after a covenant violation is caused by a transfer of control rights to banks, who can use their enhanced bargaining power, either directly or indirectly, to influence managerial choices (Aghion and Bolton, 1992; Dewatripont and Tirole, 1994; Baird and Rasmussen, 2006; Chava and Roberts, 2008; Roberts and Sufi, 2009; Nini, Smith and Sufi, 2011). Since banks prefer less public dissemination of information, increased weight on banks' preferences should lead to a decline in borrowers' disclosure after a covenant violation.

The above control rights view of the role of covenants follows from the theoretical literature on optimal contracting in the presence of agency conflicts between managers and capital providers (e.g., Aghion and Bolton, 1992; Dewatripont and Tirole, 1994). In these models, provision of contingent control rights emerges endogenously to enhance pledgeable

income and to facilitate financing by mitigating agency problems between managers and capital providers. Under this view covenants define the circumstances or rules for the optimal allocation of control rights between shareholders and banks. Note that the violation of a covenant by itself does not provide banks with the right to decide firm policies; banks can, however, use the threat of demanding immediate re-payment of the outstanding principal and withholding any further credit to wield significant influence over firm policies. It is also important to note that the influence of banks is not limited to policies within the scope of credit agreements, but extends to policies outside its scope.⁹

The influence of banks on firms' policies after a covenant violation can manifest through both direct and indirect mechanisms and my research design cannot empirically distinguish between the two. Banks can directly influence firms' policies by explicitly negotiating with managers. In some cases, such direct intervention may take place through acquisition of board seats by banks (Gilson, 1990). As noted by Nini et al. (2011), exercise of direct influence, however, is likely to be unobservable and "behind-the-scenes" in nature because banks may be reluctant to appear excessively controlling of the management due to concerns regarding lender liability.¹⁰

Banks can also exercise an indirect influence over firm policies through their enhanced ability to influence the welfare of managers and the board of directors, who then better

⁹ Examples of policies influenced through changes in credit agreement include new capital raising efforts, acquisitions, investments in working capital and tangible assets, asset sales and dividend payments. Examples of firm policies influenced, although outside the scope of credit agreements, include management turnover (Gilson, 1989; Nini, Smith and Sufi, 2011), board composition (Gilson, 1990), managerial compensation (Gilson and Vetsuypens, 1993), and hiring of turnaround specialist (Nini, Smith and Sufi, 2011). Also note that by influencing the composition of the top management team and the board of directors, banks can, at least indirectly, use their control rights to influence virtually every aspect of a firm's operations.

¹⁰ The bankruptcy doctrine of equitable subordination permits the Court to subordinate a lender's claims if it determines that the lender obtained an advantage at the expense of other creditors as a result of its control over its borrowers' management (Daniels and Triantis, 1995; Berlin and Mester, 2001).

internalize the banks' payoffs and give greater weight to banks' preferences. Prior literature has provided significant evidence in support of this indirect effect. For example, Gilson (1989) finds a significant increase in bank initiated senior management turnover in financially distressed firms.¹¹ Moreover, after resignation these managers are not employed by any exchanged listed firm for at-least three years. Gilson and Vetsuypens (1993) find that managers of financially distressed firms who keep their job often experience a large reduction in their compensation, and sometimes their compensation is explicitly tied to the value of the banks' claims. Gilson (1990) finds that banks' influence also extends to the board of directors. Banks frequently appoint new directors and acquire board seats in distressed firms. Moreover, directors who resign hold significantly fewer seats on other boards following their departure. Anticipating banks' increased ability to impose large personal costs after a covenant violation through executive turnover and compensation, managers should better internalize banks' payoffs and give greater weight to banks' preferences.

3. Research design

I estimate various versions of the following two models to test my hypothesis which predicts a decline in borrowers' disclosure after a covenant violation:

$$\Pr(Disclosure_{i,t+1}) = f(\alpha + \beta Violation_{i,t} + \Gamma Control_{i,t} + \Phi \Delta Control_{i,t+1} + \Omega \Delta Control_{i,t} + Ind_i + Time_t + \varepsilon_{i,t+1}) \quad (1)$$

$$Disclosure_{i,t+1} = \alpha + \beta Violation_{i,t} + \Gamma Control_{i,t} + \Phi Control_{i,t+1} + Firm_i + Time_t + \varepsilon_{i,t+1} \quad (2)$$

where $DISCLOSURE_{i,t+1}$ is a measure of firm i 's disclosure released during the three month period following the end of quarter $t+1$. I estimate the probit specification in Equation (1) for

¹¹ Nini, Smith and Sufi (2011) report similar findings using a relatively recent sample of covenant violations.

dichotomous disclosure measures and linear specification in Equation (2) for continuous disclosure measures.¹² $VIOLATION_t$ is an indicator that equals one if the firm was in violation of a financial covenant during quarter t and zero otherwise. $CONTROL_{i,t}$ ($CONTROL_{i,t+1}$) denotes a vector of control variables measured at the end of quarter t ($t+1$), and $\Delta CONTROL_{i,t}$ ($\Delta CONTROL_{i,t+1}$) denotes a vector of changes in control variables from quarter $t-1$ to quarter t (quarter t to quarter $t+1$). IND_i , $FIRM_i$, and $TIME_t$ denote industry, firm, and time fixed effects, respectively. Since the purpose of the empirical analysis is to identify shifts in the equilibrium disclosure choices caused by covenant violation, I measure covenant violation with a quarter long lag to the release of the disclosure. The measurement of all control variables and the motivation for including them in the above models is described in Appendix A.5.¹³

To better identify the causal relation between banks' preferences and disclosure, I also implement a quasi-regression discontinuity design similar to the one employed by Roberts and Sufi (2009), and Nini, Smith, and Sufi (2011). This research design exploits the discontinuity in banks' bargaining power at the covenant threshold. To implement this approach, I include higher-order nonlinear transformations of underlying variables on which covenants are typically written in the empirical specifications to approximately control for the effect of smooth functions

¹² Note that inclusion of firm fixed effects along with time dummies effectively converts Equation (2) to a difference-in-differences specification (Bertrand, Duflo, and Mullainathan, 2004). However, inclusion of firm fixed effects is not feasible in nonlinear Probit or Logit models with panels of relatively smaller length due to the incidental parameters problem that induces bias in coefficient estimates (e.g., Greene, 2004). Therefore, I instead include industry fixed effects and changes in control variables in Equation (1). My inferences, however, remain unaltered if I estimate a linear probability model for dummy dependent variables using Equation (2).

¹³ Analyst related variables are not available for a significant number of smaller firms in my sample. These small firms also have a significantly higher incidence of covenant violation and thus are likely to add significant statistical power to my empirical tests. In order to avoid excluding these firms, I assign a value of zero to analyst related variables for firms that are missing in IBES and include an indicator variable for lack of coverage in IBES as an additional control variable. This approach for dealing with missing observations, initially recommended by Cohen and Cohen (1975), allows one to mitigate concerns regarding selection biases and enhance statistical power by capitalizing on information inherent in observations for which values for an important group of variables do not exist.

of the variables that determine the distance to covenant violation.¹⁴ This helps ensure that the effect of covenant violation is identified primarily through discontinuous changes in disclosure occurring at the covenant threshold, and which can therefore be more appropriately attributed to a discontinuous shift in banks' bargaining power at the covenant threshold. Accordingly, the coefficient on *VIOLATION* can be interpreted as the effect of banks' increased bargaining power on firms' disclosure. It is important to note that this interpretation is valid even if *VIOLATION* is correlated with some omitted variable, provided that this omitted variable is not discontinuous at the covenant threshold. This feature of my research design helps ensure that my inferences are not confounded by other determinants of disclosure that change around covenant violations, even if I cannot control for them perfectly.

The remainder of section 3 describes my main measures of disclosure (Section 3.1), the covenant violation data (Section 3.2), and the final sample (Section 3.3).

3.1 Measuring disclosure through management forecasts

While I examine multiple disclosure channels, my main inferences are based on the analysis of the incidence of management forecasts, which provide an appealing setting to examine this research question for two reasons. First, management earnings forecasts constitute an important and highly visible form of discretionary disclosure, omission of which can confer significant information advantage to banks. For example, Beyer et al. (2010) note that on average, management forecasts account for 15.67% of the variation in quarterly stock returns. Second, the incidence of a management forecast can be measured with minimal measurement error for a large sample of firms.

¹⁴ Following Nini et al. (2011), I include third-order polynomials of the following five variables on which covenants are typically written: (i) the ratio of operating cash flow to lagged assets, (ii) book leverage ratio, (iii) the ratio of interest expense to lagged assets, (iv) the ratio of net worth to assets, and (v) current ratio defined as current assets divided by current liabilities. Henceforth, I will refer to these variables collectively as "covenant controls".

Therefore, my primary disclosure measure is an indicator for the issuance of a management forecast during a fiscal quarter (*FCAST*).¹⁵ I measure *FCAST* using data from the First Call Company Issued Guidelines database (First Call). To ensure meaningful coverage of firms, I fix 1995 as the first year in my sample (Miller, 2002). I drop firms that never appear in the First Call database during my sample period.

To accommodate the possibility that some firms may vary the precision of the information communicated in management forecasts while continuing to issue them, I also examine changes in forecast informativeness around covenant violations. I use the following five measures of forecast informativeness (See Appendix A.1 for further detail on variable construction).¹⁶

1. *PRECISION* – equals 4 for point forecasts, 3 for range forecasts, 2 for open-ended forecasts, and 1 for completely qualitative forecasts.
2. *SPECIFICITY* – equals negative of the top of the forecast range less the bottom of the forecast range deflated by the pre-release share price for all range estimates; it equals zero for point estimates. Tighter estimates indicate greater forecast informativeness.
3. *HORIZON* – equals the number of days between the release date of the forecast and the end date of the fiscal period being forecasted. Timelier disclosure of private information through forecasts with longer horizons indicates greater forecast informativeness.

¹⁵ I retain earnings pre-announcements – forecasts issued on or after the corresponding fiscal-period end – in my sample to allow for the possibility that these pre-announcements may also contain non-earnings or soft information. My findings are, however, robust to exclusion of pre-announcements.

¹⁶ Note that, as pointed out by Leuz and Verrecchia (2000), the theory is sufficiently broad as to allow the notion of “increased levels of disclosure” to be interpreted either as an increase in the quantity of disclosure or an increase in the informativeness of the disclosure or both. Therefore, all of my measures of disclosure can be considered as proxies for the extent to which managers release their private information, regardless of whether the measure appears to capture the notion of the quantity (e.g., dummy for management forecast) or the informativeness (e.g., specificity of the management forecast) of disclosures.

4. *ACCURACY* – equals negative of the absolute difference between the earnings forecast and the actual earnings deflated by the pre-release share price; for range forecasts, the average of the range endpoints is used to compute *ACCURACY*.
5. *ΔLAMBDA* – equals the difference between the natural logarithm of the average adverse selection component of the bid-ask spread before and after the forecast release date, where both averages are measured over 5 trading days.¹⁷ More informative forecasts should lead to greater reduction in the adverse selection component of bid-ask spread (Rogers, 2008).

Since all five variables are defined such that a greater value indicates greater forecast informativeness, I expect them to be negatively associated with covenant violation.

3.2 Covenant violation data

I obtain covenant violation data from Nini, Smith and Sufi (2011)'s database, which provides this information for a large sample of firms in the Compustat database.¹⁸ One potential concern with this database is that it does not distinguish between covenant violations related to bank debt and public debt. I do not, however, believe this is a source of significant concern for three reasons. First, the above limitation of the data is likely to bias my tests against finding results consistent with my predictions because outside creditors, in contrast to banks, are likely to prefer greater disclosure. Second, covenants in private debt contracts are usually "tighter" relative to covenants in public debt contracts (DeAngelo, DeAngelo and Skinner, 1994; Sweeney, 1994; Kahan and Tuckman, 1995) and are, therefore, likely to get violated prior to the covenants in public debt contracts. Finally, the existence of cross-default provisions in most loan

¹⁷ I thank Dan Taylor for sharing data on the adverse selection component of the bid-ask spread. This data is available until year 2007 leading to a smaller sample size for *ΔLAMBDA* relative to other measures for which data is available till 2009.

¹⁸ Readers are referred to the Appendix in Nini, Smith and Sufi (2011) for further details on the procedure used for the construction of this dataset.

agreements (Beatty, Liao, and Weber, 2011) ensures that the violation of a covenant in a public debt contract automatically puts the borrower in default on its private debt. Despite the above mitigating features, to further allay this concern, I also check whether my findings hold on a subsample of firms that do not have a long-term public debt rating issued by Standard & Poor's since these firms are likely to have been financed only using private debt.¹⁹

3.3 Sample and descriptive statistics

Table 1, Panel A provides descriptive statistics for the sample used to analyze the likelihood of issuance of a management forecast. The final sample comprises a maximum of 115,661 firm-quarter observations from 1995 to 2009 for which information on all required variables is available. The unconditional likelihood of forecast issuance in the sample is 31%, and the likelihood of a firm being in covenant violation in any quarter is 5.2%. Panel B provides descriptive statistics for my measures of forecast informativeness for the subsample of firm-quarter observations with a management forecast. This sample comprises a maximum of 35,826 observations. The likelihood of covenant violation in any quarter among firms that issue management forecasts is 2.8%, which is significantly lower than the unconditional likelihood of covenant violation.

4. Analysis of management forecasts

4.1 Primary results

Table 2 provides evidence on the relation between the likelihood of management forecast issuance and covenant violation by presenting estimates of Equation (1) with an indicator for the issuance of a management forecast as the dependent variable. If firms' disclosure choices following a covenant violation reflect greater weight on banks' preferences, then a reduction in

¹⁹ Himmelberg and Morgan (1995) note that virtually all bond and commercial paper issues are rated by Standard and Poor's.

the likelihood of issuance of management forecasts should be observed. Column (i) presents estimates for the full sample. Consistent with my prediction, the coefficient on *VIOLATION* is negative and statistically significant, indicating an average decline of 3.9% in the probability of issuance of a management forecast after a covenant violation. Estimates in Column (ii) show that this finding is robust to inclusion of higher order nonlinear transformations of variables on which covenants are written, suggesting that the estimated effect of covenant violation indeed represents the effect of discontinuous increase in banks' bargaining power. Finally, in Column (iii), I restrict the estimation to a subsample of firms without a long-term debt rating to focus on firms that are likely to have been financed exclusively through private debt. The coefficient on *VIOLATION* remains negative and significant, indicating that my results for the full sample are not driven by covenant violations in public debt contracts.

The decline in the likelihood of the issuance of a management forecast is also economically significant: a 4% decline following a covenant violation represents approximately 13% of the unconditional probability of the issuance of a forecast in my sample. To further assess the economic significance of this finding, I compare the effect of covenant violation to the effect of the book-to-market ratio, which is an important determinant of firms' disclosure choices in its own right. The effect of a covenant violation is approximately as large as the effect of a 1.9 standard deviations increase in the book-to-market ratio.

Table 3 presents several robustness checks to further ensure that my findings reflect changes in firms' disclosures around covenant violations rather than persistent cross-sectional differences between disclosure policies of covenant violators and non-violators. First, Columns (i) through (vi) present estimates of various versions of Equation (1) that differ in the relative timing of the measurement of covenant violation and forecast issuance. Estimates show that the

coefficient on *VIOLATION* is negative and significant only if it is measured with a lag relative to *FCAST* and is not significantly different from zero when it is measured contemporaneously or in future quarters relative to *FCAST*. This time series pattern, which is consistent with my hypothesis, cannot be explained by unobserved persistent differences between violators and non-violators. In addition, estimates in Columns (vii) through (ix) show that my findings are robust to (1) inclusion of an indicator for forecast issuance in the same quarter of the previous year as a control variable, (2) estimation of a linear probability model for *FCAST* with firm fixed effects, and (3) estimation of Equation (1) after removing all firms that never violate a covenant. These findings provide further validation for my identification strategy.

In an untabulated analysis, I also examine whether my findings hold for a subsample of firms that issued forecasts on a regular basis prior to covenant violation.²⁰ This analysis reveals a significant decline in the probability of forecast issuance after a covenant violation for regular forecasters. This indicates that my findings likely reflect a shift in firms' equilibrium disclosure choices and not just a temporary omission of forecasts after violations. This result also mitigates concerns that my inferences may be confounded by other determinants of disclosure that exhibit temporary changes around covenant violations (e.g., uncertainty) since the subsample of regular forecasters is likely to be composed of firms that were committed to issuing forecasts regardless of temporary changes in economic conditions.

I next examine whether some firms respond to a covenant violation by reducing the precision of the information communicated in the forecasts rather than by abstaining from issuing forecasts. Table 4 presents the analysis using five measures of forecast informativeness for the subsample of firms that continue to issue forecasts. The model for *PRECISION*, which is

²⁰ Following Rogers, Skinner, and Van Buskirk (2009), I classify a firm as regular forecaster during a quarter if it issued a forecast in at least three of the four preceding quarters.

an ordinal variable, has been estimated using ordered probit. Models for all other measures of forecast informativeness have been estimated using the linear specification with firm fixed effects in equation (2). To allow for the possibility that forecast properties may be systematically different for quarterly and annual forecasts, I include indicators for quarterly and annual forecasts as additional control variables. Estimates show that the coefficient on *VIOLATION* is not significantly different from zero in any specification. This suggests that borrowers cater to banks' preferences for opacity by omitting the forecasts altogether rather than by issuing forecasts of lower informativeness.

4.2 Effect of the severity of covenant violation

The increase in the banks' bargaining power should be greater following more severe covenant violations, which reflect a material decline in borrowers' credit quality, relative to violations that reflect a temporary and less severe decline in performance with no material change in credit quality. This variation in the banks' bargaining power could, for example, be the result of better access to non-bank sources of capital available to borrowers with better credit quality. Accordingly, I expect the decline in disclosure to be greater following more severe covenant violations as these violations provide banks with a greater ability to extract concessions from their borrowers. To test this prediction, I measure the severity of violations using the length of time during which the borrower remains in violation. Borrowers with a more permanent decline in credit quality are likely to violate covenants frequently and thus remain in violation for longer periods. In contrast, violations for healthier borrowers are likely to be one-off events caused by a temporary and less severe decline in performance.

I test this prediction by estimating a modified version of Equation (1) that includes eight lags of *VIOLATION*. The dependent variable is the indicator for management forecast issuance.

Table 5, Panel A presents the analysis. Column (i) presents the individual effect of each covenant violation dummy; Column (ii) presents the cumulative effect. Consistent with my prediction, the decline in the likelihood of a forecast increases monotonically with the number of quarters in which a firm was in violation of a covenant. In particular, it increases from 2.1% for a firm that was in violation for only the previous quarter (t) to 16.3% for a firm that was in violation for the previous eight quarters (t to $t-7$).

In a related analysis, presented in Panel B, I also examine whether some firms revert back to higher levels of disclosure once they are no longer in violation of covenants. Specifically, I test whether the effect of prior violations (quarters t to $t-7$) is mitigated if the firm is not in violation during the current quarter ($t+1$), by estimating a modified version of Equation (1) that includes dummies for covenant violation during quarters t to $t-7$ and their interaction terms with the indicator for violation during quarter $t+1$. Column (i) (Column (ii)) presents the cumulative effect of covenant violations during the previous two years separately if the firm was (was not) in violation during quarter $t+1$; Column (iii) presents the difference between the effects reported in Columns (i) and (ii). Estimates show that while the effect of violations during the previous year (quarters t to $t-3$) does not significantly depend on whether the firm is currently in covenant violation, the decline in the likelihood of forecast issuance due to violations during quarters $t-4$ to $t-7$ is significantly lower if the firm is not in violation during the current quarter. This finding suggests that some firms gradually (with a lag of one year) revert to higher levels of disclosure once they are no longer in covenant violation and the bargaining power of banks decreases.

4.3 Cross-sectional analysis

This section explores the cross-sectional variation in the effect of covenant violation on the likelihood of the issuance of a management forecast. For continuous partitioning variables, I

estimate augmented versions of Equation (1) that include three dummy variables indicating the membership of the partitioning variable to the bottom, middle or top tercile of the distribution, and their interaction terms with the indicator for covenant violation as additional covariates. For dummy partitioning variables, Equation (1) is augmented with the partitioning variable and its interaction term with *VIOLATION*. Table 6, Panel A presents the analysis for continuous partitioning variables, and Panel B presents the analysis for dummy partitioning variables.

4.2.1 Reliance on bank financing

Greater reliance of firms on bank financing should increase the size of banks' economic benefit from a decline in firms' disclosure, and should also increase banks' bargaining power relative to dispersed shareholders and bondholders. Therefore, managers of firms with greater reliance on bank financing should be more likely to cater to banks' preferences after a covenant violation due to the greater incentives and ability of the banks to extract concessions from such firms. Accordingly, I expect the decline in disclosure following a covenant violation to be larger for firms that are more reliant on bank financing.

I use three measures of reliance on bank financing to test this prediction. First, I use book leverage measured at least one quarter before measuring covenant violation. Consistent with my prediction, Column (i) of Table 6, Panel A shows that covenant violations have no effect on the likelihood of a forecast issuance for firms with relatively low reliance on debt capital (i.e., those with book leverage in the bottom tercile), but that the effect is negative and significant for the middle and top terciles.

My second measure, *FRACPVTASSETS*, provides a more direct estimate of firms' reliance on bank financing, and therefore, unlike book leverage, provides a measure of inside banks' bargaining power over both shareholders and bondholders. The limitation of this measure

is that it is available only for a smaller sample of firms with private loans reported in the Loan Pricing Corporation's Dealscan database. For this analysis, I construct a sample of covenant violations on private loans (dealscan sample) by closely following the approach of Chava and Roberts (2008), which I describe in more detail in Appendix B. *FRACPVTASSETS* is estimated as the total amount that can be borrowed from banks (or deal size) under the deal used for identifying covenant violation scaled by total book value of assets. The estimates presented in Column (ii) of Panel A show that the decrease in the probability of a forecast following a covenant violation increases monotonically from the bottom tercile (coefficient of 4%) to the top tercile (coefficient of 11%), and is significantly different from zero only for the top tercile. Collectively, these results suggest that the decline in disclosure following covenant violations manifests only for firms that exhibit a significant reliance on bank financing.

Finally, I examine the effect of the fraction of the borrowers' total debt financed by banks (*FRACPVTDEBT*). This analysis offers an especially useful test to mitigate concerns regarding omitted correlated variables. If the decline in disclosure following a covenant violation is indeed caused by the transfer of control rights to well informed inside banks, then the covenant violation should not have any effect on firms that are primarily financed by public debt, as dispersed bondholders do not gain from a decline in disclosure. A finding of a similar decline in disclosure both for firms primarily financed by banks and for firms primarily financed by public debt would raise concerns that my results may be driven by some omitted variable correlated with covenant violation. *FRACPVTDEBT* is available only for the dealscan-sample and is estimated as deal size scaled by total debt. Estimates in Column (iii) of Panel A show that the effect of covenant violation is not significantly different from zero for the bottom tercile of *FRACPVTDEBT*, which is likely to be comprised of firms that are relatively more reliant on public debt. The effect of

covenant violation is negative and significant for the middle and top terciles of *FRACPVTEBT*, and increases monotonically in magnitude from the bottom to the top tercile. This suggests that my findings are unlikely to be driven by omitted correlated variables such as uncertainty, as it is not obvious why the effect of an increase in uncertainty around covenant violations would be systematically greater for firms primarily financed by public debt relative to firms primarily financed by bank debt.²¹

4.2.2 Relationship banking

Banks typically acquire private information about borrowers through repeated interactions over the course of the lending relationship. Consequently, a bank that is a transaction-specific lender, rather than a long-term relationship lender, is unlikely to possess significant private information and is therefore unlikely to experience meaningful economic gains from decreased disclosure. Thus, I expect the decline in disclosure after a covenant violation to be larger for firms that are reliant on relationship banking to obtain private debt. I test this prediction on the dealscan-sample using an indicator that equals one if the loan used to identify covenant violation is a relationship loan (*RELATIONSHIP*). I consider a loan to be a relationship loan if the lead arranger of the loan was also the lead arranger for any one of the prior loans taken by the borrower. Estimates presented in Column (i) of Table 6, Panel B show that the effect of covenant violation on the likelihood of the issuance of a management forecast is negative and significant only for relationship loans. This indicates that the decline in disclosure

²¹ It is useful to note that these findings are unlikely to be caused by systematic differences between firms that vary in their mix of private and public debt as my empirical specification includes the main effect of the proportion of private debt as a control variable. To further guard against this concern, however, in untabulated analysis, I estimate this specification after including known determinants of firms' mix of public and private debt that are not part of my main empirical specification. My choice of these variables is guided by prior literature on the determinants of firms' mix of private and public debt (e.g., Houston and James, 1996; Johnson, 1997; Krishnaswami et al., 1999; Cantillo and Wright, 2000; Denis and Mihov, 2003). My inferences remain unaltered after including these control variables.

manifests only for firms whose lenders have significant access to private information about them, and therefore are likely to gain the most from a decline in disclosure.

4.2.3 Growth opportunities

Excessive reliance on a single source of capital should be costlier for firms with higher growth opportunities as it increases their vulnerability to ex-post extraction of rents by the capital provider (Houston and James, 1996). Therefore, all else being equal, managers of firms with higher growth opportunities should be less willing to cater to banks' demand for opacity since opacity would further increase firms' reliance on bank financing by making capital from outside investors less accessible. On the other hand, the greater size of the economic benefit associated with higher growth opportunities should enhance banks' incentive to maintain their information monopoly over the borrower through reduced disclosure. Therefore, the effect of growth opportunities on the relation between covenant violation and disclosure is theoretically ambiguous. I use the book-to-market ratio as a proxy for a firm's growth opportunities. Estimates in Column (iv) of Panel A show that the reduction in the probability of a management forecast caused by covenant violation increases monotonically from the bottom tercile to the top tercile of book-to-market ratio, and it is not significantly different from zero for the bottom tercile. These findings suggest that managers' propensity to cater to banks' demand for reduced disclosure is tempered by a concern for increased reliance on a single source of capital to finance future growth opportunities.

4.2.4 Earnings timelines

The information advantage conferred to banks by the omission of an earnings forecast should increase with the timeliness with which earnings incorporate information about firm performance. I therefore expect banks' demand for withholding earnings forecasts to be stronger

in industries in which earnings are more timely, and therefore are an important channel through which managers communicate their private information. I adopt two approaches to measure variation in earnings timeliness across different industries. For the first approach, I follow Basu (1997) and estimate earnings timeliness at the 3-digit SIC level as the R-squared from annual cross-sectional regressions of earnings on stock returns, an indicator for negative stock returns, and the interaction between this indicator and stock returns (*TIMELINESS*). I measure *TIMELINESS* in the fiscal year prior to the fiscal year of the quarter in which covenant violation is measured. For the second approach, I use an indicator for industries with high levels of intangible assets (*HIINTANGIBLE*) as the partitioning variable.²² This approach considers the earnings of firms in high-intangible industries to be less timely than the earnings of firms in other industries.

Column (v) of Table 6, Panel A presents the analysis with *TIMELINESS*, and Column (ii) of Panel B presents the analysis with *HIINTANGIBLE* as the partitioning variable. Both specifications provide evidence consistent with my prediction. The decline in the probability of earnings forecast issuance increases monotonically from the bottom tercile to the top tercile of *TIMELINESS*, and is not significantly different from zero for the bottom tercile. Furthermore, the decline is significantly different from zero only for firms that do not belong to high-intangible industries. Collectively, the analysis indicates that the banks' influence on earnings forecasts issuance only manifests in industries in which the lack of a forecast provides banks with a meaningful information advantage.²³

²² Following Collins, Maydew and Weiss (1997), I classify industries with SIC codes 282 (plastics and synthetic materials), 283 (drugs), 357 (computer and office equipment), 367 (electronic components and accessories), 48 (communications), 73 (business services), and 87 (engineering, accounting, R&D and management related services) as high-intangible industries.

²³ In untabulated analysis, I also test this prediction using firm age as a partitioning variable as earnings of younger firms are likely to be less timely and less informative than earnings of older and mature firms. Consistent with my

5. Additional tests

5.1 Other disclosure channels

To determine whether managers' propensity to disclose less after a covenant violation also manifests through other disclosure channels and to mitigate concerns regarding substitution between different disclosure channels, this section presents an analysis of disclosures in the Management Discussion and Analysis section (MD&A) of 10-K and 10-Q filings (Section 5.1.1) and earnings press releases (Section 5.1.2). This analysis is based on disclosure measures constructed from an automated search of MD&As and earnings press releases for key terms and expressions as described in Appendices A.2 and A.3.

5.1.1 Management Discussion and Analysis

I use two empirical proxies to capture the precision of the private information released through MD&As.²⁴ My first proxy measures the extent of forward-looking information released in MD&As, and is computed as the natural logarithm of one plus the proportion of words that indicate forward-looking information (*PROPFORWARD*). This proxy is motivated by several studies that find that the information in MD&As that is incremental to other sources of information and that is useful for forecasting future firm performance is primarily contained in forward-looking statements (Bryan, 1997; Clarkson, Kao and Richardson, 1999; Barron, Kile

prediction, I find that the decline in disclosure does not manifest in the bottom tercile of the age distribution and is significant only in the middle and top terciles.

²⁴ While analysis based on readability (untabulated) as a proxy for the informativeness of MD&As, measured using the FOG index (Li, 2008), provides evidence consistent with my hypothesis, I do not use it to support my inferences due to significant concerns regarding the construct validity of the FOG index specific to the setting of covenant violations. Covenant violations can be associated with several significant changes such as executive turnover, changes in board composition, renegotiation of financial contracts, and asset sales. If the description of these potentially complex events requires use of longer and complex sentences, then an association between covenant violation and the FOG index, instead of reflecting an increase in comprehension difficulty deliberately induced by managers, may simply reflect significant changes accompanying a covenant violation.

and O’Keffe, 1999; Li, 2010). I predict a reduction in forward-looking disclosures in MD&As after a covenant violation.

My second proxy is intended to capture the overall precision of MD&A disclosures and is calculated as the natural logarithm of one plus the proportion of numeric terms (*PROPNUMERIC*). Greater use of verifiable numeric terms not only provides investors with more precise information about the firm’s financial condition, but also enhances the credibility of the “soft” information contained in MD&As (Dye and Sridhar, 2004; Demers and Vega, 2010).²⁵

Table 7 presents the analysis. The only significant finding from this analysis, as seen in Column (ii), is that MD&As filed subsequent to a covenant violation contain significantly fewer numeric disclosures. Estimates in Column (iii) and (iv) show that this finding is robust to inclusion of nonlinear transformations of covenant controls and to estimation on a sample of firms without a public debt rating. I do not, however, find any changes in the extent of forward-looking disclosures in MD&As around covenant violations. To the extent numeric disclosures provide more precise and verifiable information to investors, the above evidence is consistent with my prediction and indicates that managers provide less informative MD&As after a covenant violation.

5.1.2 Earnings press releases

My first set of measures to capture the extent of private information released by managers through earnings press releases is a series of five indicators for the disclosure of specific accounting information. The first three indicators capture the presence of a balance sheet (*BS*), an income statement (*IS*), and a cash flow statement (*CF*) in the press release. The next two variables focus on investments and indicate the disclosure of capital expenditure (*DISCAPEX*)

²⁵ Consistent with this claim, Demers and Vega (2010) document greater market reaction to soft information in earnings press releases that contain more numeric terms.

and the disclosure of research and development expenditure (*DISCRD*) in the earnings press release.

I use two additional measures of the informativeness of earnings press releases: (i) the extent of forward-looking disclosures, and (ii) the extent of numeric disclosures. The computation of these two variables for earnings press releases is analogous to that for MD&As.

The first set of analyses, presented in Table 8, examine the effect of covenant violations on the likelihood of disclosure of specific accounting information in earnings press releases. To ensure that my results for disclosure of capital expenditures, and R&D spending are not spuriously driven by a reduction in investments following a covenant violation, I include the levels of capital expenditure and R&D spending scaled by total book value of assets (*CAPEX* and *R&D*) as additional control variables. Estimates in Panel A of Table 8 show that earnings press releases issued after a covenant violation are significantly less likely to contain a cash flow statement, and are significantly less likely to disclose capital expenditures. I do not find any significant relation between covenant violation and the likelihood of disclosure of a balance sheet, an income statement, and research and development expenditure. Sensitivity analyses presented in Panel B show that these findings are robust to inclusion of nonlinear transformations of covenant controls, and to estimation on a sample of firms without a long-term debt rating. The second set of analyses, presented in Table 9, which examine the effect of covenant violation on the extent of forward-looking disclosures and the extent of numeric disclosures, do not reveal any significant findings.

The above findings on their own provide only limited evidence in support of my hypothesis because the information advantage conferred to banks by the omission of cash flow statements, and capital expenditures from press releases is likely to be small. These findings,

however, complement my previous results by further mitigating concerns that my inferences may be confounded by an increase in litigation risk or uncertainty around covenant violations. Unlike management forecasts, the disclosure of a cash flow statement or capital expenditures for a prior period is not forward-looking, and thus the decision of whether to disclose these items is unlikely to be driven either by concerns regarding future litigations related to unattained forecasts, or by managers' inability to make forecasts under heightened uncertainty. To the extent my empirical specifications do not perfectly control for uncertainty or litigation risk, this feature of my findings further enhances the credibility of my inferences.

5.2 Effect on analyst forecast errors and the adverse selection component of the bid-ask spread

In this section, I examine the effect of covenant violations on two indirect but composite measures of disclosure: analyst forecast errors (*AFE*) and the adverse selection component of the bid-ask spread in equity prices (*LAMBDA*). The construction of these variables is described in Appendix A.4. A reduction in the amount of public disclosure after a covenant violation should lead to an increase in *AFE* by reducing the amount of information available to analysts, and should also lead to an increase in *LAMBDA* by increasing the information gap between informed and uninformed traders in equity markets. I test these predictions by estimating Equation (2) with *AFE* and *LAMBDA* as the dependent variables.²⁶

Table 10 presents the analysis. Consistent with my predictions, estimates in Columns (i) and (ii) show that, following a covenant violation, there is a significant increase in analyst

²⁶ The model for *AFE* differs from previously estimated version of Equation (2) in the composition of control variables. The model for *AFE* does not include forecast dispersion and analyst following as control variables, and, following Lang and Lundholm (1996), includes firms' historical return-earnings correlation (*CORRETEARN*), and earnings surprise (*SURPRISE*) as control variables. *CORRETEARN* is measured as the R-squared from regressions of annual earnings on annual stock return, a dummy for a negative stock return, and the interaction between stock return and the dummy for negative stock return. *SURPRISE* is measured as the absolute value of the difference between current quarter's earnings and earnings for the same quarter in the previous fiscal year, scaled by stock price two days prior to the earnings announcement.

forecast errors and the adverse selection component of the bid-ask spread. Estimates in Columns (iii) through (vi) show that these findings are robust to implementation of a quasi-regression discontinuity approach, and to estimation on a subsample of firms that are likely to have been financed exclusively through private debt. Since analyst forecast errors and the adverse selection component of the bid-ask spreads reflect the combined effect of all public disclosures made by managers, these findings enhance the credibility of my main results by mitigating concerns that my analysis may have overlooked some important disclosure channel.

5.3 Are my findings driven by lack of dependence on equity markets?

I now examine whether my findings are driven by firms' lack of ability or need to raise capital from equity markets after a covenant violation. Firms that do not anticipate raising capital from equity markets may reduce costly disclosure. To rule out this alternative explanation for my findings, I estimate Equations (1) and (2) on a subsample of firms that were net issuers of equity during the one year period following the quarter of the measurement of disclosure. Following Fama and French (2008), I classify a firm as a net issuer of equity during a period if the ratio of its split-adjusted outstanding shares at the end of the period to the number of split-adjusted outstanding shares at the beginning of the period is greater than one. Estimates presented in Panel A of Table 11 show that all of my findings are robust to estimation on a sample of firms that were net issuers of equity, indicating that these findings cannot be explained by firms' lack of dependence on equity capital.

5.4 Can Substitution of shareholder monitoring by bank monitoring explain my findings?

If shareholders are less concerned about managerial slack when firms are under close scrutiny from banks around covenant violations, they may rationally choose to not duplicate banks' costly monitoring effort and, consequently, may demand less disclosure that is required to

aid their monitoring efforts. Note that the implementation of the regression discontinuity approach helps ensure that my inferences are not confounded by this alternative explanation since it is not obvious why this substitution effect will manifest discontinuously at the covenant threshold. However, I conduct an additional test to further mitigate this concern. Specifically, I confine my sample to firms that are less likely to rely on shareholder monitoring prior to covenant violation, and therefore are unlikely to exhibit a substitution of monitoring between shareholders and banks following a covenant violation. I identify firms that are less likely to rely on shareholder monitoring as firms with high levels of insider ownership, since insider ownership can act as a substitute for direct monitoring by outside shareholders (e.g., Prendergast, 2002). Specifically, I estimate Equations (1) and (2) on a sample of firms whose percentage of outstanding equity held by their officers and directors falls in the top tercile of the distribution. I measure insider ownership at least one quarter prior to the quarter of covenant violation using Thomson Reuters's compilation of insider trades that are filed with the Securities and Exchange Commission. Estimates presented in Panel B of Table 11 show that my results are also found on a sample of firms with high insider ownership. Accordingly, my results are unlikely to be due to substitution of shareholder monitoring by bank monitoring following a covenant violation.

6. Conclusion

This study examines whether banks' incentives to enhance their share of the surplus from the lending relationship by suppressing disclosure to other competing capital providers can have an economically significant influence over borrowers' disclosure choices. Banks that actively monitor borrowers acquire private information that gives them an information advantage relative to competing outside banks and dispersed shareholders and bondholders that do not actively monitor and that are relatively more reliant on public disclosures to obtain information about the

firm. Banks benefit from this information advantage as it allows them to retain a greater share of the surplus generated by their monitoring efforts. Banks will, therefore, prefer less public dissemination of information that is available to them privately.

I test the above theory by examining changes in firms' disclosure choices around covenant violations, which trigger a transfer of control rights to banks who can then use their enhanced bargaining power to impose their preferences on various firm policies, including disclosure policies. Analysis reveals a significant decline in firms' disclosures after a covenant violation. This evidence is consistent with manifestation of banks' preferences for opacity in borrowers' disclosure choices.

Appendix A: Description of measures of disclosure and control variables

A.1. Management forecasts

Measures of disclosure based on management earnings forecast are constructed using data from First Call Company Issued Guidelines database for years 1995 to 2009. The table below describes these measures.

Variable name	Description
<i>FCAST</i>	<i>FCAST</i> is an indicator for the issuance of a management forecast during a quarter.
<i>PRECISION</i>	<i>PRECISION</i> equals 4 for point forecasts, 3 for range forecasts, 2 for open-ended forecasts, and 1 for completely qualitative forecasts.
<i>SPECIFICITY</i>	<i>SPECIFICITY</i> equals negative of the top of the forecast range less the bottom of the forecast range deflated by the pre-release share price for all range estimates; it equals zero for point estimates
<i>HORIZON</i>	<i>HORIZON</i> is the number of days between the release date of the forecast and the end date of the fiscal period being forecasted.
<i>ACCURACY</i>	<i>ACCURACY</i> is the negative of the absolute difference between the earnings forecast and the actual earnings deflated by the pre-release share price; for range forecasts, the average of the range endpoints is used to compute <i>ACCURACY</i> .
$\Delta LAMBDA$	$\Delta LAMBDA$ equals the difference between the natural logarithm of the average adverse selection component of the bid-ask spread before and after the forecast release date, where both averages are measured over 5 trading days; measurement of the adverse selection component of the bid-ask spread follows the methodology of Madhavan, Richardson, and Roomans (1997) as modified by Armstrong, Core, Taylor, and Verrecchia (2011).

A.2. Management discussion and analysis

To construct my sample of MD&As, I use a python script to first download all 10-Ks and 10-Qs from the SEC edgar website filed between the years 1995 and 2010, and then extract the MD&A section after removing all HTML tags from the filing. The script then searches the text of MD&As for relevant terms and expressions to compute various measures of disclosure defined in the below table.

Variable name	Description
<i>PROPFORWARD</i>	<i>PROPFORWARD</i> is the proportion of words that indicate forward-looking information. Words indicating forward-looking information, identified in Li (2010), which I use to compute <i>PROPFORWARD</i> , are: “will,” “should,” “can,” “could,” “may,” “might,” “expect,” “anticipate,” “believe,” “plan,” “hope,” “intend,” “seek,” “project,” “forecast,” “objective,” and “goal.” Following Li (2010), I exclude words “expected,” “anticipated,” “forecasted,” “projected,” “believed” when such words follow “was,” “were,” “had,” and “had been”; because situations like these typically indicate a sentence that is not forward-looking in nature.
<i>PROPNUMERIC</i>	<i>PROPNUMERIC</i> is the proportion of numeric terms in the text.

A.3. Earnings press releases

To construct my sample of earnings press releases, I use 8-K filings that were triggered by an SEC rule effective March 28, 2003, which required public companies to furnish a Form 8-K within five business days after issuing any press release or other disclosure about the company's quarterly or annual financial results that contains material nonpublic information. Press releases attached to 8-Ks under this rule are available either under Item 12 or Item 2.02 of the 8-K. I use a python script to first download all 8-K filings with Item 12 or Item 2.02 from the Sec Edgar website. The script then extracts the earnings press release from the relevant section of the filing after removing all HTML tags. Finally, the script searches the text of the press releases for relevant terms and expressions to compute various measures of disclosure defined in the below table.

Variable name	Description
<i>IS</i>	<i>IS</i> is an indicator for the presence of an income statement. I identify the presence of an income statement by searching the text of the press release for various versions of the phrases: "statement of earnings", "statement of income", and "statement of operations".
<i>BS</i>	<i>BS</i> is an indicator for the presence of a balance sheet. I identify the presence of a balance sheet by searching the text of the press release for various versions of the phrases: "consolidated balance sheet", "condensed balance sheet", and "unaudited balance sheet".
<i>CF</i>	<i>CF</i> is an indicator for the presence of a cash flow statement. I identify the presence of a cash flow statement by searching the text of the press release for various versions of the phrase "cash flow".
<i>DISCAPEX</i>	<i>DISCAPEX</i> is an indicator for the disclosure of capital expenditure. <i>DISCAPEX</i> equals 1 if the text of the press release mentions "capital expenditure" or "capital spending".
<i>DISCRD</i>	<i>DISCRD</i> is an indicator for the disclosure of research and development expenditure. <i>DISCRD</i> equals 1 if the text of the press release contains one of the following phrases: "research and development expenditure", "research and development expense", "research and development spending", and "research and development activity".
<i>PROPFORWARD</i>	Similar to MD&As (see A.2).
<i>PROPNUMERIC</i>	<i>PROPNUMERIC</i> is the proportion of numeric terms in the text.

A.4. Composite measures of disclosure

Below table describes two indirect but composite measures of disclosure that reflect the combined effect of all public disclosures made by managers.

Variable name	Description
<i>AFE</i>	<i>AFE</i> is the absolute value of the difference between the median analyst earnings forecast and actual earnings scaled by stock price prevailing two days prior to the earnings announcement. It is measured using all active earnings forecasts available for the fiscal quarter following the quarter $t+1$.
<i>LAMBDA</i>	<i>LAMBDA</i> is the daily average of the adverse selection component of the bid-ask spread in equity prices during quarter $t+1$. Measurement of the

	adverse selection component of the bid-ask spread follows the methodology of Madhavan, Richardson, and Roomans (1997) as modified by Armstrong, Core, Taylor, and Verrecchia (2011).
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A.5. Control variables

All control variables have been constructed using information on financial reports from COMPUSTAT, analyst related information from IBES database, stock price information from CRSP database, and information on institutional ownership from Thomson financial 13f institutional holdings database.

Variable Name	Description
$LOGASSETS_t$	$LOGASSETS_t$ is natural logarithm of the book value of total assets at the end of quarter t . This variable is used to control for firm size as larger firms may disclose more due to economies of scale in disclosure.
$BKLEV_t$	$BKLEV_t$ is the book value of the total long-term debt divided by the total book value of assets at the end of quarter t . Firms that exhibit a greater reliance on dispersed shareholders to finance their assets may face greater demand for disclosure relative to, for instance, firms that exhibit greater reliance on bank debt.
$BTOM_t$	$BTOM_t$ is the ratio of the book value of total assets to the sum of the firm's market capitalization and book value of liabilities measured at the end of quarter t . This variable is used to control for a firm's growth opportunities. Firms with higher growth opportunities may disclose more in anticipation of approaching capital markets to finance future projects.
$RETURN_t$	$RETURN_t$ is the stock return during quarter t . This variable is used to control for firm performance, as firms are unlikely to disclose below a threshold level of news about firm value (Verrecchia, 1983).
$PROBLIT_t$	$PROBLIT_t$ is the probability of litigation at the end of quarter t estimated using the probit model in the Appendix B of Rogers and Stocken (2005). This variable is included to control for the confounding effect of a potential increase in litigation risk around covenant violations, as managers may disclose less in the face of heightened litigation risk (Baginski et al., 2002; Rogers and Van Buskirk, 2009).
$VOLATILITY_t$	$VOLATILITY_t$ is the standard deviation of daily stock returns during quarter t . This variable used to control for uncertainty regarding future firm performance. If missing forecasts is costly, then managers may be less inclined to issue them under heightened uncertainty.
$NUMANALYSTS_t$	$NUMANALYSTS_t$ is the number of analysts following a firm at the end of quarter t . It is estimated as the number of next quarter earnings forecasts available in IBES. This variable is used to proxy for shareholder demand for disclosure.
$BADNEWS_t$	$BADNEWS_t$ is an indicator variable that equals one if a firm experienced negative stock return during quarter t . This variable is used to control for bad news, as managers, concerned about potential litigations that follow large negative earnings surprises, may issue forecasts to preempt such litigations (Skinner, 1994, 1997).

<i>ISSUE_t</i>	<i>ISSUE_t</i> is an indicator variable that equals one if the firm issued common and preferred stock in quarter $t+1$ in excess of 20% of its market capitalization at the end of the quarter. This variable is used to control for incentives of firms to disclose more in anticipation of raising significant capital in equity markets.
<i>NUMSEGMENTS_t</i>	<i>NUMSEGMENTS_t</i> is the number of business segments in which a firm operates. Since COMPUSTAT segment data is available only annually, <i>NBUS_t</i> for quarter t is measured using segment data for the fiscal year quarter t is part of. This variable is used to proxy for the complexity of a firm's operations. Firms with more complex operations are expected to provide greater disclosure to meet greater demand for information.
<i>ROA_t</i>	<i>ROA_t</i> is the firm's return on assets during quarter t measured as the income before extraordinary items scaled by total book value of assets. This variable is used to control for firm performance.
<i>BLOCK_t</i>	<i>BLOCK_t</i> is an indicator variable for the presence of a blockholder, which is defined as any institutional investor that owns greater than 5% of firm's outstanding equity. Information on institutional ownership is obtained from Thomson financial 13f institutional holdings database. Presence of a blockholder for quarter t is identified using the most recent 13f filing made prior to the end of quarter t . This variable is used to control for Blockholder demand for disclosure.
<i>ERC_t</i>	<i>ERC_t</i> is the firms earnings response coefficient estimated by regressing two-day $[0, +1]$ cumulative market adjusted stock returns on the earnings news of up to 16 preceding quarters, where day 0 is the earnings announcement date; the earnings news of a past quarter equals earnings per share for that quarter minus the most recent prior median analyst earnings forecast, scaled by closing share price on day -1. <i>ERC_t</i> is used to control for the informativeness of a firm's earnings. When earnings are less informative, firms may voluntarily disclose more non-earnings information (Chen et al., 2002; Lougee and Marquardt, 2004). Alternatively, if investors believe earnings to be informative, managers may be more likely to provide forecasts for future earnings (Lennox and Park, 2006).
<i>SURPRISE</i>	<i>SURPRISE</i> for management forecast sample is measured as the absolute value of the difference between earnings per share (EPS) for the quarter during which the management forecast is released and the most recent median analyst earnings forecast during the previous quarter scaled by share price one day before the earnings announcement date. For the MD&A and earnings press release sample, <i>SURPRISE</i> is measured as the absolute value of the difference between EPS for the fiscal quarter of the earnings press release or MD&A and the most recent median analyst forecast scaled by share price one day before the earnings announcement. This variable is used to control for the likely greater shareholder demand for information when reported earnings exhibit greater deviation from expected earnings (Chen et al., 2002; Lennox and Park, 2006).
<i>DISPERSION_t</i>	<i>DISPERSION_t</i> is the dispersion in analysts' quarterly earnings forecasts

	at the end of quarter t scaled by the actual earnings. This variable is used as a proxy for uncertainty regarding future firm performance.
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Appendix B: Construction of sample of covenant violations on private loans

This appendix describes the procedure used for the construction of the sample with an indicator for the violation of a covenant in private debt contracts using information from Loan Pricing Corporation's Dealscan database. The procedure closely follows the approach of Chava and Roberts (2008) and involves the following steps:

1. I first identify loans that are bound by a current ratio or a net worth covenant. I focus on these two covenants for two reasons. First, the accounting measures used for these covenants are relatively standardized and unambiguous, and thus allow for a cleaner identification of covenant violation. Second, these covenants appear most frequently in the dealscan database.
2. I then compute the firms' actual current ratio and net worth ratio at the end of each fiscal quarter using data from Compustat and compare it with the corresponding threshold ratio to determine whether the firm was in violation of a covenant.
3. In case a loan has both net worth and current ratio covenants, I use the tighter of the two covenants to identify a covenant violation.
4. In case of multiple loans at a point in time, I use the loan with the tightest covenants to identify covenant violation.
5. Some of the loans undergo amendments over time which may lead to change in covenant threshold. Since I do not have information on change in covenant thresholds for amended loans, I exclude them from my sample.
6. Net worth covenants sometimes have a "buildup" provision which specifies the changes in the covenant threshold over time with a fraction of positive net income or a fraction of stock issuances. Since I do not have information on the fraction of net income or stock issuance used to adjust the thresholds, I exclude all loans with a buildup provision from my sample.

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Table 1: Descriptive Statistics

This table provides descriptive statistics for various disclosure measures based on management forecasts, and for the indicator for the violation of a covenant (*VIOLATION*). *FCAST* is an indicator for the issuance of a management forecast. *PRECISION* equals 4 for point forecasts, 3 for range forecasts, 2 for open-ended forecasts, and 1 for completely qualitative forecasts. *SPECIFICITY* equals negative of the top of the forecast range less bottom of the forecast range deflated by the pre-release share price for all range estimates; it equals zero for point estimates. *HORIZON* equals the number of days between the release date of forecast and the end date of fiscal period being forecasted. *ACCURACY* is the negative of the absolute difference between the earnings forecast and the actual earnings deflated by the pre-release share price; for range forecasts the average of the range endpoints is used to compute *ACCURACY*. *ΔLAMBDA* equals the difference between the natural logarithm of the average adverse selection component of the bid-ask spread before and after the forecast release date, where both averages are measured over 5 trading days; measurement of the adverse selection component of the bid-ask spread follows the methodology of Madhavan et al. (1997) as modified by Armstrong et al. (2011).

	Mean	StDev	25 th Percentile	50 th Percentile	75 th percentile
Panel A: Full sample					
FCAST	0.310	0.462	0.000	0.000	1.000
VIOLATION	0.052	0.222	0.000	0.000	0.000
Panel B: Subsample of firm-quarters with a management forecast					
PRECISION	3.074	0.666	3.000	3.000	3.000
SPECIFICITY	-0.002	0.004	-0.003	-0.001	-0.000
HORIZON	120.143	112.282	48.000	96.000	180.667
ACCURACY	-0.007	0.016	-0.006	-0.002	-0.001
ΔLAMBDA	0.042	0.762	-0.163	0.049	0.268
VIOLATION	0.028	0.165	0.000	0.000	0.000

Table 2: Covenant violation and management forecast issuance

This table presents probit regression estimates of Equation (1). Dependent variable (*FCAST*) is an indicator for the issuance of a management forecast. *VIOLATION* is an indicator variable that equals 1 if the firm was in the violation of a debt covenant. All estimates reflect marginal effects computed at the mean of the independent variables. All specifications include industry and time dummies. For brevity, coefficient estimates on control variables in changes have been omitted. Coefficient estimates on *ERC* have been multiplied by 100. Standard errors have been obtained by clustering at the firm level. Statistical significance (two-sided) at the 10%, 5% and 1% level is denoted by *, **, and ***, respectively.

	Full sample	Higher powers of covenant controls	No public debt rating
	(i) $FCAST_{t+1}$	(ii) $FCAST_{t+1}$	(iii) $FCAST_{t+1}$
$VIOLATION_t$	-0.039*** (-4.194)	-0.047*** (-4.135)	-0.029*** (-3.092)
<i>CONTROLS</i>			
$LOGASSETS_t$	0.028*** (7.209)	0.030*** (6.443)	0.016*** (3.579)
$BKLEV_t$	-0.021 (-1.012)	0.610*** (3.888)	-0.011 (-0.493)
$BTOM_t$	-0.064*** (-4.519)	-0.085*** (-4.629)	-0.037*** (-2.728)
$RETURN_t$	-0.037** (-2.293)	-0.014 (-0.669)	-0.042*** (-2.740)
$PROBLIT_t$	0.051*** (4.186)	0.040*** (2.660)	0.079*** (6.313)
$VOLATILITY_t$	-0.082*** (-6.268)	-0.058*** (-3.568)	-0.067*** (-5.380)
$NUMANALYSTS_t$	0.004*** (3.352)	0.002* (1.827)	0.005*** (3.549)
$BADNEWS_t$	-0.009 (-1.028)	-0.003 (-0.239)	-0.011 (-1.184)
$ISSUE_t$	0.004 (0.177)	0.012 (0.435)	0.001 (0.037)
$NUMSEGMENTS_t$	0.015*** (4.121)	0.012*** (2.942)	0.012*** (2.604)
ROA_t	0.796*** (10.82)	0.363** (2.374)	0.701*** (10.38)
$BLOCK_t$	0.038*** (4.794)	0.047*** (4.792)	0.041*** (5.296)
ERC_t	0.041*** (3.489)	0.057*** (3.865)	0.028** (2.357)
$SURPRISE_{t+1}$	0.423*** (3.743)	0.445*** (3.134)	0.573*** (4.919)
$DISPERSION_t$	0.008 (1.499)	0.000 (0.087)	0.012* (1.863)
Observations	115,661	82,449	83,039

Table 3: Covenant violation and management forecast issuance: robustness checks

This table presents various robustness checks of the relation between the incidence of management earnings forecast and covenant violation. Dependent variable (*FCAST*) is an indicator for the issuance of a management forecast. *VIOLATION* is an indicator variable that equals 1 if the firm was in the violation of a debt covenant. Columns (i) through (vi) present probit regression estimates of various versions of Equation (1) that differ in the relative timing of the measurement of covenant violation and forecast issuance. Column (vii) presents estimate of Equation (1) after including a dummy for the issuance of a management forecast in the same quarter of the previous year as an additional control variable. Column (viii) presents estimate of a linear probability model with firm fixed effects by estimating Equation (2) with *FCAST* as the dependent variable. Column (ix) presents estimate of Equation (1) on a subsample obtained after removing all firms that never violate a covenant during the sample period. All estimates reflect marginal effects computed at the mean of the independent variables. All probit specifications include industry and time dummies, and the linear specification in Column (viii) includes firm and time dummies. For brevity, coefficient estimates on all control variables have been omitted. Standard errors have been obtained by clustering at the firm level. Statistical significance (two-sided) at the 10%, 5% and 1% level is denoted by *, **, and ***, respectively.

	Relative timing of covenant violation and forecast issuance						With lagged forecast	With firm fixed effects	Subsample of only covenant violators
	(i) $FCAST_{t+1}$	(ii) $FCAST_{t+1}$	(iii) $FCAST_{t+1}$	(iv) $FCAST_{t+1}$	(v) $FCAST_{t+1}$	(vi) $FCAST_{t+1}$	(vii) $FCAST_{t+1}$	(viii) $FCAST_{t+1}$	(ix) $FCAST_{t+1}$
$VIOLATION_{t+3}$	0.003 (0.391)								
$VIOLATION_{t+2}$		-0.001 (-0.087)							
$VIOLATION_{t+1}$			-0.006 (-0.684)						
$VIOLATION_t$				-0.039*** (-4.194)			-0.051*** (-5.849)	-0.038*** (-6.430)	-0.029*** (-3.529)
$VIOLATION_{t-1}$					-0.050*** (-5.317)				
$VIOLATION_{t-2}$						-0.059*** (-6.021)			
Observations	110,679	113,453	115,661	115,661	112,995	109,948	115,117	115,661	56,738

Table 4: Covenant violation and forecast informativeness

This table presents estimates of specifications with various measures of management forecast informativeness as dependent variables. All measures of forecast informativeness have been defined in the caption of Table 1. *VIOLATION* is an indicator variable for violation of a debt covenant. Model for the ordinal variable *PRECISION*, which otherwise is similar to Equation (1), has been estimated using ordered probit. Specifications with continuous dependent variables have been estimated using the linear specification in Equation (2). The ordered probit specification includes industry and time dummies, and all linear specifications include firm and time dummies. All estimates, except for estimates in Column (i), reflect marginal effects. For brevity, coefficient estimates on control variables in changes in the ordered probit specification, and coefficient estimates on contemporaneous control variables in linear specifications, except on *SURPRISE*, have been omitted. Coefficient estimates on dummies for quarterly and annual forecasts have also been omitted. All coefficients in Column (ii) have been multiplied by 1000. Standard errors have been obtained by clustering at the firm level. Statistical significance (two-sided) at the 10%, 5% and 1% level is denoted by *, **, and ***, respectively.

	(i) PRECISION _{t+1}	(ii) SPECIFICITY _{t+1}	(iii) HORIZON _{t+1}	(iv) ACCURACY _{t+1}	(v) Δ LAMBDA _{t+1}
VIOLATION _t	-0.038 (-0.868)	0.128 (0.551)	-3.092 (-0.971)	-0.001 (-0.977)	0.003 (0.105)
<i>CONTROLS</i>					
LOGASSETS _t	0.004 (0.385)	1.130*** (8.029)	1.972 (0.916)	0.002*** (4.798)	0.010 (0.566)
BKLEV _t	-0.010 (-0.168)	-2.030*** (-3.883)	4.674 (0.686)	-0.003** (-1.985)	-0.003 (-0.042)
BTOM _t	-0.167*** (-3.300)	-3.290*** (-9.363)	-14.100*** (-3.557)	-0.012*** (-10.230)	-0.075 (-1.407)
RETURN _t	0.029 (0.327)	1.460*** (4.383)	-4.952 (-1.245)	0.005*** (3.716)	0.059 (1.315)
PROBLIT _t	-0.013 (-0.323)	-0.193 (-1.213)	-4.489* (-1.685)	-0.000 (-0.648)	0.019 (0.598)
VOLATILITY _t	0.003 (0.054)	-0.795*** (-2.675)	7.795** (2.165)	-0.003*** (-2.750)	0.065 (1.152)
NUMANALYSTS _t	0.008*** (2.745)	0.039*** (3.259)	0.107 (0.439)	-0.000 (-0.046)	0.002 (1.224)
BADNEWS _t	-0.021 (-0.623)	-0.295*** (-3.479)	-2.653 (-1.560)	-0.000 (-0.977)	0.004 (0.258)
ISSUE _t	-0.217** (-2.324)	-0.034 (-0.093)	7.620 (1.197)	-0.003 (-1.298)	0.130** (2.059)
NUMSEGMENTS _t	0.004 (0.434)	0.061 (1.213)	-1.168 (-1.236)	-0.000 (-0.210)	-0.001 (-0.255)
ROA _t	0.712** (2.328)	-10.600*** (-5.148)	10.310 (0.457)	0.010 (1.538)	0.025 (0.118)
BLOCK _t	0.049 (1.604)	-0.199* (-1.861)	4.006** (2.025)	0.000 (0.741)	-0.045** (-2.464)
ERC _t	-0.000 (-0.635)	0.002** (2.522)	0.000 (0.029)	-0.000 (-0.349)	0.000 (0.553)
SURPRISE _{t+1}	-2.073*** (-4.573)	-17.900*** (-5.514)	-61.310** (-1.994)	-0.291*** (-15.54)	-0.691 (-1.550)
DISPERSION _t	0.045* (1.697)	-0.297*** (-2.608)	-0.598 (-0.318)	0.001 (1.314)	0.018 (1.228)
R-squared		18%	40%	19%	0.5%
Observations	35,826	30,298	35,826	29,010	28,792

Table 5: Effect of the severity of covenant violation on the likelihood of management forecast issuance

This table provides evidence on the effect of the severity of covenant violation on the likelihood of the issuance of a management forecast by presenting various versions of probit regression estimates of Equation (1). Dependent variable ($FCAST_{t+1}$) is an indicator for the issuance of a management forecast. Panel A presents estimates from Equation (1) augmented with 8 lags of the indicator for covenant violation ($VIOLATION$). Panel B presents estimates from Equation (1) augmented with 8 lags of $VIOLATION$ and their interaction terms with the indicator for covenant violation during the current quarter $t+1$. All estimates reflect marginal effects computed at the mean of the independent variables. All specifications include industry and time dummies. For brevity, coefficient estimates on control variables have been omitted. Standard errors have been obtained by clustering at the firm level. Statistical significance (two-sided) at the 10%, 5% and 1% level is denoted by *, **, and ***, respectively.

Panel A: Effect of the length of covenant violations			
	(i) Individual effect	(ii) Cumulative effect	
VIOLATION _t	-0.021** (-2.280)		
VIOLATION _{t-1}	-0.021** (-2.471)	-0.042*** (-3.332)	
VIOLATION _{t-2}	-0.027*** (-3.160)	-0.069*** (-4.356)	
VIOLATION _{t-3}	-0.009 (-0.989)	-0.078*** (-4.138)	
VIOLATION _{t-4}	-0.029*** (-3.445)	-0.106*** (-4.965)	
VIOLATION _{t-5}	-0.011 (-1.306)	-0.117*** (-5.016)	
VIOLATION _{t-6}	-0.021** (-2.382)	-0.138*** (-5.427)	
VIOLATION _{t-7}	-0.025*** (-2.630)	-0.163*** (-5.646)	
Observations	89,088	89,088	
Panel B: Effect of current violation on the effect of prior violations			
	(i) VIOLATION _{t+1} =1	(ii) VIOLATION _{t+1} =0	(iii) Difference: (i) - (ii)
<i>Cumulative effect of violations over:</i>			
Quarters <i>t</i> to <i>t-3</i>	-0.066** (-2.057)	-0.101*** (-4.811)	0.035 (0.936)
Quarters <i>t-4</i> to <i>t-7</i>	-0.183*** (-4.359)	-0.070*** (-3.342)	-0.113** (-2.502)
Observations	89,088	89,088	89,088

Table 6: Heterogeneity in the effect of covenant violation

This table presents evidence on the cross-sectional variation in the effect of covenant violation on the likelihood of the issuance of a management forecast. Partitioning variables examined are: (i) book-leverage (*BKLEV*), (ii) fraction of assets financed by private loans (*FRACPVTASSETS*), (iii) fraction of total debt that is private (*FRACPVTDEBT*), (iv) book-to-market ratio (*BTOM*), (v) indicator for relationship loan (*RELATIONSHIP*), (vi) earnings timeliness (*TIMELINESS*), and (vii) indicator for high-intangibles industry (*HIINTANGIBLE*). For continuous partitioning variables, an augmented version of Equation (1) is estimated that includes three dummy variables indicating the membership of the partitioning variable to the top, middle or bottom tercile of the distribution, and their interaction terms with the dummy for covenant violation (*VIOLATION*). For dummy partitioning variables, Equation (1) is estimated after augmenting it with the partitioning variable and its interaction term with *VIOLATION*. For brevity, only the marginal effect of *VIOLATION* at different levels of partitioning variables has been presented. Standard errors have been obtained by clustering at the firm level. Statistical significance (two-sided) at the 10%, 5% and 1% level is denoted by *, **, and ***, respectively.

Panel A: Continuous partitioning variables					
	(i) BKLEV	(ii) FRACPVTASSETS	(iii) FRACPVTDEBT	(iv) BTOM	(v) TIMELINESS
Bottom Tercile	0.004 (0.212)	-0.045 (-1.186)	-0.041 (-1.343)	0.012 (0.595)	-0.001 (-0.0632)
Middle Tercile	-0.062*** (-4.480)	-0.053 (-1.643)	-0.072** (-2.516)	-0.050*** (-3.385)	-0.048*** (-3.241)
Top Tercile	-0.048*** (-3.470)	-0.106*** (-3.646)	-0.086** (-2.483)	-0.052*** (-4.318)	-0.067*** (-4.470)
Top - Bottom	-0.052** (-2.205)	-0.061 (-1.322)	-0.045 (-1.016)	-0.063*** (-2.766)	-0.066*** (-3.210)
Observations	115,082	12,445	11,414	115,406	102,582
Panel B: Dummy partitioning variables					
	(i) RELATIONSHIP	(ii) HIINTANGIBLE			
No	-0.035 (-1.167)	-0.050*** (-4.541)			
Yes	-0.113*** (-3.999)	-0.016 (-1.051)			
Diff	-0.078** (-1.976)	0.034* (1.851)			
Observations	10,732	115,661			

Table 7: Covenant violation and MD&A section disclosures

This table presents estimates of Equation (2) with natural logarithm of one plus the proportion of words in MD&A that indicate forward-looking information (*PROPFORWARD*), and natural logarithm of one plus the proportion of numeric terms in MD&A (*PROPNUMERIC*) as dependent variables. *VIOLATION* is an indicator variable that equals 1 if the firm was in the violation of a debt covenant. All specifications include firm and time dummies. For brevity, coefficient estimates on contemporaneous control variables, except *SURPRISE*, have been omitted. All coefficients in Column (i) have been multiplied by 100. Coefficient estimates on *ERC* in Columns (ii)-(iv) have been multiplied by 100. Standard errors have been obtained by clustering at the firm level. Statistical significance (two-sided) at the 10%, 5% and 1% level is denoted by *, **, and ***, respectively.

	Full sample		Higher powers of covenant controls	No public debt rating
	(i)	(ii)	(iii)	(iv)
	PROPFORWARD _{t+1}	PROPNUMERIC _{t+1}	PROPNUMERIC _{t+1}	PROPNUMERIC _{t+1}
VIOLATION _t	0.000 (0.044)	-0.015*** (-2.738)	-0.022*** (-3.766)	-0.017*** (-2.597)
<i>CONTROLS</i>				
LOGASSETS _t	0.014 (1.166)	-0.012* (-1.704)	-0.008 (-0.992)	-0.020* (-1.879)
BKLEV _t	-0.221*** (-6.279)	0.068*** (3.583)	0.027 (0.587)	0.012 (0.259)
BTOM _t	-0.055*** (-2.788)	-0.002 (-0.220)	-0.015 (-1.093)	-0.007 (-0.449)
RETURN _t	-0.029** (-2.536)	0.008 (1.338)	0.008 (1.040)	0.012 (1.375)
PROBLIT _t	0.039** (2.504)	-0.036*** (-4.220)	-0.035*** (-3.751)	-0.026** (-2.300)
VOLATILITY _t	0.108*** (7.660)	-0.058*** (-8.812)	-0.044*** (-6.392)	-0.052*** (-5.717)
NUMANALYSTS _t	0.000 (0.196)	0.002* (1.692)	0.001 (0.463)	0.001 (0.839)
BADNEWS _t	0.032*** (4.505)	-0.010*** (-2.679)	-0.010** (-2.280)	-0.007 (-1.318)
ISSUE _t	-0.028 (-1.288)	0.002 (0.218)	-0.002 (-0.131)	0.003 (0.214)
NUMSEGMENTS _t	-0.008 (-1.240)	0.006* (1.701)	0.006 (1.493)	0.010 (1.610)
ROA _t	-0.693*** (-7.554)	0.320*** (6.773)	0.191*** (2.766)	0.250*** (3.180)
BLOCK _t	0.002 (0.225)	-0.005 (-0.778)	0.000 (0.0704)	0.002 (0.295)
ERC _t	0.000 (0.120)	-0.000 (-0.092)	-0.003 (-0.330)	-0.010 (-0.811)
SURPRISE _{t+1}	0.440* (1.836)	-0.320** (-2.391)	-0.206 (-1.404)	-0.183 (-0.948)
DISPERSION _t	-0.003 (-0.496)	0.000 (0.0967)	-0.001 (-0.467)	0.004 (1.004)
R-squared	13%	11%	11%	12%
Observations	88,711	88,711	64,647	43,957

Table 8: Panel A - Covenant violation and disclosure of accounting information in earnings press releases

This table presents probit regression estimates of Equation (1) with a series of indicator variables to capture the disclosure of specific accounting information in the earnings press release as dependent variables. *IS*, *BS*, *CF*, *DISSCAPEX*, and *DISCRD* are indicator variables for the presence of an income statement, balance sheet, cash flow statement, capital expenditure, and Research & development expenditure in the earnings press release respectively. *VIOLATION* is an indicator variable that equals 1 if the firm was in the violation of a debt covenant. All estimates reflect marginal effects computed at the mean of the independent variables. All specifications include industry and time dummies. For brevity, coefficient estimates on control variables in changes have been omitted. All coefficient estimates on *ERC* have been multiplied by 1000. Standard errors have been obtained by clustering at the firm level. Statistical significance (two-sided) at the 10%, 5% and 1% level is denoted by *, **, and ***, respectively.

	(i) IS _{t+1}	(ii) BS _{t+1}	(iii) CF _{t+1}	(iv) DISSCAPEX _{t+1}	(v) DISCRD _{t+1}
VIOLATION _t	0.005 (0.271)	-0.014 (-0.741)	-0.041** (-2.048)	-0.060*** (-2.967)	0.001 (0.0381)
<i>CONTROLS</i>					
LOGASSETS _t	-0.018** (-2.573)	-0.015* (-1.876)	0.076*** (9.050)	0.079*** (8.830)	-0.001 (-0.153)
BKLEV _t	0.011 (0.322)	-0.033 (-0.785)	0.144*** (3.192)	0.213*** (4.541)	-0.235*** (-4.394)
BTOM _t	-0.020 (-0.684)	-0.022 (-0.648)	-0.056 (-1.629)	0.054 (1.450)	-0.044 (-1.044)
RETURN _t	0.091*** (2.742)	0.064 (1.623)	0.021 (0.517)	0.048 (1.092)	0.092** (2.150)
PROBLIT _t	0.041* (1.936)	0.047** (2.012)	-0.058** (-2.295)	-0.058** (-2.151)	0.095*** (3.559)
VOLATILITY _t	0.022 (0.797)	0.045 (1.538)	0.019 (0.609)	0.000 (0.0100)	0.040 (1.216)
NUMANALYSTS _t	0.003 (1.586)	0.003 (1.585)	-0.001 (-0.634)	-0.002 (-0.930)	0.004** (2.051)
BADNEWS _t	0.004 (0.269)	-0.014 (-0.802)	-0.027 (-1.466)	0.030 (1.583)	0.034* (1.771)
ISSUE _t	0.005 (0.134)	-0.078 (-1.531)	0.018 (0.351)	0.007 (0.130)	0.047 (0.805)
NUMSEGMENTS _t	-0.009 (-1.412)	-0.012* (-1.738)	-0.008 (-0.998)	-0.005 (-0.620)	-0.011 (-1.369)
ROA _t	-0.494*** (-2.909)	-0.494*** (-2.745)	0.104 (0.567)	0.102 (0.478)	-0.448 (-1.410)
BLOCK _t	-0.010 (-0.554)	0.033 (1.563)	0.030 (1.416)	0.034 (1.444)	0.024 (0.951)
ERC _t	0.107 (0.506)	0.085 (0.355)	0.475* (1.769)	-0.221 (-0.806)	0.002 (0.008)
SURPRISE _{t+1}	0.667* (1.731)	-0.880** (-2.145)	-0.085 (-0.181)	-0.128 (-0.276)	0.288 (0.574)
DISPERSION _t	-0.008 (-1.285)	-0.005 (-0.657)	0.021** (2.302)	-0.005 (-0.501)	0.004 (0.391)
CAPEX _{t+1}				0.858*** (4.716)	-0.632*** (-2.873)
R&D _{t+1}				-1.748*** (-2.877)	7.043*** (9.438)
Observations	39,401	39,369	39,369	39,355	39,231

Table 8 (Cont'd): Panel B - Robustness checks

This table presents robustness of the findings shown in Panel A of Table 8 to inclusion of higher powers of covenant controls, and to estimation on a subsample of firms without debt rating. All estimates reflect marginal effects computed at the mean of the independent variables. All specifications include industry and time dummies. For brevity, coefficient estimates on control variables in changes have been omitted. Standard errors have been obtained by clustering at the firm level. All coefficient estimates on *ERC* have been multiplied by 1000. Statistical significance (two-sided) at the 10%, 5% and 1% level is denoted by *, **, and ***, respectively.

	Higher powers of covenant controls		No public debt rating	
	CF _{t+1} (i)	DISCCEPEX _{t+1} (ii)	CF _{t+1} (iii)	DISCCEPEX _{t+1} (iv)
VIOLATION _t	-0.062*** (-2.794)	-0.085*** (-3.915)	-0.040* (-1.741)	-0.050** (-2.535)
<i>CONTROLS</i>				
LOGASSETS _t	0.0679*** (7.289)	0.080*** (7.743)	0.081*** (7.153)	0.090*** (8.523)
BKLEV _t	0.449* (1.831)	0.316 (1.138)	0.020 (0.362)	0.074 (1.476)
BTOM _t	-0.063 (-1.515)	0.057 (1.273)	-0.091** (-2.308)	0.035 (0.976)
RETURN _t	-0.033 (-0.707)	0.052 (0.958)	0.030 (0.633)	0.019 (0.430)
PROBLIT _t	-0.037 (-1.320)	-0.051* (-1.671)	-0.048 (-1.558)	-0.057** (-2.052)
VOLATILITY _t	-0.009 (-0.258)	-0.013 (-0.305)	0.051 (1.274)	0.056 (1.491)
NUMANALYSTS _t	-0.001 (-0.416)	-0.003 (-1.370)	-0.003 (-1.138)	-0.003 (-1.288)
BADNEWS _t	-0.033 (-1.585)	0.027 (1.225)	-0.020 (-0.869)	0.015 (0.764)
ISSUE _t	0.059 (1.039)	0.075 (1.136)	-0.004 (-0.0647)	0.015 (0.265)
NUMSEGMENTS _t	-0.021** (-2.570)	-0.004 (-0.492)	-0.009 (-0.861)	0.001 (0.152)
ROA _t	0.076 (0.294)	-0.046 (-0.161)	0.080 (0.412)	-0.008 (-0.041)
BLOCK _t	0.040* (1.658)	0.043* (1.658)	0.025 (0.981)	0.017 (0.711)
ERC _t	0.357 (1.163)	0.067 (0.209)	0.592* (1.846)	-0.107 (-0.366)
SURPRISE _{t+1}	-0.438 (-0.832)	-0.088 (-0.161)	0.100 (0.184)	0.207 (0.437)
DISPERSION _t	0.023** (2.444)	-0.007 (-0.622)	0.020* (1.670)	0.008 (0.678)
CAPEX _{t+1}		0.831*** (4.173)		0.899*** (4.964)
R&D _{t+1}		-1.652** (-2.340)		-1.195** (-2.257)
Observations	30,210	30,255	26,335	26,322

Table 9: Covenant violation and forward-looking and numeric disclosures in earnings press releases

This table presents estimates of Equation (2) with natural logarithm of one plus the proportion of words that indicate forward-looking information (*PROPFORWARD*), and natural logarithm of one plus the proportion of numeric terms (*PROPNUMERIC*) in earnings press releases as dependent variables. *VIOLATION* is an indicator variable that equals 1 if the firm was in the violation of a debt covenant. All specifications include firm and time dummies. For brevity, coefficient estimates on contemporaneous control variables, except *SURPRISE*, have been omitted. Standard errors have been obtained by clustering at the firm level. All estimates in Column (i) have been multiplied by 1000. Statistical significance (two-sided) at the 10%, 5% and 1% level is denoted by *, **, and ***, respectively.

	(i) PROPFORWARD _{t+1}	(ii) PROPNUMERIC _{t+1}
VIOLATION _t	0.068 (0.539)	-0.002 (-0.233)
<i>CONTROLS</i>		
LOGASSETS _t	-0.004 (-0.0248)	-0.010 (-1.010)
BKLEV _t	-0.663 (-1.617)	0.023 (0.787)
BTOM _t	-0.194 (-0.713)	-0.001 (-0.058)
RETURN _t	-0.492** (-2.475)	0.0126 (0.925)
PROBLIT _t	0.135 (0.884)	-0.014 (-1.285)
VOLATILITY _t	-0.282* (-1.958)	0.016* (1.760)
NUMANALYSTS _t	0.022 (1.222)	0.001 (1.026)
BADNEWS _t	0.095 (1.142)	-0.008 (-1.429)
ISSUE _t	-0.138 (-0.547)	-0.041** (-2.443)
NUMSEGMENTS _t	-0.119 (-1.574)	0.004 (0.674)
ROA _t	-3.320*** (-3.145)	0.196*** (3.103)
BLOCK _t	0.153 (1.221)	-0.000 (-0.003)
ERC _t	0.000 (0.251)	-0.000 (-0.395)
SURPRISE _{t+1}	8.320*** (2.904)	-0.743*** (-3.777)
DISPERSION _t	0.026 (0.506)	-0.002 (-0.662)
R-squared	7%	10%
Observations	36,211	36,211

Table 10: Effect of covenant violations on analyst forecast errors and adverse selection component of bid-ask spread

This table presents estimates of Equation (2) with analyst forecast error (*AFE*) and the adverse selection component of bid-ask spread (*LAMBDA*) as the dependent variables. *VIOLATION* is an indicator for covenant violation. All specifications include firm and time dummies. For brevity, coefficient estimates on contemporaneous control variables, except *SURPRISE*, have been omitted. Coefficient estimates on *NUMANALYSTS*, *BADNEWS*, *BLOCK*, and *DISPERSION* have been multiplied by 1000. Standard errors have been obtained by clustering at the firm level. Statistical significance (two-sided) at the 10%, 5% and 1% level is denoted by *, **, and ***, respectively.

	Full Sample		Higher powers of covenant controls		No public debt rating	
	(i) AFE _{t+1}	(ii) LAMBDA _{t+1}	(iii) AFE _{t+1}	(iv) LAMBDA _{t+1}	(v) AFE _{t+1}	(vi) LAMBDA _{t+1}
VIOLATION _t	0.004*** (5.180)	0.001*** (4.215)	0.003*** (3.937)	0.001*** (3.835)	0.004*** (4.123)	0.001*** (4.115)
<i>CONTROLS</i>						
LOGASSETS _t	-0.003*** (-7.801)	-0.001*** (-9.079)	-0.002*** (-5.236)	-0.001*** (-4.613)	-0.003*** (-6.667)	-0.002*** (-14.590)
BKLEV _t	0.008*** (6.730)	0.002*** (8.786)	-0.017*** (-2.772)	-0.001 (-0.346)	0.008*** (5.203)	0.003*** (9.117)
BTOM _t	0.012*** (15.12)	0.003*** (13.540)	0.012*** (12.650)	0.003*** (10.830)	0.013*** (13.080)	0.003*** (14.840)
RETURN _t	-0.007*** (-10.35)	-0.001*** (-10.510)	-0.009*** (-9.518)	-0.002*** (-8.030)	-0.005*** (-6.566)	-0.002*** (-11.420)
PROBLIT _t	-0.003*** (-5.445)	-0.002*** (-5.286)	-0.003*** (-4.514)	-0.001*** (-3.645)	-0.003*** (-4.172)	-0.001*** (-10.380)
VOLATILITY _t	0.011*** (11.60)	0.002*** (7.141)	0.011*** (9.322)	0.002*** (5.582)	0.008*** (7.881)	0.002*** (7.282)
NUMANALYSTS _t		0.074*** (6.452)		0.062*** (4.261)		0.106*** (10.150)
BADNEWS _t	-0.859*** (-3.461)	-0.355*** (-2.656)	-1.090*** (-3.714)	-0.431** (-2.347)	-0.313 (-0.944)	-0.113 (-1.471)
ISSUE _t	0.003** (2.057)	0.000 (1.354)	0.001 (0.638)	0.000 (0.391)	0.004*** (2.930)	0.000 (1.300)
NUMSEGMENTS _t	-0.000 (-0.620)	-0.000 (-0.491)	-0.000 (-1.408)	0.000 (0.006)	-0.001*** (-2.908)	-0.000** (-1.974)
ROA _t	-0.047*** (-9.622)	-0.003*** (-2.889)	-0.032*** (-4.109)	-0.001 (-0.798)	-0.044*** (-8.198)	-0.001 (-1.558)
BLOCK _t	-0.408 (-1.448)	-0.150* (-1.749)	-0.301 (-0.918)	-0.066 (-0.621)	-0.213 (-0.544)	-0.127 (-1.125)
ERC _t	-0.005** (-2.327)	0.002*** (4.655)	-0.003 (-1.512)	0.002*** (3.314)	-0.002 (-0.676)	0.003*** (4.775)
SURPRISE _{t+1}	0.006*** (2.706)	-0.012*** (-7.466)	0.006** (2.407)	-0.011*** (-7.387)	0.005 (1.544)	-0.012*** (-6.102)
DISPERSION _t		0.022 (1.076)		0.012 (0.542)		0.060* (1.868)
CORRETEARN _t	-0.003*** (-5.324)		-0.003*** (-4.466)		-0.003*** (-3.794)	
R-squared	15%	4%	15%	3%	13%	12%
Observations	79,191	106,053	59,857	77,088	49,995	76,761

Table 11: Additional robustness checks

This table presents estimates of Equations (1) and (2), which model various measures of disclosures, on a subsample of firms: (i) that were net issuers of equity in the year following the quarter of the measurement of disclosure, and (ii) whose insiders' percentage ownership of their firms' stock falls in the top tercile of the distribution. Firms that are net issuers of equity are identified as firms whose ratio of shares outstanding at the end of the measurement period to the ratio of outstanding shares at the beginning of the measurement period is greater than one. VIOLATION is an indicator for covenant violation. All probit specifications include industry and time dummies, and all linear specifications include firm and time dummies. All estimates reflect marginal effects computed at the mean of the independent variables. Coefficient estimates in Column (vi) have been multiplied by 10. For brevity, coefficient estimates on control variables have been omitted. Standard errors have been obtained by clustering at the firm level. Statistical significance (two-sided) at the 10%, 5% and 1% level is denoted by *, **, and ***, respectively.

Panel A: Subsample of net equity issuers						
	Mgmt. forecasts	MD&As	Earnings press releases		Composite measures	
	(i) FCAST _{t+1}	(ii) PROPNUMERIC _{t+1}	(iii) CF _{t+1}	(iv) DISCCAPEX _{t+1}	(v) AFE _{t+1}	(vi) LAMBDA _{t+1}
VIOLATION _t	-0.038*** (-3.648)	-0.018*** (-2.583)	-0.040* (-1.694)	-0.058** (-2.484)	0.003*** (3.940)	0.003** (2.348)
Observations	77,701	43,328	26,222	26,221	53,031	71,583
Panel B: Subsample with high insider ownership						
	Mgmt. forecasts	MD&As	Earnings press releases		Composite measures	
	(i) FCAST _{t+1}	(ii) PROPNUMERIC _{t+1}	(iii) CF _{t+1}	(iv) DISCCAPEX _{t+1}	(v) AFE _{t+1}	(vi) LAMBDA _{t+1}
VIOLATION _t	-0.048*** (-3.809)	-0.002*** (-3.365)	-0.072** (-2.281)	-0.086*** (-3.597)	0.004*** (2.807)	0.007** (2.354)
Observations	37,360	30,464	13,205	13,209	23,054	35,262