Why Disclose Privately? Shareholder Litigation Risk and Managers' Private Disclosure of Earnings Warnings

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Abstract

This paper examines managers' private disclosure in the context of shareholder litigation risk. To measure private disclosure of earnings warnings to analysts, I construct a proxy based on sell-side analysts' forecast revisions (relative to benchmark forecasters) around earnings announcements. I first show that my proxy for private earnings warnings is both positively associated with analysts' private access to managers and predictive of future firm performance. I then use this proxy and exploit plausibly exogenous changes in shareholder litigation risk based on judge ideology to understand managerial incentives to engage in private disclosure. Consistent with theory, I find managers' propensity to provide private earnings warnings increases when shareholder litigation risk increases. This effect is concentrated among firms with high proprietary costs and among firms without commitments to public guidance. I conclude that, in response to increases in shareholder litigation risk, managers use private disclosure to indirectly influence market earnings expectations, and they consider disclosure-channel-specific costs and benefits when choosing to do so.

Keywords: Litigation risk, private disclosure, private communication, sell-side analysts
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1. Introduction

A fundamental premise underlying the theoretical and empirical literature on disclosure is that firms can choose to withhold private information or use public voluntary disclosure to reveal this information (e.g., Verrecchia [2001]; Beyer et al. [2010]). Yet, firms can also disclose information *privately* to select market participants, such as sell-side analysts, to indirectly influence market prices (Ajinkya and Gift [1984]; King et al. [1990]).[1] Although some prior studies have examined both the existence and the economic consequences of firms' private communication with select groups of large investors and analysts (e.g., Green et al. [2014]; Soltes [2014]; Brown et al. [2015]; Bushee et al. [2018]), our understanding of managers' motivation to strategically engage in private communication as an alternative voluntary disclosure channel is relatively limited.

In this paper, I examine the effect of shareholder litigation risk on managers' use of private voluntary disclosure. I broadly define voluntary disclosure as managers' dissemination of pieces of information to ensure stock prices incorporate this information in a timely manner. Because the forecasts of analysts transmit earnings expectations into market prices (Gleason and Lee [2003]), I argue that private communication with analysts can be an effective strategy to adjust market expectations (Ajinkya and Gift [1984]) and reduce or potentially avoid costly shareholder litigation associated with sudden stock price declines (Skinner [1997]).[2] Therefore, I examine whether managers disclose earnings warnings privately to analysts when the threat of shareholder litigation increases. This idea goes back to Skinner [1997], who posits firms have incentives to disclose information privately (i.e., leak information) to analysts to avoid a sudden stock price crash associated with a single adverse news release.

Although Regulation Fair Disclosure (Reg FD) prohibits the communication of new material information between firms and select market participants, firms are still allowed to

[1] Throughout the paper, I use "analysts" to refer to sell-side analysts employed at brokerage houses, unless specifically noted otherwise.

[2] Firms commonly refer to the risk of legal costs as a possible consequence of sudden stock price drops in their public disclosures. For instance, Vishay Precision Group, Inc. states that a "significant drop in our stock price could expose us to costly and time consuming litigation" (see their 10-K, 2021Q4).

provide pieces of *non-material* information that can be combined into a material mosaic. For example, firms can emphasize public information, corroborate analysts' private signals (Solomon and Soltes [2015]), and review analysts' models (SEC [2010]). In addition, mounting empirical evidence suggests the Securities and Exchange Commission's (SEC) enforcement actions have been insufficient in deterring managers' selective disclosure of information (e.g., Solomon [2005]; Bengtzen [2017]; Allee et al. [2022]), and other evidence further suggests managers have spent considerable time in private meetings with select groups of large investors and analysts in recent years (e.g., Soltes [2014]; Brown et al. [2015]; Bushee et al. [2017, 2018]; Campbell et al. [2021]; Choy and Hope [2021]). Therefore, private communication presents a plausible disclosure channel for managers; yet, whether and the extent to which this channel is used in the context of shareholder litigation risk is an empirical question.

To identify firms' use of a private disclosure channel, I construct a measure based on analysts' revisions of earnings forecasts around earnings announcements. The choice to focus on analysts' forecast revisions rests on the widespread evidence of manager-analyst private communication post-Reg FD (Soltes [2014]; Choy and Hope [2021]), analyst reliance on private conversations with managers to update their forecasts (Brown et al. [2015]), and the evidence that analysts' forecast revisions are priced by the market (Stickel [1989]; Gleason and Lee [2003]; Beyer et al. [2010]). Accordingly, I argue firms can disclose information privately to analysts to indirectly adjust market prices in the desired direction. A key feature of my measurement is that I compare analysts' forecast revisions with those of a group of benchmark forecasters. To construct my benchmark group, I use data from Estimize, a crowdsourced financial forecasting platform, which prior research has shown provides useful information to the capital market (e.g., Jame et al. [2016]). I assume benchmark forecasters and analysts respond similarly to public information in earnings announcements, but benchmark forecasters have no private interactions with managers. By benchmarking analysts against the forecaster group, I attempt to hold constant analysts' revisions due to public

disclosure and—together with elements of my research design—isolate analysts' incremental revisions due to managers' private disclosure. Doing so is important, because firms' private interactions with analysts typically concentrate around earnings announcements (Soltes [2014]; Choy and Hope [2021]), and analysts respond to other public signals around earnings announcements (e.g., Abarbanell [1991]; Clement et al. [2011]).

Using a firm-quarter sample from 2012 through 2020 with coverage from I/B/E/S analysts and Estimize benchmark forecasters, I first validate my empirical proxy for private disclosure at both the analyst level and the firm level. I expect analysts who are more likely to have access to private meetings with managers receive private earnings warnings. Comparing similar analysts forecasting earnings of the same firm at the same point in time (i.e., with the inclusion of firm-year-quarter fixed effects and analyst-level controls), I confirm analysts' probability of receiving bad news privately is positively associated with the favorability of their outstanding stock recommendations, a proxy for private access to managers. Next, I find my measure of managers' propensity to leak bad news to analysts predicts firms' reporting of (i) future losses, (ii) decreases in future earnings, and (iii) decreases in future cash flows from operations. These combined results are consistent with the idea that managers leak bad news to analysts when they anticipate future adverse events.

I then use my proxy for private disclosure to test whether firms' propensity to leak bad news to analysts increases when shareholder litigation risk increases. I use Huang et al.'s [2019] measure of shareholder litigation risk, which exploits plausibly exogenous variation in the appointment of federal judges at the circuit level and captures the extent to which judges presiding over a case would take a pro-shareholder versus pro-firm standpoint. In estimating the relation between shareholder litigation risk and firms' propensity to provide private earnings warnings, I include both circuit and year-quarter fixed effects in my main design. In this way, I isolate within-circuit variation in shareholder litigation risk and private earnings warnings provisions over time, and in assessing the relation between the two, I account for macro-level shocks that affect all circuits at the same time. I also control for various firm

and earnings characteristics. Utilizing this research design, I find managers increase their propensity to engage in private disclosure when shareholder litigation risk increases.

An alternative explanation for this result is that my private disclosure proxy reflects differential sophistication between analysts and benchmark forecasters in processing public information signals (instead of differential access to private communication) and that this difference introduces variation endogenous to within-circuit changes in shareholder litigation risk.[3] Therefore, in a robustness test, I reconstruct my proxy and benchmark analyst forecast revisions only against other professional analysts—namely, buy-side and independent analysts who have skills comparable to sell-side analysts—to capture managers' propensity to leak bad news to analysts. My results continue to hold.[4] Consistent with Skinner [1997], these results suggest that, in response to increases in shareholder litigation risk, managers use private disclosure to indirectly influence market earnings expectations.

To further understand the various costs and benefits managers face in deploying private communication, I then exploit cross-sectional variation in firms' public disclosure costs and incentives. First, I argue that higher proprietary costs should be associated with an increased propensity to engage in private disclosure when shareholder litigation risk increases. Using measures of research and development costs and industry competition, I find some evidence that my results are concentrated among firms with high proprietary costs. Second, partitioning the sample based on whether firms provide public voluntary disclosure, I find some evidence that my results are concentrated among firms that do *not* provide public earnings guidance. Overall, these results suggest managers' decision to engage in private disclosure

[3] According to Blankespoor et al. [2020], firm disclosures can be more broadly viewed as private information, because of the costs entailed in monitoring, acquiring, and analyzing information from these disclosures.

[4] Another alternative explanation for my main result is that increases in shareholder litigation risk alter firms' and analysts' incentives to produce pessimistically biased forecasts. To the extent that pessimism is also correlated with my proxy for private earnings warnings, my main coefficient estimates would be biased. To partially alleviate this concern, I include a measure of forecast pessimism in Internet Appendix Table IA.4. Although the measure of forecast pessimism is positively correlated with my proxy, integrating this measure in my research design does not affect the relation between shareholder litigation risk and my proxy. In another robustness test in Internet Appendix Table IA.5, I exploit the granularity of my data and test the relation between shareholder litigation risk and firms' private earnings warnings at the *analyst* level. Doing so allows me to control for analyst characteristics directly, and I find similar results.

is a function of the costs and benefits of each disclosure channel and point to the complex interdependencies among firms' disclosure choices (see, e.g., Beyer et al. [2010]; Heinle et al. [2022]).

This study contributes to the literature on managers' disclosure choices in response to shareholder litigation risk. Prior research has largely addressed firms' decision to withhold private information or provide public voluntary disclosure, finding mixed evidence (e.g., Skinner [1994]; Kasznik and Lev [1995]; Johnson et al. [2001]; Field et al. [2005]; Rogers and Van Buskirk [2009]; Chen et al. [2011]; Bourveau et al. [2018]; Houston et al. [2019]; Huang et al. [2019, 2020]).[5] For example, using the same variation in shareholder litigation risk, Huang et al. [2019] find limited evidence that firms increase their public bad news disclosures before earnings announcements when shareholder litigation risk increases. By contrast, I study an alternative disclosure strategy conjectured in Skinner [1997, pp. 253–254], who posits firms would benefit from disclosing bad news privately to analysts in order to avoid having a sudden stock price drop that typically triggers litigation. Although directly observing whether managers use private disclosure as a channel to leak bad news is not possible, evidence of a positive and significant association between shareholder litigation risk and my private earnings warnings proxy suggests this mechanism exists. Expanding our understanding of such a private disclosure channel is important, given the mixed evidence on public voluntary disclosure and recent survey evidence suggesting that focusing on public guidance alone may provide limited insights into managers' disclosure choices in response to shareholder litigation risk (Call et al. [2022]).

Beyond providing evidence on the use of a private disclosure channel in the context of shareholder litigation risk, I further examine managers' disclosure choices in the presence of

[5]Although not the focus of Matsumoto [1998, 2002] and Wang [2007], both use an industry measure of litigation risk as a control variable in tests examining firms' likelihood of guiding analysts' expectations. Matsumoto [1998, 2002] does not find evidence consistent with a relation between litigation risk and firms' use of expectations management to reduce analysts' forecasts. Wang [2007] finds evidence consistent with a negative association between litigation risk and the magnitude of private guidance in the pre-Reg FD period. My study differs by focusing on the post-Reg FD period, using a finer measure to capture variation in firms' propensity to leak bad news to analysts, and using a more precise measure of shareholder litigation risk (Huang et al. [2019]).

other disclosure-channel-specific costs and benefits. In this way, I add to the recent research that considers nuances in firms' equilibrium disclosure strategies (e.g., Stocken and Verrecchia [2004]; Bagnoli and Watts [2007]; Beyer et al. [2010]; Heinle et al. [2022]). For example, Heinle et al. [2022] argue firms can use various disclosure channels, including mandatory financial statements, earnings conference calls, social media platforms, and public guidance to reveal private information. They find managers substitute between aggregated (e.g., earnings forecasts) and disaggregated (e.g., financial statements) disclosures conditional on differential disclosure costs. I add to this discussion by considering the trade-offs managers face in providing public and private voluntary disclosures when various disclosure frictions exist.

Finally, this study adds to the nascent literature on managers' private communication with select market participants (e.g., Green et al. [2014]; Soltes [2014]; Brown et al. [2015]; Solomon and Soltes [2015]; Bushee et al. [2017, 2018]; Johnson et al. [2020]; Campbell et al. [2021]; Choy and Hope [2021]; Allee et al. [2022]; Kirk and Piao [2022]). Based on prior evidence that managers perceive a low risk of Reg FD enforcement (Allee et al. [2022]) and enjoy flexibility in communicating non-material pieces of information to select large investors and analysts (e.g., Solomon and Soltes [2015]), I exploit a unique setting and introduce a large-sample measure to study managers' motivation to privately communicate with analysts.

2. Background and conceptual underpinnings

2.1. Public voluntary disclosure and the risk of shareholder litigation

Federal securities class-action lawsuits are filed by shareholders who allege they suffered economic losses due to firms' violations of the securities law. Most claims are filed under Rule 10b-5 of the 1934 Exchange Act on which basis plaintiffs must show price impact and prove loss causation, that is, establish that the economic losses associated with the buying or selling of the security are due to firms' misrepresentation or omission of material information (Perry and Conover [2015]; Aganin [2021]).[6] Plaintiffs commonly file lawsuits following stock

[6]Shareholders can also file state-level class-action or derivative lawsuits. However, in today's judiciary system, federal-level lawsuits have become the means for shareholders to make claims (Thompson and Sale [2003]).

price drops based on claims that firms have intentionally delayed or withheld bad news (e.g., Skinner [1994, 1997]; Francis et al. [1994]; Levine [2014]).[7] To reduce the costs of shareholder lawsuits, managers have incentives to disclose bad news (Healy and Palepu [2001]).

Empirical evidence on public voluntary disclosure is largely consistent with this thesis. Skinner [1994] finds managers publicly disclose bad news prior to earnings announcements to avoid the negative consequences associated with the reporting of large negative earnings surprises. Recent research using plausibly exogenous variation in shareholder litigation risk supports early empirical evidence. For example, Naughton et al. [2019] study the effect of the US Supreme Court's *Morrison* ruling, which reduced shareholder litigation risk for foreign cross-listed firms under Rule 10b-5 of the Exchange Act, and find a reduction in public voluntary disclosure in the post-*Morrison* period. Examining various legal changes, Houston et al. [2019] conclude litigation risk increases the frequency of managers' public voluntary disclosures, in particular, disclosures that preempt bad earnings news.[8] Finally, Huang et al. [2019] use variation in federal judge ideology to proxy for managers' perceived shareholder litigation risk. They, however, find limited evidence that firms increase public bad news pre-disclosures when shareholder litigation risk increases. Their mixed results suggest that, in some cases, increasing public voluntary disclosure is not a beneficial strategy to reduce the legal costs associated with shareholder lawsuits (see, e.g., Kasznik and Lev [1995]; Skinner [1997]).

In fact, other evidence suggests managers carefully decide when to disclose additional information for which they can be held accountable and choose to reduce public voluntary disclosure to limit public scrutiny (Graham et al. [2005]). Johnson et al. [2001] examine the impact of the Private Securities Litigation Reform Act of 1995 on firms' public vol-

Even merger and acquisition filings and claims under Section 11 of the 1933 Exchange Act are shifted from state- to federal-level courts (Aganin [2021]).

[7] According to the Supreme Court ruling on *Basic Inc. v. Levinson* in 1988, plaintiffs can rely on the integrity of the market in which prices reflect all available, material information to make such claims. This court ruling has led to a multi-billion dollar industry for plaintiff law firms (Langevoort [2009]).

[8] Also, Field et al. [2005] find a positive association between litigation risk and earnings warnings. Cao and Narayanamoorthy [2011] report an increase of bad news disclosure and a decrease of good news disclosure when litigation risk, proxied by Directors' and Officers' liability insurance premiums, increases.

untary disclosure choices and find an increase in earnings warnings shortly before earnings announcements. Because the regulation reduced shareholder litigation risk by limiting the incidence of lawsuits based on unsubstantiated claims, this result suggests that heightened litigation risk reduces firms' bad news pre-disclosures. Rogers and Van Buskirk [2009] study a sample of sued firms and find these firms reduce the provision of public disclosures in the post-litigation period.[9] Furthermore, Billings and Cedergren [2015] note that the majority of managers facing large negative news "keep quiet," that is, they do not publicly warn investors about the impending negative news.

In light of this mixed evidence, I follow Skinner [1997] and argue firms could also disclose information privately (i.e., leak information) about expected bad news to analysts. Because private information leakages can influence stock prices gradually instead of triggering sudden stock price drops associated with a single adverse news release, private communication can be an effective disclosure strategy. Yet, this alternative disclosure channel remains largely unexplored, which can partly be attributed to the unobservability of firms' private information flows and the perception that Reg FD has successfully curbed *all* manager-analyst private interactions.[10]

[9] Also, Bourveau et al. [2018] find the implementation of universal demand laws, which reduced litigation risk, increases firms' voluntary disclosure. Huang et al. [2020] note the seemingly contradictory evidence is due to the study's focus on long-term (instead of short-term) management forecasts and emphasize the importance of accounting for managers' differential incentives for voluntary disclosure in the short versus long term (see Healy and Palepu [2001]). Explanations for mixed evidence in other work include measurement error in the litigation risk proxy (e.g., Kasznik and Lev [1995]; Field et al. [2005]; Huang et al. [2020]), time trends in class actions across industries (see Aganin [2021]), and reverse causality; that is litigation risk can affect corporate disclosure policies (La Porta et al. [2006]) and voluntary disclosures can affect litigation costs (Francis et al. [1994]; Skinner [1997]).

[10] Billings et al. [2021] examine changes in firms' provision of information through various public disclosure channels *following* litigation. They further document the frequency and probability of positive earnings surprises increase in the post-litigation period and argue that managers' increased provision of information through public disclosure channels helps managers to lower analysts' expectations. In this paper, I argue managers react to changes in *ex-ante* shareholder litigation risk and strategically engage in a private disclosure channel to leak bad news to analysts.

2.2. Private disclosure and the risk of regulatory enforcement actions

In 2000, the SEC adopted Reg FD to curb the disclosure of *new material* information to select groups of analysts and investors. Although some prior research finds Reg FD reduced private information flows to analysts and investors (e.g., Gintschel and Markov [2004]; Francis et al. [2006]; Koch et al. [2012]), the overall evidence is inconclusive (see Beyer et al. [2010]). According to interview and survey analyses conducted by Brown et al. [2015], Reg FD changed the information environment initially, but managers have learned to adapt their private communication strategies without risking Reg FD liability. Importantly, managers can still communicate *non-material* pieces of information that analysts can combine into a material mosaic (Unger [2001]), which gives managers significant leeway in communicating privately with market participants post-Reg FD.[11]

Several studies provide evidence on the existence and importance of private meetings between managers and select large investors or analysts (e.g., Brown et al. [2015]; Bushee et al. [2017, 2018]; Campbell et al. [2021]; Allee et al. [2022]). For instance, Green et al. [2014] find investment conferences hosted by brokerages enable the host analysts to gather private information, because host analysts' earnings forecasts are more timely and more accurate following the conferences. Soltes [2014] examines proprietary records of a large publicly traded firm and finds the vast majority of private meetings occur over the phone (85%). Although analysts and managers do not meet during the two-week blackout period before earnings announcements, a significant proportion of meetings occurs within 72 hours after public announcements. Using taxi-ride patterns in New York City, Choy and Hope [2021] confirm manager-analyst private meetings cluster around earnings announcements.

The lack of clarity about what "material" and "private" information entail has led to disagreeing views over the right interpretation of the ruling among both regulators and firms (Soltes [2014]) and to disagreement among SEC Commissioners about the appropriate

[11] According to the SEC, "an issuer is not prohibited from disclosing a non-material piece of information to an analyst, even if, ... that piece helps the analyst complete a "mosaic" of information that ... is material," and also argue that "an issuer cannot render material information immaterial simply by breaking it into ostensibly non-material pieces" (SEC [2000]; Posner [2019]).

enforcement actions (Jordan [2004]). Since the enactment of Reg FD in 2000, the SEC has taken action in only 18 cases, most of which were settled with no or limited penalties (see Internet Appendix Table IA.1). When the SEC took the *Siebel Systems* case to court in 2005, a federal judge dismissed the case because it could not be proven that the firm's private statements to select market participants were material. This event marked a milestone in firms' perception about Reg FD liability concerns and signaled to managers that they are unlikely to be held liable when providing information selectively (e.g., Solomon [2005]; Fisch [2013]; Allee et al. [2022]). After this case, the SEC investigated or imposed penalties in only 10 other cases, six of which involved firms' private communication with analysts.[12]

2.3. Hypothesis development

Firms are frequently the target of securities class-action lawsuits based on shareholder claims that firms have intentionally misled, delayed, or omitted material information in line with the securities law.[13] Because sudden stock price drops associated with negative information shocks are typically followed by shareholder legal actions, firms have incentives to disclose bad news (e.g, Skinner [1994, 1997]). In this paper, I argue firms can use *private* disclosure to analysts to reduce the price impact of adverse news events. Because analysts' revisions of earnings forecasts are priced by the market (e.g., Gleason and Lee [2003]) and managers spend considerable time meeting privately with analysts post-Reg FD (e.g., Soltes [2014]), private disclosure can be an effective strategy to indirectly alter market prices. Although private communication is not limited to bad news, in this setting, managers have incentives to pre-disclose adverse news in response to an increased risk of shareholder litigation following large drops in stock price (Healy and Palepu [2001]).

The idea of private information disclosure to analysts is, however, not new. Skinner [1997] posits that firms prefer gradual stock price declines associated with private information leakages rather than sudden stock price crashes associated with a single adverse news

[12]See Levine [2022] for a discussion of a recent Reg FD enforcement action.
[13]In 2020 alone, a total of 334 class-action lawsuits were filed in state and federal courts (Aganin [2021]).

release. Other arguments in support of private communication as an alternative disclosure channel include evidence that public voluntary disclosures do not necessarily sufficiently protect against shareholder legal actions (Kasznik and Lev [1995]; Skinner [1997]) and that public disclosure costs can prevent managers from issuing additional public disclosures. For example, the costs of public voluntary disclosure increase with market uncertainty, because managers are required to frequently update their public forecasts when they are no longer appropriate (Chen et al. [2011]). Similarly, when proprietary information would be revealed to outsiders through public disclosures (e.g., Verrecchia [1983]), managers might prefer private disclosure to analysts (King et al. [1990]). Finally, prior research finds the majority of managers facing impending negative news do not *publicly* inform investors about upcoming earnings disappointments (e.g., Billings and Cedergren [2015]); however, managers could use private disclosure to adjust earnings expectations. Henceforth, I predict that managers' propensity to disclose earnings warnings privately to analysts increases when shareholder litigation risk increases and state the hypothesis in alternative form:

H: Managers' propensity to disclose short-term earnings warnings privately is higher when the risk of shareholder litigation is higher.

Ex ante, whether managers use private disclosure to analysts to leak information to the market is unclear, given that Reg FD prohibits the communication of *new material* information to select groups of analysts. Thus, managers are expected to trade off the costs and benefits associated with each disclosure choice. When the costs of private disclosure outweigh its benefits, the firm may refrain from doing so. For instance, firms' requirement to promptly disclose 8-K filings to release material information has led to a decline in public voluntary disclosure (Noh et al. [2019]), which can either mean firms now choose private disclosure or firms similarly refrain from leaking news. Given the complex interdependencies and complementarities in firms' disclosure choices (e.g., Beyer et al. [2010]), whether firms also use private disclosure to analysts to leak bad news to the market is an empirical question.

3. Measurement of key variables, research design, data, and sample

3.1. Measuring managers' private disclosure of earnings warnings

When testing theories about managers' choice to privately communicate, researchers face an econometric challenge because private communication is unobservable. To overcome this issue, researchers use either confidential data (Soltes [2014]; Solomon and Soltes [2015]), interview and survey analyses (Brown et al. [2015]; Durney et al. [2022]), or innovative research designs, such as corporate jet (Bushee et al. [2018]) or taxi-ride patterns (Choy and Hope [2021]; Kirk and Piao [2022]). To identify systematic differences in managers' private disclosure practices, I construct a large-sample measure based on analysts' revisions of earnings forecasts around earnings announcements. The choice to focus on analysts is two-fold: (i) there is widespread evidence of manager-analyst private communication post-Reg FD (e.g., Soltes [2014]) and (ii) analysts' forecast revisions are priced by the market (e.g., Beyer et al. [2010]). Because analysts respond to a wealth of information at the earnings announcement, I use a group of benchmark forecasters to control for analysts' earnings forecast revisions following public news and aim to isolate analysts' incremental revisions due to managers' private information leakages. Exploiting the simple difference between analysts' and benchmark forecasters' earnings forecast revisions, I construct my proxy for private earnings warnings disclosure at both the analyst level and the firm level.

Figure 1 depicts the timeline underlying the calculation of the main variables. At quarter $q-1$ earnings announcements, analysts update earnings forecasts for quarter q using information disclosed both publicly and privately. At the same time, a group of benchmark forecasters revises quarter q forecasts. I assume these forecasters do not receive managers' private information. To capture variation in analysts' private access to information from managers, I examine the difference between analysts' and benchmark forecasters' revisions of forecasts around quarter $q-1$ earnings announcements. Individual analyst and mean benchmark forecasters' revisions are calculated as the difference between $t+10$ and $t-1$ earnings forecasts, where t is the day of the quarter $q-1$ earnings announcement. The choice

of this measurement window is motivated by Soltes [2014], who finds manager-analyst private meetings cluster shortly after public announcements.

First, the analyst-level measure captures whether an analyst has likely received bad news privately from managers, and is defined as follows:

$$Private\_Warn_{jiq} = \begin{cases} 1, & \text{if } \Delta F_{jiq}^{Analyst} < \Delta F_{iq}^{Control} \\ 0, & \text{otherwise.} \end{cases} \tag{1}$$

Above, superscripts *Analyst* and *Control* refer to analysts and the control group of benchmark forecasters, respectively. The subscripts j, i, and q refer to analyst, firm, and year-quarter, respectively. ΔF is an analyst's (the benchmark group's mean) earnings forecast revision. $Private\_Warn_{jiq}$ is an indicator variable that is equal to 1 when analysts' forecast revisions are strictly lower than those of the benchmark group, and 0 otherwise. In other words, $Private\_Warn_{jiq}$ helps flag analysts who are likely to receive bad news privately around earnings announcements. Second, I construct a firm-level measure ($Firm\_Private\_Warn_{iq}$) based on the fraction of analysts (N) who likely received private earnings warnings at the $q-1$ earnings announcement:

$$Firm\_Private\_Warn_{iq} = \frac{1}{N_{iq}} \sum_{j=1}^{N_{iq}} Private\_Warn_{jiq}. \tag{2}$$

Both measures rest on the assumption that I have a convincing control group of benchmark forecasters to capture analysts' forecast revisions following public news. To construct such a control group, I obtain data from Estimize, an online financial forecasting platform.[14] The data provide detailed insights into a diverse pool of forecasters and allows me to examine users' disclosed background characteristics as summarized in Internet Appendix Figure IA.1. Based on Brown and Khavis [2018], who confirm the reliability of Estimize users' background profiles, I retain user observations identified as professional (buy-side and independent analysts) and non-professional (e.g., academics, students, and working professionals) forecasters.

[14]Further information on Estimize's institutional setting is provided by Jame et al. [2016].

I eliminate those identified as sell-side analysts.[15] Finally, I drop anonymous user observations to reduce noise in the control group and reduce the concern that their intentions to forecast are less credible (Dyer and Kim [2021]). I argue the primary remaining difference between analysts and benchmark forecasters is their access to private information selectively disclosed by managers, and elements of my research design help isolate this difference.

Although individuals sharing their private information freely on online platforms is not intuitive, prior research suggests they do so to improve their reputation (Wasko and Faraj [2005]), career, and problem-solving skills (Lakhani et al. [2007]) and to be entertained (Brabham [2008]). In the online forecasting context, Crawford et al. [2018] find buy-side analysts disclose their private information on SumZero to advance their career, improve their reputation with investing clients, and ensure market prices reflect fundamental values. On Estimize, users' incentives include learning, reputation building, and the contribution to an unbiased set of market expectations. The participation and content are free. The platform promotes users' accuracy and protects data quality by reviewing and verifying users' submitted forecasts.[16]

Recent studies confirm the usefulness of online financial forecasting to the capital market. For instance, Jame et al. [2016] find crowdsourced estimates convey unique information to the market because these forecasts are less biased and contain more recent information than those of analysts. Importantly, the value of online financial forecasting increases with the diversity of users (Adebambo et al. [2017]). Campbell et al. [2019] also find non-professional research reports written on SeekingAlpha, a social media platform for investment research, provide unique value to investors.

[15] I retain forecasts submitted by buy-side analysts, because prior research finds they rely more strongly on quantitative and less on privately disclosed information relative to sell-side analysts (Brown et al. [2015]).
[16] See also Estimize.com and Gillam et al. [2017] for information about Estimize user incentives.

3.2. Measuring shareholder litigation risk

I capture variation in managers' perceived shareholder litigation risk using data on judge ideology from Huang et al. [2019]. Drawing from the political science literature and exploiting variation in judges' appointment by Democrats versus Republicans, Huang et al. [2019] classify judge ideology by the extent to which a judge presiding over a case would take a liberal versus conservative standpoint. Because federal judges are appointed by the president, and presidents tend to choose judges with a similar political orientation, judges appointed by Democrats versus Republicans can be classified on a liberal (pro-shareholder) versus conservative (pro-firm) dimension (e.g., Baum [1992]; Grofman and Brazill [2002]; Epstein et al. [2007]).[17] The intuition underlying this classification is that judges use discretion in their decision-making and that managers are aware of the implications of judge ideology for their court case outcomes. Importantly, because a president can appoint a federal judge when there is a vacancy in the court, and court vacancies commonly occur when judges retire or die, judge ideology varies substantially across circuits and within circuits over time. Using this classification of federal judge ideology, Huang et al. [2019] define managers' perceived litigation risk as the probability that a panel of three randomly selected federal circuit court judges is dominated by liberal judges.

Huang et al. [2019] measure judge ideology at the federal circuit level, because judges in circuit and district courts rule on most of these lawsuits, and securities class-action lawsuits are rarely if ever heard by the Supreme Court (Choi and Pritchard [2012]).[18] Because circuit judges can review and reverse decisions made by district judges, and district judges care about their reputation and career, prior research finds the ideology of circuit judges is strongly reflected in rulings by district judges (Choi et al. [2012]). Therefore, Huang et al.

[17] In the political science literature, researchers have introduced and measured judge ideology before Huang et al. [2019] (see, e.g., Bonica and Sen [2021] for a summary). According to prior research, judges' and justices' political orientations influence case outcomes (Posner [2005]; Coffee [2015] and Epstein et al. [2012], respectively), and liberal judges impose a greater threat to firms' success in winning in court (Sullivan and Thompson [2004]; Ventoruzzo and Fedderke [2016]).

[18] The US federal court system is based on three pillars: (i) district courts, (ii) circuit courts, and (iii) the Supreme Court. It has 13 circuit courts (12 regional circuits) and 94 district courts. Only about 1%-2% of cases appealing to the Supreme Court are heard. See uscourts.gov.

[2019] capture shareholder litigation risk directly at the circuit level and indirectly at the district level, where securities class-action lawsuits are heard.[19]

3.3. Research design

I estimate the relation between the threat of shareholder litigation and firms' propensity to disclose private earnings warnings by estimating the following equation:

$$Firm\_Private\_Warn_{iq} = \beta_1 Litigation\_Risk_{cm} + \mathbf{X}\delta + \alpha_c + \alpha_q + \varepsilon_{iq}, \tag{3}$$

where subscripts i, q, c, and m refer to firm, year-quarter, circuit, and year-month, respectively. *Firm_Private_Warn* is the self-constructed proxy for firms' propensity to leak bad news to analysts. *Litigation_Risk* captures firms' shareholder litigation risk based on variation in federal judge ideology that is linked to firms' headquarters. Specifically, *Litigation_Risk* varies at the circuit-year-month level, and for each firm's $q-1$ earnings announcement, I attach the litigation risk variable from a month before. The choice to attach on a monthly basis ensures that I plausibly represent a manager's most recent expectation of litigation risk at the time of the earnings announcement.[20] $\mathbf{X}$ is a set of observable linear control variables primarily measured at the firm-quarter level. I estimate equation (3) using OLS and cluster standard errors at the state level.[21] I expect β_1 to be positive: firms are more likely to leak bad news to analysts when shareholder litigation risk increases.

Specific features of my empirical research design help isolate the variation in firms' shareholder litigation risk to predict their private disclosure decisions. First, I present a firm-specific and time-varying proxy for managers' propensity to disclose bad news privately (see

[19] See Huang et al. [2019] for a detailed summary of the theory and the development of the proxy. Although the assumption that plaintiffs go to the court where they expect the most favorable outcomes seems reasonable, this practice is infrequent at the circuit level. The relevant circuit court is geographically linked to a firm's headquarters. This observation suggests firms can form expectations about the probability that a panel of three randomly selected judges in a circuit would decide in favor of shareholders.

[20] Alternative design choices include (i) the replacement of year-quarter fixed effects with year-month fixed effects (see Internet Appendix Table IA.2) and (ii) the measurement of litigation risk at the circuit-year-quarter (untabulated). My main inferences are robust to these alternative design choices.

[21] By choosing state-level clustering, I follow Huang et al. [2019]. My main inferences are robust to various clustering methods such as clustering standard errors at the firm level and two-way at the firm and year-quarter levels, and also, calculating wild bootstrapped standard errors (see Internet Appendix Table IA.3).

Section 3.1). I acknowledge concerns that my private disclosure proxy might capture analysts' incentives to bias earnings forecasts (e.g., Brown et al. [2015]) and to selectively update earnings forecasts when new information becomes available (e.g., Berger et al. [2019]).[22] In Section 4.2, I present validation tests of my empirical proxy at both the analyst level and the firm level. Second, I use Huang et al.'s [2019] proxy for shareholder litigation risk (see Section 3.2). The key identifying assumption is that *Litigation_Risk* is plausibly exogenous to firms' disclosure strategies. A concern is that my measure of *Litigation_Risk* is not as good as randomly assigned. For instance, although judges are randomly assigned to a case, judge ideology can capture location- and time-specific characteristics (see, e.g., Bonica and Sen [2021] for a summary). Huang et al. [2019] present a number of validation tests that support the use of judge ideology as a proxy for shareholder litigation risk. Given that litigation risk is a function of the likelihood that shareholders can take legal action against a firm, they find judge ideology has a significant and economically meaningful impact on firms' likelihood of being sued and court case outcomes.[23]

I further include an extensive list of linear control variables associated with the independent and dependent variables. I include controls for firms' issuance of public guidance at the $q-1$ earnings announcement, because firms can use various disclosure channels such as public voluntary disclosure in response to shareholder litigation risk (Skinner [1994, 1997]). I control for the level of the earnings surprise, whether a firm meets or misses earnings targets,

[22] For example, if *Firm_Private_Warn* reflects analysts' incentives to provide pessimistic forecasts, and if these incentives are affected by changes in shareholder litigation risk, then my estimates would be biased. To partially reduce this concern, I include a measure of forecast pessimism in Internet Appendix Table IA.4. I follow Veenman and Verwijmeren [2018] and construct a new variable, *Pessimism*, based on firm-level consensus and analyst-level forecast errors. I find *Pessimism* is positively and significantly associated with *Firm_Private_Warn*. However, the inclusion of *Pessimism* as a linear control variable does not materially affect the relation between shareholder litigation risk and my proxy. Moreover, with the inclusion of firm fixed effects, the coefficient on *Pessimism* is no longer differentiable from zero. Finally, in an alternative research design, I compare firms to those with similar levels of forecast pessimism (but different levels of shareholder litigation risk) by interacting *Pessimism* with year-quarter fixed effects. The results of this analysis confirm my main inferences.

[23] For example, across firms, judge ideology plays a larger role when the fraction of institutional investors is higher. Over time, an increase in litigation risk predicts an increase in the frequency of lawsuit filings. Also, the market reacts negatively (positively) to the appointment of liberal (conservative) judges (see Huang et al. [2019]). Yet, a concern remains that estimated judge ideology is different from actual judge ideology and thus understates the true effect of litigation risk.

and whether a firm reports a loss at the $q-1$ earnings announcement to account for analysts' and benchmark forecasters' reactions to public news and anticipated firm performance. Other control variables that are associated with *Litigation_Risk* include industry returns, market returns, state political leanings, and whether a firm operates in a high-litigation-risk industry (Huang et al. [2019]). I also include variables linked to managers' strategic disclosure choices, namely, firm size, book-to-market, sales growth, and analyst coverage (Johnson et al. [2020]), and the fraction of institutional investors, because analysts cater to the needs of large investors (Brown et al. [2015]; Johnson et al. [2020]). Appendix A presents the variable definitions.

Finally, I include a set of fixed effects. Circuit fixed effects α_c control for time-invariant factors across circuits. Year-quarter fixed effects α_q control for macroeconomic trends, changes in the political landscape, and trends in class-action lawsuit filings over time. In this way, I isolate within-circuit variation in shareholder litigation risk and managers' use of private disclosure over time, and in assessing the relation between the two, I account for macro-level shocks that affect all circuits at the same time.[24]

3.4. Data sources and sample construction

I combine data from CRSP, Compustat, Estimize, I/B/E/S, Thomson Reuters Institutional (13f) Holdings, and Huang et al. [2019] from calendar-quarters 2012Q1 through 2020Q4.[25] My initial sample consists of US firms from CRSP-Compustat Merged, including firms with a primary listing on NYSE/AMEX/Nasdaq and firm-quarter observations with non-negative assets and non-missing SIC, CIK, and reporting date identifiers. I attach

[24] My empirical results are robust to alternative design choices, such as (i) including firm fixed effects in the main design (see Table 5), (ii) including year-month fixed effects, (iii) including industry-circuit and industry-year-quarter fixed effects, and (iii) transforming the independent variable to an indicator variable capturing low- versus high-litigation-risk values and including year-month fixed effects (see Internet Appendix Table IA.2). Furthermore, results are robust to reconstructing the panel at the *analyst*-firm-year-quarter level and including skill-year-quarter (or experience-year-quarter) fixed effects, addressing concerns that my results are driven by the differential composition of analysts' skills or experience across circuits (see Internet Appendix Table IA.5).

[25] Prior to 2012Q1, Estimize coverage is too limited for my study.

the sample of earnings forecast revisions from I/B/E/S Unadjusted Detail History and Estimize, after linking each Estimize firm to common firm identifiers, namely, GVKEY, CUSIP, PERMNO, CIK, and I/B/E/S ticker.

I focus on quarter q earnings forecasts that are revised around the days of quarter $q-1$ earnings announcements (see Section 3.1 and Appendix A). The construction of forecast revisions depends on accurately identified earnings reporting dates on I/B/E/S and Estimize. To identify precise reporting dates, I use the reporting date when at least two of the following datasets agree on the date variable: CRSP-Compustat, Estimize, and I/B/E/S. For the 13 firm-quarter observations for which the datasets record different reporting dates, I hand-collect the dates from firms' 8-K's and websites.

I next attach data on the linear control variables, firms' historical headquarter location, and Huang et al.'s [2019] proxy for litigation risk. Huang et al. [2019] measure judge ideology at the circuit-year-month level and capture the risk of securities class-action lawsuits filed in firms' headquarter location. Because Compustat backfills firms' historical headquarter addresses and provides only the current location, which can increase measurement error and bias results (Jennings et al. [2021]), I use the recorded historical headquarter locations (HLOC) from CRSP-Compustat Merged. Finally, I attach hand-collected data on a state's political leaning (*Blue_State*), which captures the voting outcome of the presidential elections per state and over time.[26]

Panel A of Table 1 outlines the sample selection for analysts. Panel B of Table 1 presents the sample selection for the control group of benchmark forecasters. The selected group of benchmark forecasters consists of 10.37% buy-side analysts, 11.74% independent analysts, and 77.89% non-professional forecasters (5,065 benchmark forecasters). Within the group of buy-side analysts, hedge fund (189) and asset manager (166) are the most frequently identified professions. Within the group of non-professional benchmark forecasters, most

[26]The linear control variables are measured at quarter $q-1$ fiscal year-end. Earnings news control variables are attached to the days of quarter $q-1$ earnings announcements. Litigation risk varies at the circuit-year-month level, and for each firm's $q-1$ earnings announcement, I attach the litigation risk variable from a month before.

users are employed in Information Technology (1,035) and Finance (428) and are students (699) (untabulated). Finally, Panel C of Table 1 summarizes the main sample selection. My final sample consists of 14,884 firm-quarter observations, reflecting 1,720 unique firms from 2012Q1 through 2020Q4. In analyst-firm-quarter-level tests, I require the availability of additional analyst-level control variables using the I/B/E/S Recommendations Detail and Unadjusted Detail History files. For these tests, I obtain a final sample of 153,209 analyst-firm-quarter observations, including 3,288 unique analysts following 1,682 unique firms.

4. Results

4.1. Descriptive statistics

Panel A of Table 2 presents the summary statistics for the firm-quarter sample. Around quarter $q-1$ earnings announcements, the control group's mean revision of quarter q earnings forecasts is -0.84 cents ($\Delta F^{Control}$), while analysts' mean revision of forecasts is -3.01 cents ($\Delta F^{Analyst}$) (unscaled). The negative sign suggests forecasts are on average revised more downwards than upwards. In a similar vein, in 62% of firm-quarters, the mean analyst forecast revision is negative (i.e., downwardly revised) compared with 31% of firm-quarters for the mean benchmark forecasters' revisions (untabulated).

To illustrate the differences in forecast revisions between analysts and benchmark forecasters, Figure 2 displays both groups' median earnings forecast revisions in event time. The graph illustrates that analysts strongly lower their forecasts (scaled by price) around event days 0 and 1, which is when the firm reports quarterly earnings. By contrast, benchmark forecasters do not lower their forecasts as strongly. Similarly, my firm-level private disclosure proxy, *Firm_Private_Warn*, captures variation in managers' extent to leak bad news to analysts. Figure 3 presents the distribution of *Firm_Private_Warn* and illustrates that many firms cluster in the extremes. A value of 1 (0) suggests all analysts following firm i in quarter $q-1$ are likely to receive bad (good or no) news from managers.

The mean of *Litigation_Risk* is 46%. A value less than 50% indicates a panel of three

randomly selected circuit court judges is more likely to decide in favor of firms rather than shareholders. Generally, substantial variation exists in litigation risk across circuits, which stems from changes in federal circuit judges' composition. During my sample period, the 2nd and 9th Circuits are the most liberal and the 7th and 8th Circuits are the most conservative. For instance, in the 9th Circuit, the probability that a panel is dominated by liberal judges is 65%, but in the 8th Circuit, the probability is only 3%. Litigation risk also varies over time. While the 1st Circuit becomes more liberal (18% in 2012 to 42% in 2020), the 7th Circuit becomes more conservative over time (15% in 2012 to 3% in 2020) (untabulated).[27] Figure 4 further presents the annual distribution of shareholder litigation risk based on judge ideology. In line with the presidential elections during my sample period, I find an increase in judge ideology from 2012 until 2017 and a subsequent decrease.

Regarding firm controls, I find 25% of firm-quarters provide public guidance and 26% report a loss. The median firm-quarter earnings surprise is 3.38 cents per share. This finding corresponds to an average of 75% of firm-quarters that meet or beat quarterly earnings consensus forecasts. The sample statistics are skewed toward large (median MV is 5.8 billion USD) and high growth firms (median BM is 0.28). The mean (median) analyst coverage is 12 (11) and the mean (median) fraction of institutional investors is 61% (76%). Because this sample requires the availability of Estimize benchmark forecasters who initiated coverage of large firms in the initial years (Schafhäutle and Veenman [2022]), the sample firms are larger than those in related research (e.g., Green et al. [2014]).

Panel B of Table 2 presents the mean values of the variables split on low versus high litigation risk.[28] First, I find a higher mean value for *Firm_Private_Warn* in the high-litigation-risk partition. This difference is significant at the 10% level and consistent with my hypothesis that managers' propensity to provide private earnings warnings increases

[27] Most sample firms are headquartered in the 2nd, 5th, and 9th Circuits. In a "leave-one-out" robustness test (i.e., I drop each circuit one at a time), I consistently document a positive relation between shareholder litigation risk and private earnings warnings provisions (untabulated).

[28] I drop the variables underlying my firm-level private disclosure proxy because they are not used to test the hypothesis. I also drop *Market_Ret* because this variable is estimated for the entire pool of firms in a given quarter.

when shareholder litigation risk increases. Related to prior research (e.g., Skinner [1994]), I find a level difference in public guidance between firms operating in the high- versus the low-litigation-risk partition. However, this difference is not statistically significant at conventional levels. Second, some significant differences exist among the covariates. Firms in the high-litigation-risk partition are more likely to be larger, exhibit higher sales growth, and be younger than in the low-litigation-risk partition. These insights highlight the need to control for observable firm characteristics in my empirical analyses.

Finally, Panel D of Table 2 illustrates the sample distribution across industries and associated mean values of *Firm_Private_Warn*. Most firms are identified in (i) business equipment, (ii) wholesale, retail, and some services, and (iii) other industries. Firms' propensity to leak bad news to analysts is highest in the consumer non-durables (62.09%), followed by chemicals and allied products (55.24%) and consumer durables (55.06%). By contrast, firms' propensity to privately disclose earnings warnings is lowest in regulated industries, namely, utilities (39.71%) and oil, gas, and extraction (42.1%).

4.2. Validation tests

4.2.1. Do analysts with preferred access to managers receive private earnings warnings?

In this section, I validate the analyst-level measure, *Private_Warn*, as a proxy for the probability that analysts receive bad news from managers. According to prior research, private access to managers varies predictably across analysts. Analysts who issue positive stock recommendations are more likely to gain private access to managers than those who issue negative stock recommendations (e.g., Chen and Matsumoto [2006]; Mayew [2008]). If *Private_Warn* captures variation in analysts' private access to managers, I expect analysts who hold a favorable (unfavorable) view of the firm to be more (less) likely to receive bad news information. To test this prediction, I estimate the following linear probability model:

$$\begin{aligned} Private\_Warn_{jiq} = {} & \beta_1 Strong\_Buy_{jiq} + \beta_2 Buy_{jiq} + \beta_3 Sell_{jiq} \\ & + \beta_4 Strong\_Sell_{jiq} + \mathbf{X}_{jiq}\delta + \alpha_{iq} + \varepsilon_{jiq}. \end{aligned} \tag{4}$$

Above, j, i, and q refer to analyst, firm, and year-quarter, respectively. *Strong_Buy*, *Buy*, *Sell*, and *Strong_Sell* are indicator variables that refer to an analyst's final outstanding stock recommendation and proxy for analysts' preferred access to managers. α_{iq} are a set of firm-year-quarter fixed effects that flexibly control for time-varying firm-level shocks. Using this design, I exploit within-firm-year-quarter variation in analysts' characteristics. $\mathbf{X}$ is a set of control variables that help rule out that alternative analysts' attributes, for example, ability or incentives, determine *Private_Warn*. Standard errors are clustered at the analyst level. I expect that $\beta_1 > 0$, $\beta_2 > 0$, $\beta_3 < 0$, and $\beta_4 < 0$.

I control for analysts' forecast accuracy, general and firm experience, and brokerage size. The latter reflects analysts' skills and level of resource availability (Clement [1999]). Because forecast accuracy also proxies for private access to managers (Chen and Jiang [2006]; Ke and Yu [2006]), analysts who issue more accurate forecasts may receive private information. I further control for brokerage house reputation (Hong and Kubik [2003]; Hilary and Hsu [2013]) and analysts' busyness based on the number of firms in their portfolio (Clement [1999]). The former recognizes that variation exists in analysts' credibility to move stock prices, but also potentially lowball their estimates. The latter captures analysts' reliance on private information from managers when they are busy.

Panel C of Table 2 presents summary statistics on the analyst-level variables. In line with prior research (e.g., Mayew [2008]), most analysts hold a favorable view of firms (mean *Strong_Buy* is 19% and mean *Buy* is 34%), and only 1% and 5% of analysts have an outstanding *Sell* and *Strong_Sell* stock recommendation, respectively.[29] The mean brokerage house has 100 analysts employed in a given year. Analysts have between 0 and 25 years of firm experience and between 0 and 34 years of general experience. The average analyst covers 15 firms, but substantial variation exists across analysts' busyness covering between 1 and 39 firms. Finally, 33% of analysts are employed at prestigious brokerage houses, including Deutsche Bank, Goldman Sachs, JPMorgan Chase & Co., Merrill, and Morgan Stanley.[30]

[29] I exclude neutral stock opinions from my summary table.

[30] Brokerage names are hand-collected based on the masked ESTIMID and in combination with analysts'

Column (1) in Table 3 presents results from estimating equation (4). To facilitate interpretation of the independent variables and to control for the skewness in their distributions, all continuous variables are sorted into quarterly decile ranks (Bernard and Thomas [1990]) and subsequently transformed to a [-0.5; 0.5] scale (Mashruwala et al. [2006]). Consistent with my expectations, I find the probability of receiving private earnings warnings from managers is positively associated with the favorability of analysts' outstanding stock recommendations. Relative to neutral recommendations, *Strong_Buy* and *Buy* recommendations increase the probability that analysts receive private earnings warnings from managers by 3 and 2.4 percentage points, respectively.

The negative coefficient on *Accuracy* suggests less accurate analysts are substantially less likely to receive private earnings warnings.[31] The coefficient on *Brokerage_Size* is positive and significant, suggesting analysts who work for larger rather than smaller brokerage houses are 3.3 percentage points more likely to receive bad news. Because brokerage size captures not only analysts' resource availability but also their reputation, firms might provide preferred access to analysts employed at prestigious brokerages. Other analyst experience controls are not significantly related to the probability that analysts receive bad news privately. Consistent with the idea that busy analysts rely on firms' private signals, I find the busiest analysts are 2.8 percentage points more likely than the least busy to receive private earnings warnings. Finally, *Reputation* does not incrementally explain *Private_Warn*, after controlling for other variables.[32]

In column (2), I use an alternative specification to test whether my results are sensitive to the empirical design choice. I include linear controls for (i) public earnings announcement news to hold analysts' reactions to public information constant, (ii) the fraction of institutional investors, because analysts care about their credibility with investing clients

names provided by IBES recommendations detail.

[31] This variable is constructed such that larger values indicate a higher degree of inaccuracy.

[32] Because analysts from prestigious brokerages have a greater influence on stock prices than less prestigious analysts (Stickel [1992]; Gleason and Lee [2003]), another strategy could be to leak bad news to high-reputation analysts (e.g., Mayew [2008]). I find no evidence for this alternative strategy (untabulated).

(Brown et al. [2015]), and (iii) the number of analysts covering a firm, because managers could be time constrained in privately speaking to all analysts. I also include firm and year-quarter fixed effects to control for time-invariant unobserved firm factors and unobserved time trends, respectively. Because analysts' stock recommendations are sticky over time, I do not include analyst fixed effects, to reduce concerns of limited variation within the explanatory variables of interest (deHaan [2021]).[33] Estimated coefficients and levels of statistical significance are similar to those in column (1). Two results stand out. First, because analysts quickly revise their forecasts following firms' public earnings guidance (Cotter et al. [2006]), the insignificant relation between *Public_Guidance* and *Private_Warn* increases confidence that my proxy captures firms' private disclosure of information, which can be an alternative channel through which managers leak bad news.[34] Second, managers' provision of private earnings warnings is not determined by the number of analysts covering a firm.

4.2.2. The predictive ability of managers' private earnings warnings

The above results suggest managers leak earnings warnings to analysts with whom they are more likely to meet privately. A potential alternative explanation is that analysts who issue favorable stock recommendations please managers with pessimistic earnings forecasts, which help the firm meet the consensus forecasts (e.g., Richardson et al. [2004]; Ke and Yu [2006]). To reduce this concern, I present a second validation test. I argue if my measure indeed captures variation in firms' propensity to disclose bad news privately to analysts, it should be associated with future adverse performance.

Empirically, I define an indicator variable equal to 1 when a firm reports negative earnings at the quarter q earnings announcement (*Future_Loss*), and 0 otherwise. Additionally, I

[33] The results are robust to alternative fixed effects structures, such as (i) firm, year-quarter, and analyst fixed effects (untabulated) and (ii) analyst-firm fixed effects (untabulated). Also, estimating equation (4) using a logit model and employing Mayew's [2008] methodology to use peer group adjusted measures (analyst j relative to other analysts following firm i at quarter $q-1$), I confirm signs and statistical significance levels of my estimates (untabulated).

[34] The insignificant association is unsurprising given the empirical strategy to estimate private disclosure and the assumption that I can use a benchmark group to control for analysts' reaction to public news.

estimate future adverse performance, using two continuous variables, and account for changes in earnings and cash flow from operations, because firms could report a loss already in the previous quarter (*Future_ΔEarn* and *Future_ΔCFO*, respectively). I include common firm controls associated with firms' likelihood to report bad news and disclosure strategies, and include year-quarter fixed effects. All continuous variables are sorted into quarterly decile ranks and subsequently transformed to a [-0.5; 0.5] scale to control for skewness in the variables' distributions and facilitate the interpretation of the results.

Table 4 presents the results on the relation between private earnings warnings and future adverse performance. The results consistently suggest *Firm_Private_Warn* is positively associated with future adverse performance. That is, the reporting of quarterly losses, decreases in earnings, and decreases in cash flows from operations are more likely for firms that disclose bad news privately to analysts. For instance, I find a one-unit increase in firms' propensity to leak bad news is associated with a 6-percentage-point increase in firms' probability of reporting a loss in the next quarter. The results remain largely the same when I include firm fixed effects (untabulated). Combined, the results increase confidence that my measure captures firms' private signals instead of analysts' incentives to play the earnings game.

4.3. Shareholder litigation risk and managers' private earnings warnings

Table 5 presents the main results on the relation between shareholder litigation risk and firms' propensity to disclose bad news privately. My hypothesis predicts this relation is positive. In column (1), I present the results for the most basic design that includes only circuit fixed effects to isolate within-circuit variation over time in shareholder litigation risk and private earnings warnings. In column (2), I test my main design and examine within-circuit variation over time while linearly controlling for firm and earnings characteristics and flexibly controlling for overall time trends. In column (3), I exchange circuit fixed effects with firm fixed effects and also omit *Litigation_Industry* because it is collinear with firm

fixed effects.[35]

I find managers are more likely to disclose earnings warnings privately when shareholder litigation risk increases. In estimating the relation between shareholder litigation risk and private earnings warnings, I find the coefficient equals 0.153 in column (2).[36] In terms of economic magnitude, after accounting for the variation explained by the fixed effects and linear control variables, a one standard deviation change in litigation risk is associated with about 2% of a standard deviation change in managers' private earnings warnings disclosure.[37] The effect of litigation risk on managers' private disclosure of bad news is larger when isolating within-firm variation, as presented in column (3), with a coefficient of 0.229.[38] Similarly, in terms of economic magnitude, a one standard deviation change in litigation risk is associated with about 3% of a standard deviation change in managers' propensity to leak bad news.

Note the results hold after controlling for firms' public voluntary disclosure. I also find firms are less likely to leak bad news to analysts when they report positive earnings surprises and meet the consensus forecast. In column (2), control variables associated with managers' incentives to guide expectations are largely unrelated to managers' propensity to leak bad news. The coefficients on firm size and growth are not significant, but the coefficient on sales

[35] My results hold with the inclusion of additional linear control variables, namely, return volatility, return skewness, turnover, and leverage. However, doing so further reduces the sample size. My inferences also remain the same after the inclusion of analyst coverage as a linear control variable. Analyst coverage is not among the set of linear control variables in my main research design, because it is the denominator of the dependent variable.

[36] In assessing the change in the baseline estimate from column (1) to the estimate with the key controls in column (2), I follow Oster [2019] and estimate the Oster delta (δ), which is an estimate of the effect that unobservable variables would need to have to possibly overturn my result (i.e., $\hat{\beta} = 0$). The Oster test relies on the assumption that movements in the estimated beta coefficients and R-squared are informative about the omitted variable bias in the full set of observable and unobservable variables. I find a δ of 0.94, suggesting the effect of unobservable variables would need to be at least 94% as important as the observed control variables employed in my main model. I find an even larger δ when comparing column (1) to column (3) (see Table 5).

[37] Specifically, I residualize both $Firm\_Private\_Warn$ and $Litigation\_Risk$ by the linear control variables and fixed effects in equation (3), and I use the standard deviations of those residuals to calculate the economic magnitude, i.e., $\frac{\hat{\beta} \times \hat{\sigma}(Litigation\_Risk_r)}{\hat{\sigma}(Firm\_Private\_Warn_r)}$.

[38] Although firms rarely shift headquarters across circuits, in those few cases, firm fixed effects do not subsume circuit fixed effects. My results are similar when I include circuit-firm fixed effects (untabulated).

growth is negative and significant at the 10% level. These results further strengthen the idea that I capture firms' private disclosure of earnings warnings rather than incentives to guide expectations.[39] I conclude that, in response to increases in shareholder litigation risk, managers use private disclosure as an alternative voluntary disclosure channel to indirectly influence market earnings expectations and valuations.

4.3.1. Adjusting the definition of benchmark forecasters

The inferences drawn in the previous sections depend on my ability to capture variation in firms' propensity to leak earnings warnings to analysts. However, my key variables hinge on the idea that I can reasonably compare analysts' and benchmark forecasters' revisions of earnings forecasts to capture private information flows between analysts and managers. Although the assumption that benchmark forecasters have no access to private meetings with managers is reasonable, these forecasters may lack the ability to extract value-relevant signals from public disclosures (Blankespoor et al. [2020]). If this difference in ability induces variation that is endogenous to within-circuit changes in shareholder litigation risk, then the estimate of the relation between shareholder litigation risk and my private disclosure proxy could be biased.

Internet Appendix Figure IA.1 outlines the distribution of all contributors on Estimize, of which about 5,000 unique users fulfill the selection criteria for my main benchmark group. From this group, about 78% are non-professional forecasters, such as working professionals and students. Using non-professional forecasters in my benchmark group raises particular concerns about their ability and skills to interpret public earnings news and extract value-relevant signals from public disclosures. Hence, I drop all non-professional forecaster obser-

[39] A concern is that analysts and benchmark forecasters use different definitions in their computations of non-GAAP earnings, which adds noise to my proxy for firms' private disclosure. I argue this is unlikely the case given that Estimize informs its users about the (firm-specific) computation of non-GAAP earnings in line with the definition of both sell-side analysts and firms. To alleviate remaining concerns, I redo the tests performed in Table 5 and exclude observations for which the absolute difference between unadjusted non-GAAP actuals recorded on I/B/E/S and Estimize differs by more than 1 cent (=1,602 firm-quarter observations). I find coefficient estimates and significance levels remain relatively unchanged.

vations from my group of benchmark forecasters and keep only buy-side and independent analysts. In Table 6, I report the results on the relation between shareholder litigation risk and firms' propensity to leak bad news to analysts using this alternative benchmark group. Using a sample of 8,929 firm-quarter observations, my results continue to hold. Focusing on within-circuit (firm) and year-quarter variation, I find the coefficient on *Litigation_Risk* equals 0.170 (0.288) and is significant at the 10% (1%) level.[40]

4.4. Heterogeneity in public disclosure costs and incentives

Next, I exploit cross-sectional variation in firms' public disclosure costs and incentives. First, I expect other costs associated with public disclosures influence firms' choice to use private disclosure. One such public disclosure cost is the possible revelation of proprietary information. Prior evidence suggests firms have incentives to withhold public disclosures when proprietary costs are high (e.g., Verrecchia [1983]; Healy and Palepu [2001]; and Beyer et al. [2010] for a summary). Therefore, to avoid public-disclosure-specific costs, firms might prefer private disclosure to analysts who include managers' private information in their summary statistics, such as earnings forecasts (King et al. [1990]). To estimate proprietary costs, I use variation in research and development expenses (*R&D*) (e.g., Kothari et al. [2002]; Koh and Reeb [2015]). I also exploit variation in the level of industry competition (*Competition*), as measured by the Herfindahl-Hirschman index (e.g., Verrecchia and Weber [2006]; Berger and Hann [2007]). I estimate equation (3) by partitioning firms in the sample into below- versus above-median values of *R&D* and *Competition*. Panel A of Table 7 presents the results. For presentation purposes, I do not report the control variables. I find some evidence that the results are concentrated among firms in the high-proprietary-cost partitions. Although the coefficient on *Litigation_Risk* is not significant at conventional levels in the *R&D* sample partition (t-stat=1.46), the coefficient is highly statistically significant in the *Competition* sample partition (t-stat=3.16). I also find the difference between the

[40]The use of this alternative benchmark group leads to unchanged inferences in my validation tests. See Internet Appendix Table IA.6.

low- and high-competition partitions is significant at the 10% level (t-stat=2.01).

Second, I test whether firms' private disclosure strategies are concentrated among firms that do not engage in public earnings guidance during the quarter. I posit that because each disclosure channel comes with disclosure-specific costs and benefits and because complex interdependencies exist in firms' set of disclosure choices (see, e.g., Beyer et al. [2010]; Heinle et al. [2022]), managers are more likely to choose private disclosure when they have not already committed to public disclosure. Also, recent survey evidence suggests litigation risk is not a first-order concern of firms that engage in public guidance but rather of firms that do *not* issue public guidance (Call et al. [2022]). Therefore, I expect my findings to be concentrated among non-guiding firms. To test this thesis, I partition the sample based on an indicator variable, *Public_Guidance*, that is equal to 1 when managers provide public guidance for q at the $q-1$ earnings announcement, and 0 otherwise. Alternatively, I modify *Public_Guidance* to instead be equal to 1 when firms provide public guidance for q at any point in time during $q-1$. Panel B of Table 7 presents the results. As predicted, I find firms' private disclosure strategies are concentrated among those firms that do not publicly guide, but the differences between the coefficients are not statistically significant at conventional levels.

Together, my results suggest firms are (i) more likely to engage in private disclosure when public disclosure costs are high and (ii) less likely to engage in private disclosure when they commit to public guidance. However, these results should be interpreted with caution because most of the differences between the coefficients are not statistically significant at conventional levels.

5. Discussion and conclusion

This paper examines managers' private disclosure of earnings warnings in response to changes in shareholder litigation risk. I use a self-constructed, firm-level, and time-varying measure of managers' propensity to disclose bad news privately to analysts, and I exploit

plausibly exogenous variation in shareholder litigation risk based on the ideology of federal circuit judges presiding over a case. Consistent with theory, I find managers' propensity to provide private earnings warnings to analysts increases when shareholder litigation risk increases. I further find managers' use of a private disclosure channel is concentrated among firms with high proprietary costs and among firms without commitments to public guidance. My study contributes to the literatures on firms' disclosure choices in response to shareholder litigation risk and firms' private communication with analysts. Particularly, I aim to address the question about *when* and *why* managers choose to disclose information privately. I find evidence consistent with shareholder litigation risk—in concert with other disclosure costs and benefits—being a moderating force that motivates private disclosure.

My inferences are subject to the following caveats. First, because directly observing managers' private disclosure strategies is not possible, my empirical proxy may insufficiently capture managers' private information flows to analysts. Quantifying the propensity of private bad news disclosures from managers to analysts by benchmarking analyst forecast revisions against a control group requires that (i) analysts incorporate information disclosed both publicly and privately immediately after earnings announcements, (ii) the control group does not receive managers' private information and has comparable information processing skills to analysts, and (iii) my research design accounts for other factors that may drive differences between the two groups. The validation tests support my empirical proxies and the robustness test helps to rule out that differences in skills between analysts and benchmark forecasters are what drive the results. However, I cannot fully rule out that analysts are simply better at uncovering bad news (Blankespoor et al. [2020]) or that analysts form expectations about firms' litigation risk, which are reflected in more negative forecast revisions relative to the control group. Second, because I require a group of benchmark forecasters to capture variation in managers' private bad news disclosures, my sample period is relatively short. Therefore, this paper is limited in its ability to generate other insights, such as from exploiting the implementation of Reg FD or the SEC's Reg FD enforcement actions. Third,

given the setting exploited in this study, I focus on firms headquartered in the US. My results are likely to be more relevant for firms operating in the US judiciary and legal system and not generalizable to firms located in other countries. For instance, differences between civil law and common law countries and other institutional differences across countries (e.g., quality of enforcement) (e.g., La Porta et al. [1997]) can have important implications for both firms' litigation risk and their disclosure strategies.

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Appendix A. Variable definitions

Variable	Definition
Key variables:	
$\Delta F^{Analyst}$	Analysts' quarter q earnings forecast revision in cents per share measured at quarter $q-1$ earnings announcements. Individual analyst's revisions are computed as the $t+10$ minus $t-1$ most recent outstanding forecasts including forecasts in the 120 calendar-day window leading up to $t+10$ and $t-1$, respectively. t is the day of firms' quarter $q-1$ earnings announcements. I use unadjusted earnings forecasts and subsequently adjust for stock splits using cumulative stock split factors from CRSP. (Source: I/B/E/S unadjusted detail history, CRSP)
$\Delta F^{Control}$	Benchmark forecasters' quarter q earnings forecast revision in cents per share measured at quarter $q-1$ earnings announcements. Forecast revisions are computed as $t+10$ minus $t-1$ mean consensus forecasts, where t is the day of firms' quarter $q-1$ earnings announcements. The consensus (mean) forecasts are self-constructed including each benchmark forecaster's most recent earnings forecast for quarter q in the 120 calendar-day window leading up to $t+10$ and $t-1$, respectively. When benchmark forecasters issue multiple forecasts for the same firm-date-time combination, the mean of these forecasts is used. Forecasts submitted by sell-side analysts or anonymous users and forecasts flagged as unreliable from the website's management are dropped from the sample. I use unadjusted earnings forecasts and subsequently adjust for stock splits using cumulative stock split factors. (Source: Estimize)
Litigation_Risk	Shareholder litigation risk is estimated as the probability that a panel of three randomly selected judges in a given circuit decides in favor of shareholders versus the firm. Data is obtained from Huang et al. [2019]. Litigation risk varies at the circuit-year-month level, and for each firm's $q-1$ earnings announcement, I attach the litigation risk variable from a month before. See Section 3.2 for further information and Huang et al. [2019] for a detailed description of the variable measurement.
Firm_Private_Warn	Fraction of analysts following a firm who receive bad news from managers, estimated as the sum of analysts for which $Private\_Warn = 1$, divided by the number of all analysts following firm i at quarter $q-1$.
Private_Revision	Extent of analysts' forecast revisions that is likely informed by private communication with managers, estimated as the difference between $\Delta F^{Analyst}$ and $\Delta F^{Control}$.
Private_Warn	Probability that an analyst receives bad news from managers. An indicator variable equal to 1, when $\Delta F^{Analyst} < \Delta F^{Control}$, and 0 otherwise.
Firm-quarter variables:	
Age	Firm age at the end of fiscal quarter $q-1$, measured as the natural log of the number of years since the firm first appeared on Compustat. (Source: Compustat quarterly)

Variable	Description
Blue_State	An indicator variable capturing a state's political leaning based on the outcomes of the presidential elections in the United States measured at the end of fiscal quarter $q-1$. *Blue_State* is equal to 1 if a state votes for the Democratic Party, and 0 otherwise. Variables are hand-collected from wikipedia.
Competition	Industry concentration estimated using the Herfindahl-Hirschman index for each firm's two-digit SIC industry at the end of fiscal quarter $q-1$, measured as the sum of squares of a firm's sales (SALESQ) divided by the sum of squares of industry sales. (Source: Compustat quarterly)
BM	Book-to-market ratio at the end of fiscal quarter $q-1$, measured as the ratio of book value of common equity (CEQQ) to MV. (Source: Compustat quarterly)
ES	Earnings surprise in cents per share measured at quarter $q-1$ earnings announcements. ES is calculated as the difference between I/B/E/S actual earnings per share and the self-constructed I/B/E/S consensus forecast, scaled by the final closing stock price from Compustat quarterly. The consensus (mean) forecast is constructed from individual analysts' most recent earnings forecasts made within 120 days of the announcement date. I use unadjusted earnings forecasts and actuals and subsequently adjust for stock splits using cumulative stock split factors from CRSP. (Source: I/B/E/S unadjusted detail history and actuals, CRSP)
Future_ΔCFO	Changes in firm quarterly cash flows from operations at quarter q. The difference between firm's quarterly cash flows from operations at quarter q minus cash flows from operations at quarter $q-1$ and scaled by total assets (ATQ) at quarter $q-1$. Quarterly cash flows from operations are based on Compustat's annual item (OANCFY), following Collins and Hribar [2000]. (Source: Compustat quarterly)
Future_ΔEarn	Changes in firm quarterly earnings at quarter q. The difference between firm's quarterly earnings (IBQ) at quarter q minus earnings at quarter $q-1$ and scaled by total assets (ATQ) at quarter $q-1$. (Source: Compustat quarterly)
Future_Loss	Firm reports a quarterly loss at quarter q earnings announcement. An indicator variable equal to 1 when quarterly earnings (IBQ) are negative, and 0 otherwise. (Source: Compustat quarterly)
Industry_Ret	Cumulative equally-weighted monthly industry returns (RET) over the prior twelve months leading up to the end of fiscal quarter $q-1$. *Industry_Ret* is the average of firms' equally weighted monthly returns within a four-digit SIC code. (Source: CRSP monthly)
Inst_Hold	Fraction of institutional investors (InstOwn_Perc) holding firm i's shares at the end of fiscal quarter $q-1$. (Source: Thomson/Refinitiv Institutional (13f) Holdings)
Litigation_Industry	A firm belongs to a high-litigation-risk industry at the end of fiscal quarter $q-1$, following Francis et al. [1994]. *Litigation_Industry* is equal to 1 when a firm's four-digit SIC code falls in one of the following groups: biotechnology (2833–2836, 8731–8734), computers (3570–3577, 7370–7374), electronics (3600–3674), and retail (5200–5961), and 0 otherwise. (Source: Compustat quarterly)

Variable	Description
Loss	Firm reports a quarterly loss at quarter $q-1$ earnings announcements. An indicator variable equal to 1 when quarterly earnings (IBQ) are negative, and 0 otherwise. (Source: Compustat quarterly)
Market_Ret	Cumulative value-weighted monthly market returns (VWRETD) over the prior twelve months leading up to the end of fiscal quarter $q-1$. (Source: CRSP monthly)
MBE	Firm meets or beats analysts' consensus forecast at quarter $q-1$ earnings announcements. An indicator variable equal to 1 when $ES \geq 0$, and 0 otherwise.
MV	Firm size at the end of fiscal quarter $q-1$, measured as the natural log of the total market value of equity at the end of the fiscal quarter (PRCCQ $\times$ CSHOQ). (Source: Compustat quarterly)
N	Analyst coverage measured at quarter $q-1$ earnings announcements. The natural log of the number of analysts with an outstanding forecast of quarter q earnings per share within 120 days of the announcement date. (Source: IBES unadjusted detail history)
Public_Guidance	Firms' public guidance for quarter q. When the outcome variable is *Firm_Private_Warn*, this variable is defined as an indicator variable equal to 1, when firms issue public guidance at day t or $t+1$, where t is the day of quarter $q-1$ earnings announcements, and 0 otherwise. In tests where the outcome variable is measured at quarter q earnings announcements, this variable is defined as an indicator variable equal to 1, when firms issue public guidance for quarter q, and 0 otherwise. (Source: I/B/E/S recommendation unadjusted detail)
R&D	Research and development expense at the end of fiscal quarter $q-1$, measured as the ratio of (nonnegative) research and development expense (XRDQ) to total assets (ATQ). I do not replace missing values of XRDQ with zeroes (e.g., Koh and Reeb [2015]). (Source: Compustat quarterly)
Sales_Growth	Sales growth at the end of fiscal quarter $q-1$, measured as the average growth of sales (SALEQ) from quarter $q-2$ to quarter $q-1$. (Source: Compustat quarterly)
Analyst variables:	
Accuracy	Analyst's absolute forecast error measured before quarter $q-1$ earnings announcements. Forecast error is the difference between the actual earnings per share and an analyst's most recent outstanding earnings forecast. I use unadjusted earnings forecasts and actuals and subsequently adjust for stock splits using cumulative stock split factors from CRSP. (Source: IBES unadjusted detail history, CRSP)
Analyst_Exp	Analyst's general working experience, measured as the difference between the date of an analyst's first appearance on I/B/E/S and the day of quarter $q-1$ earnings announcements, divided by 365. (Source: IBES unadjusted detail history)
Buy	An indicator variable equal to 1 when an analyst's most recent outstanding stock recommendation before the $q-1$ earnings announcement is a buy, and 0 otherwise. (Source: IBES recommendations detail)

Variable	Description
Brokersize	Number of analysts (AMASKCD) who are associated with a brokerage house (ESTIMID) in a given year and measured before quarter $q-1$ earnings announcements. (Source: IBES recommendations detail)
Firm_Exp	Analyst's firm experience, measured as the number of years an analyst has covered a firm. *Firm_Exp* is the difference between the date of an analyst's first recorded earnings forecast on I/B/E/S and the day of quarter $q-1$ earnings announcements, divided by 365. (Source: IBES unadjusted detail history)
N_Firms	Number of firms (including firm i) an analyst follows over quarter $q-1$. (Source: IBES unadjusted detail history)
Reputation	An indicator variable capturing prestigious brokerage houses. *Reputation* is equal to 1 when the brokerage house (ESTIMID) is identified as one of the ten largest brokerage houses in the sample, i.e., CLALEXHK, FBOSTON, FRCLAYSC, GOLDMAN, JEFFEREG, JPMORGAN, LAWRENCE, MACQUARI, MERRILL, and MORGAN, and 0 otherwise. (Source: IBES recommendations detail)
Sell	An indicator variable equal to 1 when an analyst's most recent outstanding stock recommendation before the $q-1$ earnings announcement is a sell, and 0 otherwise. (Source: IBES recommendations detail)
Strong_Buy	An indicator variable equal to 1 when an analyst's most recent outstanding stock recommendation prior to the $q-1$ earnings announcement is a strong buy, and 0 otherwise. (Source: IBES recommendations detail)
Strong_Sell	An indicator variable equal to 1 when an analyst's most recent outstanding stock recommendation before the $q-1$ earnings announcement is a strong sell, and 0 otherwise. (Source: IBES recommendations detail)

Figure 1
Setting and timeline

This figure displays a timeline including the prior- and current-quarter earnings announcements which correspond to the sample selection procedures and calculation of key variables. In essence, this paper focuses on quarter q forecasts that are revised around the days of firms' quarter $q-1$ earnings announcements to estimate the probability that analysts' forecast revisions are due to private communication with managers. To hold constant the extent of analysts' forecast revisions due to public information, I benchmark analysts' forecast revisions against a control group. The empirical strategy is motivated by Schafhäutle and Veenman [2022], who find analysts strongly revise quarter q earnings forecasts around the days of quarter $q-1$ earnings announcements, which is when manager-analyst private meetings peak (Soltes [2014]). The combined factors that analysts update earnings forecasts for quarter q at quarter $q-1$ earnings announcement using information disclosed both publicly (e.g., 10-K, 10-Q, management forecasts) and privately (e.g., private meetings with managers) and benchmark forecasters only observe public information signals lies at the heart of the empirical strategy to estimate managers' private disclosure. Appendix A summarizes the variable construction in detail.

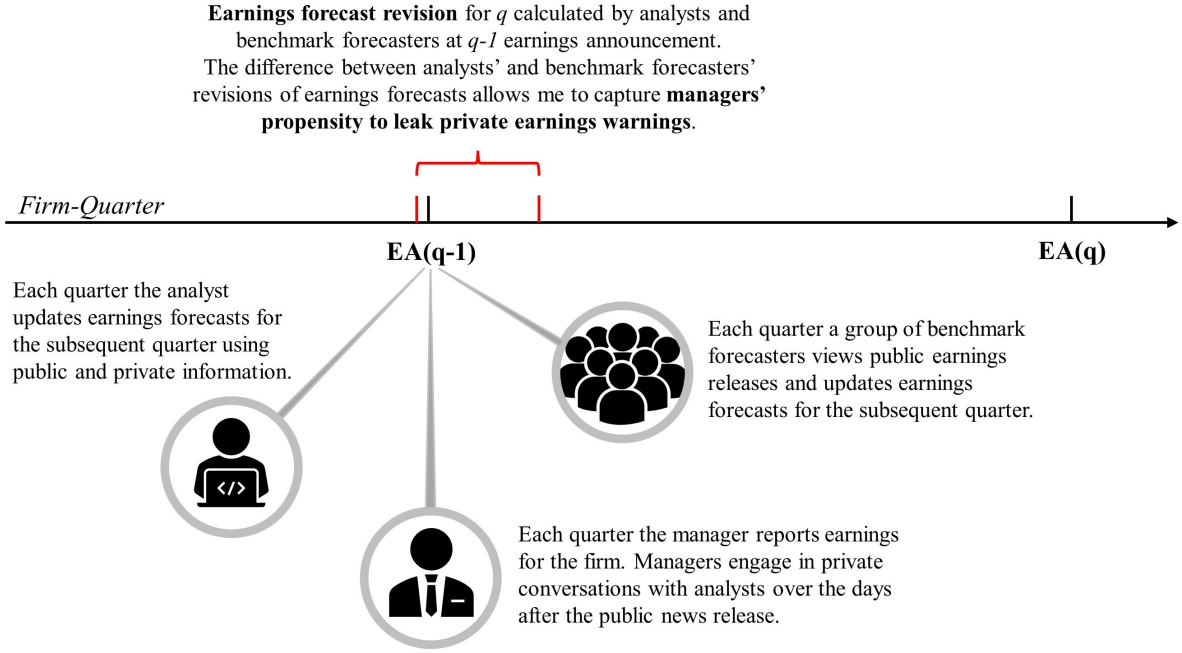

Figure 2

Earnings forecast revisions in event time

This figure presents earnings forecast revisions of analysts and benchmark forecasters over the days around prior-quarter earnings announcements ($t = 0$). On each of the event days [-3, 10] around firms' prior-quarter earnings announcements, I first compute analysts' and benchmark forecasters' median consensus earnings forecasts per firm-quarter and scale by prior-quarter closing price. Appendix A summarizes the variable construction in detail (see $\Delta F^{Analyst}$ and $\Delta F^{Control}$). All t consensus forecasts are constructed over the $[t-120, t]$ forecasting horizon. For instance, on day -3, the consensus forecast is constructed from forecasts issued in the [-123,-3] window. I then calculate sample median consensus forecasts. Second, I calculate changes in analysts' and benchmark forecasters' median consensus forecasts between $t-1$ and t to illustrate the groups' daily median earnings forecast revisions. The solid blue line depicts analysts' median earnings forecast revisions and the dashed pink line depicts benchmark forecasters' median earnings forecast revisions during the [-3,10] forecasting horizon. The x-axis denotes the event-days (calendar days) relative to firms' prior-quarter earnings announcements. The y-axis denotes the median earnings forecast revision scaled by price in cents per share. Negative (positive) values illustrate downward (upward) revisions and zero values illustrate no revision in earnings forecasts.

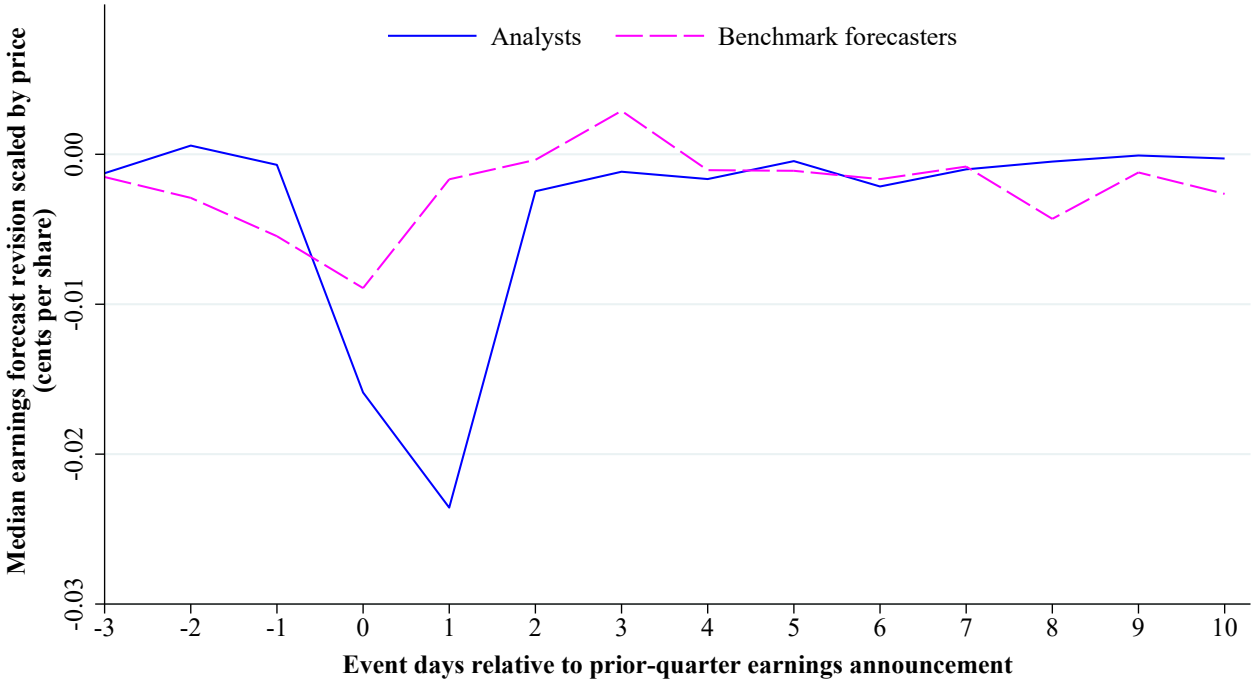

Figure 3

Distribution of managers' propensity to provide private earnings warnings

This figure presents the frequency distribution of my measure of managers' propensity to communicate privately with analysts about bad news. *Firm_Private_Warn* is measured based on the differential forecasting behavior between sell-side analysts and benchmark forecasters. The x-axis reflects the fraction of analysts following a firm who are likely to receive private earnings warnings from managers. The variable is constructed such that a value closer to 1 indicates that managers' propensity to leak bad news to the majority of analysts increases. Appendix A summarizes the variable construction in detail.

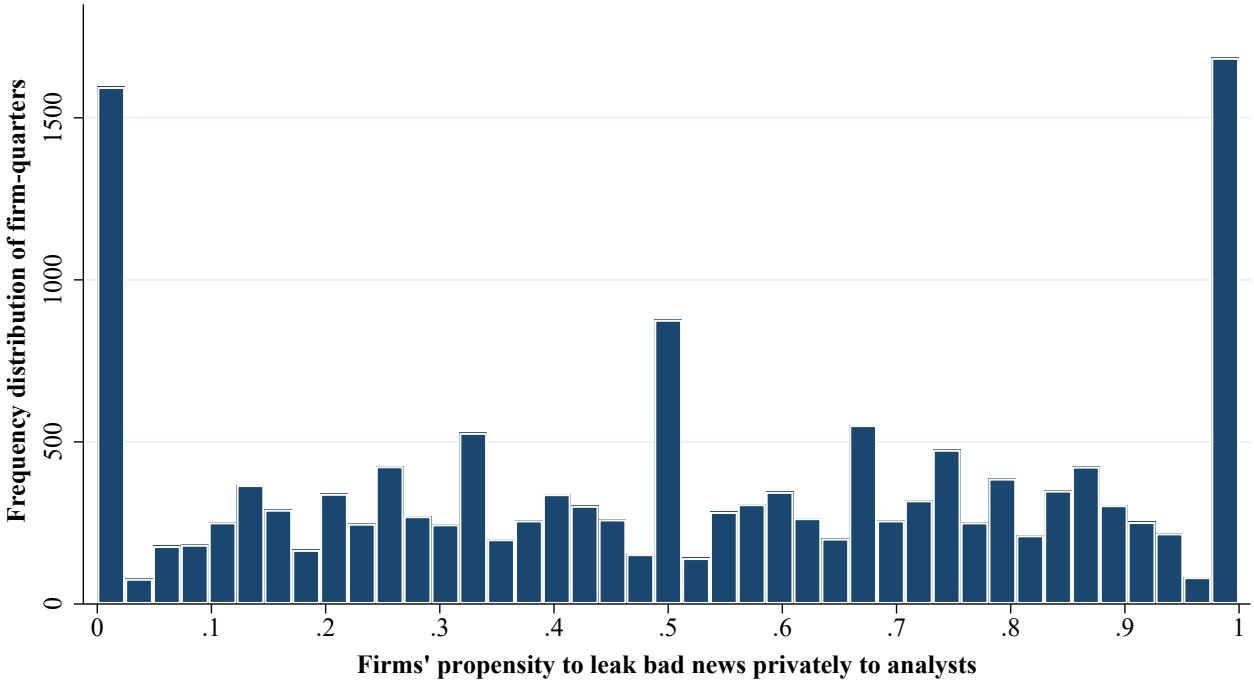

Figure 4
Shareholder litigation risk over time

This figure illustrates the annual distribution of shareholder litigation risk from 2012 to 2020 using data from Huang et al. [2019]. Litigation risk is estimated as the probability that a panel of three randomly selected judges in a given circuit is dominated by liberal judges. A panel dominated by liberal judges is more likely to decide in favor of shareholders rather than the firm. The y-axis displays the probability that the panel of three randomly selected judges consists of at least two liberal judges, i.e., appointed by a Democratic president. A higher value of judge ideology indicates that the federal circuit court is more liberal. The distribution of judge ideology is illustrated using boxplots, which highlight the minimum values, 25th percentiles, 50th percentiles, 75th percentiles, and maximum values. The red dotted line displays the mean of judge ideology over time. Appendix A summarizes the variable construction in detail. For more information on the construction of judge ideology, see Huang et al. [2019].

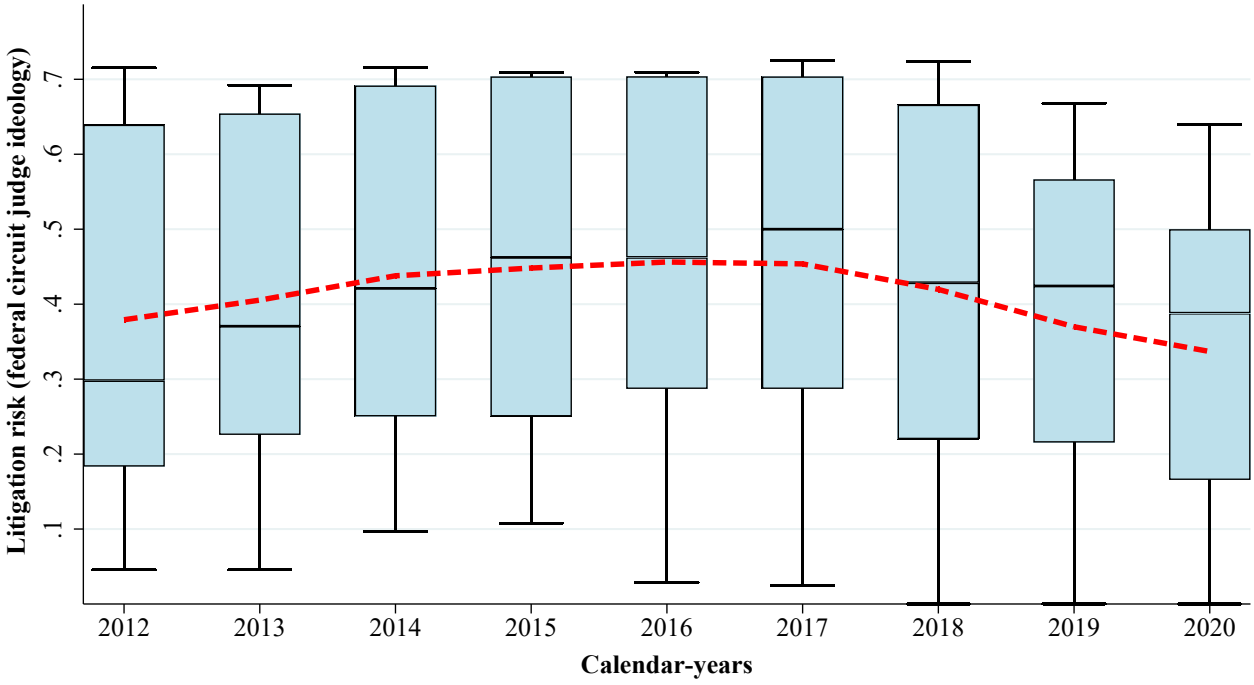

Table 1
Sample selection

Panel A: Sample selection of analysts' forecast revisions

	jiq
Analyst-firm-quarter observations from I/B/E/S unadjusted detail history from 2012Q1–2020Q4 using the following criteria: (1) Availability of prior- and current-quarter reporting dates across CRSP-Compustat Merged, I/B/E/S, and Estimize (2) Firm reports in US Dollars, reported earnings and estimates are adjusted for stock splits using split adjustment factors from CRSP (3) Analyst is identifiable with unique analyst code	1,006,763
Less: Analyst has no outstanding quarter q forecast over the $[t-1; t-120]$ window, where t is the day of quarter $q-1$ earnings release	-207,237
Less: Analyst has no outstanding quarter q forecast over the $[t+10; t-109]$ window, where t is the day of quarter $q-1$ earnings release	-4,643
I/B/E/S forecast revision sample	794,883
Less: Missing analyst controls	-78,997
Less: Missing benchmark sample forecast revisions	-562,677
Final sample of analysts' forecast revisions used in analyst-level tests	153,209

Panel B: Sample selection of the benchmark group's forecast revisions

	jiq
Benchmark forecaster-firm-quarter observations from Estimize from 2012Q1–2020Q4 using the following criteria: (1) Firm can be linked to common firm identifiers on CRSP, Compustat, and I/B/E/S (2) Availability of prior- and current-quarter reporting dates across CRSP-Compustat Merged, I/B/E/S, and Estimize (3) Reported earnings and estimates are adjusted for stock splits using split adjustment factors from Estimize	964,826
Less: Forecasts that are flagged by Estimize and considered unreliable	-11,478
Less: Data errors, i.e., forecasts recorded after the reporting date	-957
Less: Forecasts contributed by sell-side analysts or anonymous users	-145,565
Less: Forecaster has no outstanding quarter q forecast over the $[t-1; t-120]$ window, where t is the day of quarter $q-1$ earnings announcement	-746,762
Less: Forecaster has no outstanding quarter q forecast over the $[t+10; t-109]$ window, where t is the day of quarter $q-1$ earnings announcement	-2,011
Final sample of individual benchmark forecasters' forecast revisions	58,053

Panel C: Main sample selection

	iq	i
Unique firm-quarter observations obtained from CRSP-Compustat Merged for the period from 2012Q1–2020Q4	193,597	8,360
Less: Firms with missing key variables	-23,815	-968
Less: Firms not listed on NYSE, AMEX, or NASDAQ	-1,971	-9
Less: Firms with missing analyst forecast revisions	-53,725	-1,695
Less: Firms with missing benchmark group forecast revisions	-96,684	-3,684
Less: Firms with missing litigation risk proxy	-1,288	-151
Less: Firms with missing control variables	-1,230	-126
Final firm-quarter sample	14,884	1,720

Notes: Panel A presents the sample selection of I/B/E/S analysts' forecast revisions for a panel of analyst-firm-year-quarter observations (jiq). Panel B presents the sample selection of the benchmark group's forecast revisions for a panel of benchmark forecaster-firm-year-quarter observations (jiq). The benchmark group's earnings forecast data is obtained from Estimize, an online financial forecasting platform, dating back to 2012Q1. I identify precise earnings announcement date variables to construct analysts' and benchmark forecasters' earnings forecast revisions. To do so, I use the earnings announcement date when at least two of the following three data providers agree on the reporting date: CRSP-Compustat, Estimize, and I/B/E/S. For the 13 firm-quarter observations that have no overlapping reporting dates, I hand-collect the reporting dates from firms' press releases and websites. Panel C presents the sample selection of the final firm-quarter sample with fiscal quarters from 2012Q1–2020Q4.

Table 2
Descriptive statistics

Panel A: Descriptive statistics firm-quarter sample

	N	Mean	SD	Min	Median	Max
Key variables:						
$\Delta F^{Analyst}$	14,884	-0.0301	0.153	-0.79	0	0.47
$\Delta F^{Control}$	14,884	-0.0084	0.0556	-0.34	0	0.1567
Private_Revision	14,884	-0.0217	0.1531	-0.73	-0.0025	0.55
Firm_Private_Warn	14,884	0.5146	0.3244	0	0.5	1
Litigation_Risk	14,884	0.4566	0.2224	0	0.5	0.7249
Firm and market linear control variables:						
Public_Guidance	14,884	0.2512	0.4337	0	0	1
ES	14,884	0.0562	0.1718	-0.5576	0.0338	0.8592
Loss	14,884	0.2606	0.439	0	0	1
MBE	14,884	0.7526	0.4315	0	1	1
MV (in million USD)	14,884	25,334.75	54,150.03	122.7324	5,807.93	346,615.2
BM	14,884	0.3738	0.4146	-0.5242	0.2764	2.5126
Sales_Growth	14,884	0.0264	0.1842	-0.5684	0.0212	0.88
N	14,884	11.9594	7.2662	1	11	33
Inst_Hold	14,884	0.6089	0.3678	0	0.7614	1
Age	14,884	27.0278	16.2508	4	24	58
Industry_Ret	14,884	0.1006	0.2454	-0.5091	0.0978	0.9111
Market_Ret	14,884	0.1224	0.0802	-0.0972	0.1431	0.2875
Litigation_Industry	14,884	0.4507	0.4976	0	0	1
Blue_State	14,884	0.6416	0.4796	0	1	1
Other outcome and partitioning variables:						
Future_Loss	14,884	0.2655	0.4416	0	0	1
Future_ΔEarn	14,877	-1.6522	440.9179	-2,290	1	2,077
Future_ΔCFO	14,883	10.4709	751.1886	-3,730	2.51	3,726
R&D	8,869	205.2848	685.2934	-0.4	23.99	10,388
Competition (in %)	14,881	7.05	7.35	1.01	4.01	85.54

Panel B: Descriptive statistics for low- vs. high-litigation-risk sample

	Litgation_Risk >= .5		*Litgation_Risk < .5*			
	N	Mean	N	Mean	Mean diff.	t-stat.
Firm_Private_Warn	7,529	0.527	7,355	0.5018	0.0252*	1.80
Public_Guidance	7,529	0.2702	7,355	0.2318	0.0383	0.58
ES	7,529	0.06	7,355	0.0523	0.0077	1.43
Loss	7,529	0.281	7,355	0.2397	0.0413	0.89
MBE	7,529	0.7674	7,355	0.7373	0.0301	1.55
MV (in million USD)	7,529	30,118.06	7,355	20,438.27	9,679.79***	2.69
BM	7,529	0.3309	7,355	0.4177	-0.0868*	-1.96
Sales_Growth	7,529	0.0314	7,355	0.0214	0.0100**	2.16
N	7,529	12.2925	7,355	11.6185	0.674	0.95
Inst_Hold	7,529	0.6116	7,355	0.6062	0.0053	0.25
Age	7,529	24.057	7,355	30.0689	-6.0120***	-2.75
Industry_Ret	7,529	0.1156	7,355	0.0852	0.0303**	2.50
Litigation_Industry	7,529	0.5155	7,355	0.3844	0.131	1.60
Blue_State	7,529	0.851	7,355	0.4272	0.4238***	2.70

Panel C: Descriptive statistics analyst-level sample

	N	Mean	SD	Min	Median	Max
Key variables:						
Private_Warn	153,209	0.5162	0.4997	0	1	1
Strong_Buy	153,209	0.1931	0.3947	0	0	1
Buy	153,209	0.3438	0.4750	0	0	1
Sell	153,209	0.0090	0.0945	0	0	1
Strong_Sell	153,209	0.0506	0.2192	0	0	1
Analyst controls:						
Accuracy	153,209	0.1308	0.2117	0	0.06	1.4
Brokersize	153,209	100.3079	100.4347	2	55	327
Firm_Exp	153,209	5.8670	5.3370	0	4.3041	24.5699
Analyst_Exp	153,209	13.5076	8.6618	0.274	12.3781	33.9425
N_Firms	153,209	14.8464	8.0812	1	15	39
Reputation	153,209	0.3288	0.4698	0	0	1

Panel D: Sample distribution across industries

	N	%	Mean Y
Consumer Non-Durables	856	5.75	0.6209
Consumer Durables	358	2.41	0.5506
Manufacturing	1,229	8.26	0.5464
Oil, Gas, and Coal Extraction	814	5.47	0.421
Chemicals and Allied Products	226	1.52	0.5524
Business Equipment	4,885	32.82	0.5086
Telephone and Television Transmission	391	2.63	0.5226
Utilities	230	1.55	0.3971
Wholesale, Retail, and some Services	1,856	12.47	0.5269
Healthcare, Medical Equipment, and Drugs	1,364	9.16	0.4889
Finance	1,071	7.20	0.4937
Other	1,604	10.78	0.5223
Total	14,884	100	0.5147

Notes: Panel A presents descriptive statistics for the firm-quarter variables. Panel B presents descriptive statistics for the main variables used in the firm-quarter tests split into low- and high-litigation-risk values. The differences in the sample means are constructed and t-statistics are presented based on standard errors clustered at the state level. The *, **, *** reflect two-tailed significance levels at 0.1, 0.05, and 0.01, respectively. Panel C presents descriptive statistics for the variables used in the analyst-level test. For brevity, Panel C presents the dependent and analyst-level variables but excludes other control variables as displayed in Panel A. All continuous variables are winsorized at the 1st and 99th percentiles of their distributions. For presentation purposes, the raw versions of variables are displayed, such that ES is not scaled by stock price, *Future_ΔEarn* and *Future_ΔCFO* are not scaled by total assets, and MV, N, and *Age* are not log-transformed. Panel D presents the sample distribution across industries. Here, Y refers to *Firm_Private_Warn*. Appendix A summarizes the variable definitions in detail.

Table 3

Do analysts with preferred access to managers receive private earnings warnings?

	(1) *Private_Warn*	(2) *Private_Warn*
Strong_Buy	0.030***	0.031***
	(6.97)	(7.37)
Buy	0.024***	0.024***
	(7.36)	(7.19)
Sell	-0.055***	-0.059***
	(-3.81)	(-3.72)
Strong_Sell	-0.033***	-0.033***
	(-5.28)	(-4.85)
Accuracy	-0.049***	-0.029***
	(-7.00)	(-5.24)
Brokersize	0.033***	0.033***
	(3.94)	(3.85)
Firm_Exp	-0.002	-0.003
	(-0.32)	(-0.45)
Analyst_Exp	-0.000	-0.002
	(-0.04)	(-0.40)
N_Firms	0.028***	0.035***
	(4.50)	(5.67)
Reputation	-0.008	-0.008
	(-1.38)	(-1.37)
Public_Guidance		0.005
		(0.83)
ES		-0.110***
		(-14.61)
Loss		-0.023***
		(-5.32)
MBE		-0.043***
		(-7.81)
Inst_Hold		0.033***
		(3.52)
N		-0.009
		(-0.80)
Fixed effects:		
Firm-year-quarter	yes	–
Firm	–	yes
Year-quarter	–	yes
Observations	153,209	153,209
Adjusted R^2	32.8%	7.9%

Notes: This table presents the results of the relation between analyst access to managers and the probability that analysts receive private earnings warnings from managers estimated using OLS. The dependent variable is *Private_Warn* and the independent variables are *Strong_Buy*, *Buy*, *Sell*, and *Strong_Sell*. Appendix A summarizes the variable definitions in detail. All continuous variables are ranked to quarterly deciles and subsequently transformed to a [-0.5,0.5] scale. In column (1), I include firm-year-quarter fixed effects. In column (2), I add linear controls and include firm and year-quarter fixed effects. T-statistics are presented in parentheses below the coefficient estimates and based on standard errors clustered at the analyst level, and *, **, *** reflect two-tailed significance levels at 0.1, 0.05, and 0.01, respectively.

Table 4
The predictive ability of managers' private earnings warnings

	(1) *Future_Loss*	(2) *Future_ΔEarn*	(3) *Future_ΔCFO*
Firm_Private_Warn	0.060***	-0.006***	-0.007***
	(6.18)	(-8.54)	(-5.15)
Loss	0.506***	0.016***	0.005***
	(36.28)	(20.83)	(2.84)
MV	-0.191***	0.005***	0.002*
	(-13.24)	(7.04)	(1.88)
BM	0.029**	-0.004***	-0.003***
	(2.11)	(-5.94)	(-2.65)
Sales_Growth	0.020*	-0.005***	-0.007***
	(1.82)	(-5.72)	(-2.60)
Age	-0.095***	0.003***	-0.002**
	(-6.46)	(4.69)	(-2.12)
Fixed effects:			
Year-quarter	yes	yes	yes
Observations	14,884	14,877	14,883
Adjusted R^2	37%	7.1%	2.1%

Notes: This table presents the results of the relation between managers' propensity to leak bad news to analysts and future adverse performance estimated using a linear probability model in column (1) (estimated using OLS) and using OLS in columns (2) and (3). In column (1), future adverse performance is an indicator variable equal to 1 when a firm reports negative earnings at the quarter q earnings announcement (*Future_Loss*), and 0 otherwise. In column (2), future adverse performance is a continuous variable to account for firms' changes in quarterly earnings (*Future_ΔEarn*). In column (3), future adverse performance is a continuous variable to account for firms' changes in cash flows from operations (*Future_ΔCFO*). Appendix A summarizes the variable definitions in detail. All continuous variables are ranked to quarterly deciles and subsequently transformed to a [-0.5,0.5] scale. I include year-quarter fixed effects. T-statistics are presented in parentheses below the coefficient estimates and based on standard errors clustered at the firm level, and *, **, *** reflect two-tailed significance levels at 0.1, 0.05, and 0.01, respectively.

Table 5
Shareholder litigation risk and managers' private earnings warnings

Y=Firm_Private_Warn	(1) Basic design	(2) Main design	(3) Add firm fixed effects
Litigation_Risk	0.203***	0.153**	0.229***
	(5.83)	(2.66)	(4.68)
Public_Guidance		0.002	0.011
		(0.23)	(0.87)
ES		-0.106***	-0.143***
		(-7.18)	(-6.58)
Loss		-0.006	-0.022*
		(-1.11)	(-1.76)
MBE		-0.036***	-0.028**
		(-4.63)	(-2.26)
MV		-0.001	0.152***
		(-0.05)	(3.32)
MB		0.004	0.055***
		(0.28)	(2.73)
Sales_Growth		-0.023**	-0.003
		(-2.30)	(-0.25)
Inst_Hold		0.015	0.035*
		(1.51)	(1.79)
Industry_Ret		-0.003	0.000
		(-0.34)	(0.04)
Market_Ret		0.028*	-0.003
		(2.01)	(-0.24)
Litigation_Industry		-0.002	
		(-0.27)	
Blue_State		0.020*	-0.028
		(1.79)	(-1.14)
Fixed effects:			
Circuit	yes	yes	–
Year-quarter	–	yes	yes
Firm	–	–	yes
Oster test δ		*0.94*	*2.6*
Observations	14,884	14,884	14,884
Adjusted R^2	0.7%	4.6%	13.2%

Notes: This table presents the results of the relation between shareholder litigation risk and managers' propensity to leak bad news to analysts estimated using OLS. The dependent variable is *Firm_Private_Warn* and the independent variable is *Litigation_Risk*. In column (1), I include circuit fixed effects. In column (2), I include linear controls and circuit and year-quarter fixed effects. In column (3), I exchange circuit with firm fixed effects. To assess the sensitivity of my results to unobservable variables, I follow Oster [2019] and estimate the effect that unobservable variables would need to have to eliminate the association between *Litigation_Risk* and *Firm_Private_Warn*, with the design in column (1) as a baseline model and the design in column (2) as the main model. The δ estimates the importance of unobservable variables relative to observable variables that are required to overturn my results. To estimate δ, I set R_{max} equal to $1.3 \times \hat{R} = 0.059$ as suggested by Oster [2019]. I repeat the Oster test calculation from column (1) to column (3). Appendix A summarizes the variable definitions in detail. All continuous variables are ranked to quarterly deciles and subsequently transformed to a [-0.5,0.5] scale. T-statistics are presented in parentheses below the coefficient estimates and based on standard errors clustered at the state level, and *, **, *** reflect two-tailed significance levels at 0.1, 0.05, and 0.01, respectively.

Table 6
Alternative group of benchmark forecasters

Y=Firm_Private_Warn	(1) Main design	(2) Add firm fixed effects
Litigation_Risk	0.170*	0.288***
	(1.79)	(4.25)
Public_Guidance	0.009	0.013
	(1.01)	(0.96)
ES	-0.123***	-0.145***
	(-7.50)	(-5.99)
Loss	-0.011	-0.022
	(-1.41)	(-1.30)
MBE	-0.034***	-0.029*
	(-3.70)	(-1.85)
MV	-0.000	0.184***
	(-0.03)	(3.34)
MB	0.021	0.106***
	(1.05)	(3.83)
Sales_Growth	-0.037***	-0.009
	(-2.78)	(-0.80)
Inst_Hold	0.025*	0.026
	(1.98)	(1.24)
Industry_Ret	0.006	0.017
	(0.62)	(1.52)
Market_Ret	0.027	-0.013
	(1.47)	(-0.66)
Litigation_Industry	-0.005	
	(-0.38)	
Blue_State	0.022*	-0.042
	(1.78)	(-1.60)
Fixed effects:		
Circuit	yes	–
Year-quarter	yes	yes
Firm	–	yes
Observations	8,929	8,929
Adjusted R^2	5.8%	17.3%

Notes: This table presents the results of the relation between shareholder litigation risk and managers' propensity to leak bad news to analysts estimated using OLS. The dependent variable is *Firm_Private_Warn* and the independent variable is *Litigation_Risk*. To construct *Firm_Private_Warn*, I use an alternative benchmark group using only professional forecasters, namely, buy-side and independent analysts. Appendix A summarizes the variable definitions in detail. All continuous variables are ranked to quarterly deciles and subsequently transformed to a [-0.5,0.5] scale. I include circuit and year-quarter fixed effects in column (1) and firm and year-quarter fixed effects in column (2). T-statistics are presented in parentheses below the coefficient estimates and based on standard errors clustered at the state level, and *, **, *** reflect two-tailed significance levels at 0.1, 0.05, and 0.01, respectively.

Table 7

Heterogeneity in public disclosure costs and incentives

Panel A: Using variation in proprietary costs

Y=Firm_Private_Warn	(1)	(2)	(3)	(4)
	R&D		*Competition*	
	low	high	low	high
Litigation_Risk	0.023	0.199	0.035	0.290***
	(0.18)	(1.46)	(0.42)	(3.16)
Difference in coefficients:	0.176		0.255*	
	(0.89)		(2.01)	
Controls	yes	yes	yes	yes
Fixed effects:				
Circuit	yes	yes	yes	yes
Year-quarter	yes	yes	yes	yes
Observations	4,434	4,434	7,446	7,438
Adjusted R^2	5.6%	6%	5.2%	4.5%

Panel B: Using variation in the provision of public guidance

Y=Firm_Private_Warn	(1)	(2)	(3)	(4)
	Public_Guidance at $q-1$ EA		*Public_Guidance* during $q-1$	
	no	yes	no	yes
Litigation_Risk	0.184**	0.059	0.195**	0.051
	(2.18)	(0.41)	(2.21)	(0.38)
Difference in coefficients:	0.125		0.144	
	(0.65)		(0.75)	
Controls	yes	yes	yes	yes
Fixed effects:				
Circuit	yes	yes	yes	yes
Year-quarter	yes	yes	yes	yes
Observations	11,145	3,739	10,917	3,967
Adjusted R^2	4.6%	6.1%	4.6%	5.9%

Notes: This table presents the results of the relation between shareholder litigation risk and managers' propensity to leak bad news to analysts estimated using OLS. In Panel A, I estimate the relation for firms in the low- versus high-proprietary-costs partition using below- and above-median values of the partitioning variables *R&D* and *Competition*. In Panel B, I estimate the relation for firms that issue versus do not issue public guidance (*Public_Guidance*), where *Public_Guidance* accounts for the issuance of public guidance either during the days of the earnings announcement in columns (1) and (2) or during the quarter in columns (3) and (4). The dependent variable is *Firm_Private_Warn* and the independent variable is *Litigation_Risk*. Appendix A summarizes the variable definitions in detail. All continuous variables are ranked to quarterly deciles and subsequently transformed to a [-0.5,0.5] scale. I include circuit and year-quarter fixed effects. T-statistics are presented in parentheses below the coefficient estimates and based on standard errors clustered at the state level, and *, **, *** reflect two-tailed significance levels at 0.1, 0.05, and 0.01, respectively.

Internet Appendix

Why Disclose Privately? Shareholder Litigation Risk and Managers' Private Disclosure of Earnings Warnings

Sandra Schafhäutle

January 2023

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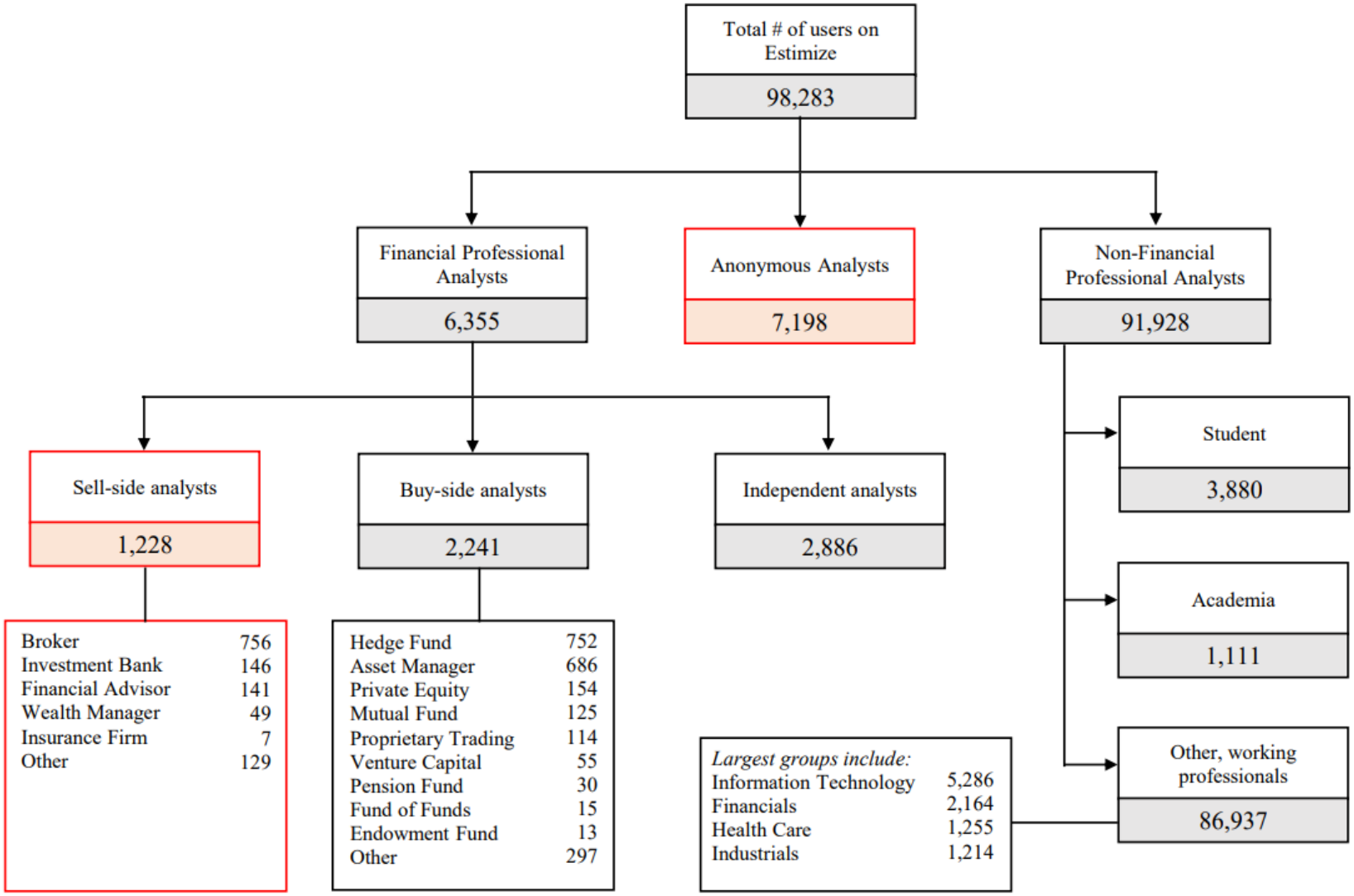
Figure IA.1
Overview of benchmark forecasters' background information



Notes: This figure provides an overview of the full sample of unique users, referred to as benchmark forecasters, in the Estimize sample over the period from 2012Q1 to 2020Q4. On Estimize, new users can identify themselves as professional or non-professional analysts and further specify their professional profiles. I refer to anonymous analysts when a user's profession is unknown, i.e., not disclosed on the website. To select my benchmark forecaster group, I eliminate anonymous and sell-side analysts, highlighted in red. I retain buy-side and independent analysts as well as non-professional analysts. In a robustness test, I also exclude non-professional forecasters. It is important to note that this is the full sample of benchmark forecasters before applying strict sample selection criteria to the data. The final group of benchmark forecasters used in this sample consists of 10.37% buy-side analysts, 11.74% independent analysts, and 77.89% non-professional forecasters (5,065 benchmark forecasters). Within the group of buy-side analysts, hedge fund (197) and asset manager (169) are the most frequently identified professions. Within the group of non-professional benchmark forecasters, most users are employed in Information Technology (1,035) and Finance (428).

Table IA.1
SEC enforcement actions

Date	Issuer	Individual	Proceeding	Claim	Enforcement
Nov/02	Raytheon Company	F. A. Caine (CFO)	administrative	Selectively disclosed material information to research analysts during private phone calls	$0
Nov/02	Secure Computing Corporation	J. McNulty (CEO)	administrative	Selectively disclosed material information to two institutional advisors	$0
Nov/02	Siebel Systems, Inc.	–	judicial (District of Columbia)	Selectively disclosed material information during a private technology conference	$250K
Nov/02	Motorola, Inc.	–	–	Selectively disclosed material information to analysts during private phone calls	$0 (sole report of investigation to remind firms of Reg FD)
Sep/03	Schering-Plough Corporation	R. J. Kogan (CEO)	administrative	Selectively disclosed material information to investors and research analysts during private events	$1M (firm), $50K (CEO)
Sep/04	Senetek PLC	–	administrative	Selectively corrected and updated the research report of analysts	$0
Mar/05	Flowserve Corporation	S. Greer (CEO)	judicial (District of Columbia)	Selectively disclosed material information to research analysts in private meetings	$350K (firm), $50K (CEO)

Sep/05	Siebel Systems, Inc.	K. Goldman (CFO), M. H. (Senior Vice President)	judicial (Southern District of New York)	Selectively disclosed material information to investors and research analysts during private events	$0 (dismissed by court)
Sep/07	Electronic Data Systems Corporation	–	administrative	Selectively disclosed material information to analysts	$0
Sep/09	–	C. A. Black (CFO, American Commerical Lines, Inc.)	administrative	Selectively disclosed private guidance to analysts via email	$25K
Mar/10	Presstek, Inc.	E. J. Marino (CEO)	administrative	Selectively disclosed material information to investors	$400K (firm), $50K (CEO)
Oct/10	Office Depot, Inc.	S. A. Odland (CEO), P. A. McKay (CFO)	administrative	Selectively disclosed private guidance to investors and research analysts during private events	$1M (firm), $50K (CEO and CFO)
Nov/11	Fifth Third Bancorp	–	administrative	Selectively disclosed private information to investors about redemption of securities	$0
Apr/13	Netflix, Inc.	R. Hastings (CEO)	administrative	Posting material information on Hastings' personal facebook webpage	$0 (SEC drops charges)
Sep/13	–	L. D. Polizzotto (head of investor relation, First Solar, Inc.)	administrative	Selectively disclosed material information to investors and research analysts	$50K
Sep/18	Tesla, Inc.	E. Musk (CEO)	judicial (Southern District of New York)	Posting material, misleading and false, information on Musk's twitter account	$20M (firm), $20M (CEO)

Aug/19	TherapeuticsMD	–	administrative	Selectively disclosed material information to research analysts in private messages	$200K
Mar/21	AT&T, Inc.	C. C. Womack; K. D. Evans; M. J. Black (investor relation)	judicial (Southern District of New York)	Selectively disclosed material information to research analysts during private phone calls	$6.25M (firm), $25K (investor relation)

Notes: This table provides an overview of SEC enforcement actions against firms ("issuer") and persons who act on behalf of the firm ("individual"), because of violations of Reg FD. The first column presents the date of the SEC's enforcement actions ("date"). The second and third columns report the issuer or individual against whom claims are made. The fourth column reports whether the case is proceeded in a federal court or in front of an administrative agency ("proceeding"). When the case is pursued in court, I report the federal district. The fifth column summarizes the SEC's main claim made against the issuer or individual ("claim") and the sixth column summarizes the SEC's enforcement actions. Even when the SEC does not require firms to pay a penalty, firms would receive a cease-and-desist order which requires the issuer or individual to immediately stop violating the securities law. Mostly, firms settle with the SEC directly and cases are not heard in front of a court. All information is hand-collected and extracted from publicly available sources.

Table IA.2
Alternative research design choices

Y=Firm_Private_Warn	(1)	(2)	(3)	(4)	(5)	(6)
	Fixed effects 1		Fixed effects 2		*X* is 0/1	
Litigation_Risk	0.176***	0.248***	0.157***	0.232***	0.032***	0.033***
	(3.00)	(5.06)	(2.98)	(4.26)	(4.12)	(3.06)
Controls	yes	yes	yes	yes	yes	yes
Fixed effects:						
Circuit	yes	–	–	–	yes	–
Firm	–	yes	–	yes	–	yes
Year-quarter	–	–	–	–	–	–
Year-month	yes	yes	–	–	yes	yes
Industry-circuit	–	–	yes	–	–	–
Industry-year-quarter	–	–	yes	yes	–	–
Clustering:						
State	yes	yes	yes	yes	yes	yes
Observations	14,884	14,884	14,884	14,884	14,884	14,884
Adjusted R^2	5%	13.6%	6.7%	13.8%	5%	13.6%

Notes: This table presents the results of the relation between shareholder litigation risk and managers' propensity to leak bad news to analysts estimated using OLS. In my main design in Table 5, I include circuit and year-quarter fixed effects. In an alternative design, I add firm fixed effects. To examine whether my main inferences are robust to alternative design choices, I use two alternative fixed effects designs. In columns (1) and (2), I exchange year-quarter fixed effects with year-month fixed effects. In columns (3) and (4), I exchange year-quarter fixed effects with industry-year-quarter fixed effects (and in column (3), I also exchange circuit fixed effects with industry-circuit fixed effects to produce a within-industry estimation). Finally, in columns (5) and (6), I exchange the continuous *X* variable (*Litigation_Risk*) by an indicator variable that is equal to 1 when firms operate in a high-litigation-risk circuit, and 0 otherwise. I also include year-month instead of year-quarter fixed effects in this design similar to columns (1) and (2). Appendix A summarizes the variable definitions in detail. All continuous variables are ranked to quarterly deciles and subsequently transformed to a [-0.5,0.5] scale. T-statistics are presented in parentheses below the coefficient estimates. Standard errors are clustered at the state level, and *, **, *** reflect two-tailed significance levels at 0.1, 0.05, and 0.01, respectively.

Table IA.3
Alternative standard error clustering choices

Y=Firm_Private_Warn	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Cluster 1		Cluster 2		Wild bootstrap 1		Wild bootstrap 2	
	Main design	Add firm fixed effects	Main design	Add firm fixed effects	Main design	Add firm fixed effects	Main design	Add firm fixed effects
Litigation_Risk	0.153**	0.229***	0.153**	0.229***	0.153**	0.229***	0.153**	0.229***
	(2.07)	(3.41)	(2.72)	(3.72)	(2.66)	(4.68)	(2.32)	(5.88)
Estimated p-values:					*(0.034)*	*(0.001)*	*(0.039)*	*(0.004)*
Controls	yes	yes	yes	yes	yes	yes	yes	yes
Fixed effects:								
Circuit	yes	yes	yes	yes	yes	yes	yes	yes
Year-quarter	yes	–	yes	–	yes	–	yes	–
Firm	–	yes	–	yes	–	yes	–	yes
Clustering:								
Firm	yes	yes	yes	yes	–	–	–	–
Year-quarter	–	–	yes	yes	–	–	yes	yes
State	–	–	–	–	yes	yes	–	–
Circuit	–	–	–	–	–	–	yes	yes
Observations	14,884	14,884	14,884	14,884	14,884	14,884	14,884	14,884
Adjusted R^2	4.6%	13.2%	4.5%	13.2%	4.6%	13.2%	4.6%	13.2%

Notes: This table presents the results of the relation between shareholder litigation risk and managers' propensity to leak bad news to analysts estimated using OLS. In my main specification in Table 5, I cluster standard errors at the state level (n *clusters* = 48). To examine whether my main inferences are robust to alternative clustering choices, I implement four alternative clustering choices. In columns (1) and (2), I cluster standard errors at the firm level (n *clusters* = 1,720). In columns (3) and (4), I two-way cluster standard errors at the firm and year-quarter levels (n *clusters* = 1,720 and n *clusters* = 36, respectively). In columns (5) and (6), I use wild bootstrapped standard errors based on the main model using state clustering, and in columns (7) and (8), based on an augmented model using two-way circuit and year-quarter clusters. I present the results both for the main design that includes circuit and year-quarter fixed effects and the design that adds firm fixed effects. The dependent variable is *Firm_Private_Warn* and the independent variable is *Litigation_Risk*. Appendix A summarizes the variable definitions in detail. All continuous variables are ranked to quarterly deciles and subsequently transformed to a [-0.5,0.5] scale. T-statistics are presented in parentheses below the coefficient estimates, and *, **, *** reflect two-tailed significance levels at 0.1, 0.05, and 0.01, respectively.

Table IA.4
Alternative specification: Accounting for predictable forecast pessimism

Y=Firm_Private_Warn	(1)	(2)	(3)	(4)	(5)	(6)
	Main design		Add firm fixed effects		Alternative fixed effects design	
Litigation_Risk	0.153**	0.154***	0.229***	0.227***	0.174***	0.202**
	(2.66)	(2.74)	(4.68)	(4.62)	(3.44)	(2.40)
Pessimism		0.041***		-0.002		
		(3.50)		(-0.09)		
Controls	yes	yes	yes	yes	yes	yes
Fixed effects:						
Circuit	yes	yes	–	–	–	–
Firm	–	–	yes	yes	–	–
Year-quarter	yes	yes	yes	yes	–	–
Circuit × Pessimism	–	–	–	–	yes	–
Firm × Pessimism	–	–	–	–	–	yes
Year-quarter × Pessimism	–	–	–	–	yes	yes
Clustering:						
State ($n = 48$)	yes	yes	yes	yes	yes	yes
Observations	14,884	14,707	14,884	14,707	14,707	14,707
Adjusted R^2	4.6%	4.8%	13.2%	13.3%	5.3%	13.9%

Notes: This table presents the results of the relation between shareholder litigation risk and managers' propensity to leak bad news to analysts estimated using OLS. Analysts have incentives to cater to managers with pessimistic earnings forecasts, and variation introduced by these incentives could be reflected in *Firm_Private_Warn*. If these incentives are affected by changes in shareholder litigation risk, then my research design would suffer from omitted variable bias. To assess the nature of this omitted variable bias in my research design, I include a decile-ranked measure of forecast pessimism. I construct *Pessimism* following Veenman and Verwijmeren [2018], using firm-level consensus and analyst-level forecast errors. Specifically, *Pessimism* is the sum of the relative frequency with which the firm beat (versus missed) consensus analyst forecasts in the past 12 quarters and the relative frequency with which any firm beat (versus missed) an individual analyst's forecasts in the past 12 months, averaged across the analysts forecasting earnings for the current firm-quarter. Column (1) presents the main design, and column (2) adds *Pessimism* as a linear control variable. In columns (3) and (4), I repeat this exercise with firm fixed effects. In columns (5) and (6), I interact *Pessimism* with circuit and firm fixed effects, respectively. I also interact *Pessimism* with year-quarter fixed effects. In this alternative fixed effects design, I estimate my coefficient by comparing similarly-pessimistic firms over time. These tests confirm my main analysis. Appendix A summarizes the variable definitions in detail. All continuous variables are ranked to quarterly deciles and subsequently transformed to a [-0.5,0.5] scale. T-statistics are presented in parentheses below the coefficient estimates. Standard errors are clustered at the state level, and *, **, *** reflect two-tailed significance levels at 0.1, 0.05, and 0.01, respectively.

Table IA.5
Alternative specification: Analyst-level design and relative groups

Y=Private_Warn	(1)	(2)	(3)	(4)	(5)	(6)
	Benchmark coefficients in analyst-level design		Analyst fixed effects design using similarly skilled analysts		Analyst fixed effects design using similarly experienced analysts	
Litigation_Risk	0.153***	0.229***	0.121**	0.211***	0.119**	0.213***
	(2.69)	(4.85)	(2.27)	(4.70)	(2.13)	(4.67)
Firm controls	yes	yes	yes	yes	yes	yes
Fixed effects:						
Circuit	yes	–	–	–	–	–
Firm	–	yes	–	–	–	–
Year-quarter	yes	yes	–	–	–	–
Circuit × Analyst	–	–	yes	–	yes	–
Firm × Analyst	–	–	–	yes	–	yes
Year-quarter × Accuracy	–	–	yes	yes	–	–
Year-quarter × Experience	–	–	–	–	yes	yes
Clustering:						
State ($n = 48$)	yes	yes	yes	yes	yes	yes
Analyst ($n = 3,583$)	yes	yes	yes	yes	yes	yes
Observations	178,054	178,054	178,039	178,039	178,041	178,041
Adjusted R^2	2%	9%	8.6%	11.2%	8.6%	11.2%

Notes: This table presents the results of the relation between shareholder litigation risk and managers' propensity to leak bad news to analysts estimated using OLS. Unlike Table 5, I estimate the relation of interest at the *analyst*-firm-year-quarter level, using equal weights for each firm-year-quarter in the sample. Columns (1) and (2) reconstruct the coefficient of interest, using the main design (circuit and year-quarter fixed effects) and firm fixed effects design (firm and year-quarter fixed effects), respectively. The estimated coefficients in columns (1) and (2) present the benchmark coefficients for the subsequent columns (3) through (6). In columns (3) through (6), I use this analyst-level design and construct more granular relative groups. Particularly, in columns (3) and (4), I compare a sell-side analyst to similarly-skilled analysts; and in columns (5) and (6), I compare a sell-side analyst to similarly-experienced analysts. To do so, I define a fixed analyst-level characteristic, using each analyst's mean of *Accuracy* and *Experience*, respectively, and subsequently splitting the analyst-level sample means at the median value. Note that limited conditional support within analyst-year-quarter inhibits the reliable estimation of a full within-analyst design (i.e., circuit × analyst and year-quarter × analyst fixed effects). Appendix A summarizes the variable definitions in detail. All continuous variables are ranked to quarterly deciles and subsequently transformed to a [-0.5,0.5] scale. T-statistics are presented in parentheses below the coefficient estimates. Standard errors are two-way clustered at the state and analyst levels, and *, **, *** reflect two-tailed significance levels at 0.1, 0.05, and 0.01, respectively.

Table IA.6

Validation tests: Alternative group of benchmark forecasters

	(1)	(2)	(3)	(4)	(5)
	Table 3		Table 4		
	Y=Private_Warn		*Future_Loss*	*Future_ΔEarn*	*Future_ΔCFO*
Strong_Buy	0.033***	0.034***			
	(6.79)	(6.87)			
Buy	0.025***	0.024***			
	(6.40)	(5.88)			
Sell	-0.080***	-0.081***			
	(-4.93)	(-4.71)			
Strong_Sell	-0.032***	-0.032***			
	(-4.45)	(-3.88)			
Firm_Private_Warn			0.044***	-0.005***	-0.005***
			(3.65)	(-4.77)	(-3.02)
Controls	yes	yes	yes	yes	yes
Fixed effects:					
Firm-year-quarter	yes	–	–	–	–
Firm	–	yes	–	–	–
Year-quarter	–	yes	yes	yes	yes
Clustering:					
Analyst ($n = 3,000$)	yes	yes	–	–	–
Firm ($n = 1,360$)	–	–	yes	yes	yes
Observations	104,279	104,279	8,929	8,927	8,929
Adjusted R^2	33%	9.8%	37%	7.1%	1.9%

Notes: In this table, I redo the analysis of Tables 3 and 4 using an alternative group of benchmark forecasters in the construction of my main dependent variable. Particularly, to construct *Firm_Private_Warn*, I use only professional forecasters, namely, buy-side and independent analysts. Appendix A summarizes the variable definitions in detail. All continuous variables are ranked to quarterly deciles and subsequently transformed to a [-0.5,0.5] scale. T-statistics are presented in parentheses below the coefficient estimates. Standard errors are clustered at the analyst level in columns (1) and (2) or at the firm level in columns (3) through (5); and *, **, *** reflect two-tailed significance levels at 0.1, 0.05, and 0.01, respectively.