Journal of Accounting and Economics 61 (2016) 391–413

Contents lists available at ScienceDirect

Journal of Accounting and Economics

journal homepage: www.elsevier.com/locate/jae

# The informational feedback effect of stock prices on management forecasts ☆

Luo Zuo

*Samuel Curtis Johnson Graduate School of Management, Cornell University, USA*

A R T I C L E I N F O

*Article history:*
Received 29 November 2014
Received in revised form
9 March 2016
Accepted 10 March 2016
Available online 17 March 2016

*JEL classification:*
G10
G14
G30
G31
M41

*Keywords:*
Learning from prices
Private information
Probability of informed trading
Mutual fund redemptions
Management forecasts
Corporate disclosure

A B S T R A C T

Using management earnings forecasts over the period 1996–2010, I find that the sensitivity of forecast revisions to contemporaneous stock returns is increasing in the amount of investors' private information in prices. This effect remains after controlling for various confounds and is robust to the use of mutual fund redemptions as a shock to price changes that is exogenous to fundamental news. Furthermore, investors' private information helps managers improve their forecast accuracy. Together, these findings suggest that stock prices contain information that managers do not otherwise have regarding firms' fundamentals, and that managers incorporate this information in their earnings forecasts.



## 1. Introduction

Management forecasts are a key voluntary disclosure mechanism (see Beyer et al. (2010) for a recent review). Prior research presents evidence that managers disclose earnings forecasts to provide additional information and guidance to the market and the market significantly reacts to them (e.g., Patell, 1976; Jennings, 1987; Anilowski et al., 2007; Rogers et al.,

☆ This paper is based on my dissertation at the Massachusetts Institute of Technology. I am thankful to John Core (dissertation co-chair), Michelle Hanlon, Antoinette Schoar, Rodrigo Verdi, and Ross Watts (dissertation co-chair) for their guidance and encouragement. I am indebted to Joanna Wu (editor) and Dawn Matsumoto (reviewer) for comments and suggestions that substantially improved the paper. I also appreciate the helpful comments of Sanjeev Bhojraj, Robert Bloomfield, Brian Bushee, Qi Chen, Patricia Dechow, Ilia Dichev, Richard Frankel, Pingyang Gao, Itay Goldstein, Wayne Guay, Yaniv Konchitchki, S.P. Kothari, Charles Lee, Robert Libby, Joshua Madsen, Gregory Miller, Jeffrey Ng, Christopher Noe, Shivaram Rajgopal, Sugata Roychowdhury, Catherine Schrand, Nemit Shroff, Ewa Sletten, Eric So, Phillip Stocken, Daniel Taylor, Joseph Weber, Eric Yeung, Liandong Zhang and seminar participants at Boston College, Columbia University, Cornell University, Dartmouth College, Emory University, Harvard University, the Massachusetts Institute of Technology, Northwestern University, Stanford University, the University of California – Berkeley, the University of Pennsylvania, Washington University in St. Louis and the 2013 American Accounting Association Annual Meeting. I gratefully acknowledge financial support from Cornell University, the MIT Sloan School of Management and the Deloitte Foundation.

*E-mail address:* luozuo@cornell.edu

http://dx.doi.org/10.1016/j.jacceco.2016.03.001
0165-4101/

2009). This stream of literature primarily views information flows between firms and capital markets as one way – from firms to capital markets. In this paper, I investigate the effect of price information on management forecasts and provide evidence on whether information flows also occur from capital markets to firms. My study builds on a growing literature in financial economics that financial markets can affect the real economy through the managerial learning channel (see Bond et al. (2012) for a review). The idea that market prices are a useful source of information goes back to Hayek (1945). In essence, stock prices aggregate diverse pieces of information from different traders who have no other means of communicating with managers outside the trading process (e.g., Grossman and Stiglitz, 1980; Glosten and Milgrom, 1985; Kyle, 1985). As a result, stock prices can reveal traders' private information that is otherwise not available to managers, and hence affect managers' beliefs about their own firms' fundamentals.

I hypothesize that managers learn from investor information in stock prices when revising annual earnings forecasts.[1] To test this hypothesis, it is not sufficient to simply regress forecast revisions on stock returns over the revision period for at least two reasons. First, a positive relation between contemporaneous stock returns and forecast revisions does not imply causality from the former to the latter: it could arise from correlated information channels that affect both or from reverse causality. Second, even assuming a causal explanation, stock price changes may lead to earnings forecast revisions due to reasons unrelated to learning, such as managers catering to investor sentiment (Lee and So, 2015). One approach used in the literature to address these concerns is to conduct analyses showing that the effect is greater in exactly those firms where theory would predict that the learning channel is likely to be stronger. For example, Chen et al. (2007) show that the correlation between prices and investment is stronger when stock prices contain more information not available to managers and argue that this evidence is consistent with the managerial learning hypothesis.

I follow the empirical methodology of Chen et al. (2007) and examine whether the amount of private information in prices has a positive effect on the association between forecast revisions and stock returns over the revision period. The logic of this approach is as follows. If, at a given point in time, managers forecast future earnings, they will use all the information available to them at that point. This set includes the information aggregated in stock prices, as well as managers' private information that has not found its way to prices yet. In this environment, forecast revisions will be more sensitive to price movements when stock prices contain more information that is new to managers. Noise or information that managers already had will move prices but not affect the revised forecast and thus will decrease the sensitivity of forecast revisions to price changes.

To construct the sample, I start with all management annual earnings forecasts issued between January 1996 and December 2010. I identify 15,977 management forecast revisions over this period. To measure the extent of informed trading and thus the amount of investors' private information in stock prices, I follow Chen et al. (2007) and use a modified version of the probability of privately informed trading. This measure starts with the probability of informed trading (*PIN*), estimated following Easley et al. (2002), and is adjusted for insider trading to remove the effect of managers' private information. It is derived from a structural market microstructure model, in which trades come from either noise traders or informed traders. It directly measures the probability of informed trading by outsiders, and thus captures the amount of investors' private information in stock prices.

I first show that the sensitivity of management forecast revisions to stock returns over the revision period is increasing in the amount of private information in stock prices. This result is consistent with the hypothesis that stock prices with a larger amount of private information provide managers with more new information about their own firms' fundamentals, which, in turn, affects their earnings forecasts. In the test, I include several design features to alleviate the concern related to the delay in management forecasting (i.e., public information is impounded into stock prices immediately whereas it is incorporated into forecast revisions with a delay due to the relatively low frequency of management forecasts). First, I directly control for public information about future earnings (i.e., common signals that affect both forecast revisions and stock returns) using two proxies: contemporaneous consensus analyst forecast revisions and the sum of management's forecast error associated with any quarterly earnings announcement that occurs between the two forecast dates. Second, I control for the effect of the time lag (between the two forecasts) on the revision–return relation because this lag increases the probability that the correlation between stock returns and management forecast revisions is simply due to managers' delay in revising their forecasts. Third, I control for a number of firm-specific and forecast-specific characteristics that may be correlated with my measure of private information and affect the revision–return relation (e.g., the richness of the information environment).

Next, I conduct a test using mutual fund redemptions as a shock to price changes that is exogenous to fundamental news (Edmans et al., 2012) and therefore should result in a lower revision–return sensitivity. The evidence supports this prediction. These first two tests provide evidence that investors' private information affects management forecasts. To provide more direct support on the decision usefulness of investor information, I investigate the effect of price information on management forecast accuracy. I find that the improvement in forecast accuracy is positively related to the magnitude of contemporaneous stock returns; and, more importantly, this positive relation is stronger when the amount of private information in prices is higher, suggesting that investors' private information is new to managers and helps managers

[1] Throughout the paper, I use the wording "investor information" or "price information" to mean investors' private information that is otherwise not available to managers except through the trading process.

improve their earnings forecast accuracy. Together, these findings lend support to the managerial learning hypothesis and the existence of information flows from capital markets to firms.

In testing the managerial learning hypothesis, prior studies usually abstract away from managers' strategic behavior (e.g., Chen et al., 2007; Foucault and Fresard, 2014). While it is unclear under which setting (i.e., investment or forecast) managers' strategic bias is more severe, here I discuss some potential concerns in the management forecast setting. Because management forecasts are voluntary, managers might strategically withhold disclosures (e.g., Bergman and Roychowdhury, 2008; Kothari et al., 2009; Sletten, 2012) or strategically bias disclosures (e.g., Matsumoto, 2002; Rogers and Stocken, 2005; Ajinkya et al., 2005; Cotter et al., 2006). For example, Rogers and Stocken (2005) show that managers are more likely to bias their forecasts when their ability to do so undetected is higher. Ajinkya et al. (2005) document that management forecasts are more optimistically biased in firms with weaker corporate governance (proxied by fewer outside directors or lower institutional ownership). These results suggest that managers may be strategically revising their forecasts rather than truthfully disclosing their beliefs.

To alleviate concerns about managers' strategic decision to issue a forecast, I repeat my analysis for a sample of forecasters that appear to have a forecasting policy (i.e., to consistently forecast each quarter with the earnings announcement). These firms are unlikely to strategically withhold forecasts because their disclosure policies are set and costly to deviate from. My inferences are unchanged when I repeat the tests for this subsample of regular forecasters.

The possibility that some managers have incentives to distort their forecasts will be of great concern to my analysis if the likelihood that managers bias their forecasts is negatively related to the extent of privately informed trading. Under that scenario, managers are more likely to reveal their true information (i.e., less likely to bias their forecasts) when there is more privately informed trading, and the positive effect of investor information on the revision–return relation could be partly driven by managers being disciplined by investors (rather than learning from investors). However, this is unlikely to be the case. Prior literature suggests that more opaque firms attract more informed trading because opacity increases the profitability of private information acquisition (Verrecchia, 1982; Diamond, 1985; Brown and Hillegeist, 2007; Maffett, 2012; Gao and Liang, 2013).[2] I find a similar pattern in my sample: smaller firms with less analyst following, less accurate analyst forecasts and lower institutional ownership experience more privately informed trading.[3] If anything, managers in those firms (that experience more informed trading) are more likely to bias their forecasts because their ability to do so undetected and undisciplined is higher with more opaque information environments (Rogers and Stocken, 2005) and weaker corporate governance (Ajinkya et al., 2005). Therefore, this potential bias likely weakens the power of my tests and works against finding a managerial learning effect.[4]

My study is mainly related to two strands of literature. First, it contributes to a growing literature on how financial reporting and disclosure are shaped by feedback from capital markets (e.g., Badertscher, 2011; Sletten, 2012; Li and Zhang, 2015). In particular, it provides evidence in support of information flows from capital markets to firms in the context of corporate disclosure. An implicit assumption in most prior disclosure research is that managerial information completely subsumes that of outside investors.[5] Dye and Sridhar (2002) note that the current disclosure literature fails to recognize that information flows between capital markets and firms need not be just from firms to capital markets (as recognized in prior research), but also be from capital markets to firms. My study highlights the *two-way* information flows between firms and capital markets. It is important to note that by primarily focusing on the information flows from capital markets to firms in this study, I do not deny the importance of the other direction of information flows (i.e., from firms to capital markets). My evidence does not imply that information flows from firms to capital markets are not important. Rather, it suggests that conditional on issuing management forecasts (to provide managerial information to the market), managers take into account the information in stock prices and incorporate that into their forecasts.

Second, my study contributes to the long-standing and important debate on whether financial markets affect the real economy through the managerial learning channel. The well-known study of Morck et al. (1990, p. 198) argues that "the explanatory power of relative stock returns for investment is unlikely to be evidence that the stock market provides new information to managers, since managers probably learn little from the market about their own firms' idiosyncratic prospects." Thus, the authors conclude that the stock market is somewhat of a sideshow. In contrast, recent evidence suggests that stock prices influence investment decisions in the real economy. These studies document the effect of price information on managers' observable actions, as manifested by, for example, higher investment sensitivity to stock prices (Chen et al., 2007), and argue that the stock market affects corporate investment decisions because managers learn from the private information in stock prices about their own firms' fundamentals. I study the effect of price information on managers' beliefs

[2] Two related theory papers (McNichols and Trueman, 1994; Chen et al., 2014) reach different conclusions (with different settings).

[3] The negative correlation between the extent of privately informed trading and the *level* of institutional ownership does not suggest that informed trading measured by *PIN* is driven by non-institutional investors. Maffett (2012) documents a similar negative relation between the extent of privately informed trading *by institutional investors* and the level of institutional ownership.

[4] It is also important to note that prior empirical evidence suggests that *on average* management forecasts are credible even though these disclosures are voluntary and forward-looking in nature (Healy and Palepu, 2001). Managers have incentives to provide accurate forecasts because earnings forecast errors can impose severe legal costs on them (Francis et al., 1994; Kasznik 1999) and inaccurate forecasts can also result in a loss of reputation, thereby lowering stock prices and managerial compensation (Trueman, 1986; Kasznik, 1999; Stocken, 2000).

[5] Two papers are notable exceptions: McNichols (1989) and Hutton et al. (2012). However, neither paper examines managers' learning behavior.

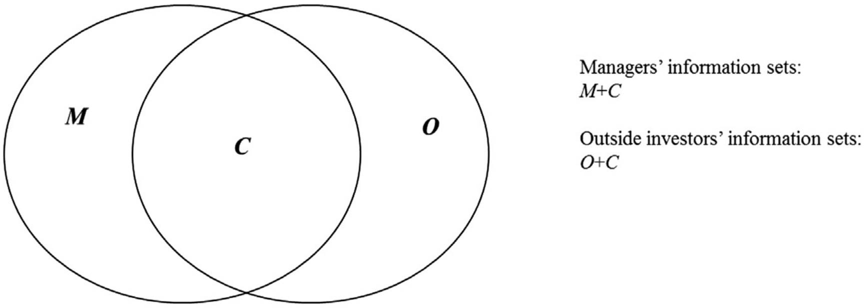


**Fig. 1.** The information sets of managers and outside investors. Fig. 1 provides an illustration of the information sets of managers and outside investors. Managers' information sets contain their private information, *M*, and common information, *C*, while outside investors' information sets include their private information, *O*, and common information, *C*.

about their own firms' fundamentals, as proxied by their earnings forecasts. My evidence corroborates those prior studies and provides a new angle on the managerial learning channel through which the stock market affects the real economy.

As with prior studies (Chen et al., 2007; Foucault and Fresard, 2014), I do not claim that my empirical methodology based on cross-sectional predictions can unambiguously identify a *causal* effect of stock prices on management earnings forecasts. The inferences in the paper hinge critically on my measure of private information in prices and whether this measure captures the cross-sectional variation in the amount of investors' private information that is in prices. Admittedly, possible omitted variable bias and measurement error might interfere with the interpretation of the results. However, I believe that the extensive set of tests conducted in the paper mitigates this concern to a large extent.

The remainder of the paper is organized as follows. Section 2 reviews related literature on the managerial learning hypothesis. Section 3 describes the data, research design and the construction of the main variables. Section 4 presents the main empirical results. Section 5 provides some additional analyses. Section 6 concludes and discusses some avenues for future research.

## 2. Related literature

A key building block of the managerial learning hypothesis is that markets could produce information new to managers. The idea that markets may have an advantage in producing some types of information goes back to Hayek (1945). In essence, markets aggregate many small pieces of dispersed information from numerous different participants, who have no other means of communicating with firm managers outside the trading process (e.g., Dow and Gorton, 1997; Subrahmanyam and Titman, 1999). The presumption needed for managerial learning is not that managers are less informed overall than investors are, but only that managers do not have perfect information about every decision-relevant factor, and so investors collectively may possess some information that managers do not have. This information more likely concerns external environments, such as macroeconomic conditions, industry competition or consumer demand. Moreover, even for some information that exists within a firm's scope, corporate bureaucracy can hinder its collection, if the information is difficult to standardize or to interpret or is incentive incompatible with the information possessors (e.g., Rajan and Zingales, 2003). Trading in the stock market elicits this information from profit-driven traders.

Fig. 1 provides an illustration: managers and outside investors share a common information set *C*. Managers have private information *M* that is not held by outside investors, and outside investors have private information *O* that is not held by managers. It could be the case that *M* is much larger than *O*, i.e., that managers have a significant information advantage over investors. However, as long as *O* is nonempty, managers can learn something from stock prices if, through trading, investors' information *O* is incorporated into stock prices.[6] The managerial learning channel is described vividly in the words of Hayek (1945, pp. 524–525): "the "man on the spot" cannot decide solely on the basis of his limited but intimate knowledge of the facts of his immediate surroundings. There still remains the problem of communicating to him such further information as he needs to fit his decisions into the whole pattern of changes of the larger economic system… this problem can be solved, and in fact is being solved, by the price system."

The markets' information production ability has been documented in prior studies. For example, Roll (1984) shows that citrus futures markets improve weather forecasting relative to traditional meteorological forecasts, and Wolfers and Zitzewitz (2004) show that prediction markets provide better forecasts of election outcomes than polls and other devices. Anecdotal evidence also suggests that managers generally view market information as valuable. David Allen, Managing Director (2003–2008) and Chief of Staff (2000–2008) of British Petroleum, stated: "I have deep faith in markets and a huge

[6] Here "investors" refer to investors as a group, not individual investors. Information asymmetry among investors (i.e., informed versus uninformed with respect to a particular piece of information) exists. A particular investor may have some piece of information (i.e., informed) or no information at all (i.e., uninformed). Together as group, they possess information *C* (also held by managers) and *O* (not held by managers).

respect for them. Within the company you have, at least in theory, access to all the information, but there is only you. Outside you have imperfect information but a lot of brains. If you accept these two different realities and use that creatively, you can learn a lot" (Miller et al., 2006, p. 5). Fergus MacLeod, an award winning sell-side analyst, was appointed the head of BP Investor Relations for his ability "to speak the market's language and translate its views back to the company" (Miller et al., 2006, p. 3).

However, Miller et al. (2006) also note in their case study that some managers argue that the market's data could not offer much insight as it is less complete than a firm's internal information, particularly about detailed operations or short term plans. Relatedly, Roll (1986) argues that managers view their proprietary information as superior to the aggregate public information set when bidding to acquire other firms. Simon (1973, p. 270) argues that "the scarce resource is not information; it is processing capacity to attend to information." Shroff (2014) provides evidence supporting that managers have limited attention and do not fully process all the information available within the firm. Thus, it remains an empirical question as to whether investors possess some information that managers lack and whether managers are able and willing to extract this information from prices.

A growing number of studies provide empirical evidence consistent with the managerial learning hypothesis (e.g., Luo, 2005; Chen et al., 2007; Bakke and Whited, 2010; Foucault and Fresard, 2012, 2014; Loureiro and Taboada, 2015; Edmans and Jayaraman, 2016). Most empirical studies document the effect of price information on firm investment, and use firms' investment outcomes to draw inferences on managerial learning from the stock market. For example, Chen et al. (2007) show that the investment sensitivity to Tobin's Q is stronger when there is more private information injected into prices through the trading process and argue that this evidence is consistent with the managerial learning channel. I apply the empirical methodology of Chen et al. (2007) to the management forecast setting and examine whether the amount of private information in prices has a positive effect on the association between forecast revisions and stock returns over the revision period.

Compared with the investment setting, the management forecast setting offers a new insight into unobservable managerial beliefs about firm fundamentals. Even though publicly disclosed earnings forecasts are still proxies for managers' internal forecasts of project payoffs, the link between these two types of forecasts is likely to be strong. Hemmer and Labro (2008) show analytically that the decision usefulness of information reported externally is inherently tied to the quality of information for internal decision-making. Dichev et al. (2013) conduct a large survey and a dozen interviews with top financial executives, primarily Chief Financial Officers (CFOs), and find that there is "a tight link between internal and external reporting" (p. 10) and that CFOs embrace the idea of "one number," that is, "a single earnings metric that shapes both their interactions with external stakeholders and internal decision-making" (p. 1). Several recent empirical studies make inferences about unobservable managerial information based on observable external information (e.g., Goodman et al., 2014; Gallemore and Labro, 2015; Heitzman and Huang, 2016). Therefore, the management forecast setting provides a new and arguably more direct test of whether managers glean information from prices, and complements prior studies based on firms' investment outcomes.

An additional advantage of looking at management forecasts is that the quality of earnings forecasts can be more objectively assessed than the quality of capital investment. The ex post comparison of earnings forecasts with actual earnings allows me to test not only whether investors' private information affects management forecasts, but also whether in equilibrium this information improves management forecast accuracy. Evidence of such a link can provide more direct support on the decision usefulness of investor information.

## 3. Sample selection, research design and variable construction

### 3.1. Sample selection

I use the "Company Issued Guidance" (CIG) database maintained by Thomson First Call to obtain all management annual earnings forecasts issued between January 1996 and December 2010.[7] I only include point and range forecasts, and exclude one-sided directional forecasts and qualitative forecasts that are not specific enough to determine numerical values as well as earnings pre-announcements. To determine a numeric value for each forecast, I use the value of the point forecasts and the midpoint of the range forecasts.[8] I identify forecast revisions when a firm issues more than one forecast for a given annual earnings realization and the new forecast is not simply a reiteration of the old one.[9]

For each forecast revision identified above, I obtain related stock price and return data from the Center for Research in Security Prices (CRSP), financial statement data from Compustat, intraday transaction data from Trade and Quote (TAQ), analysts' forecast and actual earnings per share data from Institutional Brokers' Estimate System (I/B/E/S), and data on

[7] The potential incompleteness of the CIG database (Chuk et al., 2013) is not of first order concern in my analysis since I examine whether managers incorporate investors' private information into forecasts once issuance decisions have been made. In addition, the managerial learning hypothesis does not hinge on whether these disclosures are voluntary or mandatory because of materiality concerns (Heitzman et al., 2010).

[8] I use the low (high) end of the range forecasts in cases where the CIG code indicates that EPS would be at the low (high) end of the range.

[9] The results are largely unchanged when I include forecast reiterations in the tests.

**Table 1**
Number of management forecast revisions and firms by year.

| Year | Number of forecast revisions | Number of unique firms |
|---|---|---|
| 1996 | 31 | 25 |
| 1997 | 52 | 43 |
| 1998 | 115 | 78 |
| 1999 | 174 | 124 |
| 2000 | 234 | 161 |
| 2001 | 871 | 515 |
| 2002 | 1260 | 665 |
| 2003 | 1399 | 732 |
| 2004 | 1879 | 888 |
| 2005 | 1924 | 882 |
| 2006 | 2013 | 919 |
| 2007 | 1740 | 844 |
| 2008 | 1600 | 753 |
| 2009 | 1273 | 604 |
| 2010 | 1412 | 633 |
| Total | 15,977 | 2116 |

This table presents the number of management forecast revisions and firms by calendar year (based on the date of the revised forecast). The sample period is from 1996 to 2010. Note that the initial forecast and the revised forecast do not have to be issued in the same calendar or fiscal year.

insider trading and institutional ownership from Thomson Reuters. The final sample contains 15,977 management forecast revisions (with non-missing variables) for 2116 firms over the period 1996 to 2010.[10]

Table 1 presents the number of management forecast revisions and firms by calendar year (based on the date of the revised forecast). Note that the initial forecast and the revised forecast do not have to be issued in the same calendar or fiscal year. The number of management forecast revisions varies substantially from year to year, ranging from 31 forecast revisions in 1996 to 2013 revisions in 2006. There are fewer observations in the earlier years (1996 and 1997) because First Call began compiling management forecast data more systematically in 1998 (Anilowski et al., 2007). The large increase in the number of management forecast revisions in 2001 is due to the passage of Regulation Fair Disclosure (Reg FD).[11]

### 3.2. Measure of investor information

In equilibrium, different stocks may have different amounts of investor information in their prices due to different benefits and costs of information production (Grossman and Stiglitz, 1980; Bloomfield, 2002). While it is difficult to directly measure the amount of investor information in prices that is new to managers, prior studies (e.g., Chen et al., 2007) suggest that the extent of informed trading in the stock market is positively associated with the amount of this information. Following this logic, I use a modified version of the probability of privately informed trading to proxy for the amount of investor information in prices. I construct this measure (*INFO*) in two steps. First, I estimate the probability of informed trading (*PIN*) following Easley et al. (2002). Second, I adjust it for insider trading following Chen et al. (2007).

The *PIN* measure is based on a structural market microstructure model, in which trades come from either noise traders or informed traders (Easley et al., 1996, 1997a, 1997b). It directly measures the probability of informed trading and thus captures the amount of investor information in stock prices. The trading process is modeled in the following way. The daily arrival rates of noise traders that submit buy and sell orders are $\varepsilon_b$ and $\varepsilon_s$, respectively. The probability that some traders acquire new (private) information about the fundamental value of the firm is $\alpha$. Given an information event occurs, the arrival rate of informed traders is $\mu$. Then, the probability of informed trading (*PIN*) in a given stock for a given period will be $\alpha\mu/(\alpha\mu+\varepsilon_b+\varepsilon_s)$. *PIN* should be low for stocks with little fluctuation in their daily buy and sell orders, which are more likely to arise from liquidity or noise trading. Likewise, *PIN* should be high for stocks that display frequent large deviations from their normal order flows. Consistent with the theoretical prediction of Easley and O'Hara (2004), Easley et al. (2002) find that stocks with high *PIN* earn higher returns that compensate investors for the high risk of private information.[12]

[10] I also obtain equity offering data from the Securities Data Company (SDC) and exclude observations associated with an equity offering (announcement or issuance) over the forecast revision period (494 observations). This restriction ensures that stock price changes over the forecast revision period are driven by trading conducted between investors. All results are virtually unchanged when I include these 494 observations in the final sample. Quarterly earnings forecast revisions happen much less often. Following the same procedure, I identify 2947 such revisions between 1996 and 2010. All results are similar when I include this set of revisions in the tests.

[11] All inferences are unchanged when I restrict my sample to the period post Reg FD. It is also interesting to note that forecast revisions happen most often in October and July (2925 and 2652 revisions respectively) and least often in March, June and December (503, 509 and 583 revisions respectively). However, there is no discernible difference in the distribution of forecast revisions across *INFO* decile for any calendar month (based on the date of the revised forecast) or fiscal year-end month.

[12] The pricing effect of *PIN* is not without controversy. See, for example, Duarte and Young (2009), Armstrong et al. (2011), Akins et al. (2012), and Lai et al. (2014).

To remove the effect of managers' private information on the *PIN* measure, I follow Chen et al. (2007) and adjust the *PIN* measure for insider trading. Specifically, I calculate *INFO* for the outside informed traders as *PIN* × (1–*Insider*), where *Insider* is the percentage of insider transactions to the total number of all transactions over the period in which *PIN* is calculated. Compared with the *PIN* measure, *INFO* more directly captures investors' private information (unknown to managers).[13]

As noted in Easley et al. (2002, p. 2203), the estimated *PIN* is very stable across years, both individually and cross-sectionally. I find a similar pattern for the *INFO* variable over my sample period. Specifically, the correlation between *INFO* and lagged *INFO* calculated over two consecutive non-overlapping years is 0.52. I use *INFO* as my main proxy for the amount of investor information in stock prices.

### 3.3. Empirical specification

To assess the managerial learning hypothesis, I use the following regression equation:

$$\begin{aligned} Forecast\_Revision = {} & \beta_1 Return + \beta_2 Analyst\_Revision + \beta_3 Quarterly\_Error \\ & + \beta_4 Return \times INFO + \beta_5 Analyst\_Revision \times INFO \\ & + \beta_6 Quarterly\_Error \times INFO + \beta_7 INFO + \Gamma Controls \\ & + Industry\ \ FE + Year\ \ FE + \epsilon. \end{aligned} \tag{1}$$

The timeline of how I measure the key variables, i.e., *Forecast_Revision*, *Return*, *Analyst_Revision*, *Quarterly_Error*, and *INFO*, is presented in Fig. 2. Managers of firm *i* issue an initial forecast of earnings for fiscal year *t* on date $d_1$ ($MF_{i,t}^{d_1}$) and then issue a subsequent forecast (for the same earnings realization) on date $d_2$ ($MF_{i,t}^{d_2}$). The key dependent variable *Forecast_Revision* is the difference between those forecast values, scaled by the stock price two days before the issuance of $MF_{i,t}^{d_1}$.[14] All variables (here and below) are adjusted for stock splits and stock dividends. I require the time interval between the two forecasts to be more than 10 days and less than a year to exclude potential outliers.

*Return* is the buy-and-hold return over the period from the day after the issuance of $MF_{i,t}^{d_1}$ to one day before the issuance of $MF_{i,t}^{d_2}$. I use raw returns instead of market-adjusted or industry-adjusted returns because both market and industry returns relate to future earnings. It is important to note that information on external factors such as macroeconomic conditions or industry trends can have different implications for different firms (even for firms within the same industry) and should not be simply viewed as market or industry returns. I use this return accumulation period to capture investor information released subsequent to the initial forecast (that would be incorporated into the next revision).

I control for public information about future earnings (i.e., *C* as in Fig. 1) using two variables, consensus analyst forecast revisions (*Analyst_Revision*) and management's quarterly forecast errors (*Quarterly_Error*). *Analyst_Revision* is measured as the difference between the prevailing consensus analyst forecast on date $d_2$ ($AF_{i,t}^{d_2}$) and that on date $d_1$ ($AF_{i,t}^{d_1}$), scaled by the stock price two days before the issuance of $MF_{i,t}^{d_1}$.[15] $AF_{i,t}^{d_1}$ ($AF_{i,t}^{d_2}$) is calculated as the average of all analyst forecasts (for the same earnings realization as the management forecast) issued within 90 days before firm *i* releases $MF_{i,t}^{d_1}$ ($MF_{i,t}^{d_2}$).[16]

*Quarterly_Error* is defined as the sum of management's quarterly forecast error ($MQE_{i,t}^{d_3}$) associated with any quarterly earnings announcement that occurs between the two forecast dates (excluding the day of the issuance of $MF_{i,t}^{d_1}$ but including the day of the issuance of $MF_{i,t}^{d_2}$), scaled by the stock price two days before the issuance of $MF_{i,t}^{d_1}$. A simple example serves to illustrate the underlying logic. Suppose $MF_{i,t}^{d_1}$ is issued at quarter one's earnings announcement. It will be equal to $A(Q_1)+F(Q_2)+F(Q_3)+F(Q_4)$ (where $A(Q_q)$ is the actual earnings and $F(Q_q)$ is the forecast for quarter *q*). Suppose $MF_{i,t}^{d_2}$ is made at quarter three's earnings announcement. It will be comprised of $A(Q_1)+A(Q_2)+A(Q_3)+F(Q_4)$. If there is no change in the forecast for quarter four's earnings between the two forecast dates, the revision will equal $A(Q_2)–F(Q_2)+A(Q_3)–F(Q_3)$, i.e., the sum of managers' *Q2* and *Q3* forecast errors.[17] Note that managers' *Q1* forecast error does not affect the annual forecast revision because I assume that $MF_{i,t}^{d_1}$ issued on *Q1*'s earnings announcement fully incorporates this information. Note also that I restrict *Quarterly_Error* to only include those quarterly errors that directly affect the revised annual forecast $MF_{i,t}^{d_2}$ (that is, those quarterly earnings belong to the same fiscal year as the annual earnings forecast). For quarterly earnings without management forecasts, I use the prevailing consensus analyst forecast as a proxy. If there are no interim quarterly earnings announcements between the two forecast dates, I code *Quarterly_Error* as zero.

---

[13] All inferences remain unchanged when I use *PIN* unadjusted for insider trading. My results are also robust to alternative measures of *PIN* (Brown and Hillegeist, 2007; Duarte and Young, 2009), and the use of price nonsynchronicity as an alternative measure of investors' private information (Roll, 1988; Morck et al., 2000; Durnev et al., 2004).

[14] All inferences remain unchanged when I use the absolute value of $MF_{i,t}^{d_1}$ as the scaler instead.

[15] Since analysts also incorporate some price information when forecasting earnings (e.g., Lys and Sohn, 1990; Abarbanell, 1991; Clement et al., 2011), the effect of stock returns on management forecast revisions after controlling for consensus analyst forecast revisions provides a lower bound estimate of the amount of investor information that is useful to managers (in the sense that it is not fully incorporated into analyst forecast revisions). All inferences are unchanged if I do not include analyst forecast revisions in the regressions.

[16] In computing the average, I remove excluded estimates and stopped estimates defined by I/B/E/S. Results are virtually unchanged when I use the median (instead of average) of all analyst forecasts as the consensus forecast. To further filter out stale forecasts from the consensus, I exclude analyst forecasts that are outstanding for more than 60 (or 30) days prior to management forecasts in a robustness check. The sample size is reduced somewhat but my inferences remain unchanged.

[17] For quarterly earnings with multiple management forecasts, I use the quarterly forecast issued closest to the day of the issuance of $MF_{i,t}^{d_1}$.

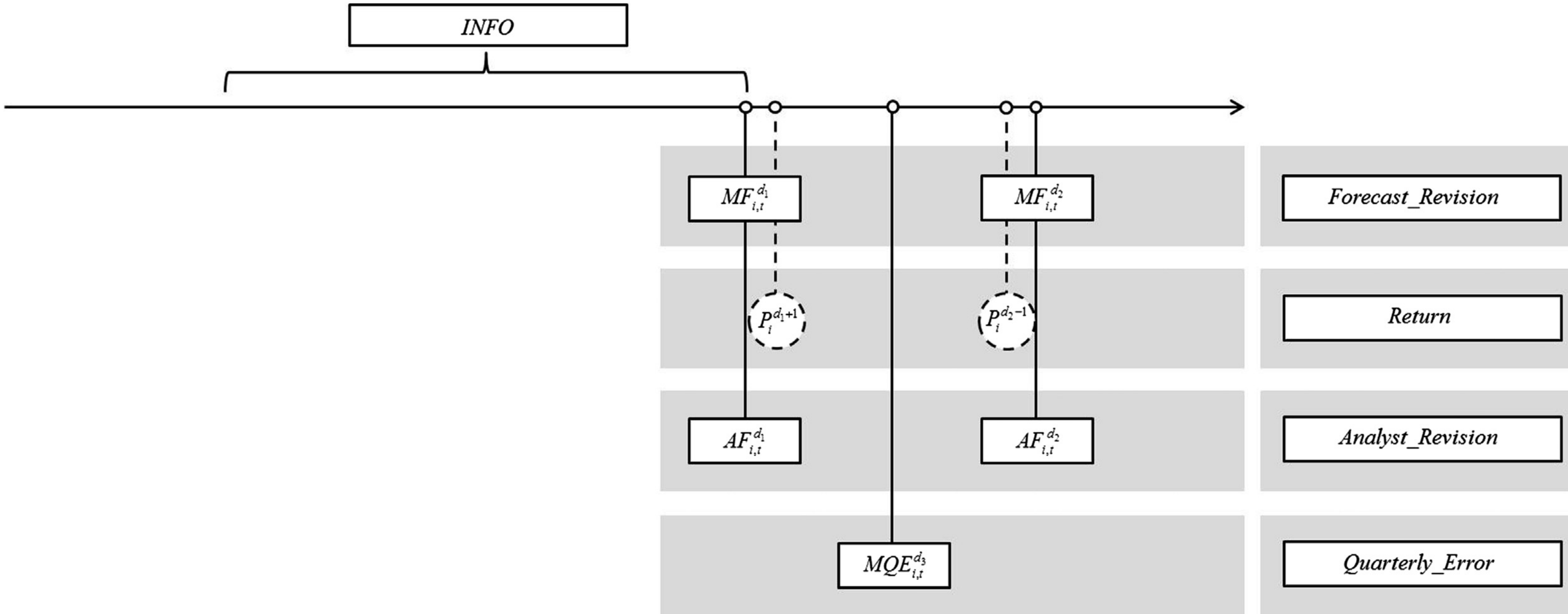


**Fig. 2.** Timeline. Fig. 2 presents a timeline of how I measure the key variables, i.e., *Forecast_Revision*, *Return*, *Analyst_Revision*, *Quarterly_Error*, and *INFO*. Managers of firm $i$ issue an initial forecast of earnings for fiscal year $t$ on date $d_1$ ($MF_{i,t}^{d_1}$) and then issue a subsequent forecast (for the same earnings realization) on date $d_2$ ($MF_{i,t}^{d_2}$). The key dependent variable *Forecast_Revision* is the difference between those forecast values, scaled by the stock price two days before the issuance of $MF_{i,t}^{d_1}$. *Return* is the buy-and-hold return over the period from the day after the issuance of $MF_{i,t}^{d_1}$ (stock price $P_i^{d_1+1}$) to one day before the issuance of $MF_{i,t}^{d_2}$ (stock price $P_i^{d_2-1}$). *Analyst_Revision* is measured as the difference between the prevailing consensus analyst forecast on date $d_2$ ($AF_{i,t}^{d_2}$) and that on date $d_1$ ($AF_{i,t}^{d_1}$), scaled by the stock price two days before the issuance of $MF_{i,t}^{d_1}$. *Quarterly_Error* is defined as the sum of management's quarterly forecast error ($MQE_{i,t}^{d_3}$) associated with any quarterly earnings announcement that occurs between the two forecast dates (excluding the day of the issuance of $MF_{i,t}^{d_1}$ but including the day of the issuance of $MF_{i,t}^{d_2}$), scaled by the stock price two days before the issuance of $MF_{i,t}^{d_1}$. *INFO* is the probability of privately informed trading net of all insider transactions measured over the year prior to $MF_{i,t}^{d_1}$.

*INFO* is the probability of privately informed trading net of all insider transactions (defined above) measured over the year prior to $MF_{i,t}^{d_1}$. As noted above, this variable is highly correlated over time for a given firm. I use lagged *INFO* to capture some firm characteristic that results in *Return* containing more private information (i.e., high *O* as in Fig. 1).[18] I use the decile rankings of *INFO* (rescaled to range from zero to one) to reduce measurement error caused by outliers and to facilitate the interpretation of the coefficients. The variable of interest is *Return* × *INFO*. I predict that the coefficient on *Return* × *INFO* is positive.

In the above framework, I do not control for unobservable managerial information (i.e., *M* as in Fig. 1).[19] *M* is in *Forecast_Revision* (the dependent variable) but not in *Return* (the independent variable) so variation in *M* would not affect the coefficient on *Return*. In other words, *M* is unlikely to introduce omitted variable bias. It is also important to note that I do not argue that only investors' private information in stock prices is new to managers. It could be the case that some public information, such as the realization of GDP or the unemployment rate, gets impounded into stock prices at the same time when it is revealed to managers. My prediction will hold as long as, on average, investor information increases the amount of information present in prices that is new to managers and thus increases the extent to which they rely on stock prices when revising their earnings forecasts. An effect driven by public information (i.e., *C* as in Fig. 1) that is not controlled for by consensus analyst forecast revisions or management's quarterly forecast errors should result in a positive association between management forecast revisions and stock returns, but it is unlikely to explain why the revision–return relation is stronger when there is more private information in prices.[20]

Other control variables include: (1) *Size*, the book value of total assets measured at the end of the most recent fiscal year prior to the issuance of $MF_{i,t}^{d_1}$; (2) *Coverage*, the number of analysts covering the firm immediately before the issuance of $MF_{i,t}^{d_1}$; (3) *Book_to_Market*, the ratio of the book value of equity to the market value of equity, measured at the end of the most recent fiscal year prior to the issuance of $MF_{i,t}^{d_1}$; (4) *Horizon*, the number of days between the date of $MF_{i,t}^{d_2}$ and the estimate period end date; and (5) *Gap*, the number of days between $MF_{i,t}^{d_1}$ and $MF_{i,t}^{d_2}$. I include *Size* and *Coverage* to control for the general information environment of the firm. I include *Book_to_Market* and *Horizon* to control for the difficulty in forecasting earnings. I include *Gap* to control for managers' delay in revising their forecasts. I use the natural logarithm of *Size*, *Coverage*, *Horizon* and *Gap*. I include both industry and year fixed effects in the regressions, where industry fixed effects are based on the Fama-French 49 industries, and year fixed effects are based on the year the revised forecast ($MF_{i,t}^{d_2}$) is issued.

### 3.4. Descriptive statistics

Table 2 presents the descriptive statistics of the variables used in my analysis. Panel A of Table 2 presents the summary statistics for the sample. All variables are defined in Appendix A. All continuous variables are winsorized at the top and bottom 1% levels to mitigate the influence of extreme values. There is a large variation in *Forecast_Revision*, with a mean value of −0.143, a median value of 0.062, and a standard deviation of 0.930. The average (median) number of days between the revised forecast date and the estimate period end date (*Horizon*) is 164 (156) days. The average (median) number of days between the initial forecast and the revised forecast (*Gap*) is 87 (90) days.

Panel B of Table 2 presents the Pearson (above diagonal) and Spearman (below diagonal) correlations for the variables used in my analysis of forecast revisions. Because the patterns for these two correlations are quite similar, I focus on Pearson correlations in the following discussions. The correlation between *Forecast_Revision* and *Return* is positive (0.30) and statistically significant at the 1% level; *Analyst_Revision* and *Quarterly_Error* also have positive and statistically significant correlations with both *Forecast_Revision* (0.51 and 0.47 respectively) and *Return* (0.23 and 0.15 respectively). These high correlations among variables suggest that *Analyst_Revision* and *Quarterly_Error* capture public information reasonably well, and controlling for *Analyst_Revision* and *Quarterly_Error* is important in the tests.

To further assess the relative strength of *Return*, *Analyst_Revision* and *Quarterly_Error* in explaining *Forecast_Revision*, I regress *Forecast_Revision* on *Return*, *Analyst_Revision* and *Quarterly_Error* individually and jointly, where all variables are standardized to have a mean of zero and a standard deviation of one to facilitate comparisons (see Appendix B). In the multiple regression analysis (column 4), the coefficient on *Analyst_Revision* or *Quarterly_Error* is much larger than that on *Return*, consistent with the prior that public information (i.e., *C* as in Fig. 1) is of first order importance relative to investors' private information (i.e., *O* as in Fig. 1) in explaining forecast revisions. In addition, *Return*, *Analyst_Revision* and *Quarterly_Error* jointly explain 36.7% of the variation in *Forecast_Revision*. The remaining variation in *Forecast_Revision* can be explained by measurement error in proxies for public information and investors' private information, and more importantly, by (unobservable) managers' private information (i.e., *M* as in Fig. 1). The important role of managers' private information in

[18] I use lagged *INFO* to alleviate endogeneity concerns. The inferences are unchanged when I use contemporaneous *INFO*.

[19] In this framework, *M* refers to managers' private information regarding future earnings as of the date of the revised forecast. Managerial information released to the public (e.g., through corporate disclosure) after the initial forecast but before the revised forecast is captured by *C* (i.e., common information between managers and investors).

[20] This statement holds as long as the cross-sectional correlation between the amount of public information and the amount of investors' private information is not significant or at least not positive. Empirical evidence suggests that *PIN* does not capture cross-sectional differences in the amount of public information. Easley et al. (1998) find that *PIN* is negatively related to analyst following. The correlation between *INFO* and analyst following (analyst forecast accuracy) in my sample is −0.46 (−0.23). More discussion on this issue is provided in Section 5.3.1.

**Table 2**
Descriptive statistics.

Panel A: Summary statistics

| | Observations | Mean | Median | SD |
|---|---|---|---|---|
| *Forecast_Revision* | 15,977 | −0.143 | 0.062 | 0.930 |
| *Return* | 15,977 | 0.025 | 0.026 | 0.176 |
| *Analyst_Revision* | 15,977 | −0.118 | 0.000 | 0.857 |
| *Quarterly_Error* | 15,977 | 0.002 | 0.000 | 0.507 |
| *INFO* | 15,977 | 0.124 | 0.113 | 0.061 |
| *Size* | 15,977 | 6133 | 1242 | 14,863 |
| *Coverage* | 15,977 | 10 | 8 | 7 |
| *Analyst_Accuracy* | 15,977 | −0.407 | −0.120 | 0.781 |
| *Book_to_Market* | 15,977 | 0.460 | 0.390 | 0.312 |
| *Beta* | 15,977 | 0.993 | 0.886 | 0.717 |
| *D_Neg* | 15,977 | 0.424 | 0.000 | 0.494 |
| *Horizon* | 15,977 | 164 | 156 | 99 |
| *Institution_Own* | 15,977 | 0.633 | 0.726 | 0.297 |
| *Turnover* | 15,977 | 0.905 | 0.745 | 0.613 |
| \|*Sentiment*\| | 15,977 | 0.495 | 0.444 | 0.320 |
| *Gap* | 15,977 | 87 | 90 | 42 |
| \|*MFFlow*\| | 11,692 | 0.298 | 0.143 | 0.455 |
| Δ*Accuracy* | 15,398 | 0.451 | 0.180 | 0.909 |

Panel B: Pearson (above diagonal) and Spearman (below diagonal) correlations

| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) | (17) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| (1) | *Forecast_Revision* | | **0.30** | **0.51** | **0.47** | **−0.06** | **0.05** | **0.07** | **0.04** | **−0.10** | **−0.04** | **−0.23** | 0.02 | **0.05** | 0.00 | **−0.04** | **−0.06** | 0.00 |
| (2) | *Return* | **0.32** | | **0.23** | **0.15** | **0.05** | **−0.03** | **−0.05** | **−0.06** | **0.07** | **−0.03** | **−0.76** | **0.05** | 0.02 | **−0.03** | **−0.07** | **0.05** | **−0.04** |
| (3) | *Analyst_Revision* | **0.50** | **0.27** | | **0.42** | **−0.02** | 0.01 | **0.03** | **0.02** | **−0.08** | **−0.05** | **−0.18** | −0.01 | **0.04** | −0.01 | −0.02 | **−0.04** | 0.00 |
| (4) | *Quarterly_Error* | **0.50** | **0.18** | **0.40** | | **−0.03** | **0.03** | **0.06** | **0.04** | **−0.06** | 0.00 | **−0.11** | 0.01 | **0.03** | 0.01 | 0.00 | **−0.03** | −0.01 |
| (5) | *INFO* | **−0.02** | **0.03** | 0.00 | **−0.07** | | **−0.48** | **−0.46** | **−0.23** | **0.20** | 0.00 | 0.00 | **−0.02** | **−0.18** | **−0.17** | **0.12** | **0.07** | **0.06** |
| (6) | *Size* | 0.01 | **−0.02** | −0.01 | **0.03** | **−0.55** | | **0.55** | **0.12** | **0.04** | **−0.22** | **−0.03** | 0.01 | **0.20** | **−0.11** | **−0.05** | **−0.08** | **−0.11** |
| (7) | *Coverage* | 0.02 | **−0.03** | 0.02 | **0.12** | **−0.51** | **0.57** | | **0.37** | **−0.30** | **0.02** | 0.01 | **0.04** | **0.16** | **0.26** | **−0.05** | **−0.08** | **−0.08** |
| (8) | *Analyst_Accuracy* | **−0.09** | **−0.06** | **−0.07** | **−0.03** | **−0.21** | **0.17** | **0.34** | | **−0.26** | **−0.02** | **0.03** | 0.01 | **0.10** | **0.06** | **−0.05** | **−0.04** | **0.07** |
| (9) | *Book_to_Market* | 0.00 | **0.06** | **−0.02** | **−0.05** | **0.14** | **0.08** | **−0.27** | **−0.31** | | **−0.05** | **−0.04** | **−0.05** | **−0.07** | **−0.13** | **0.10** | **0.04** | −0.02 |
| (10) | *Beta* | **0.02** | **−0.03** | 0.00 | **0.06** | 0.01 | **−0.21** | 0.02 | **−0.11** | **−0.07** | | **0.06** | −0.01 | **−0.05** | **0.32** | **−0.08** | **0.03** | **−0.03** |
| (11) | *D_Neg* | **−0.26** | **−0.86** | **−0.23** | **−0.15** | 0.01 | **−0.03** | 0.01 | **0.02** | **−0.05** | **0.06** | | **−0.02** | **−0.03** | **0.03** | **0.06** | **−0.02** | 0.03 |
| (12) | *Horizon* | **0.03** | **0.04** | **−0.02** | **−0.06** | **−0.03** | **0.03** | **0.07** | **0.03** | **−0.06** | **−0.02** | **−0.03** | | 0.01 | **0.03** | 0.01 | **−0.10** | −0.02 |
| (13) | *Institution_Own* | **0.04** | 0.01 | **0.02** | **0.06** | **−0.19** | **0.17** | **0.17** | **0.10** | **−0.03** | 0.01 | **−0.02** | 0.01 | | **−0.03** | −0.02 | **−0.03** | **0.05** |
| (14) | *Turnover* | **0.04** | **−0.03** | **0.04** | **0.10** | **−0.18** | **−0.08** | **0.28** | 0.01 | **−0.17** | **0.34** | **0.04** | **0.03** | **0.18** | | 0.00 | **−0.05** | **−0.15** |
| (15) | \|*Sentiment*\| | **−0.02** | **−0.07** | −0.01 | **−0.02** | **0.12** | **−0.05** | **−0.05** | 0.00 | **0.07** | **−0.09** | **0.06** | **0.04** | **−0.06** | **−0.04** | | 0.02 | **−0.11** |
| (16) | *Gap* | −0.02 | **0.05** | **0.03** | **0.03** | **0.07** | **−0.08** | **−0.07** | **−0.03** | **0.03** | 0.01 | −0.01 | **−0.19** | **−0.03** | **−0.06** | 0.02 | | 0.01 |
| (17) | \|*MFFlow*\| | 0.02 | **−0.03** | **0.03** | **0.03** | **−0.07** | 0.02 | **0.05** | **0.16** | **−0.08** | **−0.04** | 0.01 | 0.01 | **0.16** | **−0.04** | **−0.15** | −0.02 | |

This table presents the descriptive statistics of the variables used in my analysis. The sample period is from 1996 to 2010. Panel A presents the summary statistics for the sample, and Panel B presents the Pearson (above diagonal) and Spearman (below diagonal) correlations for the variables used in my analysis of forecast revisions. All variables are defined in Appendix A. All continuous variables are winsorized at the top and bottom 1% levels to mitigate the influence of extreme values. In Panel B, numbers in bold are significant at the 1% level (two-tailed).

explaining forecast revisions is consistent with prior literature that management forecasts provide additional information and guidance to the market and the market significantly reacts to them (e.g., Patell, 1976; Jennings, 1987; Anilowski et al., 2007; Rogers et al., 2009).

### 3.5. Controlling for confounding effects

Prior literature suggests that analysts are not efficient in impounding public information and are biased in making forecasts (see Gu and Wu (2003) and So (2013) for a summary of various explanations). Hence, my proxy for public information based on *Analyst_Revision* likely contains measurement error. Although *Quarterly_Error* can help get some of the important pieces of public information that may not be completely captured in *Analyst_Revision*, a positive relation between *Forecast_Revision* and *Return* (after controlling for *Analyst_Revision* and *Quarterly_Error*) can still be partly driven by public information, and not solely attributed to managers learning from prices. To address this concern, I examine whether the amount of private information in prices has a positive effect on the revision–return relation and focus on the coefficient on $Return \times INFO$. This empirical methodology based on cross-sectional predictions follows the investment sensitivity literature in which it is infeasible to control for public information about a firm's growth opportunities (e.g., Chen et al., 2007; Foucault and Fresard, 2014).

However, of concern here is the possibility that *INFO* is correlated with variables that affect the revision–return relation but are omitted from the analysis. For example, *INFO* is highly correlated with *Size* and *Coverage* (with correlation coefficients of −0.48 and −0.46 respectively, see Panel B of Table 2), and *Size* or *Coverage* may directly affect the revision–return relation. To control for the major firm-specific and forecast-specific characteristics, I use the following two-stage approach. In the first stage, I orthogonalize *INFO* with respect to a set of control variables by performing the following regression and saving the residual:

$$\begin{aligned} INFO = {} & \beta_1 Size + \beta_2 Coverage + \beta_3 Analyst\_Accuracy \\ & + \beta_4 Book\_to\_Market + \beta_5 Beta + \beta_6 D\_Neg + \beta_7 Horizon \\ & + \beta_8 Institution\_Own + \beta_9 Turnover + \beta_{10} |Sentiment| \\ & + \beta_{11} Gap + IndustryFE + YearFE + \varepsilon. \end{aligned} \tag{2}$$

The motivations for controlling for this set of variables are as follows (more detailed discussions are provided in Section 5.3): firm size (*Size*), analyst coverage (*Coverage*), and analyst forecast accuracy (*Analyst_Accuracy*) are included in the model to control for the richness of a firm's information environment. To control for the mechanical relation between prices and earnings (i.e., prices leading earnings), I include four variables: growth opportunities (*Book_to_Market*), risk (*Beta*), a dummy variable for negative *Return* (*D_Neg*), and forecast horizon (*Horizon*). I control for two additional variables that may be correlated with *INFO* and may affect the revision–return relation: the percentage of shares held by institutional investors (*Institution_Own*), and stock market liquidity as proxied by the average daily share turnover (*Turnover*). I also control for investor sentiment (|*Sentiment*|) as managers may cater to investor sentiment. Lastly, I control for the number of days between the revised forecast and the initial forecast (*Gap*) because the lag between the two forecast dates increases the probability that the correlation between *Return* and *Forecast_Revision* is simply due to managers' delay in revising their forecasts. All variables are defined in Appendix A, and the results from this model are presented in Appendix C. The first-stage regression results indicate that larger firms with more analyst following, more accurate analyst forecasts, more growth opportunities, higher beta risk, higher institutional ownership and greater share turnover experience significantly less informed trading. Maffett (2012) documents similar findings in an international setting using a measure of informed trading by institutional investors. These results are consistent with theoretical models that more opaque information environments stimulate more private information acquisition (Verrecchia, 1982; Diamond, 1985; Gao and Liang 2013).

In the second stage, I use the residual from Eq. (2) (denoted by $INFO^{\perp}$) and re-estimate Eq. (1). The unexplained variation in *INFO* (i.e., $INFO^{\perp}$) is highly correlated with *INFO* (with a correlation coefficient of 0.79), but uncorrelated with other confounds by construction. The observed effect of $INFO^{\perp}$ on the revision–return relation results in a cleaner test of the managerial learning hypothesis since it cannot be explained by other confounds (that are included in the first stage). This two-stage approach is preferred to a one-stage approach of augmenting Eq. (1) with direct controls and their interactions with *Return*, *Analyst_Revision* and *Quarterly_Error* because not only is it parsimonious but also it avoids the potential loss of power or multi-collinearity caused by too many interaction terms (in my case, 33 interaction terms for the 11 control variables, and even more if industry or year dummies are considered).[21]

[21] This two-stage approach of controlling for confounding effects has been used in prior studies. For example, Nikolaev (2010) uses a similar approach to test the relation between debt covenants and accounting conservatism.

**Table 3**
The effect of investor information on the revision–return relation.

| | Dependent variable: *Forecast_Revision* | | |
|---|---|---|---|
| | (1) | (2) *INFO* | (3) $INFO^{\perp}$ |
| *Return* | 1.00*** | 0.52*** | 0.74*** |
| | (17.29) | (4.44) | (7.18) |
| *Analyst_Revision* | 0.35*** | 0.43*** | 0.36*** |
| | (20.66) | (10.79) | (10.18) |
| *Quarterly_Error* | 0.53*** | 0.51*** | 0.54*** |
| | (16.36) | (8.45) | (11.03) |
| ***Return*** **×** ***INFO*** | | **0.82*** ** | **0.50** ** |
| | | **(4.55)** | **(2.41)** |
| *Analyst_Revision* × *INFO* | | −0.12*** | −0.01 |
| | | (−2.60) | (−0.21) |
| *Quarterly_Error* × *INFO* | | 0.02 | −0.02 |
| | | (0.31) | (−0.33) |
| *INFO* | | −0.06** | −0.03 |
| | | (−2.03) | (−0.99) |
| *Size* | 0.01* | 0.01 | 0.01* |
| | (1.68) | (1.22) | (1.73) |
| *Coverage* | 0.03 | 0.03 | 0.03 |
| | (1.49) | (1.41) | (1.49) |
| *Book_to_Market* | −0.18*** | −0.18*** | −0.19*** |
| | (−4.41) | (−4.25) | (−4.36) |
| *Horizon* | −0.00 | −0.00 | −0.00 |
| | (−0.15) | (−0.10) | (−0.11) |
| *Gap* | −0.07*** | −0.07*** | −0.07*** |
| | (−3.82) | (−3.69) | (−3.83) |
| Industry FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Observations | 15,977 | 15,977 | 15,977 |
| Adjusted $R^2$ | 38.3% | 38.6% | 38.4% |

This table presents the results of estimating Eq. (1). The sample period is from 1996 to 2010. Column 2 presents the results based on the raw *INFO*, and column 3 presents the results based on the orthogonalized *INFO* ($INFO^{\perp}$), where I orthogonalize *INFO* by regressing *INFO* on a set of control variables and saving the residual (see Appendix C). All variables are defined in Appendix A. I use the decile rankings of *INFO* and $INFO^{\perp}$ (rescaled to range from zero to one) to facilitate interpretation of the coefficients. I use the natural logarithm of *Size*, *Coverage*, *Horizon* and *Gap*. All regressions include industry and year fixed effects. The *t*-statistics, presented in parentheses below the coefficients, are corrected for heteroskedasticity and cross-sectional and time-series correlations using a two-way cluster at the firm and year levels. ***, **, and * denote significance at the 1%, 5%, and 10% levels for two-tailed tests, respectively.

## 4. Main results

### 4.1. Basic results

Table 3 presents the results of the main test. In this and all subsequent regressions, I report *t*-statistics corrected for heteroskedasticity and cross-sectional and time-series correlations using a two-way cluster at the firm and year levels (Petersen, 2009; Gow et al., 2010). Column 1 presents the baseline regression without interaction terms. The coefficients on *Return*, *Analyst_Revision*, and *Quarterly_Error* are all positive and significant at the 1% level, as expected. The positive coefficient on *Return* is consistent with managers learning from prices, but may not be solely attributed to this channel since it can be partly driven by public information not captured by *Analyst_Revision* or *Quarterly_Error*. To formally test the managerial learning hypothesis, column 2 presents the results of estimating Eq. (1). The coefficient on the interaction term between *Return* and *INFO* (*Return* × *INFO*) is positive and significant at the 1% level, suggesting that managers respond more strongly to stock returns when stock prices contain more investor information that is new to them. The effect is also economically large. An increase in *INFO* from the bottom to the top decile more than doubles the revision–return sensitivity.

The result in column 2 provides initial support for the managerial learning channel, but one potential concern here is that *INFO* may be correlated with variables that affect the revision–return relation but are omitted from the analysis. To control for a number of confounding effects, I follow the two-stage approach described in Section 3.5. Appendix C presents the results from the first-stage regression, given by Eq. (2). Column 3 presents the second-stage results based on orthogonalized *INFO* (i.e., $INFO^{\perp}$). The coefficient on *Return* × $INFO^{\perp}$ is positive (though smaller than that on *Return* × *INFO*) and

**Table 4**
The effect of investor information on the revision–return relation: Evidence from mutual fund redemptions.

| | Dependent variable: *Forecast_Revision* | | |
|---|---|---|---|
| | (1) | (2) $\lvert MFFlow \rvert$ | (3) $\lvert MFFlow \rvert^{\perp}$ |
| *Return* | 1.01*** | 1.26*** | 1.24*** |
| | (14.67) | (13.11) | (10.65) |
| *Analyst_Revision* | 0.37*** | 0.35*** | 0.33*** |
| | (24.78) | (19.68) | (19.62) |
| *Quarterly_Error* | 0.48*** | 0.45*** | 0.47*** |
| | (13.60) | (10.95) | (11.79) |
| ***Return* × \|*MFFlow*\|** | | **−0.63*** ** | **−0.58*** ** |
| | | **(−2.84)** | **(−2.65)** |
| *Analyst_Revision* × \|*MFFlow*\| | | 0.05 | 0.10*** |
| | | (1.09) | (2.65) |
| *Quarterly_Error* × \|*MFFlow*\| | | 0.08 | 0.02 |
| | | (0.95) | (0.20) |
| \|*MFFlow*\| | | 0.09** | 0.07* |
| | | (2.52) | (1.79) |
| *Size* | 0.01** | 0.01** | 0.01** |
| | (2.27) | (2.12) | (2.08) |
| *Coverage* | 0.02* | 0.02 | 0.02 |
| | (1.74) | (1.62) | (1.60) |
| *Book_to_Market* | −0.20*** | −0.20*** | −0.19*** |
| | (−4.50) | (−4.42) | (−4.57) |
| *Horizon* | 0.00 | −0.00 | −0.00 |
| | (0.02) | (−0.05) | (−0.03) |
| *Gap* | −0.07*** | −0.08*** | −0.08*** |
| | (−3.20) | (−3.26) | (−3.03) |
| Industry FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Observations | 11,692 | 11,692 | 11,692 |
| Adjusted $R^2$ | 35.4% | 35.6% | 35.6% |

This table presents the results of estimating Eq. (1), with *INFO* replaced by |*MFFlow*|. The sample period is from 1996 to 2007 for which the mutual fund redemption data are available. Column 2 presents the results based on the raw |*MFFlow*|, and column 3 presents the results based on the orthogonlized |*MFFlow*| ($\lvert MFFlow \rvert^{\perp}$), where I orthogonalize |*MFFlow*| by regressing |*MFFlow*| on a set of control variables and saving the residual (see Appendix C). All variables are defined in Appendix A. I use the decile rankings of |*MFFlow*| and $\lvert MFFlow \rvert^{\perp}$ (rescaled to range from zero to one) to facilitate interpretation of the coefficients. I use the natural logarithm of *Size*, *Coverage*, *Horizon* and *Gap*. All regressions include industry and year fixed effects. The *t*-statistics, presented in parentheses below the coefficients, are corrected for heteroskedasticity and cross-sectional and time-series correlations using a two-way cluster at the firm and year levels. ***, **, and * denote significance at the 1%, 5%, and 10% levels for two-tailed tests, respectively.

significant at the 5% level. This positive effect of $INFO^{\perp}$ on the revision–return relation provides a strong support for the managerial learning hypothesis since it cannot be explained by other confounds that are included in the first stage. In sum, the results in Table 3 suggest that investors possess some information that managers lack and managers are able and willing to incorporate that information in forecasting future earnings.[22]

## 4.2. Evidence from mutual fund redemptions

As discussed in Section 3, *INFO* represents the probability of informed trading by investors and thus captures some firm characteristic that results in *Return* containing more private information (i.e., high *O* as in Fig. 1). Here, I use an alternative measure (|*MFFlow*|) that captures some time-period specific event that leads to more *uninformed* trading by investors (i.e., low *O* as in Fig. 1). Edmans et al. (2012) use mutual fund redemptions as a shock to price changes that is exogenous to fundamental news. They create a measure, *MFFlow*, the price pressure created by mutual fund trading that is not induced by information but by investor flows. The *MFFlow* measure is constructed using hypothetical orders projected from mutual funds' previously disclosed portfolios (rather than their actual purchases and sales), with a positive value indicating upward (buying) pressure and a negative value indicating downward (selling) pressure. Hence, this measure does not reflect mutual funds' discretionary trades that may be based on information. Instead, it captures an expansion or contraction of a fund's existing positions that is mechanically induced by investor flows to and from the fund. Those flows are in turn unlikely to be

[22] In untabulated tests, I run the regressions separately for upward revisions and downward revisions and find similar results for the two subsamples, suggesting that the learning channel is symmetric with respect to the direction of revisions.

**Table 5**
The effect of investor information on changes in forecast accuracy.

| | Dependent variable: Δ*Accuracy* | | | |
|---|---|---|---|---|
| *Var* = | (1) | (2) *INFO*$^{\perp}$ | (3) | (4) \|*MFFlow*\|$^{\perp}$ |
| \|*Return*\| | 0.64*** | 0.44*** | 0.57*** | 0.86*** |
| | (6.74) | (4.95) | (6.68) | (8.93) |
| \|*Analyst_Revision*\| | 0.45*** | 0.36*** | 0.48*** | 0.46*** |
| | (18.50) | (6.95) | (25.07) | (18.95) |
| \|*Quarterly_Error*\| | 0.02 | 0.11*** | 0.01 | −0.00 |
| | (1.41) | (2.95) | (1.43) | (−0.73) |
| **\|*Return*\| × *Var*** | | **0.33**** | | **−0.76***** |
| | | **(2.26)** | | **(−3.39)** |
| \|*Analyst_Revision*\| × *Var* | | 0.13** | | 0.02 |
| | | (2.12) | | (0.57) |
| \|*Quarterly_Error*\| × *Var* | | −0.10** | | 0.11** |
| | | (−2.52) | | (2.41) |
| *Var* | | 0.01 | | −0.04 |
| | | (0.18) | | (−1.05) |
| *Size* | −0.01 | −0.01 | −0.01* | −0.01* |
| | (−1.38) | (−1.47) | (−1.94) | (−1.90) |
| *Coverage* | −0.11*** | −0.11*** | −0.10*** | −0.09*** |
| | (−4.97) | (−4.99) | (−4.19) | (−4.09) |
| *Book_to_Market* | 0.29*** | 0.28*** | 0.28*** | 0.27*** |
| | (6.43) | (6.12) | (4.35) | (4.41) |
| *Horizon* | 0.01 | 0.02 | 0.02 | 0.02 |
| | (1.41) | (1.60) | (1.35) | (1.63) |
| *Gap* | 0.05** | 0.05** | 0.05** | 0.07** |
| | (2.20) | (2.17) | (2.11) | (2.24) |
| Industry FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Observations | 15,398 | 15,398 | 11,215 | 11,215 |
| Adjusted $R^2$ | 24.7% | 25.5% | 25.4% | 25.8% |

This table presents the results of estimating Eq. (3). |*Return*|, |*Analyst_Revision*|, and |*Quarterly_Error*| denote the absolute value of *Return*, *Analyst_Revision*, and *Quarterly_Error*, respectively. For *Quarterly_Error* that involves multiple quarterly earnings announcements, |*Quarterly_Error*| is calculated as the summation of the absolute value of each quarterly forecast error. In columns 1 and 2, the sample period is from 1996 to 2010. In columns 3 and 4, the sample period is from 1996 to 2007 for which the mutual fund redemption data are available. Columns 2 and 4 present the results based on the orthogonalized *INFO* and |*MFFlow*| (*INFO*$^{\perp}$ and |*MFFlow*|$^{\perp}$), respectively. I orthogonalize *INFO* and |*MFFlow*| by regressing each of them on a set of control variables and saving the residual (see Appendix C). All variables are defined in Appendix A. I use the decile rankings of *INFO*$^{\perp}$ and |*MFFlow*|$^{\perp}$ (rescaled to range from zero to one) to facilitate interpretation of the coefficients. I use the natural logarithm of *Size*, *Coverage*, *Horizon* and *Gap*. All regressions include industry and year fixed effects. The *t*-statistics, presented in parentheses below the coefficients, are corrected for heteroskedasticity and cross-sectional and time-series correlations using a two-way cluster at the firm and year levels. ***, **, and * denote significance at the 1%, 5%, and 10% levels for two-tailed tests, respectively.

driven by investors' views on an individual firm held by the fund, since such views would be expressed through direct trading of that firm's stock rather than a mutual fund share.

For this test, the sample is restricted to the 1996–2007 period for which the mutual fund price pressure measure is available.[23] I calculate *MFFlow* over the management forecast revision period and predict that the revision–return relation is weaker when the absolute value of mutual fund price pressure (i.e., |*MFFlow*|) is higher because, in such a case, the price changes are caused by investor flows rather than information. Table 4 presents the results. Column 1 reports the baseline regression without interaction terms for the 1996–2007 period. Column 2 reports the results of estimating Eq. (1), with *INFO* replaced by |*MFFlow*|. Consistent with my prediction, |*MFFlow*| negatively affects the revision–return relation, suggesting that managers glean less useful information from the stock market when price changes are caused by investor flows rather than information. In column 3, I find that the negative effect of price pressure on the revision–return relation persists after I control for various confounds using the two-stage approach.

### 4.3. *The effect of investor information on changes in forecast accuracy*

If investors' private information in stock prices supplements managers' information concerning future earnings and managers use investor information to improve their forecasts, more investor information should lead to a greater improvement in forecast accuracy. We should not observe such a relation if informed traders only discover information

[23] I thank Alex Edmans for providing the mutual fund price pressure measure.

already known by managers. To test this prediction, I use the following regression equation:

$$\begin{aligned}\Delta Accuracy = {} & \beta_1|Return| + \beta_2|Analyst\_Revision| + \beta_3|Quarterly\_Error| \\ & + \beta_4|Return| \times INFO + \beta_5|Analyst\_Revision| \times INFO \\ & + \beta_6|Quarterly\_Error| \times INFO \\ & + \beta_7 INFO + \Gamma Controls + Industry\ \ FE + Year\ \ FE + \epsilon. \end{aligned} \tag{3}$$

$\Delta Accuracy$ is defined as $-100 \times (|MF_{i,t}^{d_2} - Actual\ \ Earnings| - |MF_{i,t}^{d_1} - Actual\ \ Earnings|)/P_i^{d_1-2}$, where $MF_{i,t}^{d_2}$ is the earnings forecast released by firm $i$ on date $d_2$; $MF_{i,t}^{d_1}$ is the most recent earnings forecast (for the same earnings realization) released by firm $i$ prior to $MF_{i,t}^{d_2}$; and $P_i^{d_1-2}$ is the stock price two days before the issuance of $MF_{i,t}^{d_1}$. |*Return*|, |*Analyst_Revision*|, and |*Quarterly_Error*| denote the absolute value of *Return*, *Analyst_Revision*, and *Quarterly_Error*, respectively.[24] I include unsigned |*Return*|, |*Analyst_Revision*|, and |*Quarterly_Error*| in the model because both positive and negative values of those variables reflect information that potentially improves forecast accuracy.[25] I use lagged *INFO* to capture some firm characteristic that results in |*Return*| containing more private information (i.e., high *O* as in Fig. 1). The variable of interest is |*Return*| × *INFO*. I predict that the coefficient on |*Return*| × *INFO* is positive, suggesting that investor information in stock prices helps managers improve their earnings forecast accuracy.

Table 5 presents the results. Column 1 presents the baseline regression without interaction terms. The coefficients on |*Return*| and |*Analyst_Revision*| are both positive and statistically significant at the 1% level, as expected. The coefficient on |*Quarterly_Error*| is positive but only marginally significant (with *t*-statistic equal to 1.41). Column 2 shows that the coefficient on |*Return*| × $INFO^{\perp}$ is positive and statistically significant at the 5% level, indicating that investor information has a positive effect on the relation between |*Return*| and changes in forecast accuracy ($\Delta Accuracy$).[26] Column 3 reports the baseline regression over the period 1996–2007 for which the mutual fund redemption data are available, and the results are similar to those reported in column 1. Column 4 shows that the coefficient on |*Return*| × $|MFFlow|^{\perp}$ is negative and statistically significant at the 1% level, suggesting that price movement generated by mutual fund redemptions does not contain information and weakens the positive relation between |*Return*| and $\Delta Accuracy$. In sum, the results in Table 5 suggest that investor information in stock prices is new to managers and helps managers improve their earnings forecast accuracy.

## 5. Additional analyses

### 5.1. Sample selection issue

My main sample includes any firm that issues an earnings forecast and subsequently revises the initial forecast. Of concern here is that management forecasts are voluntary and managers might strategically withhold their earnings forecasts. To alleviate concerns about the selection issue (i.e., strategic decision to issue a forecast), I focus the sample on forecasters that appear to have a forecasting policy (i.e., to consistently forecast each quarter with the earnings announcement). These firms are unlikely to strategically withhold forecasts because their disclosure policies are set and costly to deviate from. Specifically, I define a firm as a regular forecaster in fiscal year *t* if it issues at least one bundled forecast in each quarter of fiscal year *t*.[27] I repeat the analysis for this subsample of regular forecasters where both the initial forecast and the revised forecast are bundled forecasts. The variability of *INFO* is reduced by 15% (0.052 for this subsample versus 0.061 for the full sample), but my inferences on managerial learning are unchanged (untabulated).

### 5.2. Alternative return windows

In my main analysis, the *Return* accumulation period starts from the day after the issuance of the initial forecast and ends one day prior to the forecast revision. This measurement window may pick up delayed market reaction to the initial forecast. To the extent that the reaction to the initial forecast does not convey any new information, including it in *Return* weakens the revision–return relation. Relatedly, this return window may pick up reactions to the revisions themselves due to information leakage or more intense information discovery by investors ahead of the earnings or forecast announcements. In a robustness check, I use the *Return* accumulation period that starts from the third day after the issuance of the initial forecast (to exclude the market reaction to the initial forecast), and/or ends on the third day before the issuance of the revised forecast (to exclude the market reaction to the revised forecast). My inferences are unchanged (untabulated).

[24] For *Quarterly_Error* that involves multiple quarterly earnings announcements, |*Quarterly_Error*| is calculated as the summation of the absolute value of each quarterly forecast error.

[25] Results are similar when I replace |*Analyst_Revision*| with a measure of changes in analyst forecast accuracy. |*Analyst_Revision*| is preferred because the calculation of changes in analyst forecast accuracy involves the actual earnings number and potentially introduces look-ahead bias in this explanatory variable.

[26] I only report the cleaner results using orthogonalized *INFO* or |*MFFlow*|. Results are similar with raw *INFO* or |*MFFlow*|.

[27] Following Rogers and Van Buskirk (2013), I define bundled forecasts as those that fall within two days of the earnings announcement date.

### 5.3. Controlling for confounding effects: Alternative specifications

Instead of performing the two-stage analysis described in Section 3.5, here I control for confounding factors in a one-stage regression. Because of the large number of interaction terms involved, I report in Panel A of Table 6 the results after controlling for each potential confound individually. I control for *Size* and *Coverage* throughout this analysis since the correlation matrix reported in Panel B of Table 2 suggests that they are most important to control for (as interaction terms). Column 1 presents the results after controlling for only *Size* and *Coverage*. Columns 2–10 present the results after controlling for one additional confound (*Var*) besides *Size* and *Coverage*. The nine variables controlled for in columns 2–10 are: *Analyst_Accuracy*, *Book_to_Market*, *Beta*, *D_Neg*, *Horizon*, *Institution_Own*, *Turnover*, |*Sentiment*|, and *Gap*, respectively.

In Panel B of Table 6, I report the results after controlling for all confounds jointly in one regression. Column 1 presents the coefficients on *Return*, *Analyst_Revision*, and *Quarterly_Error*. Column 2 presents the coefficients on *Return* × *INFO*, *Analyst_Revision* × *INFO*, *Quarterly_Error* × *INFO*, and *INFO*. Columns 3 to 13 present the coefficients on one potential confound and its interaction terms. For example, column 3 presents the coefficients on *Return* × *Size*, *Analyst_Revision* × *Size*, *Quarterly_Error* × *Size*, and *Size*.

Across all specifications in Panels A and B of Table 6, the coefficient on *Return* × *INFO* remains positive and statistically significant at the 5% level or better. Note that both the magnitude and statistical significance of the coefficient on *Return* × *INFO* in column 2 of Panel B of Table 6 are quite similar to those reported in column 3 of Table 3.[28] Because these alternative specifications shed light on whether the potential confounds affect the revision–return relation in an expected way, I discuss the effects of those confounds in Sections 5.3.1–5.3.3.

#### 5.3.1. Controlling for the richness of the information environment

Prior analyst literature indicates greater analyst inefficiency or bias in a more uncertain information environment (see Kothari (2001) for a review). To control for the richness of a firm's information environment, I use three proxies: *Size*, *Coverage*, and *Analyst_Accuracy* (all lagged). Both the coefficient on *Analyst_Revision* × *Size* (columns 1–10 of Panel A and column 3 of Panel B) and that on *Analyst_Revision* × *Analyst_Accuracy* (column 2 of Panel A and column 5 of Panel B) are positive and statistically significant, consistent with *Analyst_Revision* better capturing public information for firms with better information environments. The coefficient on *Return* × *Size* is statistically insignificant across the board. The coefficient on *Return* × *Analyst_Accuracy* (column 2 of Panel A and column 5 of Panel B) is negative and statistically significant, suggesting that *Forecast_Revision* is more sensitive to stock price information when analyst information is less accurate.

The negative and significant coefficient on *Return* × *Coverage* (e.g., in column 1 of Panel A) is consistent with Chen et al. (2007). Chen et al. (2007) interpret a similar result in their paper as suggesting that a large fraction of the information analysts have come from managers, and that the presence of analysts can attract more noise trading to the stock (Easley et al., 1998). However, column 3 of Panel A and column 4 of Panel B show that the coefficient on *Return* × *Coverage* loses statistical significance when *Book_to_Market* and its interactions are controlled for. Note that *Coverage* and *Book_to_Market* have a strong negative correlation (−0.30 in Panel B of Table 2). Hence a more plausible interpretation of the findings related to *Return* × *Coverage* is that analysts are a significant source of new information (e.g., Li et al., 2015) but this new information is more about growth opportunities (the earnings of which are not realized over the forecast period) and reduces the revision–return relation.

An additional finding is that the coefficient on *Quarterly_Error* × *Size* is negative and statistically significant, suggesting that larger firms are less sensitive to interim quarterly forecast errors.

#### 5.3.2. Controlling for the mechanical relation between prices and earnings

Prior accounting research suggests that prices lead earnings, i.e., information first gets impounded into stock prices before it is reflected in earnings (Kothari and Sloan, 1992). Of concern is the possibility that prices lead earnings to different extent for different firms, which can affect the link between forecast revisions and returns. Note that the extent to which prices lead earnings depends on price efficiency (the extent to which prices reflect all value relevant information), but not necessarily price informativeness (the amount of private information revealed by prices in equilibrium). Prices can be close to fundamentals from incorporating public information alone without reflecting any private information (Chen et al., 2014). As incorporation of investor information into stock prices takes time, it might be that stock prices with more investor information and less public information are further away from fundamentals (Chen et al., 2007).

To test whether prices lead earnings more for high *INFO* firms, I regress future earnings on lagged returns, lagged *INFO* and the interaction term between these two variables. In untabulated tests, I do not find evidence that the extent to which prices lead earnings is positively related to either *INFO* or orthogonalized *INFO* ($INFO^{\perp}$), where I orthogonalize *INFO* with respect to a set of control variables (i.e., *Size*, *Coverage*, *Analyst_Accuracy*, *Book_to_Market*, *Beta*, *D_Neg*, *Institution_Own*, *Turnover*, and |*Sentiment*|). Thus, the positive effect of *INFO* or $INFO^{\perp}$ on the revision–return relation is unlikely to be driven by the mechanical relation between prices and earnings.

[28] In Panel B of Table 6, the statistically insignificant coefficients on *Return* and *Analyst_Revision* are due to multi-collinearity: their variance inflation factors (*VIFs*) are both above 45 (much larger than the rule of thumb critical value of ten). The *VIFs* on other coefficients are below ten. These results reinforce the advantage of the two-stage approach.

**Table 6**
The effect of investor information on the revision–return relation: Alternative specifications.

Panel A: Controlling for each confound individually

| | Dependent variable: *Forecast_Revision* | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| *Var* = | (1) | (2) *Analyst_Accuracy* | (3) *Book_to_Market* | (4) *Beta* | (5) *D_Neg* | (6) *Horizon* | (7) *Institution_Own* | (8) *Turnover* | (9) *\|Sentiment\|* | (10) *Gap* |
| *Return* | 0.71*** | 0.92*** | 0.45* | 0.73*** | 0.39* | 0.53** | 0.81*** | 0.74*** | 0.69*** | 0.77*** |
| | (2.97) | (4.35) | (1.94) | (3.29) | (1.67) | (2.23) | (3.97) | (3.05) | (2.60) | (2.61) |
| *Analyst_Revision* | 0.23*** | 0.17*** | 0.24*** | 0.22*** | 0.22*** | 0.21*** | 0.21*** | 0.21*** | 0.22*** | 0.16** |
| | (4.75) | (3.66) | (4.57) | (3.66) | (3.95) | (4.36) | (4.03) | (3.75) | (4.19) | (2.51) |
| *Quarterly_Error* | 0.67*** | 0.66*** | 0.63*** | 0.63*** | 0.67*** | 0.70*** | 0.67*** | 0.71*** | 0.72*** | 0.65*** |
| | (6.44) | (6.08) | (6.41) | (5.06) | (6.59) | (6.03) | (6.52) | (5.51) | (7.59) | (6.00) |
| ***Return* × *INFO*** | **0.68*** ** | **0.61*** ** | **0.57** ** | **0.68*** ** | **0.71*** ** | **0.65*** ** | **0.65*** ** | **0.66*** ** | **0.65*** ** | **0.69*** ** |
| | **(2.79)** | **(2.72)** | **(2.26)** | **(2.82)** | **(2.99)** | **(2.67)** | **(2.82)** | **(2.88)** | **(2.80)** | **(3.12)** |
| *Analyst_Revision* × *INFO* | 0.03 | 0.04 | 0.03 | 0.03 | 0.02 | 0.02 | 0.03 | 0.03 | 0.02 | 0.01 |
| | (0.53) | (0.86) | (0.54) | (0.55) | (0.35) | (0.47) | (0.64) | (0.70) | (0.42) | (0.18) |
| *Quarterly_Error* × *INFO* | −0.09 | −0.08 | −0.09 | −0.08 | −0.08 | −0.08 | −0.09 | −0.10 | −0.08 | −0.08 |
| | (−0.88) | (−0.85) | (−0.90) | (−0.83) | (−0.79) | (−0.86) | (−0.90) | (−0.95) | (−0.75) | (−0.85) |
| *INFO* | −0.07*** | −0.07** | −0.05 | −0.07*** | −0.07*** | −0.07** | −0.06** | −0.07*** | −0.07*** | −0.07*** |
| | (−2.64) | (−2.46) | (−1.64) | (−2.73) | (−2.74) | (−2.50) | (−2.42) | (−2.89) | (−2.59) | (−2.75) |
| *Return* × *Size* | 0.23 | 0.17 | −0.09 | 0.23 | 0.24 | 0.22 | 0.25 | 0.21 | 0.23 | 0.23 |
| | (1.24) | (0.97) | (−0.47) | (1.22) | (1.27) | (1.20) | (1.31) | (1.14) | (1.28) | (1.29) |
| *Analyst_Revision* × *Size* | 0.25*** | 0.27*** | 0.25*** | 0.25*** | 0.24*** | 0.25*** | 0.24*** | 0.25*** | 0.25*** | 0.25*** |
| | (3.25) | (3.55) | (3.26) | (3.56) | (3.12) | (3.34) | (3.27) | (3.41) | (3.21) | (3.36) |
| *Quarterly_Error* × *Size* | −0.19* | −0.18* | −0.21* | −0.18* | −0.18* | −0.19* | −0.19* | −0.20** | −0.19* | −0.18* |
| | (−1.84) | (−1.82) | (−1.82) | (−1.70) | (−1.77) | (−1.90) | (−1.82) | (−1.98) | (−1.94) | (−1.67) |
| *Size* | 0.04 | 0.04 | 0.11*** | 0.03 | 0.01 | 0.04 | 0.03 | 0.04 | 0.04 | 0.03 |
| | (0.98) | (1.01) | (2.72) | (0.75) | (0.20) | (0.99) | (0.75) | (0.95) | (1.05) | (0.85) |
| *Return* × *Coverage* | −0.52*** | −0.36* | −0.20 | −0.52*** | −0.54*** | −0.55*** | −0.50*** | −0.50** | −0.53*** | −0.53*** |
| | (−2.78) | (−1.93) | (−1.15) | (−2.67) | (−2.70) | (−2.78) | (−2.63) | (−2.31) | (−2.79) | (−2.86) |
| *Analyst_Revision* × *Coverage* | 0.02 | −0.02 | 0.01 | 0.01 | 0.01 | 0.01 | 0.01 | 0.00 | 0.02 | 0.01 |
| | (0.29) | (−0.29) | (0.13) | (0.24) | (0.21) | (0.17) | (0.22) | (0.07) | (0.26) | (0.19) |
| *Quarterly_Error* × *Coverage* | −0.03 | −0.03 | −0.00 | −0.04 | −0.03 | −0.02 | −0.03 | 0.00 | −0.02 | −0.03 |
| | (−0.27) | (−0.31) | (−0.02) | (−0.32) | (−0.29) | (−0.16) | (−0.28) | (0.00) | (−0.19) | (−0.29) |
| *Coverage* | 0.13*** | 0.11*** | 0.05 | 0.13*** | 0.13*** | 0.13*** | 0.13*** | 0.13*** | 0.13*** | 0.13*** |
| | (2.78) | (2.76) | (1.39) | (2.77) | (2.79) | (2.69) | (2.72) | (2.77) | (2.78) | (2.78) |
| *Return* × *Var* | | −0.50*** | 0.65*** | −0.03 | 0.87*** | 0.42** | −0.23 | −0.05 | 0.09 | −0.10 |
| | | (−2.79) | (4.01) | (−0.20) | (6.76) | (2.49) | (−1.55) | (−0.20) | (0.63) | (−0.47) |
| *Analyst_Revision* × *Var* | | 0.14*** | −0.02 | 0.02 | 0.02 | 0.04 | 0.04 | 0.03 | 0.03 | 0.14*** |
| | | (3.77) | (−0.33) | (0.26) | (0.44) | (0.76) | (1.61) | (0.39) | (0.93) | (2.66) |
| *Quarterly_Error* × *Var* | | 0.04 | 0.07 | 0.05 | −0.01 | −0.06 | 0.00 | −0.07 | −0.12 | 0.01 |
| | | (0.69) | (0.93) | (0.89) | (−0.42) | (−0.81) | (0.02) | (−0.88) | (−1.46) | (0.17) |
| *Var* | | 0.05 | −0.18*** | −0.04 | 0.03 | 0.01 | 0.07*** | −0.00 | 0.08 | −0.07*** |
| | | (1.43) | (−5.24) | (−1.28) | (1.23) | (0.23) | (3.00) | (−0.02) | (1.13) | (−3.41) |
| Industry FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 15,977 | 15,977 | 15,977 | 15,977 | 15,977 | 15,977 | 15,977 | 15,977 | 15,977 | 15,977 |
| Adjusted $R^2$ | 38.5% | 38.7% | 38.9% | 38.6% | 38.8% | 38.6% | 38.6% | 38.5% | 38.6% | 38.8% |

Panel B: Controlling for all confounds jointly

| | Dependent variable: *Forecast_Revision* | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) |
| *Var* = | 1 | *INFO* | *Size* | *Coverage* | *Analyst_Accuracy* | *Book_to_Market* | *Beta* | *D_Neg* | *Horizon* | *Institution_Own* | *Turnover* | *\|Sentiment\|* | *Gap* |
| *Return* × *Var* | 0.26 | **0.46**$^{**}$ | −0.18 | −0.09 | −0.41$^{**}$ | 0.68$^{***}$ | −0.09 | 0.95$^{***}$ | 0.56$^{***}$ | −0.17 | −0.08 | −0.05 | −0.01 |
| | (0.86) | **(2.36)** | (−0.93) | (−0.39) | (−2.39) | (4.33) | (−0.64) | (6.87) | (3.34) | (−1.29) | (−0.38) | (−0.37) | (−0.05) |
| *Analyst_Revision* × *Var* | 0.01 | 0.03 | 0.27$^{***}$ | −0.07 | 0.12$^{***}$ | 0.00 | 0.03 | 0.01 | 0.06 | 0.03 | 0.04 | 0.02 | 0.15$^{***}$ |
| | (0.12) | (0.46) | (3.84) | (−1.21) | (3.09) | (0.09) | (0.43) | (0.19) | (1.25) | (1.38) | (0.71) | (0.51) | (3.25) |
| *Quarterly_Error* × *Var* | 0.66$^{***}$ | −0.07 | −0.20$^{*}$ | 0.05 | 0.04 | 0.07 | 0.09$^{*}$ | −0.01 | −0.06 | 0.03 | −0.10 | −0.11 | −0.01 |
| | (4.82) | (−0.72) | (−1.86) | (0.55) | (0.66) | (0.98) | (1.75) | (−0.17) | (−0.82) | (0.42) | (−1.39) | (−1.18) | (−0.12) |
| *Var* | | −0.03 | 0.06$^{*}$ | 0.05 | 0.00 | −0.19$^{***}$ | −0.01 | 0.03 | −0.02 | 0.06$^{***}$ | 0.00 | 0.09 | −0.04 |
| | | (−1.24) | (1.78) | (1.32) | (0.11) | (−5.70) | (−0.39) | (0.92) | (−0.55) | (3.39) | (0.07) | (1.31) | (−1.58) |
| Industry FE | Yes | | | | | | | | | | | | |
| Year FE | Yes | | | | | | | | | | | | |
| Observations | 15,977 | | | | | | | | | | | | |
| Adjusted $R^2$ | 39.8% | | | | | | | | | | | | |

This table presents the results of estimating augmented Eq. (1). Panel A presents the results by controlling for each confound individually, and Panel B presents the results by controlling for all confounds jointly. The sample period is from 1996 to 2010. All variables are defined in Appendix A. I use the decile rankings of *INFO*, *Size*, *Coverage*, *Analyst_Accuracy*, *Book_to_Market*, *Beta*, *Horizon*, *Institution_Own*, *Turnover*, *|Sentiment|* and *Gap* (rescaled to range from zero to one) to facilitate interpretation of the coefficients. The regression includes industry and year fixed effects. The *t*-statistics, presented in parentheses below the coefficients, are corrected for heteroskedasticity and cross-sectional and time-series correlations using a two-way cluster at the firm and year levels. $^{***}$, $^{**}$, and $^{*}$ denote significance at the 1%, 5%, and 10% levels for two-tailed tests, respectively.

To further control for this effect, I consider four variables. The first two variables, growth (proxied by *Book_to_Market*) and risk (proxied by *Beta*), are the standard determinants of earnings response coefficients (Collins and Kothari, 1989). The third variable, a dummy variable for negative *Return* (i.e., *D_Neg*), is motivated by the accounting conservatism literature (e.g., Basu, 1997; Watts, 2003a, 2003b). The last variable, *Horizon*, reflects the idea that price information has less effect on the forecast of earnings to be realized in the near future.[29]

The coefficients on *Return* × *Book_to_Market*, *Return* × *D_Neg*, and *Return* × *Horizon* are all positive and statistically significant at the 5% level or better. These results can be interpreted in the following way: information in prices for growth firms is less likely to be reflected in earnings that management is forecasting (with average (median) forecast horizon of 164 (156) days, see Panel A of Table 2), as it relates more to growth opportunities for which there would be no earnings effect in the near future; negative information in prices is more likely to be reflected in earnings that management is forecasting because of the asymmetric verifiability threshold required for economic gains versus economic losses (i.e., accounting conservatism); and information in prices is less relevant for earnings to be realized in the near future (e.g., next month).

*5.3.3. Controlling for the other confounds*

I control for two additional variables that may be correlated with *INFO* and may affect the revision–return relation: the percentage of shares held by institutional investors (*Institution_Own*), and stock market liquidity as proxied by the average daily share turnover (*Turnover*). The coefficient on *Return* × *Institution_Own* or *Return* × *Turnover* is statistically insignificant. I also control for investor sentiment (|*Sentiment*|) based on Baker and Wurgler (2006, 2007).[30] The insignificant coefficient on *Return* × |*Sentiment*| suggests that managerial catering to investor sentiment is not of first order concern in my analysis. Lastly, I control for the number of days between the revised forecast and the initial forecast (*Gap*) because the lag between the two forecast dates increases the probability that the correlation between *Return* and *Forecast_Revision* is simply due to managers' delay in revising their forecasts. The only statistically significant interaction term associated with *Gap* is *Analyst_Revision* × *Gap*, suggesting that controlling for contemporaneous public information (proxied by *Analyst_Revision*) helps to alleviate the concern related to the delay in management forecasting.

*5.4. The effect of prior investor information on subsequent market reaction*

My main analysis suggests that managers incorporate investors' private information in prices into their earnings forecasts. However, this finding does not imply that forecast revisions are *simply* based on information (the market already has) that managers learn and assemble into their forecasts. As discussed in Section 3.4, (unobservable) managerial private information plays an important role in forecast revisions and results in significant market reactions to forecast announcements. Forecast news to the public (i.e., information not captured in *C* as in Fig. 1) consists of two components: managers' private information (*M* as in Fig. 1) and investors' private information (*O* as in Fig. 1). Based on this decomposition, one prediction is that the sensitivity of the forecast announcement return to forecast news should be decreasing in *O* (as it has already been reflected in prices) and increasing in *M*. While intuitive, this prediction is difficult to test. To empirically test this prediction, I need proxies that capture the amounts of *O* and *M* in forecast news. Recall that *INFO* represents the probability of informed trading by outsiders and thus captures the amount of *O* in *Return*. However, a large amount of investor information in prices does not necessarily translate into a large amount of investor information in forecast news because managers' private information *M* is included in forecast news (but not in prices). Thus, it is not necessarily the case that a high *INFO* results in forecast news containing high *O* (and low *M*) and a weak market reaction to the forecast announcement. In untabulated test, I find that the market reaction to forecast news in managers' revision is lower when *INFO* is higher. But this result should be viewed as purely descriptive.

## 6. Conclusion

In this paper, I examine whether managers learn from investors' private information in prices and incorporate this information in their earnings forecasts. I use the extent of privately informed trading in the stock market (*INFO*) as an empirical proxy for the amount of investors' private information in stock prices. I find that managers rely more strongly on stock returns in revising their earnings forecasts when the amount of investor information in stock prices is higher. This effect remains after controlling for various confounds and is robust to the use of mutual fund redemptions (|*MFFlow*|) as a shock to price changes that is exogenous to fundamental news. In addition, the improvement in managers' forecast accuracy is positively related to the magnitude of contemporaneous stock returns; and, more importantly, this relation is stronger when *INFO* is higher or |*MFFlow*| is lower, suggesting that investor information helps managers improve their forecast accuracy. In sum, my study provides evidence that managers learn from private information in prices about their own firms'

[29] Consider an extreme case in which managers forecast earnings that will be realized tomorrow. In such a case, managers have almost perfect information about the predicted earnings and the information contained in stock prices has a much more limited effect on managers' predictions.

[30] The investor sentiment index in Baker and Wurgler (2006, 2007) is based on the first principal component of six (standardized) sentiment proxies, where each of the proxies has first been orthogonalized with respect to a set of macroeconomic conditions.

fundamentals when making earnings forecasts, and highlights the two-way information flows between firms and capital markets in the context of corporate disclosure.

An important question unexplored in this study is whether learning-in-forecasts affects the total amount of information available to the market. If uninformed investors can extrapolate information embedded in stock prices the same way as managers do (which is unlikely), then the total information they would obtain in the end is the same regardless of whether management forecasts incorporate investors' private information or not. However, it is important to note that even under this extreme scenario, learning-in-forecasts is still meaningful to investors because it can inform them that managers have observed investors' private information in prices. Management earnings forecasts are valuable to investors not only because they inform investors about management's expectations of future earnings, but also because they reveal information about managers' knowledge of the firm's economic environment and their ability to forecast future business prospects, a major component in the investment decision process (Trueman, 1986). Hence, learning-in-forecasts can give investors a more favorable assessment of managers' ability to anticipate economic environment changes and to adjust production plans accordingly, thereby translating into a higher firm market value.

More importantly, learning-in-forecasts can imply higher economic efficiency either because it reduces the duplicative efforts by investors to figure out the information in prices on their own, or because managers are better-equipped to extrapolate relevant information in prices than investors. Similar arguments have been made in Diamond (1985) in favor of public disclosure. By reducing investors' information processing costs, learning-in-forecasts provides an additional explanation for the empirical findings that disclosing management forecasts is associated with a decrease in information asymmetry and a reduction in the cost of raising equity capital (Coller and Yohn, 1997; Shroff et al., 2013). Disentangling how much of this capital market effect is due to learning-in-forecasts and how much is due to disclosing management's private information is an interesting avenue for future research.

## Appendix A. Variable definitions

| | |
|---|---|
| *Forecast_Revision* | $100 \times \left(MF_{i,t}^{d_2} - MF_{i,t}^{d_1}\right)/P_i^{d_1-2}$, where $MF_{i,t}^{d_2}$ is the earnings forecast released by firm $i$ on date $d_2$; $MF_{i,t}^{d_1}$ is the most recent earnings forecast (for the same earnings realization) released by firm $i$ prior to $MF_{i,t}^{d_2}$; and $P_i^{d_1-2}$ is the stock price two days before the issuance of $MF_{i,t}^{d_1}$. |
| *Return* | The buy-and-hold return of firm $i$ over the period from the day after the issuance of $MF_{i,t}^{d_1}$ to one day before the issuance of $MF_{i,t}^{d_2}$. |
| *Analyst_Revision* | $100 \times \left(AF_{i,t}^{d_2} - AF_{i,t}^{d_1}\right)/P_i^{d_1-2}$, where $AF_{i,t}^{d_2}$ ($AF_{i,t}^{d_1}$) is the consensus analyst forecast (for the same earnings realization as the management forecast) at the time when firm $i$ releases $MF_{i,t}^{d_2}(MF_{i,t}^{d_1})$; and $P_i^{d_1-2}$ is the stock price two days before the issuance of $MF_{i,t}^{d_1}$. |
| *Quarterly_Error* | $100 \times MQE_{i,t}^{d_3}/P_i^{d_1-2}$, where $MQE_{i,t}^{d_3}$ is the sum of management's quarterly forecast error associated with any quarterly earnings announcement that occurs between the two forecast dates (excluding the day of the issuance of $MF_{i,t}^{d_1}$ but including the day of the issuance of $MF_{i,t}^{d_2}$); and $P_i^{d_1-2}$ is the stock price two days before the issuance of $MF_{i,t}^{d_1}$. |
| *INFO* | Probability of informed trading net of all insider transactions, calculated as $PIN \times (1 - Insider)$ and measured over the year prior to $MF_{i,t}^{d_1}$, where *PIN* is the probability of informed trading, estimated following Easley et al. (2002), and *Insider* is the percentage of insider transactions to the total number of all transactions as recorded in TAQ. |
| *Size* | Total assets ($million) measured at the end of the most recent fiscal year prior to the issuance of $MF_{i,t}^{d_1}$. |
| *Coverage* | The number of analysts covering the firm immediately prior to the issuance of $MF_{i,t}^{d_1}$. |
| *Analyst_Accuracy* | $-100 \times$ \|*Analyst_Forecast – Actual Earnings*\|/*Price*, where *Actual Earnings* is the earnings of the most recent fiscal year prior to $MF_{i,t}^{d_1}$, *Analyst_Forecast* is the prevailing consensus analyst forecast of *Actual Earnings* as of the most recent fiscal year end, and *Price* is the stock price two days before the most recent fiscal year end. |
| *Book_to_Market* | The ratio of the book value of equity to the market value of equity, measured at the end of the most recent fiscal year prior to $MF_{i,t}^{d_1}$. |
| *Beta* | The market model systematic risk estimate obtained by regressing 60 monthly returns ending in the month prior to the issuance of $MF_{i,t}^{d_1}$ on the CRSP equally weighted return index. |
| *D_Neg* | A dummy variable that equals one for negative *Return*, and zero otherwise. |
| *Horizon* | The number of days between the date of $MF_{i,t}^{d_2}$ and the estimate period end date. |
| *Institution_Own* | Total common shares held by institutional investors divided by total common shares outstanding immediately prior to the issuance of $MF_{i,t}^{d_1}$. |
| *Turnover* | The average daily share turnover measured over the year prior to $MF_{i,t}^{d_1}$, where the daily share turnover is the daily volume of shares traded divided by the total number of shares outstanding, multiplied by 100. |
| \|*Sentiment*\| | The absolute value of the investor sentiment index from Baker and Wurgler (2006, 2007), measured over the year prior to $MF_{i,t}^{d_1}$. |
| *Gap* | The number of days between $MF_{i,t}^{d_1}$ and $MF_{i,t}^{d_2}$. |
| \|*MFFlow*\| | The absolute value of the price pressure created by mutual fund trading over the revision period, estimated following Edmans et al. (2012). |
| *ΔAccuracy* | $-100 \times \left(\left\|MF_{i,t}^{d_2} - Actual\ \ Earnings\right\| - \left\|MF_{i,t}^{d_1} - Actual\ \ Earnings\right\|\right)/P_i^{d_1-2}$, where $MF_{i,t}^{d_2}$ is the earnings forecast released by firm $i$ on date $d_2$; $MF_{i,t}^{d_1}$ is the most recent earnings forecast (for the same earnings realization) released by firm $i$ prior to $MF_{i,t}^{d_2}$; and $P_i^{d_1-2}$ is the stock price two days before the issuance of $MF_{i,t}^{d_1}$. |

## Appendix B. Standardized regression coefficients

| | Dependent variable: *Forecast_Revision* | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Return* | 0.30*** | | | 0.18*** |
| | (22.99) | | | (15.34) |
| *Analyst_Revision* | | 0.51*** | | 0.35*** |
| | | (67.56) | | (20.35) |
| *Quarterly_Error* | | | 0.47*** | 0.29*** |
| | | | (31.53) | (16.48) |
| Observations | 15,977 | 15,977 | 15,977 | 15,977 |
| $R^2$ | 9.0% | 26.1% | 21.7% | 36.7% |

This table presents the results of regressing standardized *Forecast_Revision* on standardized *Return*, *Analyst_Revision*, and *Quarterly_Error* individually and jointly. *Forecast_Revision*, *Return*, *Analyst_Revision*, and *Quarterly_Error* are standardized to have a mean of zero and a standard deviation of one. The *t*-statistics, presented in parentheses below the coefficients, are corrected for heteroskedasticity and cross-sectional and time-series correlations using a two-way cluster at the firm and year levels. ***, **, and * denote significance at the 1%, 5%, and 10% levels for two-tailed tests, respectively.

## Appendix C. Orthogonalizing *INFO* and |*MFFlow*|

| | (1) *INFO* | (2) \|*MFFlow*\| |
|---|---|---|
| *Size* | −1.46*** | −0.04*** |
| | (−11.70) | (−6.35) |
| *Coverage* | −1.09*** | −0.02 |
| | (−6.73) | (−0.94) |
| *Analyst_Accuracy* | −0.42*** | 0.02*** |
| | (−3.95) | (3.70) |
| *Book_to_Market* | 2.30*** | −0.02 |
| | (5.78) | (−0.49) |
| *Beta* | −0.32*** | −0.03** |
| | (−3.10) | (−2.43) |
| *D_Neg* | −0.10 | 0.02 |
| | (−1.45) | (1.17) |
| *Horizon* | −0.04 | −0.01 |
| | (−0.68) | (−0.45) |
| *Institution_Own* | −1.36*** | 0.13*** |
| | (−3.93) | (3.81) |
| *Turnover* | −1.19*** | −0.14*** |
| | (−8.00) | (−8.07) |
| \|*Sentiment*\| | 0.28 | −0.20 |
| | (1.11) | (−1.54) |
| *Gap* | 0.07 | 0.00 |
| | (0.80) | (0.06) |
| Industry FE | Yes | Yes |
| Year FE | Yes | Yes |
| Observations | 15,977 | 11,692 |
| Adjusted $R^2$ | 37.7% | 11.5% |

This table presents the results of estimating Eq. (2), i.e., orthogonalizing *INFO* and |*MFFlow*| on a set of control variables. All variables are defined in Appendix A. I use the natural logarithm of *Size*, *Coverage*, *Horizon* and *Gap*. For expositional purposes, all coefficients in column 1 are multiplied by 100. Both regressions include industry and year fixed effects. The *t*-statistics, presented in parentheses below the coefficients, are corrected for heteroskedasticity and cross-sectional and time-series correlations using a two-way cluster at the firm and year levels. ***, **, and * denote significance at the 1%, 5%, and 10% levels for two-tailed tests, respectively.

## References


Abarbanell, J.S., 1991. Do analysts' earnings forecasts incorporate information in prior stock prices changes? J. Account. Econ. 14, 147–165.
Ajinkya, B., Bhojraj, S., Sengupta, P., 2005. The association between outside directors, institutional investors, and the properties of management earnings forecasts. J. Account. Res. 43, 343–376.
Akins, B.K., Ng, J., Verdi, R.S., 2012. Investor competition over information and the pricing of information asymmetry. Account. Rev. 87, 35–58.
Anilowski, C., Feng, M., Skinner, D.J., 2007. Does earnings guidance affect market returns? The nature and information content of aggregate earnings guidance. J. Account. Econ. 44, 36–63.
Armstrong, C., Core, J., Taylor, D., Verrecchia, R., 2011. When does information asymmetry affect the cost of capital? J. Account. Res. 49, 1–40.
Badertscher, B.A., 2011. Overvaluation and the choice of alternative earnings management mechanisms. Account. Rev. 86, 1491–1518.
Baker, M., Wurgler, J., 2006. Investor sentiment and the cross-section of stock returns. J. Financ. 61, 1645–1680.
Baker, M., Wurgler, J., 2007. Investor sentiment in the stock market. J. Econ. Perspect. 21, 129–151.
Bakke, T.E., Whited, T.M., 2010. Which firms follow the market? An analysis of corporate investment decisions. Rev. Financ. Stud. 23, 1941–1980.
Basu, S., 1997. The conservatism principle and the asymmetric timeliness of earnings. J. Account. Econ. 24, 3–37.
Bergman, N.K., Roychowdhury, S., 2008. Investor sentiment and corporate disclosure. J. Account. Res. 46, 1057–1083.
Beyer, A., Cohen, D.A., Lys, T.Z., Walther, B.R., 2010. The financial reporting environment: Review of the recent literature. J. Account. Econ. 50, 296–343.
Bloomfield, R.J., 2002. The "Incomplete Revelation Hypothesis" and financial reporting. A. Horiz. 16, 233–243.
Bond, P., Edmans, A., Goldstein, I., 2012. The real effects of financial markets. Annu. Rev. Financ. Econ. 4, 339–360.
Brown, S., Hillegeist, S.A., 2007. How disclosure quality affects the level of information asymmetry. Rev. Account. Stud. 12, 443–477.
Chen, Q., Goldstein, I., Jiang, W., 2007. Price informativeness and investment sensitivity to stock price. Rev. Financ. Stud. 20, 619–650.
Chen, Q., Huang, Z., Zhang, Y., 2014. The effects of public information with asymmetrically informed short-horizon investors. J. Account. Res. 52, 635–669.
Chuk, E., Matsumoto, D., Miller, G.S., 2013. Assessing methods of identifying management forecasts: CIG vs. researcher collected. J. Account. Econ. 55, 23–42.
Clement, M.B., Hales, J., Xue, Y., 2011. Understanding analysts' use of stock returns and other analysts' revisions when forecasting earnings. J. Account. Econ. 51, 279–299.
Coller, M., Yohn, T., 1997. Management forecasts and information asymmetry: An examination of bid-ask spreads. J. Account. Res. 35, 181–191.
Collins, D.W., Kothari, S.P., 1989. An analysis of intertemporal and cross-sectional determinants of earnings response coefficients. J. Account. Econ. 11, 143–181.
Cotter, J., Tuna, I., Wysocki, P.D., 2006. Expectations management and beatable targets: How do analysts react to explicit earnings guidance? Contemp. Account. Res. 23, 593–624.
Diamond, D.W., 1985. Optimal release of information by firms. J. Financ. 40, 1071–1094.
Dichev, I.D., Graham, J.R., Harvey, C.R., Rajgopal, S., 2013. Earnings quality: Evidence from the field. J. Account. Econ. 56, 1–33.
Dow, J., Gorton, G., 1997. Stock market efficiency and economic efficiency: Is there a connection? J. Financ. 52, 1087–1129.
Duarte, J., Young, L., 2009. Why is *PIN* priced? J. Financ. Econ. 91, 119–138.
Durnev, A., Morck, R., Yeung, B., 2004. Value-enhancing capital budgeting and firm-specific return variation. J. Financ. 59, 65–105.
Dye, R.A., Sridhar, S.S., 2002. Resource allocation effects of price reactions to disclosures. Contemp. Account. Res. 19, 385–410.
Easley, D., Hvidkjaer, S., O'Hara, M., 2002. Is information risk a determinant of asset returns? J. Financ. 57, 2185–2221.
Easley, D., Kiefer, N., O'Hara, M., 1996. Cream-skimming or profit-sharing? The curious role of purchased order flow. J. Financ. 51, 811–833.
Easley, D., Kiefer, N., O'Hara, M., 1997a. The information content of the trading process. J. Empir. Financ. 4, 159–186.
Easley, D., Kiefer, N., O'Hara, M., 1997b. One day in the life of a very common stock. Rev. Financ. Stud. 10, 805–835.
Easley, D., O'Hara, M., 2004. Information and the cost of capital. J. Financ. 59, 1553–1583.
Easley, D., O'Hara, M., Paperman, J., 1998. Financial analysts and information-based trade. J. Financ. Mark. 1, 175–201.
Edmans, A., Goldstein, I., Jiang, W., 2012. The real effects of financial markets: The impact of prices on takeovers. J. Financ. 67, 933–971.
Edmans, A., Jayaraman, S., 2016. The source of information in prices and investment-price sensitivity. Working Paper.
Foucault, T., Fresard, L., 2012. Cross-listing, investment sensitivity to stock price and the learning hypothesis. Rev. Financ. Stud. 25, 3305–3350.
Foucault, T., Fresard, L., 2014. Learning from peers' stock prices and corporate investment. J. Financ. Econ. 111, 554–577.
Francis, J., Philbrick, D., Schipper, K., 1994. Shareholder litigation and corporate disclosures. J. Account. Res. 32, 137–164.
Gallemore, J., Labro, E., 2015. The importance of the internal information environment for tax avoidance. J Account. Econ. 60, 149–167.
Gao, P., Liang, P.J., 2013. Informational feedback, adverse selection, and optimal disclosure policy. J. Account.Res. 51, 1133–1158.
Glosten, L., Milgrom, P., 1985. Bid, ask, and transaction prices in a specialist market with heterogeneously informed traders. J. Financ. Econ. 14, 71–100.
Goodman, T.H., Neamtiu, M., Shroff, N., White, H.D., 2014. Management forecast quality and capital investment decisions. Account. Rev. 89, 331–365.
Gow, I.D., Ormazabal, G., Taylor, D.J., 2010. Correcting for cross-sectional and time-series dependence in accounting research. Account. Rev. 85, 483–512.
Grossman, S.J., Stiglitz, J.E., 1980. On the impossibility of informationally efficient markets. Am. Econ. Rev. 70, 393–408.
Gu, Z., Wu, J.S., 2003. Earnings skewness and analyst forecast bias. J. Account. Econ. 35, 5–29.
Hayek, F., 1945. The use of knowledge in society. Am. Econ. Rev. 35, 519–530.
Healy, P.M., Palepu, K.G., 2001. Information asymmetry, corporate disclosure, and the capital markets: A review of the empirical disclosure literature. J. Account. Econ. 31, 405–440.
Heitzman, S., Huang, M., 2016. Internal information quality and investment sensitivities to market value and cash flow. Working Paper.
Heitzman, S., Wasley, C., Zimmerman, J., 2010. The joint effects of materiality thresholds and voluntary disclosure incentives on firms' disclosure decisions. J. Account. Econ. 49, 109–132.
Hemmer, T., Labro, E., 2008. On the optimal relation between the properties of managerial and financial reporting systems. J. Account. Res. 46, 1209–1240.
Hutton, A., Lee, L.F., Shu, S., 2012. Do managers always know better? The relative accuracy of management and analyst forecasts. J. Account. Res. 50, 1217–1244.
Jennings, R., 1987. Unsystematic security price movements, management earnings forecasts, and revisions in consensus analyst earnings forecasts. J. Account. Res. 25, 90–110.
Kasznik, R., 1999. On the association between voluntary disclosure and earnings management. J. Account. Res. 37, 57–81.
Kothari, S.P., 2001. Capital markets research in accounting. J. Account. Econ. 31, 105–231.
Kothari, S.P., Shu, S., Wysocki, P.D., 2009. Do managers withhold bad news? J. Account. Res. 47, 241–276.
Kothari, S.P., Sloan, R., 1992. Information in prices about future earnings: Implications for earnings response coefficients. J. Account. Econ. 15, 143–171.
Kyle, A., 1985. Continuous auctions and insider trading. Econometrica 53, 1315–1336.
Lai, S., Ng, L., Zhang, B., 2014. Does *PIN* affect equity prices around the world? J. Financ. Econ. 114, 178–195.
Lee, C.M.C., So, E.C., 2015. Alphanomics: The informational underpinnings of market efficiency. Found. Trends Account. 9, 59–258.
Li, E., Ramesh, K., Shen, M., Wu, J.S., 2015. Do analyst stock recommendations piggyback on recent corporate news? An analysis of regular-hour and after-hour revisions. J. Account. Res. 53, 821–861.
Li, Y., Zhang, L., 2015. Short selling pressure, stock price behavior, and management forecast precision: Evidence from a natural experiment. J. Account. Res. 53, 79–117.
Loureiro, G., Taboada, A.G., 2015. Do improvements in the information environment enhance insiders' ability to learn from outsiders? J. Account. Res. 53, 863–905.
Luo, Y., 2005. Do insiders learn from outsiders? Evidence from mergers and acquisitions. J. Financ. 60, 1951–1982.
Lys, T., Sohn, P., 1990. The association between revisions of financial analysts' earnings forecasts and security price changes. J. Account. Econ. 13, 341–363.

Maffett, M., 2012. Financial reporting opacity and informed trading by international institutional investors. J. Account. Econ. 54, 201–220.
Matsumoto, D.A., 2002. Management's incentives to avoid negative earnings surprises. Account. Rev. 77, 483–514.
McNichols, M., 1989. Evidence of informational asymmetries from management earnings forecasts and stock returns. Account. Rev. 64, 1–27.
McNichols, M., Trueman, B., 1994. Public disclosure, private information collection, and short-term trading. Journal. Account. Econ. 17, 69–94.
Miller, G.S., Beyersdorfer, D., Sjoman, A., 2006. IR at BP: Investor relations and information reconnaissance. Harvard Business School Case 107-026.
Morck, R., Shleifer, A., Vishny, R.W., 1990. The stock market and investment: Is the market a sideshow? Brookings Pap. Econ. Act. 21, 157–216.
Morck, R., Yeung, B., Yu, W., 2000. The information content of stock markets: Why do emerging markets have synchronous stock price movements?. J. Financ. Econ. 58, 215–238.
Nikolaev, V.V., 2010. Debt covenants and accounting conservatism. J. Account. Res. 48, 137–175.
Patell, J.M., 1976. Corporate forecasts of earnings per share and stock price behavior: Empirical tests. Journal. Account. Res. 14, 246–276.
Petersen, M., 2009. Estimating standard errors in finance panel data sets: Comparing approaches. Rev. Financ. Stud. 22, 435–480.
Rajan, R.G., Zingales, L., 2003. Saving capitalism from the capitalists. Random House, New York.
Rogers, J.L., Skinner, D.J., Van Buskirk, A., 2009. Earnings guidance and market uncertainty. J. Account. Econ. 48, 90–109.
Rogers, J.L., Stocken, P.C., 2005. Credibility of management forecasts. Account. Rev. 80, 1233–1260.
Rogers, J.L., Van Buskirk, A., 2013. Bundled forecasts in empirical accounting research. J. Account. Econ. 55, 43–65.
Roll, R., 1984. Orange juice and weather. Am. Econ. Rev. 74, 861–880.
Roll, R., 1986. The hubris hypothesis of corporate takeovers. Journal. Bus. 59, 197–216.
Roll, R., 1988. Presidential address: $R^2$. J. Financ. 43, 540–566.
Shroff, N., 2014. Corporate investment and changes in GAAP. Working Paper.
Shroff, N., Sun, A.X., White, H.D., Zhang, W., 2013. Voluntary disclosure and information asymmetry: Evidence from the 2005 Securities Offering Reform. . Account. Res. 51, 1299–1345.
Simon, H.A., 1973. Applying information technology to organization design. Public Adm. Rev. 106, 467–482.
Sletten, E., 2012. The effect of stock price on discretionary disclosure. Rev. Account. Stud. 17, 96–133.
So, E.C., 2013. A new approach to predicting analyst forecast errors: Do investors overweight analyst forecasts? J. Financ. Econ. 108, 615–640.
Stocken, P.C., 2000. Credibility of voluntary disclosure. RAND J. Econ. 31, 359–374.
Subrahmanyam, A., Titman, S., 1999. The going public decision and the development of financial markets. J. Financ. 54, 1045–1082.
Trueman, B., 1986. Why do managers voluntarily release earnings forecasts? J. Account. Econ. 8, 53–72.
Verrecchia, R.E., 1982. Information acquisition in a noisy rational expectations economy. Econometrica 50, 1415–1430.
Watts, R.L., 2003a. Conservatism in accounting, Part I: Explanations and implications. Account. Horiz. 17, 207–221.
Watts, R.L., 2003b. Conservatism in accounting, Part II: Evidence and research opportunities. Account. Horiz. 17, 287–301.
Wolfers, J., Zitzewitz, E., 2004. Prediction markets. J. Econ. Perspect. 18, 107–126.