Learning and incentive: A study on analyst response to pension underfunding

Xuanjuan Chen, Tong Yao, Tong Yu, Ting Zhang

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Abstract
There is a long-standing debate on whether sell-side analysts learn from their experience to improve earnings forecast skills. This study shows that incentive is an important factor for understanding the “learning by doing” effect by analysts. We examine analysts’ response to a complex type of information – corporate pension underfunding. Pension underfunding negatively impacts future earnings and analysts on average underreact to such information in their earnings forecasts. More importantly, when there is a strong incentive for analysts to deliver accurate forecasts, analyst learning effectively reduces their underreaction to pension underfunding information. On the other hand, when such an incentive is absent, the analyst learning effect is not discernible in the data. Further evidence suggests that analyst learning and incentive jointly reduce stock market mispricing associated with corporate pension underfunding.

1. Introduction

A fundamental condition for the financial market to evolve toward greater efficiency is that the rationality and skills of market participants improve over time. This evolution can take place in two ways. The first is via market selection, or “survival of the fittest.” For example, noise traders are forced to quit after losing money, leaving only skillful investors in the market. The second mechanism is “learning by doing.” That is, market players can hone their skills by learning from their past trading experience or experience of performing other types of market activities. The profession of sell-side security analysts appears to be an attractive setting to examine these mechanisms, especially the learning-by-doing effect. A typical sell-side analyst enters the profession with a freshly minted MBA or a college degree; prior industry expertise is not required. But once on the job, analysts are expected to climb up a substantial learning curve and develop deep knowledge about the companies and industries they follow.

Empirical studies have so far provided mixed evidence on the effectiveness of analyst learning. The pioneering studies on this issue, e.g., Mikhail et al. (1997, 2003) and Clement (1999), use the firm-specific analyst experience – the time spent following a firm or the number of forecasts issued by an analyst – to quantify learning. They show that as analysts gain experience, the accuracy of their earnings forecasts improves and their underreaction to past earnings decreases. Conversely, Jacob et al. (1999) argue that experience may not be a pure measure of learning, as analysts with higher innate (learning-invariant) skills tend to have longer experience simply because such analysts survive longer in their jobs. They find no discernible effect of learning once using the analyst-firm fixed effects to control for the innate ability.

In this study, we provide a new perspective for understanding and evaluating the analyst learning effect. It is well-known that the analyst profession is ripe with conflicting incentives, driving analysts to deliver optimistic earnings forecasts and stock recommendations (see, e.g., Michaely and Womack, 1999; Abarbanell and Lehavy, 2003; Hong and Kacperczyk, 2011). The central question of our study is: how does incentive influence learning? The incentive issue has so far received little attention in the empirical studies on analyst learning; neither is it addressed in another active stream of literature that focuses on learning-by-doing.
among individual investors. However, unlike individual investors who trade to maximize their own profits, many financial market participants, including sell-side analysts, corporate executives, and portfolio managers, are agents performing delegated tasks. Our investigation therefore pertains to the broader issue of whether learning by these delegated agents can be evaluated independently from the incentives they face.

We examine the learning effect in a context that represents relatively complex information environment – analysts' response to information about underfunding of defined-benefit corporate pension plans when they make earnings forecasts. As noted in several existing studies (e.g., Picconi, 2006; Franzoni and Marin, 2006; Coronado et al., 2008), earnings forecasts for firms with underfunded pensions are difficult because of a combination of factors, including the complexity of pension accounting, the infrequent pension disclosure, and various direct and indirect ways pension funding affects earnings (detailed in Section 2). These studies also show that investors and analysts underreact to pension funding information when they value firms or forecast corporate earnings. Therefore, it is likely that analyst skills are important for parsing corporate pension funding information, and further, if learning improves analyst skills, the incentive should be easy to detect in such a context. Also relevant for the empirical design of this study, firms with pension plans tend to be large, with heavy institutional demand for analyst coverage, and active in capital market activities such as equity issuance, debt financing, and mergers and acquisitions. Hence, not only are these firms important for analysts' careers, but also analysts following them face a rich spectrum of incentives, making the incentive effect easy to detect. Finally, in the recent decade corporate pension underfunding is prevalent and the aggregate pension deficit has recently reached a staggering amount. Such a huge pension deficit affects subsequent corporate earnings and corporate valuation because of the mandatory pension contribution requirement and because of the important consequences on corporate financial decisions (e.g., Rauh, 2006). Assessing how effectively the financial market participants respond to such information is an important issue on its own right.

We analyze a large sample of publicly traded firms with defined-benefit pension plans during the period from 1989 to 2008. In the sample, pension underfunding is quite pervasive, as the proportion of firms with underfunded pension plans range from 32% to 92% across the years. To capture analyst response to freshly released pension information, we examine their annual earnings forecasts made within three months after the announcement of the prior year's earnings. We find that the higher the magnitude of pension underfunding, the more upward bias there is in analysts' earnings forecasts. This confirms that analyst forecasts on average underreact to corporate pension underfunding information. Following existing studies (Mikhail et al., 1997, 2003), we use the firm-specific analyst experience, which is the number of forecasts made by an analyst since the first time the analyst covers the firm, as a prima facie measure of learning. Based on a two-way sorting procedure, we show that underreaction to pension underfunding is substantially more prevalent among short-experience analysts relative to long-experience analysts. Therefore, experience attenuates analyst underreaction.

Existing studies have documented various forms of conflicting incentives that drive analysts to provide optimistic coverage, such as maintaining or developing investment banking business, generating trading commissions, and gaining access to corporate management for better information. However, there has been no comprehensive measure of an analyst's overall incentive so far. Instead of going after individual forms of competing interests, we analyze an incentive mechanism that represents the demand by institutional investors for accurate and unbiased analyst research. This effect is documented by Ljungqvist et al. (2007), in which the authors point out that because sell-side analysts depend on institutional investors for performance ratings and trading commissions, analysts are less likely to succumb to investment banking or brokerage pressure when there is a strong presence of institutional investors. Motivated by their study, we measure "investor discipline" by the fraction of institutional ownership, orthogonalized with characteristic variables related to a firm's information environment, including firm size, firm age, and stock return synchronicity. We find that investor discipline reduces analyst forecast bias associated with pension underfunding. More importantly, there is a strong interaction between the effect of experience and that of incentive. Analyst experience significantly reduces underreaction to pension underfunding among firms with high investor discipline, but insignificantly so when this incentive measure is weak.

Jacob et al. (1999) argue that analyst experience may not be a pure measure of learning, as analysts with higher innate (learning-invariant) skills tend to have longer experience simply because these analysts survive longer in their jobs. To address this issue, we use a modified Heckman selection model to control for the effect of analyst survival. In the first stage, we estimate a Probit model to determine analyst survival. Then in the second-stage regression, we examine the relation between experience and underreaction to pension underfunding by including control variables related to analyst survival estimated from the first stage. We find that under strong investor discipline, controlling for analyst survival does not explain away the effect of experience in reducing forecast bias associated with pension underfunding. On the other hand, when investor discipline is weak, experience is not significantly related to forecast bias reduction with or without the control of analyst survival.

We use an alternative measure of analyst experience based on the number of earnings forecasts an analyst has made on a firm, and obtain similar results. We are also able to detect the effect of analyst learning using an alternative test that does not suffer from the complication of the analyst experience measure. We look at two groups of firms – those suffering from pension underfunding for the first time and those repeatedly underfunding their pensions. The upward analyst forecast bias associated with pension underfunding is substantially reduced for repeated underfunders than for the first-timers, suggesting that analysts (regardless of their experience) learn from firms' past underfunding situations. Further, investor discipline reduces analyst underreaction by a larger extent for repeated underfunders than for first-timers.

Finally, we investigate whether the effect of analyst learning and incentive on forecast bias translates into reduced stock market

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1 See, e.g., Feng and Seasholes (2005), Dhar and Zhu (2006), Nicolosi et al. (2009), and Seru et al. (2010). It is interesting to note that a prominent issue in the literature on individuals' learning by trading echoes that in the analyst learning literature. Seru et al. (2010) find that investors' trading skill improvement attributable to learning is modest once controlling for the attrition of investors with poor trading skills. Essentially, the results of both Jacob et al. (1999) and Seru et al. (2010) can be interpreted as a strong effect of "survival of the fittest," but a weak or non-existent effect of learning in the financial market.

2 For example, according to our calculation illustrated in Fig. 1, the aggregate corporate pension deficit of U.S. public companies is over $400 billion in 2008.

3 We try an alternative measure of analyst experience – the number of forecasts made by an analyst since the first time the analyst covers the firm. The result remains to hold.

4 Our estimation approach is similar to that of Seru et al. (2010), who use a selection model to examine the learning-by-trading effect of individual investors by controlling for the likelihood that investors quit trading after realizing that they have poor trading skills.
mispricing. Franzoni and Marín (2006) report a notable stock market anomaly related to pension underfunding – firms with severely underfunded pensions have abnormally low future stock returns, suggesting market overvaluation of these firms. Consistent with their finding, we detect a negative relation between the magnitude of pension underfunding and subsequent stock returns in our sample. Moreover, with and without the control for analyst survival, given the magnitude of pension underfunding, firms covered by more experienced analysts tend to outperform those covered by less experienced analysts. This effect is significant when investor discipline is strong but insignificant when investor discipline is absent. Therefore, analyst learning and its interaction with incentive improve market efficiency.

Our study contributes to the existing literature by showing that incentives significantly affect analysts’ (under-)reaction to pension underfunding when they make earnings forecasts. More generally, our results suggest that incentives are important in detecting and measuring the learning effect by delegated agents in the financial market. Finally, our evidence that analyst incentives are related to stock returns for firms with underfunded pensions helps understand the stock market anomaly documented by Franzoni and Marín (2006).

The rest of the paper is organized as follows. Section 2 discusses the complexity of corporate pension information and related studies. Section 3 describes the data and sample. Empirical results are documented in Section 4. Section 5 summarizes the main contributions of this study.

2. Underreaction to pension underfunding information

Defined-benefit corporate pension plans represent firms’ obligations to employees in the form of post-retirement financial benefits determined primarily by the years of service and salaries. The Employee Retirement Income Security Act (ERISA) of 1974 requires firms to set aside a certain amount of assets to meet their pension obligations. Pension liabilities are regarded as an integral part of a firm’s financial liabilities (Treynor, 1977; Bodie et al., 1985; Friedman, 1983; Jin et al., 2006). A pension plan is considered to be underfunded if the value of pension assets drops below the present value of projected future pension obligations.

For two reasons, market participants may underreact to corporate pension funding information when they forecast firms’ future cash flows or value stocks. The first is due to the complexity of pension accounting, and the second is due to the difficulty of fully grasping the economic impact of pension underfunding. Both are associated with analysts’ limited ability or their cognitive bias in understanding pension information. Below, we discuss these two issues and related studies that span the accounting, finance, and economics literature.

2.1. Under-reaction due to complexity of pension accounting

During most of our sample period pension accounting was governed by the Statement No. 87 of Financial Accounting Standard Board (SFAS 87), which took effect in 1986 (FASB, 1985). SFAS 87 only requires that firms include prepaid pension costs/accrued pension costs and additional minimum liability on the face of balance sheet. The most relevant information to determine pension funding status, such as the value of pension assets and the present value of projected pension obligations, is buried in managerial notes and disclosed only annually. The pension assets and liability reported in the balance sheet are actuarially smoothed values, typically different from the numbers reported in the footnotes. In other words, the balance sheet and footnotes could provide different or even conflicting information on pension assets and liabilities and on the funding status of pension plans. The analysis of pension accounting value relevancy and the potential misleading effect of pension accounting can be found in several existing studies, such as Picconi (2006), Coronado and Sharpe (2003), Hann et al. (2007), Hann et al. (2007), Coronado et al. (2008), Shivdasani and Stefanescu (2009), and Comprix and Muller (2011). Fig. 1 illustrates that during the majority of the sample years, pension funding surplus/deficit revealed by the footnotes significantly deviates from that indicated by the balance sheet. In particular, based on the footnotes, on average firms are substantially underfunded in years after 2001. In contrast, based on the balance sheets, firms have a pension surplus in 2001 and 2002, and a slight pension deficit in the following three years. If, due to either limited attention or inexperience, investors and analysts focus on financial statements and ignore the footnotes, they could make incorrect conclusions about the funding status of pension plans.

Even if analysts pay attention to the footnote information about pension funding status, it may still not be easy for them to predict the impact on future earnings. This is because under SFAS 87 income statements would not instantly reflect the accurate pension expense information owing to the smoothing mechanism in the recognition of pension-related expenses. When recognizing the pension expenses in the income statement, firms do not directly record the cash contribution to pension plans as an expense. Rather, net periodic pension costs are used as pension expenses, which are the sum of service costs, interest costs, and other costs, after deducting expected returns on plan assets (see Appendix A for details). Under such accounting treatment on pension expenses, pension underfunding has a delayed effect on corporate earnings. As a consequence, if analysts are not aware that pension expenses may be underestimated in current income statements, their forecasts of future earnings could be optimistically biased.

In September 2006, the Financial Accounting Standards Board (FASB) changed the accounting rule for pension funding by releasing Statement 158 (effective for fiscal years after December 15, 2006),
which requires firms to recognize pension funding status in the balance sheet (FASB, 2006). SFAS 158 is the outcome of Phase I of FASB’s long-term project to reconsider all aspects of pension and other post-retirement benefit accounting, with the objective to provide useful and relevant pension accounting information to investors and creditors. However, SFAS 158 does not change the accounting treatment of pension expenses in the income statements. Thus, under the new pension accounting regime, investors and analysts may continue to underreact to pension funding information.²

2.2. Under-reaction due to economic consequences of pension underfunding

Pension underfunding has a direct impact on future earnings due to the mandatory pension contribution requirement. For a plan that is less than 90% funded, ERISA requires the sponsoring firm to make additional contributions to the plan to reduce the funding deficits within three to five years.³ The discretion firms have in terms of the timing of mandatory pension contributions increases the difficulty for outsiders to assess the impact of pension underfunding on future earnings. In addition to the direct impact of mandatory pension contributions, there are several other economic consequences of pension underfunding. Such consequences are even more difficult for market participants to quantify or anticipate.

First, pension underfunding has negative effects on employees’ work incentives and productivity. A number of studies, such as Lazear (1979, 1983) and Hutchens (1986, 1987, 1989), have shown that underfunded pensions could potentially reduce workers’ efforts and productivity, increase the turnover rate, and reduce productivity.

Further, pension underfunding may distort a firm’s investment decisions, for example, by causing underinvestment. This is because, first, facing financial constraints, firms may have to cut down other expenditures to make mandatory pension contributions. Second, under ERISA, minimum pension contributions have a claim equal to that of a federal tax lien; that is, minimum contributions are senior to debentures, bank loans, and the claims of other corporate creditors (Martin and Henderson, 1983). Unfunded pension liabilities are generally treated as unsecured bond. The underfunded firms therefore may face a serious “pension obligation overhang” issue similar to debt overhang (Myers, 1977; Lamont, 1995). Pension obligation overhang occurs when required pension contributions caused by pension underfunding deter new investments and capital expenditures because the benefits from new investments will go to the pension beneficiaries, not to the new investors. Rauh (2006) estimates that for every dollar of mandatory contribution to underfunded pension plans, a firm is forced to reduce its capital expenditure by $0.60–$0.70. Firms’ R&D investments and acquisition decisions could be similarly affected.

In addition, pension underfunding could decrease a firm’s debt ratings and thus increase a firm’s cost of debt capital, negatively affecting a firm’s earnings. Martin and Henderson (1983), Maher (1987), and Carroll and Niehaus (1998) find that underfunded firms have lower bond ratings. In particular, Carroll and Niehaus (1998) show that underfunded pension liabilities decrease ratings more than an equal amount of excess pension assets would increase ratings.

3. Data and sample

3.1. Pension plans and analyst earnings forecasts data

We obtain defined-benefit pension information and other financial information from Compustat. Stock return data are from CRSP. The institutional ownership data are from the 13F database of Thomson Reuters. Analyst earnings forecasts are from IBES.

We start with all firms that sponsor a defined benefit pension plan and are in the intersection of CRSP, Compustat, and IBES databases from 1989 to 2008. The sample starts in 1989 so that all sample firms comply with SFAS 87, which took effect on firms with fiscal years ending after December 15, 1986. A firm is identified as the sponsor of pension plans if it has pension assets and obligations reported in Compustat.

We further apply the following sample selection criteria. First, a firm must have at least two years of accounting data available in Compustat, in order to alleviate the selection bias induced by the way Compustat constructs data (Banz and Breen, 1986). Second, to minimize market microstructure issues, stock price must be higher than $1 at the end of the fiscal year. Third, there must be available information in Compustat and CRSP to estimate book value of equity, accruals, and past six-month stock returns. Finally, as our purpose is to examine how analysts respond to pension information, we require at least one earnings forecast made by individual analysts during the three months after the announcement of prior-year’s earnings. Our final sample has 161,686 analyst-firm-year observations.

3.2. Pension funding ratio

The following notation is used throughout the paper. In any given calendar year t, Y0 refers to the fiscal year of a firm with earnings reported in year t. Y1 refers to the fiscal year that is reported in year t + 1.

Our measure of pension funding status, the pension funding ratio, follows Franzoni and Marín (2006). In a given calendar year t, the pension funding ratio for a firm is the pension surplus (deficit) scaled by the firm’s market capitalization:

\[ FR_{it} = \frac{FVPA_{it} - PBO_{it}}{\text{Market Cap}_{it}} \] (1)

where FVPA is the fair value of plan assets and PBO is the present value of pension obligations, for the fiscal year Y0. FVPA is the sum of overfunded pension plan assets (Compustat data PPLAO) and underfunded pension plan assets (PPLAU). PBO is the sum of overfunded pension obligations (PBPPO) and underfunded pension obligations (PBPRU). Note that the above data are from the footnotes of financial statements (obtained from Compustat). Market capitalization is measured at the end of fiscal year Y0. To alleviate the influence of outliers, in each year we winsorize the top and bottom 1% of the pension funding ratios across all firms in the sample. A firm has pension overfunding (OF) if its pension funding ratio is zero or positive, and a firm has pension underfunding (UF) if the pension funding ratio is negative.

³ In addition, the real economic impact of pension underfunding, to be discussed below, is not altered by accounting changes. By inspecting the subperiod results, we find that the passage of SFAS 158 has not significantly altered our conclusions on how analysts respond to pension underfunding information.

⁴ If a plan is over 80% funded today and was more than 90% funded for the past two years, the additional contribution requirement is waived. However, the Pension Equity Act 2008 sets new funding targets. Beginning in 2008, sponsors are required to fund to 100% of all liabilities (including lump sum distributions and early retirement benefits) accrued to participants and beneficiaries within seven years. It should be further noted that the Pension Relief Bill passed in 2008 provides pension funding relief for plan sponsors affected by the economic downturn, by allowing underfunded firms to fund their plans over an extended period.

⁵ The results reported in this paper do not substantially change if we change the denominator of the pension funding ratio from market capitalization to total assets.
3.3. Measuring analyst forecast error, experience, and incentive

Existing studies on analyst learning have generally focused on the accuracy of analyst forecast, i.e., based on the absolute value of analyst forecast error. Analyst forecasts that are either too low or too high relative to realized earnings are considered inaccurate. In this study, we are interested in analysts’ response to pension underfunding information. Pension underfunding has a directional impact on future earnings; that is, the higher the magnitude of pension underfunding, the lower the future earnings. Accordingly, underreaction to pension underfunding information is measured in this paper by a positive relation between the magnitude of pension underfunding and analyst forecast error.

In each calendar year t, we first identify a firm’s announcement date of Y0 earnings. Then, we obtain individual analysts’ earnings forecasts for the coming fiscal year Y1 from the IBES unadjusted detail file, made within three months after the announcement date of Y0. We focus on this three-month window because of our interest in analysts’ responses to freshly released pension information. If during three months there are multiple earnings forecasts by the same analyst on the same firm, we only keep the last one. We calculate the earnings forecast error (FCE) as the difference between forecasted earnings per share (EPS) and actual EPS (both from IBES), scaled by the stock price at the end of Y0 (split-adjusted to have the share basis as of the time of the earnings forecast). Specifically, forecast error by analyst j on firm i for fiscal year Y1 is:

\[ FCE_{ij,t+1} = \frac{\text{Forecasted EPS}_{ij,t+1} - \text{Actual EPS}_{ij,t+1}}{\text{Stock Price}_i} \]  

(2)

The central measure of analyst learning in previous studies is an analyst’s firm-specific experience; see, e.g., Mikhail et al. (1997, 2003) and Clement (1999). We follow these studies to construct a variable EXP_{ij,t}, as the number of quarters (reported in the unit of years in the paper), prior to the time of measuring forecast error FCE, for which analyst j has issued earnings forecasts for firm i. Note that individual analyst forecast data in IBES are sparse before 1983. If an analyst’s first earnings forecast in IBES is in 1983, we have to assume this is indeed his/her first forecast on that firm. Of course, this is less of an issue since our pension sample starts in 1989.

It is possible that the analyst experience measure based on analyst working experience does not guarantee an analyst with longer time following a firm makes more earnings forecasts than the other one with shorter time. To address this concern, we alternatively measure analyst experience with the number of forecasts made by an analyst on a particular firm since an individual analyst first time covers the firm.

Moreover, a task-specific, measure of experience is recently analyzed by Clement et al. (2007). They show that past experience in covering firms incurring restructuring charges helps analysts improve their forecast accuracy when subsequently covering similar firms. For most of the analysis in this study we focus on the more general measure of firm-specific experience. However, in Section 4.5.2, when we rely on repeated pension underfunding to detect the learning effect, our learning measure has the spirit of the task-specific experience.

Finally, our measure of analyst incentive is motivated by Ljungqvist et al. (2007). They document a demand-side disciplinary effect; that is, the demand from institutional investor clients for accurate and unbiased equity research counterbalances various forms of pressure for optimistic coverage. They argue that since sell-side analysts depend on institutional investors for performance ratings and trading commissions, analysts are less likely to succumb to investment banking or brokerage pressure when demand by institutional investors is strong. Following their study, we define institutional ownership, IO, as the percentage of shares outstanding owned by institutional investors, based on the 13F data at the end of Y0.

Recent studies have documented that institutional investors have preference over firms with certain characteristics, such as size, liquidity, book-to-market ratio, past returns, dividend yield, firm age, and S&P 500 membership (e.g., Gompers and Metrick, 2001). A concern we have is that the relation between institutional ownership and analyst forecasting performance is not only due to the disciplinary effect of institutional investors, but also due to institutions’ preference for firms with more transparent information environment (whose earnings are easy to forecast). To control for this preference effect, we further orthogonalize institutional ownership with proxies for a firm’s informational environment, including firm size (log market cap end of fiscal year Y0), firm’s age as a public company (years since first appearing in Compustat), and stock return synchronicity. Stock return synchronicity is the R-square of regressing weekly stock returns onto CRPS value-weighted index returns, using data during the 12 months prior to the end of Y0. Prior studies, such as Morck et al. (2000) and Durnev et al. (2004), have argued that return synchronicity is inversely related to the amount of information incorporated into stock prices. The relation between institutional ownership and stock return synchronicity is documented by Piotroski and Roulstone (2004).

The orthogonalization is via annual cross-sectional regressions, and our incentive measure, “investor discipline” (DISC), is the estimated residual of the regression:

\[ IO_{i,t} = \beta_0 + \beta_1 \times SIZE_{i,t} + \beta_2 \times AGE_{i,t} + \beta_3 \times R2_{i,t} + \epsilon_{i,t} \]  

(3)

where i represents the i-th firm; t is for fiscal year t (Y0); IO represents institutional ownership of a firm; SIZE is the logarithm of market value of equity; AGE is firm age; R2 is the R-square of regressing weekly stock returns onto CRPS value-weighted index returns; \( \epsilon \) is the regression residual.

3.4. Sample descriptive statistics

We report summary statistics on the pension firm sample and analyst experience in Table 1. Panel A shows the number and proportion of defined-benefit (DB) firms and underfunded firms in the sample. Among all the firms in the CRSP-Compustat universe, the number of DB pension plan sponsors decreases from 1579 (38.91% of the sample firms) in 1989 to 1278 (29.94%) in 2008. Pension underfunding is quite pervasive and the proportion of firms with underfunded pensions in all DB-plan firms generally increases over the sample period – they account for 34.64% of all DB firms in 1989, and 82.86% in 2008. In 2002 the percentage of firms with underfunded pensions exceeds 92%.

In Panel B we compare financial characteristics of DB firms against non-DB firms. DB firms are larger and older than non-DB firms. For example, the average book value of total assets are respectively $15.09 billion and $2.33 billion for DB firms and non-DB firms. The average firm age for DB firms is 25.29 years while it is only 11.70 years for non-DB firms. There are also more analysts covering DB firms – there are on average 10.97 analysts covering one DB firm, while there are 6.25 analysts for each non-DB firm.

In Panel C, we compare overfunded (OF) firms with those underfunded (UF). OF firms are much larger than UF firms and hold more pension assets and liabilities. The average book value of total assets is USD26.43 billion for firms with an overfunding while it is...
USD12.27 billion for firms with an underfunding. The average pension assets (pension liabilities) for overfunded firms are USD1.58 billion (1.36 billion) while it is USD0.69 billion (0.87 billion) for underfunded firms.

Panel D reports descriptive statistics of analyst forecast experience and institutional ownership, two key measures in this study. The average firm-specific analyst experience for DB firms is 3.91 years, while it is 2.42 years for non-DB firms. Within DB firms, the average analyst experience is 4.16 years for overfunded firms, slightly higher than the average of 3.78 years for underfunded firms. Note that the standard deviation of analyst experience is also larger for DB firms – the standard deviation for DB firms is 1.05 years while that for non-DB firms is 0.87 year. The mean percentage of institutional ownership is 50.31% for DB firms and
35.94% for non-DB firms. Covering firms with heavy institutional ownership is important for analyst career and reputation, as institutional investors are the ultimate arbiter of analyst reputation (Ljungqvist et al., 2007). Thus, it seems that DB firms have a higher demand for analyst coverage from brokerage houses’ investor clients than non-DB firms.

4. Empirical results

4.1. Pension underfunding and analyst forecast errors

We sort all firms with pension underfunding into decile portfolios based on pension funding ratio FR, where FR is defined in Eq. (1) and calculated using financial information of the fiscal year t. The bottom decile (D1) includes the least underfunded firms and the top decile (D10) includes the least underfunded firms. For comparison purpose, we also breakdown overfunded firms into decile groups based on their pension funding ratios. In this case, the bottom decile (D1) includes the least overfunded firms and the top decile (D10) includes the most overfunded firms. We average all earnings forecast error (FCE) observations for the decile groups, and calculate the differences in average FCE between the extreme deciles separately for under- and overfunded firms over sample years. FCE is calculated based on earnings forecasts for the fiscal year t + 1. To control for serial correlations, the Newey–West procedure with a 2-year lag is used to compute the t-statistics of the time series average.

Table 2 presents the results. Shown in Panel A when the full sample period is included, there is a positive relation between the magnitude of underfunding and analyst forecast errors. The D1 portfolio, consisting of most underfunded firms, exhibits the largest positive forecast error of 3.21%, while the D10 portfolio, consisting of the least underfunded firms, has an average forecast error of 0.23%. The difference of 2.98% between the extreme groups is significant at the 1% level. Recall that a positive forecast error indicates optimistic forecast bias. The result is consistent with the well-documented pattern that analysts are overly optimistic with their earnings forecasts. Moreover, analysts on average tend to be more optimistic about firms with more underfunded pension plans. In other words, analysts underreact to pension underfunding information, especially for the most underfunded firms.

Conversely, across firms with overfunded pensions, the difference in the average FCEs between D10 and D1 groups is not statistically significant.9 The differential pension funding effects on analysts’ forecast errors are consistent with our expectation that the effect of pension funding on future earnings is asymmetric. While pension underfunding may generate a negative impact, pension overfunding may not necessarily have a positive impact, and analysts may not react to pension overfunding in the same way as they react to pension underfunding.

Further in Table 2, we report the differences of the average forecast errors between the most and least underfunded firms after 2000 and after 2005 in Panels B and C. These two subsample periods are chosen because pension underfunding is better publicized in years after the burst of the internet bubble in 2000, and because the new pension regulation SFAS 158 which requires firms to report market value of pension assets and liabilities in the balance sheet becomes effective after 2005. The differences of forecast errors between the most and least underfunded firms are 3.10% and 3.06% after 2000 and 2005, respectively. It appears that the greater publicity of the pension underfunding concern and increased disclosure of pension funding status do not help alleviate forecast errors made by analysts.

4.2. Effect of forecast experience on analyst underreaction

Next, we turn to the effect of analyst experience. Experience may help alleviate analyst underreaction to pension underfunding for two reasons related to the causes of such underreaction. First, experience can help an analyst develop an understanding of the importance of corporate pensions on a firm’s earnings and valuation, and thus help the analyst pay attention to the footnote information about pension funding status. Second, a more experienced analyst may better understand a firm’s investment plans and thus help the analyst pay attention to the footnote information. We use a sequential double-sorting procedure here. In each year t, we first sort firms into 11 portfolios based on the pension funding ratio (FR). In particular, all underfunded firms are sorted into decile portfolios, with D1 containing the least underfunded firms and D10 the least underfunded firms. All overfunded firms are grouped into D1 portfolio. Then, within each portfolio, we sort all FCE observations into 4 quartiles based on analyst experience

### Table 2: Pension funding ratio and analyst forecast errors.

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<td><strong>Panel A: Entire sample period</strong></td>
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<td>5</td>
<td>1380</td>
<td>0.79</td>
</tr>
<tr>
<td>6</td>
<td>1385</td>
<td>0.61</td>
</tr>
<tr>
<td>7</td>
<td>1381</td>
<td>0.58</td>
</tr>
<tr>
<td>8</td>
<td>1382</td>
<td>0.56</td>
</tr>
<tr>
<td>9</td>
<td>1384</td>
<td>0.48</td>
</tr>
<tr>
<td>10</td>
<td>1372</td>
<td>0.23</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>(t-Stat)</th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>D1–D10</td>
<td>2.98</td>
<td>0.36</td>
<td>-0.36</td>
<td>-0.73</td>
</tr>
</tbody>
</table>

This table reports the average earnings forecast errors (FCEs) for the next fiscal year across portfolios sorted on pension funding ratios for under-funded and over-funded firms respectively. Analyst forecast errors are the difference between the forecasted earnings and the actual earnings of the forecasted firm. Specifically, the FCE by analyst j on firm i for fiscal year t + 1 is: FCE_i,t+1 = StockPrice_i,t+1 / MarketCap_i,t / StockPrice_i,t / MarketCap_i,t - ActualEPS_i,t / ActualEPS_i,t. A firm’s pension funding ratio is the pension surplus (deficit) scaled by the firm’s market capitalization: FR_i,t = (FVPA - PBO) / MarketCap_i,t, where FVPA is the fair value of plan assets and PBO is the present value of pension obligations, for the fiscal year t. Under- and overfunded firms are sorted into decile portfolios based on pension funding ratios (FR). The D1 firms have the most underfunding or the least overfunding while the D10 firms have the least underfunding or the most overfunding. The average number of firms and the average FCEs, first averaged across firms then over time, are reported. In addition, the differences in the average FCEs between D1 and D10 groups for the full sample period (in Panel A), the subsample period after 2000 (in Panel B), and the subsample period after 2005 (in Panel C) are also reported. The t-statistics in the parentheses are computed using the Newey–West procedure with a 2-year lag. The full sample period is from 1989 to 2008. Forecast errors are in percentage points.

* Significance level at 10%.
** Significance level at 5%.
*** Significance level at 1%.

9 It is interesting to note that the average forecast error for overfunded firms is greater than that of the least underfunded group (D10). The pattern of forecast error for overfunded firms is likely due to different economic mechanisms, such as pension-related earnings manipulations by firms or analysts’ excessive extrapolation from past performance. We do not investigate such effects in this study.
analysts typically make late moves in their forecasts, and in this
way they have more information to formulate their forecasts. To
look into this possibility, we assess the timing of analysts’ forecasts
(TIMING), measured as the percentile rank of the forecast time for the
last forecast made by each analyst within three months after the
earnings announcements of a given firm. We perform the same two-way sorts as we did in Panel A and report the average timeliness measure for each portfolio. The t-statistics in parentheses are computed using the Newey–West procedure with a 2-year lag. The full sample period is from 1989 to 2008. Forecast errors are in percentage
points.

\* Significance level at 10%.
\*\* Significance level at 5%.
\*\*\* Significance level at 1%.

The results are presented in Panel A of Table 3. Holding firm
pension funding ratios constant, analyst forecast errors generally
decrease as the rank of analyst experience increases. But the differ-
ence in FCE across EXP quintiles is most striking among firms with
severe pension underfunding. For example, among the most under-
funded firms (D1), the average forecast errors are 3.93% for the
least experienced analysts (Q1) and 2.15% for the most experienced
analysts (Q4), with a significant difference of 1.78%. Such a
difference reinforces Hong et al. (2000) where they find inexperienced
analysts are less likely to issue timely forecasts. Our finding does
not support the conjecture that experienced analysts make more
timely forecasts than inexperienced analysts regardless of which FR groups firms belong to. For instance, the average TIMING for D1 firms is 58.85% for the most experienced
analysts, while it is 62.33% for the least experienced analysts. Their
difference of –3.48% is significantly at 1% level ($t = 4.78$). This
result reenforces Hong et al. (2000) where they find inexperienced
analysts are less likely to issue timely forecasts. Our finding does
not support the conjecture that experienced analysts make more
accurate forecasts because they issue forecasts later than inexperi-
enced peers.$^{10}$

$^{10}$ We also use the time of the first forecasts in our three month window to construct the TIMING variable and our results are even stronger: the difference in TIMING between experienced and inexperienced analysts are more negative. This indicates that, relative to their inexperienced peers, experienced analysts make more timely in their first forecasts than in their last forecasts.

\begin{table}[h]
\centering
\caption{Effect of analyst forecast errors on analyst forecast errors and timing.} \label{tab:forecasterrors}
\begin{tabular}{|c|c|c|c|c|c|}
\hline
FR rank & Analyst experience quartiles & Q1 (Low) & 2 & 3 & Q4 (High) & High–Low (t-Stat) \\
\hline
\hline
Panel A: Average forecast errors for portfolios sorted by funding ratios and analyst experience & & & & & & \\
1 (most UF) & 3.93 & 3.31 & 3.47 & 2.15 & –1.78$^{***}$ & (–2.67) \\
2 & 2.67 & 2.74 & 1.74 & 1.12 & –1.55$^{**}$ & (–2.42) \\
3 & 1.52 & 1.54 & 1.17 & 0.77 & –0.75$^{*}$ & (–2.05) \\
4 & 1.08 & 1.21 & 0.91 & 0.59 & –0.49 & (–1.71) \\
5 & 0.91 & 0.78 & 0.82 & 0.65 & –0.26 & (–1.05) \\
6 & 0.80 & 0.53 & 0.68 & 0.45 & –0.35 & (–0.76) \\
7 & 0.63 & 0.72 & 0.51 & 0.45 & –0.18 & (–1.12) \\
8 & 0.63 & 0.46 & 0.67 & 0.47 & –0.16 & (–0.64) \\
9 & 0.67 & 0.54 & 0.36 & 0.35 & –0.32 & (–1.21) \\
10 (least UF) & 0.27 & 0.18 & 0.22 & 0.22 & –0.05 & (–0.46) \\
D1 (OF) & 0.60 & 0.63 & 0.61 & 0.60 & 0.00 & (0.02) \\
\hline
D1–D10 & 3.66$^{***}$ & 3.13$^{**}$ & 3.25$^{**}$ & 1.93$^{*}$ & –1.73$^{**}$ & (–2.27) \\
(t-Stat) & (3.68) & (2.34) & (1.15) & (1.71) & (–2.27) & \\
\hline
Panel B: Average forecast timing for portfolios sorted by funding ratios and analyst experience & & & & & & \\
1 (most UF) & 62.33 & 58.76 & 59.84 & 58.85 & –3.48$^{***}$ & (–4.78) \\
2 & 62.13 & 60.20 & 58.94 & 57.97 & –4.16$^{***}$ & (–3.41) \\
3 & 60.51 & 58.77 & 57.56 & 57.86 & –2.65$^{**}$ & (–2.35) \\
4 & 59.55 & 58.32 & 57.75 & 56.88 & –2.67$^{**}$ & (–3.07) \\
5 & 60.12 & 57.93 & 56.42 & 57.49 & –2.63$^{**}$ & (–3.61) \\
6 & 59.83 & 58.08 & 56.13 & 56.61 & –3.22$^{**}$ & (–82.92) \\
7 & 58.28 & 56.54 & 57.11 & 56.09 & –2.20$^{**}$ & (–2.84) \\
8 & 59.46 & 56.11 & 56.51 & 55.35 & –4.11$^{***}$ & (–3.61) \\
9 & 58.89 & 56.65 & 56.94 & 55.11 & –3.78$^{***}$ & (–82.62) \\
10 (least UF) & 58.43 & 55.92 & 55.63 & 55.24 & –3.20$^{***}$ & (–2.47) \\
11 (OF) & 58.08 & 57.14 & 55.95 & 55.69 & –2.39$^{***}$ & (–3.71) \\
\hline
D1–D10 & 3.90 & 2.84 & 4.21 & 3.61 & –0.29 & \\
(t-Stat) & (5.17) & (2.39) & (5.38) & (3.13) & (–1.07) & \\
\hline
\end{tabular}
\end{table}
4.3. Incremental effect of investor discipline

We now examine the effect of analyst incentives based on the measure of investor discipline, DISC. As defined in Section 3.3, DISC is based on institutional ownership and indicates the demand by institutional investor clients for accurate equity research.

We first look at the effect of incentive on forecast errors, using a double sorting procedure. We sort firms into 11 portfolios based on pension funding ratio, and then sort firms within each of the 11 portfolios further into quartiles based on DISC. The results are reported in Panel A of Table 4. Across firms with similar pension funding ratios, analyst forecast errors tend to be lower as DISC increases. For example, for D1 firms, the average forecast error is 4.65% for the Q1 group (lowest investor discipline quartile) and is 2.07% for the Q4 group (highest investor discipline quartile). For D10 firms, the average forecast errors are 0.28% for the Q1 group and 0.11% for the Q4 group. The evidence suggests that investor discipline reduces analyst underreaction to pension underfunding.

Our key hypothesis, however, is that analyst experience is more effective in reducing underreaction when investor discipline is stronger. To examine this hypothesis, we employ the following triple-sorting procedure. First, all firms with underfunded pension plans are sorted into quintile portfolios based on the pension funding ratio. Q1 represents the most underfunded firms and Q5 represents the least underfunded firms. Then, within each FR portfolio, we sort firms into terciles based on DISC. Finally, within each FR-DISC sorted group, we further sort firms into quartiles based on analyst experience. This results in 60 portfolios (5 × 3 × 4). In the remaining panels of Table 4, we report the average forecast error (FCE) for each of the portfolios. These portfolios are grouped into three panels based on the investor discipline terciles.

| Table 4 |

Investor discipline, experience, pension funding ratio, and forecast error.

<table>
<thead>
<tr>
<th>FR Rank</th>
<th>Q1 (Low)</th>
<th>2</th>
<th>3</th>
<th>Q4 (High)</th>
<th>High-Low (t-Stat)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Panel A: Average forecast errors for portfolios sorted by funding ratios and investor discipline</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Investor discipline quintile</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1 (most UF)</td>
<td>4.65</td>
<td>3.38</td>
<td>2.74</td>
<td>2.07</td>
<td>-2.58*** (-3.51)</td>
</tr>
<tr>
<td>2</td>
<td>2.43</td>
<td>2.91</td>
<td>1.64</td>
<td>1.32</td>
<td>-1.11** (-2.38)</td>
</tr>
<tr>
<td>3</td>
<td>1.71</td>
<td>1.03</td>
<td>1.24</td>
<td>1.01</td>
<td>-0.70 ** (-2.36)</td>
</tr>
<tr>
<td>4</td>
<td>1.37</td>
<td>1.17</td>
<td>0.54</td>
<td>0.72</td>
<td>-0.65* (-1.97)</td>
</tr>
<tr>
<td>5</td>
<td>1.05</td>
<td>0.84</td>
<td>0.68</td>
<td>0.59</td>
<td>-0.46 (-1.85)</td>
</tr>
<tr>
<td>6</td>
<td>0.82</td>
<td>0.73</td>
<td>0.44</td>
<td>0.43</td>
<td>-0.39 (-1.52)</td>
</tr>
<tr>
<td>7</td>
<td>0.75</td>
<td>0.62</td>
<td>0.49</td>
<td>0.44</td>
<td>-0.31 (81.14)</td>
</tr>
<tr>
<td>8</td>
<td>0.68</td>
<td>0.62</td>
<td>0.45</td>
<td>0.47</td>
<td>-0.21 (-1.21)</td>
</tr>
<tr>
<td>9</td>
<td>0.59</td>
<td>0.61</td>
<td>0.42</td>
<td>0.42</td>
<td>-0.27 (-1.76)</td>
</tr>
<tr>
<td>10 (least UF)</td>
<td>0.28</td>
<td>0.31</td>
<td>0.21</td>
<td>0.11</td>
<td>-0.17 (-0.69)</td>
</tr>
<tr>
<td>D11 (OF)</td>
<td>0.78</td>
<td>0.58</td>
<td>0.53</td>
<td>0.54</td>
<td>-0.24 (-0.55)</td>
</tr>
<tr>
<td>D1–D10</td>
<td>4.37***</td>
<td>3.07***</td>
<td>2.53***</td>
<td>1.96**</td>
<td>-2.41*** (-2.64)</td>
</tr>
<tr>
<td>(t-Stat)</td>
<td>(3.72)</td>
<td>(3.37)</td>
<td>(3.22)</td>
<td>(2.43)</td>
<td></td>
</tr>
<tr>
<td>Panel B: Average forecast errors for portfolios sorted by funding ratio and analyst experience: low investor discipline tercile</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Investor discipline</td>
<td>1 (most UF)</td>
<td>3.42</td>
<td>2.67</td>
<td>2.99</td>
<td>2.97</td>
</tr>
<tr>
<td>2</td>
<td>1.90</td>
<td>1.32</td>
<td>1.42</td>
<td>1.35</td>
<td>-0.55 (-1.26)</td>
</tr>
<tr>
<td>3</td>
<td>0.83</td>
<td>0.65</td>
<td>0.53</td>
<td>0.58</td>
<td>-0.25 (-1.35)</td>
</tr>
<tr>
<td>4</td>
<td>0.59</td>
<td>0.5</td>
<td>0.43</td>
<td>0.44</td>
<td>-0.15 (-0.81)</td>
</tr>
<tr>
<td>5 (least UF)</td>
<td>0.42</td>
<td>0.30</td>
<td>0.33</td>
<td>0.26</td>
<td>-0.16 (-0.54)</td>
</tr>
<tr>
<td>D1–D5</td>
<td>3.00***</td>
<td>2.37***</td>
<td>2.67***</td>
<td>2.71***</td>
<td>-0.29 (81.14)</td>
</tr>
<tr>
<td>(t-Stat)</td>
<td>(4.33)</td>
<td>(4.10)</td>
<td>(3.29)</td>
<td>(3.12)</td>
<td></td>
</tr>
<tr>
<td>Panel C: Average forecast errors for portfolios sorted by funding ratio and analyst experience: medium investor discipline tercile</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Investor discipline</td>
<td>1 (most UF)</td>
<td>2.93</td>
<td>2.04</td>
<td>1.83</td>
<td>1.75</td>
</tr>
<tr>
<td>2</td>
<td>1.66</td>
<td>0.88</td>
<td>0.77</td>
<td>0.87</td>
<td>-0.89* (-2.13)</td>
</tr>
<tr>
<td>3</td>
<td>1.17</td>
<td>0.64</td>
<td>0.52</td>
<td>0.66</td>
<td>-0.51 (-1.76)</td>
</tr>
<tr>
<td>4</td>
<td>0.83</td>
<td>0.49</td>
<td>0.52</td>
<td>0.49</td>
<td>-0.34 (-1.14)</td>
</tr>
<tr>
<td>5 (least UF)</td>
<td>-0.09</td>
<td>0.35</td>
<td>0.35</td>
<td>0.25</td>
<td>0.32 (-0.10)</td>
</tr>
<tr>
<td>D1–D5</td>
<td>2.58***</td>
<td>1.69***</td>
<td>1.58**</td>
<td>1.44**</td>
<td>-0.84*** (-2.02)</td>
</tr>
<tr>
<td>(t-Stat)</td>
<td>(3.64)</td>
<td>(3.21)</td>
<td>(2.35)</td>
<td>(2.43)</td>
<td></td>
</tr>
<tr>
<td>Panel D: Average forecast errors for portfolios sorted by funding ratio and analyst experience: high investor discipline tercile</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Investor discipline</td>
<td>1 (most UF)</td>
<td>2.54</td>
<td>2.31</td>
<td>1.54</td>
<td>0.92</td>
</tr>
<tr>
<td>2</td>
<td>1.45</td>
<td>0.98</td>
<td>0.90</td>
<td>0.79</td>
<td>-0.66*** (-2.07)</td>
</tr>
<tr>
<td>3</td>
<td>1.04</td>
<td>0.61</td>
<td>0.62</td>
<td>0.59</td>
<td>-0.55*** (-1.99)</td>
</tr>
<tr>
<td>4</td>
<td>0.73</td>
<td>0.46</td>
<td>0.39</td>
<td>0.30</td>
<td>-0.43 (-1.67)</td>
</tr>
<tr>
<td>5 (least UF)</td>
<td>0.24</td>
<td>0.25</td>
<td>0.17</td>
<td>0.15</td>
<td>-0.09 (-0.73)</td>
</tr>
<tr>
<td>D1–D5</td>
<td>2.30***</td>
<td>2.06***</td>
<td>1.37***</td>
<td>0.77**</td>
<td>-1.53*** (-2.80)</td>
</tr>
<tr>
<td>(t-Stat)</td>
<td>(3.21)</td>
<td>(3.15)</td>
<td>(2.65)</td>
<td>(1.92)</td>
<td></td>
</tr>
</tbody>
</table>

Panel A reports average forecast errors (FCE) across portfolios double-sorted by pension funding ratios (FR) and investor discipline (DISC). In each year, sample firms are sorted into 11 portfolios based on funding ratios (FR). Within each FR portfolio, analyst forecast error observations are further sorted into quartiles based on investor discipline (DISC), which is the residual in the regression of institutional ownership on the log firm size, firm age, and stock return synchronicity. Panels B, C, D report average forecast errors for portfolios triple-sorted by FR, DISC, and EXP. Specifically, all underfunded firms are sorted into quintiles based on FR. Within each FR quintile, we sort observations into three groups based on DISC. Within each FR – DISC group, we further sort observations into quartiles based on EXP. We first compute the average FCEs for each portfolio by calculating cross-sectional means in each year and then averaging them over the sample period. The t-statistics in parentheses are computed using the Newey–West procedure with a 2-year lag. The sample period is from 1989 to 2008. Forecast errors are in percentage points.

* Significance level at 10%.
** Significance level at 5%.
*** Significance level at 1%.
Panel B reports the results for the group of portfolios with the lowest rank of investor discipline. Two patterns are worth noting. First, across analysts with similar experience, forecast errors increase with pension underfunding. For example, for the most experienced analysts (Q4), their forecast error for the quintile of most underfunded firms is 2.97%, versus 0.26% for the quintile of least underfunded firms. Second, across firms with similar pension underfunding, analyst experience does not make a substantial difference in forecast errors. In particular, for the quintile of most underfunded firms, the difference in the average forecast errors between the most experienced (Q4) and the least experienced (Q1) analysts is -0.45% (t = -0.83). Therefore, when investor discipline is weak, analysts tend to underreact to pension underfunding regardless of their experience; that is, experience does not substantially reduce underreaction.

Panels C and D report the results for the groups of portfolios with the medium and high ranks of investor discipline, respectively. The first pattern remains the same as in Panel A. That is, among analysts with similar experience, the more pension underfunding, the higher the forecast errors. However, among firms with a large magnitude of pension underfunding, analyst experience starts to matter in reducing forecast errors. For example, among firms with medium level of investor discipline, for the quintile of most underfunded firms, the spread in average forecast error between the most experienced (Q4) and the least experienced (Q1) analysts is -1.18% (t = -2.27). The spread is larger in magnitude among firms with the highest investor discipline, at -1.62% (t = -2.76). We also test if the difference in analyst experience effects between the high and low investor discipline groups reported in Panels D and B is significant. To do this, we estimate the analyst experience effects in Panels B and D in each year and then calculate the difference in the effects. The difference between the high and low DISC groups is significant at the 5% level, with a t-statistic of -2.47. The result of sorted portfolios suggests experience helps reduce underreaction when analysts have stronger incentive.

4.4. Experience and analyst survival

When interpreting the results on analyst experience, it is important to keep in mind the critique of Jacob et al. (1999). They argue that analyst experience may be related to both learning and innate (learning-invariant) skills. Therefore, a negative relation between experience and underreaction does not provide conclusive evidence on learning. Analysts’ experience is positively related to their innate skills through the survival effect — analysts with better innate skills tend to survive longer on their jobs. Because of this, even in the absence of learning, there may exist a positive relation between experience and forecasting performance across analysts.

It is interesting to note that a similar issue is encountered by Seru et al. (2010), who examine individual investors’ trading behavior. Even without trading skill improvement, investors’ trading experience may be positively linked to trading performance in the cross-section if a large proportion of investors quit trading after realizing that they have poor skills. The issue in both studies is essentially a selection bias induced by analysts’ exit decisions conditional on analysts’ ability.

The literature has employed various methods to address this selection bias. Mikhail et al. (1997, 2003) trace the forecasting performance of analysts over time to detect learning. Their approach does not suffer from the above selection bias, but requires long historical observations for an individual analyst’s forecasts. Indeed, their analysis is based on a relatively small sample of analysts (236 analysts with over 8 years of experience each in their sample). Jacob et al. (1999) use an analyst-firm fixed effect to capture analysts’ time-invariant firm-specific skills. However, when the fixed analyst-firm effect is applied to our data, an issue of statistical power emerges. Because analyst turnover is high and the majority of analysts have a short tenure, the number of analyst-firm pairings is very large relative to the time dimension of the data. To be specific, we have a total of 42,376 analyst-firm pairs, over a fourth of the total number of analyst-firm-year observations in our sample (161,686). When the analyst-firm dummies are interacted with analyst experience, another 42,376 explanatory variables are involved in the regression analysis. In such a “short” panel, the analyst-firm dummies can easily soak up the explanatory power of analyst experience even if there is a learning effect.

The approach we adopt to control for analyst survival is a modified Heckman (1976) selection model, which is similar to that of Seru et al. (2010) in analyzing individual investors’ trading experience and trading performance. We first use a Probit model to link an analyst’s continuation in covering a firm with funding ratio, analyst experience, a number of firm characteristics and brokerage house characteristics. Then, in the second-stage regression, we follow Wooldridge (1995) and Seru et al. (2010) to account for both survivorship bias and individual heterogeneity. Specifically, we account for survivorship by estimating the selection model every year and including the inverse Mills ratios in the second-stage regression model. Analyst time-invariant heterogeneity is accounted for by running the second-stage regression in first-differences. Consistent with the literature, the Probit regressions use the same set of explanatory variables as the second-stage regressions except for the instrument variables.

4.4.1. First stage: Probit regression

We construct the sample used in the selection stage analysis as follows. An analyst-firm combination is added to the selection sample if an analyst makes earnings forecasts for a firm for the first time in our sample period. Once a combination is added, it remains in the selection sample until 2008, which is the end of our data. The dependent variable of the Probit model is analyst continuation, which takes the value of one if an analyst continues to provide forecasts for a given firm in year $t$ after providing earnings forecasts for the firm in year $t - 1$, and zero if the analyst no longer covers the firm.

For the explanatory variables, we include three sets of factors that may affect analyst job continuation. The first group includes $FR(+)\mid FR(-), \mid \text{EXP},$ and the interaction of $\mid \text{FR}(-) \mid$ and $\text{EXP}. FR(+)$ equals to funding ratio if a firm’s funding ratio is positive and zero otherwise and $\text{FR}(-)$ equals to the absolute value of a firm’s funding ratio if it is negative and zero otherwise. Lin and McNichols (1998) suggests that analyst may withhold a forecast if his/her information about the firm’s future earnings is very negative. A negative funding ratio predicts poor earnings in subsequent years (Rauh, 2006). Consequently, funding ratios of a firm, particularly the magnitude of negative funding ratios potentially drive analyst coverage. In addition, more experienced analysts are more likely to keep their jobs due to better performance (e.g., Mikhail et al., 1997, 2003). EXP is thus included in the Probit regression.

The second group of variables includes various analyst and firm characteristics that may affect analyst continuation. We start with the variable BOLD, which is the absolute value of the difference between the forecast made by an individual analyst and the analyst consensus forecast in the same month, scaled by stock price at the beginning of the year. Analysts who make bold forecasts are more likely to be checked out in their career (see e.g., Clement and Tse, 2005). Therefore, the inclusion of BOLD potentially picks up the effect of analysts’ herding effect. Second, as shown earlier, experienced analysts make forecasts earlier than inexperienced analysts when the new information (i.e., pension underfunding) arrives at the market. Such timeliness of analyst forecast is related with analysts’ forecast ability. We therefore
include TIMING, defined in the same way as in Panel B of Table 3, in the regression. Third, following Jacob et al. (1999), we include the percentage of companies followed by an analyst with the same two-digit SIC code to measure the specialization of an analyst (SPEC), and the number of earnings forecasts on a firm by an analyst in the past year (FREQ). The firm characteristics are firm size (LOGSIZE), book-to-market ratios (LOGBM), and momentum (MOM).

Finally, we include two instrument variables in the analysis. In the specific setting of the Heckman procedure, good instrumental variables are correlated with the dependent variable of the first-stage regression, while they are not directly related to the dependent variable in the second-stage analysis except through the inverse Mills ratios estimated from the first-stage analysis. Consistent with these criteria, the selected instrumental variables are (i) the industry-wide research and development (R&D) expenses and (ii) the adjusted turnover rate of the brokerage house that an analyst is affiliated to. To estimate industry-wide R&D we use the two digit standard industrial classification codes to group firms into different industries. The rationale is the following. Barth et al. (2001) find that intangible assets of industrial firms proxied by R&D expenses are positively associated with analyst coverage. This suggests that R&D expenses potentially affect analyst survival, making it a viable candidate for instrumental variables. To ensure R&D expenses are uncorrelated with analyst forecast performance, we use the industry-wide intangible assets excluding the firm’s own intangible assets as the instrument. The second instrumental variable is the turnover of a brokerage house (BTURN), measured as the ratio of the number of analysts leaving a brokerage house to the total number of analysts in the brokerage house in the prior year. To address the concern that poor forecast performance of a brokerage house may drive high turnover, we orthogonalize the brokerage house turnover with the brokerage house’s average performance in the prior year. Such an adjusted turnover is apparently correlated with analyst survival, as an analyst is more likely to continue following a stock (or less likely to be replaced by other analysts) if there is a low turnover of brokerage house. By design, the variable is uncorrelated with analyst forecast performance.

Considering the purpose of the Heckman procedure is to examine the analyst learning effects across investor discipline groups, we separately estimate the Probit model for three investor discipline groups for each year and obtain the inverse Mills ratios (IMRs) for each year. For brevity, we only report the results of the Probit regression pooled across all sample years and with year fixed effects in Table 5. We find that regardless of the investor discipline levels, funding ratio and analyst experience both have significant power to predict analyst continuation. However, the interaction term \( FR \cdot DISC \cdot EXP \) is only significantly negative for the high discipline group. To test the difference in the coefficients on \( FR \cdot DISC \cdot EXP \) between the high and low DISC groups, we pool the observations of these two groups and construct a dummy variable for the high investor discipline observations, then interact the dummy with \( FR \cdot DISC \cdot EXP \) and all other variables included in the regression. The p value for the coefficient \( \approx -0.04 \) on the interaction of the dummy and \( FR \cdot DISC \cdot EXP \) is 0.02, suggesting the difference in the experience effects between the high and low discipline groups are statistically significant. In sum, our findings regarding analyst experience suggest that while more experienced analysts are more likely to continue their coverage, they are more likely to stop coverage for underfunded firms in the highly disciplined group.

In addition, the result shows that various firm and analyst characteristics are associated with the probability of analyst continuation of covering a firm. For example, FREQ is significantly positively related to analyst coverage continuation, suggesting that the more experience an analyst has on a firm, the more likely they will continue to cover the same firm.

Also noted, the coefficients on both instruments are statistically significant with predicted signs for all discipline groups. We find that industrial-wide intangible asset is positively related to analyst survival, while BTURN is negatively related to analyst survival. A joint \( \chi^2 \) test for the significance of the instruments rejects the null hypothesis that the instruments are weak at the 1% level (\( \chi^2 = 30.54 \) for low DISC firms, \( \chi^2 = 25.37 \) for medium DISC firms, \( \chi^2 = 28.18 \) for high DISC firms). We further test whether instrumental variables are over-identified based on the procedure specified in Woolridge (2003), which involves three steps. First, we estimate the first-stage regression without including instrumental variables and obtain the residuals. Second, we regress the residuals on the two selected instrumental variables and obtain the regression R-squared (\( R^2 \)). Third, we test whether the test statistic, \( nR^2 \) (\( n \) is the number of observations included in the first stage) follows a \( \chi^2 \) distribution with 1 degree of freedom. We find that the test statistics do not exceed the 5% for all DISC groups, suggesting that our instruments are not over-identified. Taken together, the results of both tests confirm that the selected instrumental variables are appropriate.

4.4.2. Second stage: panel regression

We estimate the learning regression as the following:

\[
\Delta FCE_{ij,t+1} = \beta X_{ijt} + \sum_{y=1990}^{2008} b_y IMR_{ijy} + e_{ijt}
\]

(4)

where the \( \Delta \) operator is to compute the difference in the dependent and independent variables. \( X \) includes all the explanatory variables used in the first-stage Probit regression except for two instrumental variables. \( IMR_{ijy}, y = 1990, \ldots , 2008 \), represent 19 variables, which take the value of the inverse Mills ratio from the year-specific first-stage Probit regression for the same year, and zero otherwise.\(^{11}\)

Our main hypothesis is that the learning effect is strong among high-incentive analysts/firms but weak among low-incentive analysts/firms. Under this hypothesis, we expect to see the coefficient for the variable \( FR \cdot DISC \cdot EXP \) to be more negative in the high DISC group (indicating reduction of forecast bias associated with pension underfunding) relative to that in the low DISC group. The results, reported in Table 6, are consistent with our expectation. We find that, with the presence of IMRs, the coefficient for \( FR \cdot DISC \cdot EXP \) is significantly negative in the high DISC tercile (coefficient = \(-0.17, \ t = -3.26 \)) while it is insignificant in the low DISC group (coefficient = \(-0.06, \ t = -0.52 \)). Applying the same procedure used in the first-stage analysis to test the difference in the coefficients on \( FR \cdot DISC \cdot EXP \) between the high and low DISC groups, we find that the t-statistic on the interaction between \( FR \cdot DISC \cdot EXP \) and the high DISC dummy is \(-3.29 \). This suggests that experience significantly reduces analyst underreaction when there is strong investor discipline.\(^{12}\)

In addition, several analyst and firm characteristics are significantly related to analyst forecast errors. For instance, the coefficients on \( BOLD \) are significantly negative in all regressions of the three different DISC groups. Prior studies find that analysts tend to herd their peers so that their own earnings forecasts do not deviate too much from their peer group. As previous discussed, we use \( BOLD \) to pick up analysts’ herding incentives. Clement and Tse (2005) report that herd forecasts are less accurate while bold

\(^{11}\) 1989 is omitted because it is the first sample year and we cannot estimate the difference for variables used in the regressions.

\(^{12}\) Moreover, following Jacob et al. (1999) we use the log-transformed measure of analyst experience to repeat the two-stage regression analysis reported in Tables 5 and 6 and obtain similar results.
forecasts are more accurate. Our result here is consistent with their findings. We also find that SPEC. FREQu. Mom are significantly negative associated with forecast error. This suggests that when an analyst covers more firms in the same industry, or he/she has more experience in forecasting a firm, or the forecasted firm has high momentum in its stock returns, the analyst forecast errors tend to be lower.

In summary, the empirical findings suggest that the effect of analyst learning does exist, but is only detected when analysts are properly motivated to deliver accurate forecasts. Learning appears to be more effective in reducing underreaction when investor discipline is stronger.

4.5. Further analyses

We perform various extended analyses to ensure the robustness of the results in this section. First, we compare analyst forecasts before and after the revelation of corporate pension funding and check how analyst experience and incentive affect analyst forecast revisions upon new funding information arriving at the market. Second, we apply alternative measures of analyst experience and incentives. Third, we investigate the effects of experience and incentives on forecast performance in different forecast horizons. Finally, we look at how experience and incentives affect stock market mispricing on pension underfunding.

4.5.1. Evidence based on forecast revisions around the revelation of pension funding information

In this section, we apply an alternative measure to check analysts’ responses to pension funding information – a comparison of individual analysts’ forecasts before and after firms’ announcements of pension funding status. Forecast revisions, FCR, of individual analysts are defined as the difference between the first forecasted EPS after announcements and the last forecasted EPS before announcements divided by stock price at the beginning of the fiscal year. Specifically, forecast revision by analyst j on firm i for fiscal year Y1 is:

\[ FCR_{j,t+1} = \frac{\text{Forecasted EPS}^\text{post}_j(t+1) - \text{Forecasted EPS}^\text{pre}_j(t+1)}{\text{Stock Price}_t} \]  

(5)

where Forecasted EPS^\text{pre}_j(t+1) is the first forecast on the firm i’s EPS of Year Y1 made by analyst j after the firm’s earnings announcement for year Y0 and Forecasted EPS^\text{post}_j(t+1) is the last forecast on the firm i’s EPS of year Y1 made by the same analyst j before the firm’s earnings announcement for year Y0. Stock Price_t is the stock price of firm i at the beginning of year Y0.

We address three issues here: (1) how financial analysts respond to the updates of pension funding information, (2) the influence of analyst experience on their responses, and (3) the effect of investor discipline on the relation between analyst response and experience.

The findings can be briefly summarized as the following. First, analysts are sensitive to pension underfunding information by revising their forecasts downward. The average FCR of the most underfunded group is much more negative than that of the least underfunded group. Second, more experienced analysts are more acute to pension underfunding information. They revise their forecast more negatively than inexperienced analysts to reflect the negative effect of underfunding on firm future earnings upon the arrival of the updated pension underfunding information. Finally, greater investor discipline intensifies the experience effect. These findings are consistent with the result reported in the paper based on analyst forecast errors. The results are not reported to conserve space while they are available upon request.
of firms having underfunded pensions for two or more times during the past three years.\textsuperscript{13} If analysts learn from past occurrences of pension underfunding, their misreaction to repeated pension underfunders should be substantially reduced relative to that for first-time underfunders.

We define a dummy variable \textit{REPEAT} for the repeated pension underfunders. We then perform the two-stage Heckman test that resembles the analysis reported in Tables 5 and 6, except that we replace analyst experience with the \textit{REPEAT} dummy. For brevity we do not report the first-stage Probit model in the paper. The results of the second-stage are reported in Table 7. The key variables of interest are the interactions of \(| FR(-) \cdot REPEAT | \) within the three incentive groups: LOWDISC, MEDDISC, and HIGHDISC. The main hypothesis is that, with incentive influencing the effectiveness of learning, the coefficient on \(| FR(-) \cdot REPEAT | \) would be significantly negative in the high incentive group while it is insignificant in the low incentive group.

The results are consistent with the learning effect: the coefficient for \(| FR(-) \cdot REPEAT | \) is \(-2.12\) with a t-statistic of \(-2.62\) for the high incentive group, suggesting a substantial reduction in forecast bias for repeated pension underfunders in the high incentive environment. On the other hand, the coefficient for \(| FR(-) \cdot REPEAT | \) is \(-0.55\) with a t-statistic of \(-0.62\) for the low incentive group, suggesting that the effect of learning is not visible in the low incentive environment.

4.5.3. Alternative measures of incentive

Investor discipline is not the only incentive mechanism that may motivate analysts to provide unbiased equity research. Another often-cited mechanism is analyst reputation; for example, in the form of Institutional Investor poll rankings. We do not use this measure for the following reasons. First, the individual analyst identity data are no longer available in IBES. Second, reputation is likely endogenous to analyst skills and closely related to analyst experience; thus, relying on analyst reputation as a measure of incentive may confound the effect of experience with that of incentive. Third, as pointed out by Ljungqvist et al. (2007), institutional investors are the ultimate arbiter of analyst reputation, and therefore, investor discipline may have captured the reputation effect to a large extent.

Another incentive mechanism examined in the recent literature is analyst competition. Hong and Kacperczyk (2011) show that competition among analysts reduces their forecast bias. They point out two economic channels through which this effect may take place. One is known as the independence rationale – competition likely encourages independent analysts, which in turn may generate a disciplinary pressure on other analysts. Another is based on influence cost – competition makes it costly for firms to suppress unfavorable opinions.

We construct a measure of analyst competition based on the number of analysts following a firm, further orthogonalized with firm characteristics related to firms’ information environment. We then repeat the analysis in Table 5 and 6 using this analyst competition measure to replace the investor discipline measure. The results are similar albeit somewhat weaker. For brevity we do not report the results in the paper. We further note that two issues may confound the inference based on analyst competition. First, the number of analysts covering a firm may be influenced by analysts’ selective coverage behavior; that is, an analyst may withhold coverage of a firm if his/her information about the firm is very negative (Lin and McNichols, 1998). Therefore, one has to

\begin{table}[h]
\centering
\caption{Panel regression on analyst forecast errors: controlling for analyst year.}
\begin{tabular}{lcccccc}
\hline
 & LOW DISC & MED DISC & HIGH DISC & \\
 & Coeff & t-Stat & Coeff & t-Stat & Coeff & t-Stat \\
\hline
INTERCEPT & -0.17\textsuperscript{**} & (-2.27) & 0.23\textsuperscript{***} & (3.29) & 0.05 & (0.62) \\
FR (+) & 0.01 & (0.35) & 0.03 & (0.57) & 0.06 & (1.41) \\
FR (-) & 9.73\textsuperscript{**} & (3.49) & 6.21\textsuperscript{**} & (4.05) & 6.79\textsuperscript{**} & (3.03) \\
EXP & -0.05 & (-1.49) & -0.04 & (-1.52) & -0.05 & (-1.45) \\
FR (-)\cdot EXP & -0.05 & (-0.61) & -0.08 & (-1.26) & -0.18\textsuperscript{**} & (-3.48) \\
BOLD & -1.38\textsuperscript{**} & (-3.31) & -1.44\textsuperscript{**} & (-3.19) & -1.09\textsuperscript{**} & (-2.28) \\
TIMING & -0.10 & (-0.82) & -0.08 & (-1.18) & -0.08 & (-1.53) \\
SPEC & -0.43\textsuperscript{**} & (-2.59) & -0.36\textsuperscript{**} & (-2.55) & -0.40\textsuperscript{**} & (-2.85) \\
FREQ & -0.11 & (-2.42) & -0.06\textsuperscript{**} & (-2.39) & -0.15\textsuperscript{**} & (-2.47) \\
LOGSIZE & -0.31\textsuperscript{**} & (-3.57) & -0.25\textsuperscript{**} & (-2.95) & -0.38\textsuperscript{**} & (-3.16) \\
LOGBM & 0.67\textsuperscript{**} & (3.24) & 0.63\textsuperscript{**} & (2.51) & 0.76\textsuperscript{**} & (2.77) \\
MOM & 0.67\textsuperscript{**} & (3.62) & 0.51\textsuperscript{**} & (2.68) & 0.45\textsuperscript{**} & (2.63) \\
IMR(y) & Yes & Yes & Yes & \\
Num. of Obs. & 54,050 & 53,750 & 53,886 & \\
Adj. R\textsuperscript{2} & 0.61 & 0.64 & 0.68 & \\
\hline
\end{tabular}
\end{table}

This table reports the results of panel regressions that are the second stage of the selection model. We classify observations into tercile groups each year based on the investor discipline measure and estimate the coefficients of the pooled regressions within low, medium, and high discipline groups (captioned as “LOW DISC,” “MED DISC,” and “HIGH DISC”). The dependent variable is forecast error (FR, in percentage) of analyst \( j \) on firm \( i \) for fiscal year \( t + 1 \). The explanatory variables involve the following. \( FR(+) \) is the positive part of \( FR \), \( FR(-) \) is the absolute value of the negative part of \( FR \). \( EXP \) is analyst experience. \( BOLD \) is the absolute value of the difference between forecasts made by an individual analyst and the analyst consensus forecasts in the same month, scaled by stock price. \( TIMING \) is measured by the percentile rank of the forecast time of the last forecasts made by all analysts within three months after the earnings announcements of a given firm. \( SPEC \) is the percentage of companies followed by an analyst with the same two-digit SIC code. \( FREQ \) is the number of earnings forecasts on the firm provided by the analyst during the past 12 months. \( LOGSIZE \) is the log market capitalization at end of year \( t \). \( LOGBM \) is the log book-to-market ratio for firm \( i \) at the end of year \( t \). \( MOM \) is the 12-month stock return during year \( t \). \( IMR(y) \) equals the year-\( y \) inverse Mills ratio estimated from the first-stage Probit model if the observation is for year \( y \), and zero otherwise. To control for analyst fixed effects we subtract analyst-specific means from the dependent variable and all explanatory variables, before performing the regression.

The sample period is from 1989 to 2008.

\begin{itemize}
\item \textsuperscript{*}Significance level at 10%.
\item \textsuperscript{**}Significance level at 5%.
\item \textsuperscript{***}Significance level at 1%.
\end{itemize}

4.5.2. Alternative measures of experience

In light of the concern that the number of forecasts rather than the length of an analyst’s tenure indeed measures analyst experience, we alternatively measure analyst experience using the number of forecasts on a specific firm made by an analyst since the first coverage. The result is reported in Panel A of Table 7. The analysis involves two steps. In the first step, we re-estimate the IMRs for individual analyst forecasts using the alternative experience measure based on the first-stage Probit model. Subsequently in the second step, we estimate the effect of learning in alternative incentive groups. The result shows that the coefficient on \(| FR(-) \cdot EXP | \) is insignificant in the low incentive group (coeff = \(-0.02\); t-stat = \(-0.06\)) while it is significantly negative (coeff = \(-0.06\); t-stat = \(-2.33\)) in the high incentive group. The difference in the coefficients are significant at the 1% level. Therefore, once again, the average forecast error of experienced analysts is lower than that of inexperienced analysts among firms where investor discipline is strong.

We also perform an alternative set of analysis on the learning effect, as well as the incentive effect on learning, without relying on the analyst experience measure. As noted earlier, the test is in the spirit of the task-specific learning measure of Clement et al. (2007).

In each year, firms are separated into two groups. The first group consists of firms encountering pension underfunding for the first time in the past three years. The second group consists

\textsuperscript{13} Since the first year of the sample period, 1989, is shortly after SFAS 87 took effect, the accounting treatment of pension underfunding is likely new to analysts in 1989 even when the firm encountered pension underfunding prior to this year. For this reason, all underfunded firms in 1989 are treated as first-time underfunders.
exercise caution and separate the effect of analyst competition from that of selective coverage. Hong and Kacperczky (2011) work around this issue by focusing on an exogenous event causing the reduction of analyst coverage – brokerage house mergers. However, focusing on brokerage house mergers would dramatically limit the sample size for our analysis. Second, in recent literature, the effect of competition on incentive is somewhat equivocal. For example, Becker and Milbourn (2011) document that in the credit rating industry, increased competition lowers credit rating quality. They argue that this is because competition among rating agencies weakens the reputation-related incentive for providing high-quality ratings services. It is unclear if a similar effect exists when analysts compete.

4.5.4. The effect of the forecasting horizon

Since our main interest is in analyst response to freshly released information about corporate pension funding, our analysis so far has been based on earnings forecasts made within three months after the release of the financial statements for the prior fiscal year (Y0). As a result, we are mainly looking at relatively long-horizon forecasts; that is, forecasts for earnings to be reported 9–12 months away. In contrast, existing studies on analyst learning typically focus on short-horizon forecasts. For example, Mikhail et al. (1997, 2003) and Jacob et al. (1999) examine earnings forecasts for the next fiscal quarter. Clement (1999) and Clement et al. (2007) examine the last earnings forecasts for the fiscal year made 30 days prior to the fiscal year end. It would be interesting to see, in our setting, whether learning and incentive generate different impacts on analysts' forecasting performance at various forecasting horizons.

Our analysis on the forecasting horizon effect is as follows. First, during year t, we denote the month of annual earnings announcement for Y0 as month 0. During each of the following 11 months – that is, from month 1 (one month after Y0 earnings release) to month 11 (one month prior to Y1 earnings release) – we obtain analyst earnings forecasts for Y1. By design, these forecasts have

Table 7

<table>
<thead>
<tr>
<th></th>
<th>LOW DISC</th>
<th>MED DISC</th>
<th>HIGH DISC</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Coeff</td>
<td>t-Stat</td>
<td>Coeff</td>
</tr>
<tr>
<td>INTERCEPT</td>
<td>-0.13***</td>
<td>(-13.6)</td>
<td>1.62***</td>
</tr>
<tr>
<td>FR (+)</td>
<td>0.02</td>
<td>(0.55)</td>
<td>0.04</td>
</tr>
<tr>
<td>[FR (-)]</td>
<td>0.60**</td>
<td>(3.09)</td>
<td>5.96**</td>
</tr>
<tr>
<td>REPEAT</td>
<td>-1.49***</td>
<td>(-2.25)</td>
<td>1.47***</td>
</tr>
<tr>
<td>[FR (-)] / REPEAT</td>
<td>-0.52</td>
<td>(-0.57)</td>
<td>-1.46**</td>
</tr>
<tr>
<td>BOLD</td>
<td>-1.31***</td>
<td>(2.78)</td>
<td>-1.34***</td>
</tr>
<tr>
<td>Timing</td>
<td>-0.14</td>
<td>(-1.19)</td>
<td>-0.12</td>
</tr>
<tr>
<td>SPEC</td>
<td>-0.43***</td>
<td>(-2.48)</td>
<td>-0.49***</td>
</tr>
<tr>
<td>FREQ</td>
<td>-0.13**</td>
<td>(-2.15)</td>
<td>-0.14**</td>
</tr>
<tr>
<td>LOGSIZE</td>
<td>-0.42***</td>
<td>(-3.27)</td>
<td>-0.29***</td>
</tr>
<tr>
<td>LOGBM</td>
<td>0.56**</td>
<td>(3.16)</td>
<td>0.62***</td>
</tr>
<tr>
<td>MOM</td>
<td>-0.59**</td>
<td>(-2.19)</td>
<td>-0.48***</td>
</tr>
</tbody>
</table>

This table reports the results of panel regressions that are the second stage of the selection model using an alternative learning measure. Panel A uses the number of forecasts to proxy for analyst learning and Panel B measures analyst learning with experience firms repeated underfunding. We classify observations into tercile groups each year based on the investor discipline measure and estimate the coefficients of the pooled regressions within low, medium, and high discipline groups (captioned as “LOW DISC”, “MED DISC”, and “HIGH DISC”). The dependent variable is forecast error (PCE, in percentage) of analyst j on firm i for fiscal year t + 1. The explanatory variables include the following. FR (+) is the positive part of FR. [FR (-)] is the absolute value of the negative part of FR. In Panel A, EXP is analyst experience, measured as the number of analyst forecasts on firm i since the analyst first time covers the firm. In Panel B, REPEAT is a dummy variable that equals one if a firm’s pension is underfunded during year t and underfunded during at least one of the past three years prior to year t, and zero otherwise. BOLD is the absolute value of the difference between forecasts made by an individual analyst and the analyst consensus forecasts in the same month, scaled by stock price. TIMING is measured by the percentile rank of the last forecasts made by all analysts within three months after the earnings announcements of a given firm. SPEC is the percentage of companies followed by an analyst with the same two-digit SIC code. FREQ is the number of earnings forecasts on the firm provided by the analysts during the past 12 months. LOGSIZE is the log market capitalization at end of year t. LOGBM is the log book-to-market ratio for firm at the end of year t. MOM is the 12-month stock return during year t. IMR(y) equals the year-y inverse Mills ratio estimated from the first-stage Probit model if the observation is for year y, and zero otherwise. To control for analyst fixed effects we subtract analyst-specific means from the dependent variable and log book-to-market ratio for firm i at the end of year

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horizons (i.e., time until reporting) ranging from 11 months to 1 month. We then compute the corresponding forecast errors at each horizon.

At each forecasting horizon, we perform the regression specified in Tables 5 and 6. In Fig. 2 we plot the second-stage regression coefficients for \(| FR(-) |^{\text{EXP}}\) in low, medium and high incentive groups against the corresponding forecasting horizons.

The pattern emerging from the plot is consistent with our main hypothesis that learning is most effective in the high incentive group. The coefficients for \(| FR(-) |^{\text{EXP}}\) are strongly negative during the three months after month 0 (i.e., months 1, 2, and 3) in the high discipline group. This is consistent with the results reported in Table 6, which are based on earnings forecasts during these three months. However, the coefficients are reduced visibly afterwards. During the four months prior to Y1 earnings release (i.e., months 8, 9, 10, and 11), the coefficients remain negative, but are very small in magnitude.

The pattern for the coefficients of \(| FR(-) |^{\text{EXP}}\) of the medium discipline group is similar, albeit at a weaker magnitude relative to that for the high discipline group. In contrast, the relation between forecast error and \(| FR(-) |^{\text{EXP}}\) of the low discipline group is very weak across all forecasting horizons. The coefficients are still mostly negative, but are very small in magnitude. Therefore, when the incentive is weak, learning is not effective at improving forecasting performance at any horizon.

How do we interpret these horizon patterns? One plausible explanation is that analyst experience (as a result of learning, since we control for analyst survival) is crucial to parse financial information when such information is freshly released, but the advantage of experience shrinks quickly afterwards because of the availability of more public information. That is, inexperienced analysts, while disadvantaged at interpreting complex pension information, are able to play catch-up and revise their earnings forecasts by relying more on other types of information (e.g., quarterly earnings releases) that becomes available subsequently. Note that this happens most visibly in the high-incentive environment. In the weak-incentive environment, the difference between experienced and inexperienced analysts is not apparent.

In summary, the results obtained here highlight an issue in detecting the learning effect. Learning may provide an advantage to experienced analysts at relatively long forecasting horizons, and by limiting the attention to quarterly earnings forecasts, one may underestimate the magnitude of the learning effect.

### 4.6. Analyst learning and market efficiency: evidence on stock returns

As mentioned in the introduction, analyst learning is important because it is an integral part of the mechanism through which the

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**Table 8** Learning, incentive, and stock returns.

<table>
<thead>
<tr>
<th></th>
<th>LOW DISC</th>
<th></th>
<th>MED DISC</th>
<th></th>
<th>HIGH DISC</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
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<td>t-Stat</td>
<td>Coeff</td>
<td>t-Stat</td>
<td>Coeff</td>
</tr>
<tr>
<td>INTERCEPT</td>
<td>0.22**</td>
<td>(2.32)</td>
<td>0.27*</td>
<td>(2.19)</td>
<td>0.25**</td>
</tr>
<tr>
<td>FR (+)</td>
<td>0.04</td>
<td>(0.67)</td>
<td>0.04</td>
<td>(0.72)</td>
<td>0.04</td>
</tr>
<tr>
<td>FR (-)</td>
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<td>(-2.36)</td>
<td>-0.37**</td>
<td>(-2.41)</td>
<td>-0.37**</td>
</tr>
<tr>
<td>EXP</td>
<td>0.06</td>
<td>(1.53)</td>
<td>0.05</td>
<td>(1.37)</td>
<td>0.05</td>
</tr>
<tr>
<td>FR (-)/EXP</td>
<td>-0.04</td>
<td>(-0.75)</td>
<td>0.06</td>
<td>(1.31)</td>
<td>0.10**</td>
</tr>
<tr>
<td>LOGSIZE</td>
<td>-0.17***</td>
<td>(-2.74)</td>
<td>-0.16***</td>
<td>(-2.70)</td>
<td>-0.18</td>
</tr>
<tr>
<td>LOGBM</td>
<td>0.18***</td>
<td>(2.63)</td>
<td>0.17***</td>
<td>(2.64)</td>
<td>0.17***</td>
</tr>
<tr>
<td>MOM</td>
<td>0.03</td>
<td>(0.79)</td>
<td>0.04</td>
<td>(0.80)</td>
<td>0.05</td>
</tr>
<tr>
<td>Firm Fixed Effects</td>
<td>Yes</td>
<td></td>
<td>Yes</td>
<td></td>
<td>Yes</td>
</tr>
<tr>
<td>Year fixed effects</td>
<td>Yes</td>
<td></td>
<td>Yes</td>
<td></td>
<td>Yes</td>
</tr>
<tr>
<td>Firm-year two-way clustering</td>
<td>Yes</td>
<td></td>
<td>Yes</td>
<td></td>
<td>Yes</td>
</tr>
<tr>
<td>Num. of obs.</td>
<td>7,160</td>
<td></td>
<td>7,183</td>
<td></td>
<td>7,172</td>
</tr>
<tr>
<td>Adj. R²</td>
<td>0.23</td>
<td></td>
<td>0.25</td>
<td></td>
<td>0.26</td>
</tr>
</tbody>
</table>

This table reports the results of panel regressions on stock returns. Observations are classified into tercile groups each year based on the investor discipline measure and estimate the coefficients of the pooled regressions within low, medium, and high discipline groups (captioned as “LOW DISC”, “MED DISC”, and “HIGH DISC”). The dependent variable is stock return of firm i during the 12 month period starting from four months after the earnings announcement for year t. The explanatory variables involve the following. FR (+) and FR (-) are the positive and the absolute value of the negative part of FR. EXP is analyst experience averaged over all analysts following the firm. LOGSIZE is the log market capitalization at end of year t. LOGBM is the log book-to-market ratio for firm i at the end of year t. MOM is the 12-month stock return during year t.

We include fixed firm effect and fixed year effect in the regressions. We report t-statistics based on two-way clustered standard errors. The sample period is from 1989 to 2008.

*Significance level at 10%.
** Significance level at 5%.
*** Significance level at 1%.
efficiency of the financial market is improved. In the final set of analysis, our attention switches to the effect of analyst learning and incentives on market efficiency. Specifically, we investigate if the reduced analyst forecast bias (as a result of incentive and learning) translates into reduced market mispricing. The benchmark of our analysis is a stock market anomaly. Franzoni and Marín (2006) report that firms with severely underfunded pensions have abnormally low future stock returns, suggesting that the stock market overvalues these firms. The relevant issue for our analysis is whether analyst experience and incentive help reduce this market anomaly.

We use panel regressions to investigate this issue. The dependent variable is stock return during a 12-month period starting from the fourth month after the Y0 earnings reporting. The explanatory variables include \( FR(\cdot), FR(-\cdot), EXP, FR(-\cdot)\cdot EXP \), as well as the three firm characteristics \( LOGSIZE, LOGBM, \) and \( MOM \). Note that, as the analysis is performed at the firm level, rather than for analyst-firm pairs, \( EXP \) here is the average of experience across all analysts following the same firm. For the same reason, we do not include the inverse Mills ratios in the regression as they are estimated for analyst-firm observations. Accordingly, fixed firm effects and fixed year effects are included to control for unobserved heterogeneity. The \( t \)-statistics are estimated based on the two-way (years and firms) clustered standard errors.

The total number of firm-year observations for the regression is 21,515. The results are reported in Table 8. First, confirming the pension underfunding anomaly, \( |FR(-\cdot)| \) has a significantly negative coefficient. The coefficient for \( |FR(-\cdot)|\cdot EXP \) is significantly positive in the high incentive group, suggesting that in a high-incentive environment, stocks followed by more experienced analysts on average have less pension-underfunding induced mispricing, which is consistent with the pattern of these stocks having lower forecast biases as reported in Table 6. The coefficients for \( |FR(-\cdot)|\cdot EXP \) in the medium and low incentive groups, however, are not significant, which is consistent with the result from Table 6 regarding analyst underreaction.

Overall, the findings are consistent with the interpretation that incentive and analyst learning help reduce stock market mispricing, in a way that is similar to their effect of reducing analyst forecast bias.

5. Conclusion

The main contribution of this study is to show the importance of incentive in evaluating the learning-by-doing effect of a type of delegated agents – sell-side analysts. When provided with a stronger incentive to deliver accurate earnings forecasts, analysts are more effective in learning on their jobs, and the effect of learning is more visible based on their earnings forecasts. To our knowledge, this is first empirical study to document the incentive effect on learning in the financial market.

Prior studies have debated on whether learning-by-doing exists among analysts, and have reached mixed conclusions. Our findings suggest that properly accounting for analyst incentive is important for detecting analyst learning. Our analysis also reveals two other issues in evaluating analyst learning. First, consistent with Clement et al. (2007), task complexity is an important factor – experience may be highly relevant when analysts perform relatively difficult forecasting tasks, while it may not make a difference when performing easy, general tasks. Second, the advantage of experienced analysts is more visible when providing forecasts at relatively long horizons, and is less visible in their short-horizon earnings forecasts. Therefore, focusing on long-horizon forecasts make a significant difference for the detection of the learning effect.

We also empirically link analyst learning to the reduction of stock market mispricing. Learning-by-doing, together with the effect of “survival of the fittest,” is considered an important mechanism through which participants in the financial market improve their skills over time and through which the financial market evolves toward a greater degree of efficiency. We show that the same factors influencing analyst forecasting performance – learning, incentive, and their interactions – also reduce stock market mispricing associated with corporate pension underfunding.

Acknowledgement

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Appendix A. Accounting treatment of pension expenses in income statements

The income statement reports the net periodic pension cost, which includes four parts: service cost, interest cost, other cost, and expected returns on plan assets. Specifically,

- Service cost is the present value of pension benefits earned by employees over the last year, which essentially is deferred compensation. It includes the additional contributions made by firms to make up funding shortfall.
- Interest cost comes from the growth in the projected pension liability over the last year. Different from service cost, interest cost reflects the increase in the pension obligations just due to the passage of time as employees are getting closer to receiving their pension benefits.
- Other cost includes actuarial gain, amortization of transition asset and prior service, and plan amendments.
- Expected return on plan assets is determined by multiplying the expected rate of return on plan assets and the market value of plan assets (FVPA). FVPA could be the market value of plan assets at the beginning of the year or a five-year moving average value of plan assets.

The net periodic pension cost is the sum of service cost, interest cost and other cost with a deduction of expected dollar return on pension assets. When a firm is underfunded for its pension plans, its service cost increases in the subsequent years as the firm is required to make contributions to make up the shortfall. Moreover, under both SFAS 87 and SFAS 158, the expected return, rather than the actual return, of pension assets is recognized. This has two consequences after a year with a substantial low asset return. First, it leads to an understatement of pension expenses as expected return of plan assets (higher than actual return) are deducted. Second, the difference between the actual and expected asset returns are amortized in later years, leading to a higher amount of other costs in subsequent years. In sum, the accounting treatment of pension expenses has a delayed effect on corporate earnings.