CAUSAL EFFECTS OF STOCK MARKET ON CORPORATE DECISIONS, DISCLOSURE MANDATES, AND INFORMATIONAL FEEDBACK

by

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Abstract

The accounting literature has long recognized that maintaining or increasing stock prices is one of the most important factors for managers' reporting and disclosure decisions, however, the extant literature mainly examines the reverse causality (i.e., the effect of voluntary earnings forecasts or earnings management on stock prices), due to endogeneity concerns. Chapter 1 examines managers' decisions on information disclosure in response to stock-underpricing. Using mutual fund fire sales as an exogenous source of market-disruption, we find some managers increase frequency/precision of earnings guidance in response to stock-underpricing. Other managers, especially those in firms with poorer performance and more short-term-oriented investors, engage in accrual-based earnings management. The passage of SOX, however, affects firms' response to fire sales, with firms increasing their reliance on guidance as opposed to earnings management. The shift is associated with faster post-fire-sales price recovery, suggesting that enhancing information disclosure rather than information manipulation is effective in correcting stock-underpricing.

The SEC promulgated Regulation Fair Disclosure (Reg FD) to establish a "level playing field"for investors through prohibiting the use of selective disclosure. In Chapter 2, we use Reg FD as a plausibly natural experiment to evaluate links between disclosure, private information production, and real efficiency. We find that the rule has an adverse impact on price informativeness, investment-to-price sensitivity, and firm valuewith stronger effects for firms with greater prior reliance on selective disclosure. Analyst forecast quality also appears to decline following the rule change. Interestingly, the impact of Reg FD on price informativeness and the sensitivity of investment-to-price diminishes over time, while the deterioration in analyst forecasts tends to persist. Collectively, the results highlight unintended consequences of Reg FD in inhibiting private information acquisition and, thereby, the informational feedback from stock prices to real decisions.

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Chapter 1

Stock-Market Disruptions and Managerial Response

1.1 Introduction

Recent studies show that disruptions in secondary stock markets can adversely impact firms' real economic decisions and performance. For example, stock underpricing can impede firms from raising equity capital and distort their investments (Lou and Wang, 2014; Khan, Kogan and Serafeim, 2012; Hau and Lai, 2013), resulting in lower investment efficiency (Xiao, 2016). Stock underpricing can also create negative externalities by conveying unfavorable signals to stakeholders, such as customers and suppliers (Subrahmanyam and Titman 2001; Williams and Xiao, 2016). Furthermore, firms with underpriced stocks are more likely to become takeover targets, posing a significant threat to incumbent management (Edmans, Goldstein, and Jiang, 2012). Hence, we would expect firm managers to take deliberate actions to mitigate the impact of disruptions to their firm's stock price. In this study, we investigate managerial response – in the form of voluntary disclosure and financial reporting – to stock price disruptions, and the effectiveness of these actions in alleviating stock underpricing.

We use mutual fund fire sales (Coval and Stafford, 2007; Edmans et al., 2012) as a source of exogenous disruption to stock prices. As Coval and Stafford (2007) show, mutual funds that are subject to extreme capital outflows create price pressure when they are forced to sell stocks they hold in common. This price pressure is unrelated to firm fundamentals, but can induce prolonged mispricing that, on average, takes more than a year to recover (Edmans et al. 2012). Consistent with the literature, we find that mutual fund outflow-driven price pressure can trigger substantial price drops. There is, however, considerable variation in the price impact across different stocks: Our cross-sectional tests show that firms with higher informational transparency, as reflected by higher analyst forecast quality and stock liquidity, experience less underpricing when their stocks are subject to mutual fund fire sales.

We investigate firms' response to mutual fund fire sales in terms of their financial reporting and voluntary disclosure policies. Motivated by our cross-sectional results, we hypothesize that increased voluntary disclosure can improve the information environment and hence mitigate the adverse price effect of mutual fund fire sales. Consistent with our conjecture, we find that firms tend to increase the frequency of earnings forecasts in response to mutual fund fire sales. Furthermore, these firms issue management earnings forecasts with greater precision, i.e., forecasts with point estimates as opposed to range estimates, and with lower error. Contrary to the idea that firms may use upward-biased earnings forecasts in response to mutual fund fire sales, we do not find that firms issue more management forecasts that are positively biased relative to analysts' forecasts. This is consistent with our hypothesis that firms use voluntary disclosure to improve transparency (Diamond and Verrecchia, 1991; Leuz and Verrecchia, 2000). The effort by firms to improve transparency seems to deliver benefits: we find that post-fire-sales management earnings forecasts, especially those with higher precision and lower error, are associated with moderating the price impact of fire sales and a faster price recovery.

Despite the above evidence, it is not clear whether all firms under mutual fund fire sales pressure would choose management earnings guidance as a response. It is possible that some firms, particularly those with weak expected performance, are constrained from disclosing information that is sufficiently positive to counter the price impact of mutual fund fire sales. We examine the cross-sectional variation in firms' earnings guidance policies after mutual fund fire sales, and find that firms with lower pre-fire-sales ROA do not increase the frequency of earnings forecasts in response to fire sales. Instead of disclosing information to correct stock underpricing, these firms use discretionary accruals to manipulate accounting earnings upward. While the use of earnings management is compatible with managers' incentives to counter the negative price impact of fire sales, the implications are antithetical in terms of the information environment – earnings management tends to induce informational opacity rather than transparency (Sloan, 1996). Hence, we look further into firms' use of earnings management as a response to mutual fund fire sales and its effectiveness in mitigating the negative price impact.

As earlier work by Coval and Stafford (2007) suggests, mutual fund fire sales induce significant stock underpricing which will eventually be corrected by the market. Even if firms do not take any deliberate action, stock prices will gradually be restored to their fundamental value in the long run. Hence, we expect managers that are more concerned about current stock price levels to be more proactive in responding to mutual fund fire sales. Managers' short-term focus may be keener in the absence of long-term investors. We therefore examine the cross-sectional variation in investor horizons, reflected by institutional ownership (Aghion, Van Reenen, and Zingales, 2013), presence of blockholders (Edmans, 2009), and investors' turnover rate of their portfolios (Gaspar, Massa, and Matos, 2005). We find that under-performing firms significantly increase discretionary accruals after mutual fund fire sales only when institutional ownership is low, blockholders are absent, and when investors' turnover rate is high. This result indicates that it is firms that do not have positive fundamental information to disclose, and whose managers are particularly concerned about short-term price levels, that use earnings management in response to mutual fund fire sales.

Though some managers respond to mutual fund fire sales by increasing their earnings management, we would not generally expect such a response to be effective at reversing stock mispricing. First, as we argue above, informational transparency rather than opacity is more likely to help in correcting mispricing. Second, our findings suggest that firms that use earnings management may also tend to have lower expected firm performance. Thus, it is possible that lower stock prices following mutual fund fire sales may partly reflect the fundamental value of these firms, rather than mispricing as such. Consistent with our predictions, we find that post-fire-sales discretionary accruals are not associated with mitigating the price impact of fire sales or leading to faster price recovery.

However, since firms' responses appear to be endogenous to firm characteristics, we cannot make any causal claims about the effect of disclosure policies on price recovery on the basis of these results. For instance, since firms' disclosure responses are related to firm performance, it is possible that better-performing firms will have a faster recovery from fire sales regardless of their disclosure policies. To address endogeneity in firms' disclosure policy responses to mutual fund fire sales, we make use of a quasi-natural experiment: the Sarbanes-Oxley Act of 2002 (SOX). The extant literature shows that the passage of SOX deters the use of discretionary accruals (Cohen, Dey, and Lys, 2008). Thus, the passage of SOX may force firms to shift from earnings management to earnings guidance as a response to mutual fund fire sales. Using a difference-in-differences test around the passage of SOX, we find that firms are indeed more (less) likely to use earnings guidance (discretionary accruals) in response to mutual fund fire sales after the regulatory change. Further, the passage of SOX is associated with faster price reversals post-fire-sales. These results suggest that switching from earnings management to earnings guidance helps mitigate the adverse effect of mutual fund fire sales. Supporting the causal interpretation of our results, we find that these changes are concentrated among firms that were not complaint with SOX requirements prior to 2002, i.e., did not have a majority of independent directors and a fully independent audit committee prior to SOX.

Our study contributes to three strands of literature. First, we provide evidence on the causal effect of stock prices on managerial disclosure decisions. Although the literature has long recognized that maintaining or increasing stock prices is one of the most important factors for managers' reporting and disclosure decisions, the extant literature mainly examines the reverse causality — i.e., the effect of voluntary earnings forecasts and earnings management on stock prices.[1] Several recent studies have attempted to examine the effect of stock prices on voluntary disclosure policies. For example, Sletten (2012) uses financial restatements by industry peers as negative shocks to stock prices and examines their effect on firms' voluntary disclosure policies. Li and Zhang (2015) exploit the adoption of Regulation SHO as a shock to short selling activities and find that firms tend to respond by reducing the precision of bad news forecasts. Both short selling activities and restatement by peer firms reduce firms' market value by incorporating negative information into stock prices. Our paper differs from these studies in that mutual fund fire sales reduce both price *levels* and price *efficiency*, because these trades are driven by liquidity shocks rather than information. Our finding suggests that while firms may take different actions in response to exogenous stock underpricing, improving informational transparency through accurate and unbiased disclosure appears to be the only effective measure. Hence our study contributes to the literature that documents managers' deliberate actions in shaping the information environment, so as to increase liquidity and firm value (Diamond and Verrecchia, 1991; Coller and Yohn, 1997; Balakrishnan et al., 2014), reduce investor uncertainty (Dye, 1985), mitigate stock price volatility (Billings et al., 2015), and offset the negative effects of increased complexity in mandatory disclosure (Guay et al., 2016).

Second, our study sheds light on the underlying incentives behind managers' choice of disclosure policy changes in response to exogenous underpricing: while firms on average tend to respond to underpricing with information disclosure, firms that do not have positive information to disclose but are concerned about short-term firm value respond by

[1] see, e.g., Pownall, Wasley, and Waymire, 1993; Coller and Yohn, 1997; Teoh, Welch, and Wong, 1998a,b; Baber, Chen, and Kang, 2006; Anilowski, Feng, Skinner, 2007; Rogers, Skinner, and Van Buskirk, 2009.

manipulating accounting earnings. These findings are related to the vast literature on the determinants of managerial decisions on financial reporting and disclosure policies (e.g., Verrechia, 1990; Beasley et al., 2000; Klein, 2002; Leuz et al., 2003; Armstrong et al., 2010; Hadani et al., 2011), by recognizing that managers also respond to non-fundamental shocks to stock prices in different ways due to different performance and underlying incentives.

Third, our findings on managerial responses to market disruptions add to the growing literature on the link between financial markets and real economic activities (Bond, Edmans, and Goldstein, 2011). Recent studies show that exogenous stock mispricing due to mutual fund fire sales/purchases can affect firms' financing and investment decisions (Lou and Wang, 2014; Khan, Kogan and Serafeim, 2012; Hau and Lai, 2013) and result in lower investment efficiency (Xiao, 2016). Given these real effects of stock mispricing, firm managers are expected to deliberately mitigate stock mispricing as long as they recognize it. Our evidence shows that these firms indeed adjust their financial reporting and voluntary disclosure policies to alleviate exogenous stock underpricing.

The remainder of the paper is organized as follows. We review the literature and develop our hypotheses in the next section. Section 1.3 describes the data and variable construction. Section 1.4 discusses our empirical approach and presents the results. Section 1.5 concludes.

1.2 Hypothesis Development

Management earnings forecasts are an important source of information, accounting for over 15% of the quarterly return variance and approximately 55% of accounting-based information (Beyer et al., 2010). An extensive prior literature suggests that voluntary disclosure is associated with lower information asymmetry. Theoretical models provide persuasive arguments that a commitment to higher levels of disclosure reduces information asymmetries arising either between the firm and its shareholders or among investors. This, in turn, should lower the cost of raising capital (see, e.g., Diamond and Verrecchia, 1991; Baiman and Verrecchia, 1996). Empirical studies, such as Leuz and Verrecchia (2000), support this view by showing that a strengthening of financial disclosure requirements leads to improvements in liquidity. Consistent with this notion, Coller and Yohn (1997) document that issuance of management earnings forecasts is associated with a lower subsequent bid-ask spread. A more recent literature stresses the important role of voluntary disclosure in supplementing information, especially when public information is deficient or obfuscatory. Balakrishnan et al. (2014) show that managers respond to exogenous decreases in public information by increasing voluntary disclosure. Further, this increase in disclosure is shown to have causal effects on liquidity and cost of capital. Guay et al. (2016) document that firms with more complex financial statements (i.e., 10-Ks) commit to providing higher levels of voluntary disclosure. Hence, when stocks are underpriced by mutual funds due to liquidity shocks, we would expect firms to inform the market of their true value by disclosing more information. We hypothesize that firms are likely to issue more earnings forecasts in response to stock underpricing.

H1: Firms are likely to increase issuance of voluntary earnings forecasts in response to mutual fund fire sales.

We also consider the possibility that some firms respond to stock underpricing by manipulating their accounting earnings to convey false positive information. The literature suggests that firms manage earnings using discretionary accruals to meet various earnings targets such as analysts' forecasts and management forecasts (e.g., Matsumoto, 2002; Bartov et al., 2002; Philips et al., 2003). Zahra et al., (2005) indicate that pressure and opportunity are the two common attributes for managers engaging in opportunistic manipulation. Various papers find opportunistic use of discretionary accruals, particularly around corporate events, such as IPOs and SEOs (Rangan 1998; Teoh et al.,1998ab; Kim and Park, 2005), debt covenant violations (DeFond and Jiambalvo 1994), and insider trading

(Darrough and Rangan, 2005; Agrawal and Cooper, 2015). In the case of mutual fund fire sales, we hypothesize that stock underpricing pressures some managers to engage in accrual-based earnings management.

Earnings forecasts and earnings management have opposing effects on the information environment, because earnings management tends to induce informational opacity rather than transparency. It is unlikely, therefore, that the same group of firms will adopt both strategies in response to stock underpricing. Consistent with this view, Francis et al. (2008) show that firms with better (worse) earnings quality have more (less) voluntary disclosure. Several papers document that firms' use of earnings guidance is positively related to operating performance. For example, Miller (2002) finds that all types of disclosure increase when firms have rising earnings. Similarly, Houston, Lev, and Tucker (2010) show that poor operating performance (e.g., decreased earnings and failure to meet analyst forecasts) can largely explain firms' stopping quarterly earnings guidance. In the case of mutual fund fire sales, we might expect underperforming firms to be unwilling to issue earnings forecasts. Instead, these firms could adopt upward-biased discretionary accruals in response to stock underpricing. Hence:

H2: Firms with lower expected operating performance are likely to use discretionary accruals instead of voluntary earnings forecasts in response to mutual fund fire sales.

A number of empirical studies have documented the relation between institutional ownership and information disclosure. For example, Healy et al. (1999) and Bushee and Noe (2000) show that expanded voluntary disclosure is associated with an increase in institutional ownership. Similarly, Ajinkya et al. (2005) find that the number of outside directors and institutional ownership are positively associated with the frequency and precision of forecasts. Boone and White (2015) show that exogenous increases in passive institutional investor ownership are associated with more information disclosure from managers and improvements in the information environment. Shareholders' horizon is also likely to affect firms' response to mutual fund fire sales. Institutional investors, especially blockholders, can induce greater information disclosure because of their incentives to produce and trade on fundamental information (Edmans, 2009). Institutional investors have been shown to mitigate managerial myopia and deter managers from opportunistic actions such as earnings management (Bushee, 1998; Jiambalvo, Rajgopal, and Venkatachalam, 2002; Aghion, Van Reenen, and Zingales, 2013). We infer shareholders' horizon based on the level of institutional ownership, the presence of blockholders, and the average turnover rate of the investors' portfolio (Gaspar, Massa, and Matos, 2005). We hypothesize that firms with shorter investor horizon are more likely to take deliberate steps in response to mutual fund fire sales. These firms are more likely to use earnings management as a response when they are underperforming and unwilling to convey true positive signals to the market.

H3: Firms with shorter horizon investors are more likely to use discretionary accruals in response to mutual fund fire sales pressure.

We next discuss the effectiveness of managerial response to stock underpricing. Earlier studies find that management earnings forecasts have information content that affects stock prices (Patell, 1976; Penman, 1980). Hutton and Stocken (2007) find that stock price reacts more promptly to managers' good-news forecasts when a firm has built a forecasting reputation. Rogers and Stocken (2005) suggest that the market is able to filter predicted bias from managers' forecasts. Li and Zhuang (2012) show that high-quality management guidance reduces SEO (seasoned equity offerings) underpricing. Baginski et al. (1993) find that stock price reactions to earnings forecasts depend on forecast precision, e.g., point forecasts induce greater stock market reactions relative to range forecasts.

In some cases, earnings management appears useful in influencing the decisions by some market participants. For example, Alissa et al. (2013) show that firms can affect their credit ratings through the use of earnings management. However, the extant literature shows that firms generally use earnings management to induce stock mispricing rather than correct mispricing. Teoh, Welch, and Wong (1998a,b) find that IPO and SEO firms with high accruals have worse abnormal returns after equity issuance. Xie (2001) shows that abnormal accruals are associated with stock overpricing. Chan, Chan, Jegadeesh, and Lakonishok (2006) show that high accruals are associated with lower future stock returns.

In a mutual fund fire sale, trading by funds is driven by their need for liquidity. It is akin to noise trading (i.e., non-information driven trading) and can be expected to introduce considerable noise into the price of the affected stock. Such noise trading may increase investor uncertainty about the economic fundamentals of the firm and the reliability of the information that is available. As a result, investors may be inhibited from producing and trading on information (De Long, Shleifer, and Summers, and Waldmann, 1990; Shleifer and Vishny, 1997; Dow, Goldstein, and Guembel, 2016).[2] Information disclosure from insiders can thus reduce information uncertainty, encouraging investors to collect and trade on information (since this could lower the effective cost of collecting additional information), thereby alleviating mispricing (Kyle, 1985). Hence, we predict that managerial actions such as earnings forecasts, that improve informational transparency, will be more effective in mitigating the impact of mutual fund fire sales.

H4: Management earnings forecasts, especially those with higher precision, are more effective in mitigating the price impact of mutual fund fire sales compared to earnings management.

Finally, since the choice of disclosure strategy – earnings management versus earnings guidance – is likely to be affected by firms' operating performance, we cannot give a causal interpretation to the association between strategy choice and price recovery following a mutual fund fire sale. To identify the effect of disclosure strategy we rely on a quasi-natural experiment, the passage of SOX, that has been shown to discourage earnings management. We expect firms that are affected by SOX requirements ('non-compliant' firms) to exhibit a shift toward relying on earnings guidance rather than earnings management post-SOX. As a result, non-compliant firms are expected to exhibit less underpricing and a more rapid price recovery post-SOX, relative to firms unaffected by SOX passage.

[2] For instance, investors may regard the greater uncertainty as an escalation in the cost of gathering reliable information and, thereby, choose to not gather additional information.

H5: We expect SOX passage to reduce mispricing and facilitate price recovery following mutual fund fire sales. This effect is expected to be concentrated among firms that were non-compliant with SOX requirements prior to its passage.

1.3 Data and Measurement of Main Variables

1.3.1 Data

We begin with all U.S. public firms with financial information from the intersection of CRSP and Compustat. We obtain data on analyst forecasts, institutional ownership, and management earnings forecasts from I/B/E/S, Thomson Reuters, and First Call database, respectively. Our sample covers firm-quarter observations from 1992 to 2007.[3] We exclude the following firm-quarter observations: (1) firms with missing accounting or stock price information; (2) financial institutions and insurance companies (4-digit SIC codes 6000-6999); (3) firms in regulated industries (4-digit SIC codes 4900-4999); (4) firms with market value of equity less than 5 million dollars. Our final sample consists of 211,516 firm-quarter observations.

1.3.2 Managerial Responses

The two main dependent variables in our study are *Guidance* and *DisAccrual*. *Guidance* is the natural logarithm of one plus the frequency of earnings guidance during a fiscal quarter. *DisAccrual* is discretionary accruals measured using modified Jones Model (Dechow, Sloan and Sweeney, 1995) following the earnings management literature.

We also look into management earnings forecast characteristics, including forecast news, forecast precision and forecast error. Forecast news (*NEWS*) is the difference be-

[3]The sample starts in 1992 because this is when the earnings forecasts data first became available. It ends in 2007 because during the financial crisis many funds are subject to large outflow shocks and thus it is difficult to distinguish idiosyncratic shocks on mutual fund fire sales from the systemic shock due to financial crisis.

tween the point estimate (or the midpoint estimate of a range forecast) and the consensus analyst forecast, scaled by beginning-of-quarter stock price. The consensus analyst forecast is the median of analyst forecasts at the time of the management earnings forecast. Based on the sign of forecast news, each management earnings forecast is classified as good news (*NEWS* > 0) or bad news (*NEWS* < 0). Second, following Cheng et al. (2013), we measure forecast precision (*PRECISION*) as the negative of earnings forecast width. Thus higher values of *PRECISION* indicate more precise earnings forecasts. For range estimates, forecast width is the difference between the upper- and the lower-end estimates, scaled by beginning-of-quarter stock price; for point estimates the forecast width is zero. Forecast error is the absolute difference between the forecast and the actual earnings, scaled by beginning-of-quarter stock price.

1.3.3 Mutual Fund Flow-driven Price Pressure

We follow Edmans et al. (2012) and use mutual fund fire sales as an exogenous shock to stock prices. We collect data on mutual fund holdings from Thomson Reuters and mutual fund returns from CRSP, and remove funds that specialize in a particular industry to address the concern that mutual fund flows might be driven by industry fundamentals. We calculate mutual fund outflow as:

$$Outflow_{j,t} = -F_{j,t}/TA_{j,t-1},$$

where $F_{j,t}$ is the dollar-amount outflow of fund j in quarter t, and $TA_{j,t-1}$ is the total assets of fund j at the end of the previous quarter. We only keep funds with $Outflow_{j,t}$ equal to or greater than 5% to ensure that the measure captures mutual fund trading driven by liquidity shocks. We construct the hypothetical mutual fund flow-driven pressure:

$$MFFlow_{i,t} = \sum_{j=1}^{m} \frac{F_{j,t} s_{i,j,t-1}}{VOL_{i,t}}.$$

$VOL_{i,t}$ is total dollar trading volume of stock i in quarter t, and $s_{i,j,t}$ is the dollar value of fund $j's$ holdings of stock i scaled by fund $j's$ total assets at the end of quarter t:

$$s_{i,j,t} = \frac{SHARES_{i,j,t} \times PRC_{i,t}}{TA_{j,t}}.$$

$SHARES_{i,j,t}$ is the number of shares of stock i held by fund j in quarter t, and $PRC_{i,t}$ is the price of stock i in quarter t. Finally, we have the quarterly mutual fund flow-driven price pressure, *MFFlow* as:

$$MFFlow_{i,t} = \sum_{j=1}^{m} \frac{F_{j,t} \times SHARES_{i,j,t-1} \times PRC_{i,t-1}}{TA_{j,t-1} \times VOL_{i,t}},$$

The exogeneity assumption is that price pressure created by large mutual fund outflows is not related to the firm's fundamentals. One concern is that even when mutual funds suffer from liquidity shocks, they can still trade based on information about the firm's economic prospects. As in Edmans et al. (2012), we construct $MFFlow$ using hypothetical trades inferred from mutual fund holdings prior to the large outflow, and thus this measure does not capture endogenous selection by these mutual funds. Another concern is that mutual fund outflows reflect fundamental information about the underlying stocks possessed by fund investors. To address this issue we follow Edmans et al. (2012) and remove sector funds so that fund flows are unlikely to reflect industry-level information. Admittedly, we cannot fully rule out the possibility that mutual fund outflows contain firm-specific information. However, as previous studies (e.g., Coval and Stafford, 2007; Edmans et al., 2012) as well as our Figures 2.1a and 2.1b show, a large negative value of $MFFlow$ is related to a temporary decline in the stock price that is eventually reversed, indicating that the price movement is driven by noise rather than fundamentals. More detailed definitions of our variables are provided in Appendix A.

1.3.4 Other Explanatory Variables

In the regression analysis, we control for the quality of firms' information environment and stock liquidity. *Analyst Coverage* is the number of analysts following a firm reported in I/B/E/S. *Analyst Dispersion* is the standard deviation of analyst forecasts for a fiscal quarter scaled by mean monthly stock price. We use two measures of stock liquidity: *Amihud*'s illiquidity and *Bid-Ask Spread*.

We also include a set of control variables that are considered to be related to price efficiency and/or managers' decisions on earnings forecasts and earnings management. We first control for prior stock return characteristics, including average firm-specific daily return (*MRET*), standard deviation of firm-specific stock returns (*SIGMA*), and stock trading *Turnover*, defined as the monthly trading volume scaled by the number of outstanding shares. In addition, we include the following firm characteristics: firm size (*SIZE*) is the natural log of market value of equity; *Tobin's Q* is the sum of total assets and the difference between market value and book value of common equity, divided by total assets; financial leverage (*Leverage*) is the sum of long-term debt and debt in current liabilities divided by total assets; *ROA* is the ratio of earnings before interest, taxes, depreciation and amortization (*EBITDA*) to lagged assets; *Institutional Ownership* is the ratio of the number of shares held by institutional investors to the total number of shares outstanding for a firm.

1.3.5 Descriptive Statistics

Panel A of Table 2.1 presents the summary statistics of the variables we use in the empirical analysis. We winsorize all the variables at the 1st and 99th percentiles. *MFFlow* is less than or equal to zero because it only captures outflow-driven sales. The sample mean of *MFFlow* is -0.238, indicating that the average hypothetical flow-driven pressure amounts to 0.238% of the actual trading volume of the stock. The average of *Guidance* and *DisAccrual* are 0.162 and 0.004, respectively. The summary statistics for other variables are comparable with the extant literature.

Panel B of Table 2.1 shows the correlation matrix of the variables. The negative correlation between *MF_D* and $CAR[0,0]$ (average monthly cumulative abnormal returns during the quarter of mutual fund fire sale) indicates that greater mutual fund outflow is related to a more negative concurrent stock price movement. *Guidance* is positively correlated with *Analyst*, *Size*, and negatively correlated with *Amihud*, indicating that larger firms with higher analyst coverage and high stock liquidity tend to also issue earnings forecasts more frequently. The correlation between *DisAccrual* and *ROA* is 0.146, which might be reflective of firms using discretionary accruals to inflate accounting earnings.

[Insert Table 2.1 Here]

1.4 Model Specification and Empirical Results

1.4.1 Mutual Fund Flow-driven Price Pressure and Stock Underpricing

We first examine whether mutual fund flow-driven price pressure has a significant price impact using the following model:

$$CAR_{i,t} = \beta_0 + \beta_1 MF\_D_{i,t} + \sum_k \beta_k Controls_{i,t-1} + Quarter_t + Firm_i + \varepsilon_{i,t}, \quad (1.1)$$

where $Quarter_t$ and $Firm_i$ denote time and firm fixed-effects, and $\varepsilon_{i,t}$ denotes the error term.[4] We examine the abnormal return around fire sales over various windows. CAR is defined as average monthly cumulative abnormal return over various windows. We first use CARs in the quarter of fire sales to examine the initial price impact, and then use CARs over the subsequent four and eight quarters to examine the persistence of mispricing and the timing of reversal. $MF\_D_{i,t}$ is a binary variable that equals one if *MFFlow* is in the bottom decile. We estimate standard errors robust to clustering at the firm level

[4]We include both calendar and fiscal quarter for time fixed-effects.

and heteroskedasticity. Control variables include *Analyst Coverage*, *Amihud*, *Size*, *Tobin's Q*, *Leverage*, *ROA*, *Turnover*, *MRet*, *Sigma*, and *Institutional Ownership*.[5] We predict the coefficient for $MF\_D_{i,t}$ (β_1) to be negative if mutual fund fire sales induce significant price drops.

Panel A of Table 2.2 shows the coefficient estimates for Model (1.1). In columns 1 and 2 we show that there is a significantly negative abnormal return during the quarter of mutual fund fire sales. After controlling for firm characteristics and fixed effects, stocks under price pressure experience a 4.5% negative abnormal return (1.5% per month) in that quarter. This result shows that mutual funds' trading under extreme outflows creates a significantly negative price impact on the underlying stocks. The estimates in columns 3 and 4 show that there is an additional negative abnormal return in the four quarters *after* mutual fund fire sales. However, columns 5 and 6 show that MF_D is not related to any significant abnormal return in the eight quarters after fire sales, indicating that there is a reversal in the stock price over the subsequent two years. Figures 2.1a and 2.1b present the average cumulative abnormal return around mutual fund fire sales. The figures show that mutual fund fire sales induce a negative abnormal return up to -15% which is reversed within two years. This pattern is consistent with our regression estimates and suggest that mutual fund fire sales due to extreme outflow cause stock mispricing that is corrected eventually.

We also examine whether informational transparency helps mitigate the impact of mutual fund flow-driven pressures on stock prices using the following model:

$$
\begin{aligned}
CAR_{i,t} =& \beta_0 + \beta_1 MF\_D_{i,t} \times High\_Info_{i,t-1} + \beta_2 MF\_D_{i,t} \times Median\_Info_{i,t-1} \\
& + \beta_3 MF\_D_{i,t} \times Low\_Info_{i,t-1} + \beta_4 High\_Info_{i,t-1} + \beta_5 Low\_Info_{i,t-1} \\
& + \sum_k \beta_k Controls_{i,t-1} + Quarter_t + Firm_i + \varepsilon_{i,t}
\end{aligned}
\tag{1.2}
$$

[5]Detailed description of the variables is in section 1.3.4.

We sort the sample into terciles based on proxies of information quality, and include the interaction terms between $MF\_D_{i,t}$ and binary variables indicating firms with high/medium/low levels of information quality in Model (1.2). We predict the negative price impact of mutual fund fire sales to be monotonically decreasing in magnitude across informational transparency if firms with higher information quality in the market are less vulnerable to the non–fundamental price shock. We use analysts' forecast quality and stock liquidity as proxies for informational transparency. Financial analysts are deemed to be important information producers in the market (Womack, 1996; Kelly and Ljungqvist, 2012). Stock illiquidity reflects adverse selection cost arising from information asymmetry between market participants (Easley and O'Hara, 2004). Here we consider a firm as having better information quality if the stock has higher number of analysts following, lower analyst forecast dispersion, or higher stock *liquidity* as reflected by a lower value of *Amihud's* illiquidity or *Bid-Ask Spread*.

Panel B of Table 2.2 presents the estimates of Model (1.2). Columns 1 to 4 show that β_1, β_2, and β_3 are negative and monotonically decreasing, consistent with our prediction that firms with greater analyst coverage or lower analyst forecast dispersion tend to have less negative abnormal return during mutual fund fire sales. Columns 5 to 8 report the estimates of Model (1.2) with stock liquidity as the proxy for information quality. Consistent with the hypothesis that firms with liquid stocks are less affected by mutual fund price pressure, we find that the magnitude of the coefficients for MF_D monotonically decreases along stock liquidity. Overall, the results in Panel B of Table 2.2 are supportive of our hypothesis that a good information environment, where analysts provide more precise earnings forecasts and stock liquidity is high, can help stabilize stock prices when firms experience exogenous market disruptions.

[Insert Table 2.2 Here]

1.4.2 Voluntary Disclosure as a Response to Market Disruptions

In this section, we test hypothesis $H1$ and examine whether firms respond to mutual fund fire sales by increasing voluntary disclosure. As discussed in the previous section, information transparency appears to mitigate the adverse effect of non-fundamental shocks to stock prices. It is plausible, therefore, that firms would want to enhance their informational transparency in response to such fire sale shocks. We focus on management earnings guidance and examine whether managers increase guidance frequency to improve the information environment after mutual fund fire sales using the following regression model:

$$Guidance_{i,t+1} = \beta_0 + \beta_1 MF\_D_{i,t} + \sum_k \beta_k Controls_{i,t-1} + Quarter_t + Firm_i + \varepsilon_{i,t}. \tag{1.3}$$

$Guidance_{i,t+1}$ is the natural logarithm of one plus the frequency of management earnings forecasts made in quarter $t+1$. Similar to previous analysis, we control for a set of firm characteristics including *Analyst Coverage*, *Amihud*, *Size*, *Tobin's Q*, *Leverage*, *ROA*, *Turnover*, *MRet*, *Sigma*, and *Institutional Ownership*. We also control for firm and fiscal year-quarter fixed effects so that the coefficient captures within-firm variation in the frequency of management earnings guidance around mutual fund fire sales. Table 2.3 presents the estimates of Model (1.3). The coefficient for MF_D is significantly positive across all specifications, indicating that the frequency of management earnings guidance significantly increases after mutual fund fire sales. The coefficient magnitude suggests that the effect of mutual fund fire sales on changes in guidance frequency is economically significant. For example, based on the estimate in column 3, the frequency of earnings guidance increases by 13.6% from the sample mean one quarter after mutual fund fire sales.[6]

[Insert Table 2.3 Here]

[6] The sample mean of *Guidance* is 0.162. Firms with *MFFlow* in the bottom decile tend to have an increase in *Guidance* by 0.022/0.162=13.6%.

We next look into the attributes of earnings forecasts firms issue in the presence of mutual fund price pressure. Earnings guidance can generally improve transparency in the stock market (Coller and Yohn, 1997), though it is also possible that managers use upward-biased earnings guidance to counter the effect of mutual fund fire sales. Thus it is important to know whether managers use earnings guidance to disclose useful information or convey false signals in response to stock underpricing.

We first check whether managers are more likely to issue good news in response to the negative price shocks. We classify each management earnings forecast as positive/negative relative to analysts' consensus, and use a multinomial logit model to estimate the effect of mutual fund fire sales on the two types of forecasts, with the baseline being firms not issuing any forecast. We present the estimates in columns 1 and 2 of Table 2.4. The estimates for MF_D are significantly positive and similar in magnitude for the two types of forecasts. Hence, it appears that firms are as likely to increase the issuance of positive earnings guidance as negative guidance after mutual fund fire sales.[7] An interpretation of why firms might be willing to issue negative forecasts relative to analysts' consensus is that even negative forecasts could be useful in correcting stock underpricing, as long as the forecast is not as negative as what is implied by the depressed stock price. The overall pattern is that managers increase their disclosure of unbiased information, thereby improving the quality of firm-related information in the stock market.

We also examine the effect of negative price shocks on the precision of management earnings guidance. We estimate an ordered logit model where the dependent variable equals zero if there is no forecast in the firm-quarter, one if the firm issues more range estimates, and two if the firm issues more point estimates. A positive estimate for MF_D would indicate that firms increase the precision of forecasts in response to the negative price shocks. Column 3 of Table 2.4 shows that the coefficient for MF_D is indeed significantly positive: hence, forecasts issued after mutual fund fire sales are more likely to be point forecasts as

[7] We find consistent results if we focus on firms with at least one forecast and estimate a logit model for the probability of firms issuing positive forecasts relative to negative forecast.

opposed to range forecasts.[8] Hence, the results in Table 2.4 suggest that firms issue more earnings forecasts that are unbiased and precise, thereby providing useful information to the market, as opposed to sending false signals to manipulate the market when stock prices experience non-fundamental shocks.

[Insert Table 2.4 Here]

Next, we test the effectiveness of management earnings forecasts in mitigating the price impact of mutual fund fire sales and facilitating price recovery ($H4$). We start by estimating the following model:

$$\begin{aligned} CAR[0,n]_{i,t} =& \beta_0 + \beta_1 MF\_D_{i,t} + \beta_2 MF\_D_{i,t} \times Managerial\_Response_{i,t+1} \\ & + \beta_3 Managerial\_Response_{i,t+1} + \sum_k \beta_k Controls_{i,t-1} \\ & + Quarter_t + Firm_i + \varepsilon_{i,t}. \end{aligned} \tag{1.4}$$

The dependent variables are average monthly market-adjusted cumulative abnormal returns from the beginning of the current quarter to four quarters or eight quarters after ($CAR[0,4]$ and $CAR[0,8]$), and the independent variable of interest is the interaction between mutual fund price pressure (MF_D) and $Managerial\_Response$ in terms of ***Guidance***. Here we define $Guidance$ as a binary variable that equals one if the firm issue earnings guidance in quarter $t+1$. As we show in Table 2.2 and Figures 2.1a and 2.1b, mutual fund fire sales create a significant downward pressure on the stock price even after four/eight quarters. Hence in Model (1.4), we measure the cumulative abnormal return from the beginning of the fire-sale quarter to four quarters after to see whether post-fire-sales earnings guidance is associated with an overall weaker price impact. In Panel A of Table 2.5, we show that the coefficient for $MF\_D_{i,t} \times Guidance_{i,t+1}$ is significantly positive while that for $MF\_D_{i,t}$ is negative, indicating that earnings guidance is related to an

[8] The results are robust if we focus on firm-quarter observations with at least one forecast issuance.

overall lower degree of underpricing after fire sales. We observe this weaker price change for guidance-issuing firms over both five-quarter (quarter t to $t+4$) and nine-quarter (quarter t to $t+8$) windows.

We also examine the speed of price recovery by estimating the following model among firm-quarter observations with mutual fund fire sales (i.e., *MFFLow* in the bottom decile):

$$\begin{aligned} CAR[1,n]_{i,t} =& \beta_0 + \beta_1 CAR[0,0]_{i,t} + \beta_2 CAR[0,0]_{i,t} \times Managerial\_Response_{i,t+1} \\ & + \beta_3 Managerial\_Response_{i,t+1} + \sum_k \beta_k Controls_{i,t-1} \\ & + Quarter_t + Firm_i + \varepsilon_{i,t}. \end{aligned} \tag{1.5}$$

The idea is that if stock price starts recovering after mutual fund fire sales over the subsequent n quarters, then the correlation between CAR[1,n] and CAR[0,0] should be negative. A significantly negative estimate for β_2 would then suggest that post-fire-sales earning guidance is associated with a stronger price reversal over the subsequent n quarters. We present the estimates in Panel B of Table 2.5. The estimates in column 1 show that β_2 is -0.019 and significant at the 10% level. This indicates that stock price reverses more sharply within one year after mutual fund fire sales, when managers issue more earnings forecasts as a response. In column 2 where we examine the abnormal return over the subsequent eight quarters, the coefficient for the interaction term is not significant. This is likely because price recovery largely finishes within two years whether or not earnings forecasts are issued. This observation is consistent with prior studies which show that price recovery from mutual fund fire sales is completed in two years on average (Coval and Stafford, 2007; Edmans, Goldstein, and Jiang, 2012). A plausible interpretation for the results in columns 1 and 2 jointly is that post-fire-sales price recovery is faster in the presence of earnings guidance.

In Panel C, we examine the relation between forecast characteristics and price recovery. Forecast precision is measured as the forecast width multiplied by negative 100 and scaled by beginning-of-quarter stock price, or zero for point forecasts. Forecast error is computed as the difference between the actual EPS and the manager's forecast (or midpoint of a range forecast), scaled by beginning-of-quarter stock price. We estimate Model (1.5) among firm-quarter observations that have extreme mutual fund outflows and at least one forecast issued in the subsequent quarter, and replace *Guidance* with binary variables that indicate forecasts with higher quality, as reflected by above-median level of precision or below-median level of error. The estimates in column 1 and 3 show that β_2 are -0.051 and -0.073, and significant at the 5% and 1% level, respectively. Collectively, the results indicate that it is mainly forecasts with higher quality that are related to faster price recovery.

[Insert Table 2.5 Here]

Overall, our findings suggest that managers increase the issuance of earnings guidance as a way to improve the information environment when mutual fund fire sales occur, and this change in disclosure policies appears useful in mitigating the adverse price effect of the shock. These findings are thus supportive of hypothesis $H4$.

1.4.3 Earnings Management as an Alternative Response to Market Disruptions

Despite the above evidence that earnings guidance appears effective in facilitating price recovery from mutual fund fire sales, it is not clear whether all firms, especially those with negative performance, choose to respond in this fashion. In this section, we focus on under-performing firms to examine whether they respond to mutual fund fire sales by increasing earnings guidance or resort to another approach. We begin by modifying Model (1.3) into the following form:

$$
\begin{aligned}
DisAccrual_{i,t+1} = & \beta_0 + \beta_1 MF\_D_{i,t} \times ROA\_High_{i,t-1} + \beta_2 MF\_D_{i,t} \times ROA\_Medium_{i,t-1} \\
& + \beta_3 MF\_D_{i,t} \times ROA\_Low_{i,t-1} + \beta_4 ROA\_High_{i,t-1} + \beta_5 ROA\_Low_{i,t-1} \\
& + \sum_k \beta_k Controls_{i,t-1} + Quarter_t + Firm_i + \varepsilon_{i,t}
\end{aligned}
\tag{1.6}
$$

We sort the sample into terciles based on firms' ex ante performance as reflected by lagged *ROA*, and interact MF_D with binary variables indicating firms with high/medium/low levels of expected operating performance. Based on hypothesis $H2$, we predict that firms with lower expected performance are less likely to increase the frequency of earnings guidance in response to mutual fund fire sales, and hence, β_1, β_2, and β_3 should be monotonically decreasing in magnitude. The estimates reported in column 1 of Table 2.6 confirm our prediction: the coefficient for $MF\_D_{i,t}$ monotonically increases in magnitude across lagged *ROA*, and is insignificant for firms with lagged *ROA* in the bottom tercile. Thus it appears that low expected performance deters firms from issuing voluntary forecasts.

What might under-performing firms do when they expect that greater disclosure would not necessarily aid stock price recovery? We consider another potential response – earnings management. Earnings management is usually adopted to hide poor performance or postpone a portion of current good earnings to the future periods (Watt and Zimmerman, 1978). As we discuss in hypothesis $H2$, in the case of mutual fund fire sales, firms that expect weak performance may respond to the fire sale by manipulating earnings upwards and sending false positive signals to the market. We therefore estimate Model (1.3) again using proxies of earnings management as the dependent variable. We follow the literature and use discretionary accruals based on modified Jones Model (Dechow, Sloan and Sweeney, 1995) as a proxy for earnings management. The estimates in column 2 of Table

2.6 show that when using discretionary accruals as the dependent variable, the coefficient for MF_D monotonically decreases in magnitude across lagged *ROA* and is significantly positive for firms with medium or low levels of expected performance. Hence, consistent with $H2$, firms that expect weak operating performance appear to adopt income-increasing discretionary accruals as an alternative response to mutual fund price shocks.

[Insert Table 2.6 Here]

We look further into the incentives of firms that manage earnings in response to mutual fund fire sales. As shown in the investment literature, mutual fund fire sales induce significant stock underpricing which will eventually be corrected by the market (Coval and Stafford, 2007). Hence managers that focus on long-term fundamental value do not necessarily take deliberate action to curb short-term mispricing. However, managers that are particularly concerned about current market valuation may be more proactive in responding to mutual fund fire sales. Extant literature shows that investors with short horizon can induce managerial myopia and suboptimal long-term investment decisions (Bushee, 1998; Polk and Sapienza, 2009). In this case, managers with more short-term investors may be more concerned about restoring firm value in the short term. We test our hypothesis $H3$, that firms that expect weak operating performance and also have short-term oriented shareholders are more likely to use earnings management as a response to mutual fund price shocks. We measure investor horizons based on institutional ownership (Aghion, Van Reenen, and Zingales, 2013), presence of blockholders (Edmans, 2009), and investors' turnover rate of their portfolios (Gaspar, Massa, and Matos, 2005), and estimate Model (1.6) in subsamples of firms with longer/shorter shareholder horizons. The estimates in Table 2.7 show that firms with bottom-tercile level of expected performance significantly increase discretionary accruals after mutual fund fire sales only when these firms have a below-median level of institutional ownership, with no blockholders present, or have an above-median level of investor turnover. These samples represent cases where shareholders are more short-term oriented. Therefore, our results are supportive of hypothesis $H3$:

It is firms that are underperforming, and whose managers are particularly concerned about short-term price levels, that use earnings management in response to mutual fund fire sales.

[Insert Table 2.7 Here]

Although we observe the use of both earnings guidance and earnings management in response to mutual fund fire sales, these two actions have opposite implications for the information environments: earnings guidance improves transparency while earnings management induces opacity. As our analysis shows, informational transparency rather than opacity is more likely to help correct mispricing. Further, our findings suggest that firms that use earnings management may tend to have lower expected firm performance. Hence, we expect earnings management to be associated with slower price recovery: because of greater opacity as well as lower fundamental value of the firms that choose to engage in earnings management. We test this prediction ($H4$) by repeating the price-recovery analysis for discretionary accruals.

In Panel A of Table 2.8 we present estimates of Model (1.4) for the effect of discretionary accruals on the overall price impact of fire sales. We interact MF_D with a binary variable that equals one if the firm's discretionary accrual is above sample median in quarter $t + 1$. Consistent with hypothesis $H4$, we do not find that post-fire-sales discretionary accruals mitigate the price impact of the fire sales. We also estimate the speed of price reversal using Model (1.5) among firm-quarter observations that have extreme mutual fund outflows, with the independent variable of interest being the interaction between fire-sale quarter CAR and a binary variable indicating firms with above-median discretionary accrual in the following quarter. As the estimates in Panel B indicate, discretionary accruals are also not related to a stronger price reversal in either four quarters or eight quarters following the price shocks. Hence, earnings management does not seem to mitigate the price impact of fire sales or facilitate price recovery.

1.4.4 Effectiveness of Managerial Responses: Addressing Endogeneity Concerns

Our empirical analysis indicates that firms endogenously choose between earnings guidance and earnings management as a response to mutual fund fire sales. This raises concerns about our interpretation of the price-recovery analysis. For instance, since firms' disclosure responses are related to firm performance, it is possible that better-performing firms would have had a faster recovery from fire sales regardless of their disclosure policies. Hence, we attempt to address the endogeneity by exploiting exogenous changes in disclosure policies due to regulatory changes. Specifically, we look into the passage of the Sarbanes-Oxley Act of 2002 (SOX). SOX was enacted in response to a number of accounting scandals such as those of Enron and WorldCom and was intended to enhance shareholder protection from accounting frauds. Prior studies show that the passage of SOX caused a substantial decrease in the use of discretionary accruals (Cohen, Dey, and Lys, 2008). Therefore, we use the passage of SOX to identify exogenous changes in the use of management earnings forecasts versus accrual-based earnings management during mutual fund fire sales. As we discuss in hypothesis $H5$, exogenous shift from earnings management to voluntary disclosure as a response to mutual fund fire sales due to SOX may facilitate price recovery. We first test whether SOX causes a shift in firms' response to stock underpricing using the following difference-in-differences model:

$$\begin{aligned} Managerial\_Response_{i,t+1} = &\beta_0 + \beta_1 MF\_D_{i,t} + \beta_2 MF\_D_{i,t} \times \text{Post\_SOX}_t + \beta_5 \text{Post\_SOX}_t \\ &+ \sum_k \beta_k Controls_{i,t-1} + Quarter_t + Firm_i + \varepsilon_{i,t}. \end{aligned} \tag{1.7}$$

Post_SOX$_t$ is a binary variable that equals one for firm-quarter observations after the passage of SOX. If SOX causes managers to switch from discretionary accruals to earnings guidance as a response to mutual fund price shocks, then β_2 should be significantly positive (negative) for the model of earnings guidance (discretionary accruals). Panel A of Table 2.9 presents the estimate of Model (1.7). The results in columns 1 and 2 show that, after the passage of SOX, firms increase (reduce) the use of earnings guidance (discretionary accruals) in response to stock underpricing. Thus this natural experiment can allow us to distinguish the effect of managerial response on price stability from firm attributes and managerial incentives. To further examine whether this exogenous change in the choice of action is related to faster price recovery, we estimate the following difference-in-differences model among observations with mutual fund fire sales in the bottom decile:

$$\begin{aligned} CAR[1,n]_{i,t} =& \beta_0 + \beta_1 CAR[0,0]_{i,t} + \beta_2 CAR[0,0]_{i,t} \times Post\_SOX_t + \beta_3 Post\_SOX_t \\ &+ \sum_k \beta_k Controls_{i,t-1} + Quarter_t + Firm_i + \varepsilon_{i,t}. \end{aligned} \tag{1.8}$$

Similar to Model (1.5), if exogenous increases (decreases) in earnings forecasts (discretionary accruals) due to SOX contribute to faster price recovery after mutual fund fire sales, we should expect β_2 to be significantly negative. In columns 1 and 2 of Panel B of Table 2.9, we show that the implementation of SOX is associated with stronger price reversal in the subsequent four quarters but not in eight quarters. This is consistent with the prediction that higher transparency as the result of expanded voluntary disclosure and less accounting manipulation accelerates the price recovery process.

To further strengthen the causal interpretation of the result, we look into cases where firms are more/less likely to be affected by SOX. Some firms were already compliant with the key requirements of SOX prior to the passage of the new regulation. These firms provide a good counterfactual for the effect of disclosure policies on price recovery because their disclosure policies are unlikely to change due to the new regulation. We partition the firm-quarter observations into two subsamples, in terms of whether or not firms were compliant with the key requirements of SOX (i.e., having a majority of independent directors and a fully independent audit committee) prior to its passage, and apply the above models to examine whether SOX has differential effects on disclosure policies and post-fire-sales price recovery in the two subsamples. We determine the status of SOX compliance for S&P 1,500 firms from 1998 to 2001, and perform diff-in-diff test for the compliant and non-compliant samples. As indicated, in columns 3 to 6 of Table 2.9, we find that only the non-compliant firms increase (decrease) earnings guidance frequency (discretionary accruals) in response to mutual fund fire sales. Moreover, the association between the passage of SOX and faster price recovery only holds for the non-compliant firms. These results suggest a causal relation between disclosure policy changes and price recovery from market disruptions.

[Insert Table 2.9 Here]

1.5 Conclusion

We examine managers' decisions regarding voluntary disclosure and financial reporting when stock underpricing occurs due to market disruptions. Using the mutual fund flow-driven price pressure as an exogenous market disruption that causes stock underpricing, we find that well-performing firms increase the frequency of earnings guidance, while under-performing firms engage in income-increasing accrual-based earnings management. Further investigation shows that the choice of earnings management is driven by managers' concerns over short-term price levels and their relatively poor performance. By comparing the two types of responses, we show that earnings guidance is more effective than earnings management in mitigating the adverse price impact of mutual fund fire sales. To address endogeneity issues, we examine the effect of SOX. We show that after the passage of SOX, firms that were previously not compliant with SOX's requirements were more likely to respond to fire sales by enhancing earnings guidance, rather than engaging in earnings management. These firms also appear to recover faster from mutual fund fire sales after the passage of SOX. This suggests a causal relation between disclosure policy changes and price recovery from market disruptions. Overall, our findings provide insight into firms' strategies in responding to disruptions in secondary stock markets, and highlight the importance of informational transparency in stabilizing prices during such disruptions.

Chapter 2

Does “Level Playing Field”Improve Real Efficiency?

2.1 Introduction

We examine how a change in the disclosure regime affects corporate investment decisions through its impact on stock market efficiency and managerial learning (i.e., market feedback). Several recent studies highlight that managers learn from the information in stock prices.[1] The notion is that prices can aggregate and reveal private information gleaned by millions of speculators (Hayek, 1945; Grossman, 1976). While managers may have precise information about their firms’ activities, stock prices can aggregate and convey information from many market participants (e.g., about industry conditions and competitors), that the manager may not have. In the paper, we focus on Regulation Fair Disclosure (Reg FD) to test for the effect of market learning and stock price informativeness on corporate decisions. The advantage of studying Reg FD is that it provides a plausibly natural experiment to evaluate links between disclosure, private information production, and real efficiency.

[1]Extant literature provides related evidence on investment decisions (Chen et al., 2007), mergers and acquisitions (Luo, 2005), cross-listing decisions (Foucault and Fresard, 2012), takeover activities (Bond et al., 2012), management forecasts (Zuo, 2016), and governance choices (Ferreira et al.,2011).

Reg FD was promulgated by the SEC to establish a “level playing field” for investors by prohibiting the use of selective disclosure.

Timely and accurate public disclosure has been deemed as a fundamental element of corporate policy and the foundation of securities regulations since 1930s in the United States.[2] A substantial body of literature supports the notion that the public release of corporate information can level the playing field and reduce the risk of liquidity traders to informed speculators. This may eventually translate into better resource allocation and a lower cost of capital (Diamond and Verrecchia, 1991; Easley and O’Hara, 2004). The SEC imposed Regulation Fair Disclosure in October 2000, as part of its continuing efforts to create a more level playing field for all investors. Reg FD prohibits the disclosure of all material firm-specific information to selective groups or individuals outside a firm, such as institutional investors or analysts.[3] The studies on the efficacy of the regulation report fairly mixed findings: some suggest Reg FD does level the playing field, whereas others suggest the elimination of selective disclosure created a “chilling effect”[4] on the flow of information, and the information environment deteriorated post-Reg FD.

It has been argued that price informativeness is important because prices can affect real decisions through managerial learning from stock prices (i.e., the market feedback effect (Edmans et al. 2015)). Identifying feedback effects is, however, challenging. The reason is that the mechanism involves interdependent factors: unobservable managerial learning channels and, the endogenous nature of both price informativeness and real decisions. In this context, Reg FD provides a natural experiment involving a change in the disclosure regime that can affect private information collection incentives and stock price informativeness. This shock to price informativeness provides an opportunity to examine whether changes in price informativeness exert an influence on real decisions.

[2]For example, Greenstone et al. (2006, p. 399) state:“(s)ince the passage of the Securities Act of 1933 and the Securities Exchange Act of 1934, the federal government has actively regulated U.S. equity markets. The centerpiece of these efforts is the mandated disclosure of financial information.”.

[3]If an inadvertent material information is selectively disclosed, a public announcement is required within 24 hours in a Form 8-K filing or through a media capable of mass and unbiased distribution (see SEC [2000a]).

[4]A reduction in the total amount of information available in the market (Koch et al., 2013).

Our first step is to show that the enactment of Reg FD does reduce price informativeness as measured by stock return nonsynchronicity.[5] Prior literature suggests that information effects of Reg FD should not be uniform across firms and are likely to hinge on a firm's proprietary costs of public disclosure. Consistent with this argument, we find the negative effect on price informativeness is stronger for firms facing higher proprietary information costs or with more analyst following in the pre-Reg FD period. In other words, the regulatory impact is generally greater for firms with characteristics indicative of more selective disclosure prior to the passage of the rule. Based on prior literature (Agrawal et al., 2006; Brown and Hillegeist, 2007; Wang, 2007; Albring et al., 2016), we utilize three such characteristics to capture firms' preferences for selective disclosure, including firms with: (1) higher analyst following, (2) positive R&D expenditures, and (3) more competition in product market. The first measure captures the extent to which firms tend to be impacted by Reg FD since analysts constitute the major participants of private conference calls preceding Reg FD; while the other two measures are proxies for proprietary information costs of public disclosure. Thus, the first part of our study collectively suggests that price informativeness suffers more for firms most affected by Reg FD. This evidence echoes Wang (2007), that documents that firms that are classified as pre-FD "private disclosers"[6] tend to have higher proprietary costs. They are, therefore, are more likely to replace private disclosure with nondisclosure rather than with public disclosure under the new disclosure regime. As a result, those firms experience substantial deterioration in their information environments.

Our results indicate that the effects of Reg FD on price informativeness only persist over a relatively short horizon (say over three to four years) on average. This suggests the possibility that firms may have been able to adapt to the rule and to disclose information in ways that do not violate Reg FD, yet are also protective of their proprietary costs. There

[5]The proxy for the amount of private information in price follows Chen et al. (2007), Dasgupta et al. (2010), and Edmans and Jayaraman (2016), that is (1-R^2), where R^2 is obtained from an expanded market model.

[6]This classification of firms reflects the extent to which managers provided private earnings guidance to analysts prior to Reg FD.

is evidence that some investors or analysts continue to have “selective access”to meet privately with executives despite the passage of this regulation (Bushee et al., 2013; Green et al., 2014; Soltes, 2014; Solomon and Soltes, 2015)[7]; nonetheless, firms’ alternative channels for disseminating private information appear to have become more limited. The horizon tends to be shorter for the firms that are affected more. However, this pattern of recovery is less likely for geographically dispersed firms,[8] suggesting that the local firms have advantages in private communication with investors.

We investigate the salient question of whether the negative effect of Reg FD on price informativeness affects real decisions (e.g., investment decisions). Since prior evidence shows a strong positive correlation between price informativeness and the investment-to-price sensitivity (Chen et al., 2007), we conjecture that the investment-to-price sensitivity is more likely to be impaired for the group of firms that experience larger decreases in their price informativeness in post-Reg FD regime. We conduct analyses by using difference-in-differences (DID) methodology. Reg FD helps distinguish a treated and control group of firms and assess the differential effect. Although Reg FD covers all U.S. public firms[9], it most impacts firms that previously relied on selective disclosure.[10] Hence, it is sensible to distinguish between a treated and control group of firms based on a firm’s pre-Reg FD preference for selective disclosure which, as discussed above, is largely determined by its proprietary costs of public disclosure and the degree of exposure to be Reg FD’s “target”. In our main analyses, we conduct the DID analysis by first interacting Tobin’s Q and the time dummy variable that indicates the passage of Reg FD as the explanatory variable of interest. We then partition the sample into treated and control groups in terms of the three firm characteristics (i.e., analyst followings, R&D expenditure, and industry competition)

[7]For example, one-on-one meetings scheduled for different subsets of participating investors to privately meet with CEOs, CFOs, and others in senior management, broker-hosted investor conferences, etc.

[8]Garcia and Norli (2012) develops a textual-based measure for geographic dispersion, categorizing firms into truely local and dispersed firms. We obtain this data from http://leeds-faculty.colorado.edu/garcia/page3.html.

[9]Only ADR firms are legally exempt from this regulation.

[10]It is also likely to be the same group of firms that inherently have more reliance on selective disclosure.

that we use to proxy for greater reliance on selective disclosure. The comparisons between estimate coefficients on the interaction terms for the two sets of firms shed light on the differential economic consequences caused by Reg FD.

Our findings with regard to investment decisions are threefold. First, we show that on average, the sensitivity of investment to price decreases significantly after Reg FD in our full sample period, from 1993 to 2007, which is 7 years before and after the enactment of Reg FD. Second, we find that the negative effects are only significant for subsamples that are identified as being more affected by Reg FD. In other words, firms with high analyst followings, positive R&D expenses, and more competition in product market are more likely to experience significant decreases of the investment to price sensitivity.[11] Third, we provide evidence that the negative impact of Reg FD on investment to price sensitivity becomes insignificant in a longer horizon among our sample years. This pattern echoes the first part of our study, in which we find that the negative impact on price informativeness is also diminished in long-run. Further, by separating the short-term (i.e., from 2001 to 2004) and long-term (i.e., from 2005 to 2007) effects, we show sharper differential consequences on investment decisions among subsamples. Therefore, the results are consistent with our conjectures, as only firms that are classified as treated groups are more likely to experience deterioration in their investment efficiency, and the sensitivity of investment to price tends to be restored with the passage of time following Reg FD's adoption. Taken together, given the existing argument that the managerial learning is more likely to take place when there is more private information impounded into stock prices (Chen et al., 2007; Gao and Liang, 2013), we provide empirical evidence on how Reg FD weakens information feedback effect through impeding private information channels, leading to less efficient investment decisions.

Despite a large body of research on the consequences of Reg FD, heretofore, there is

[11] We have two measures for investment: capital expenditure and the sum of capital expenditure and R&D expenses. When the first measure is adopted, the coefficient of interest is significant for treatment group, and insignificant for control group; although the differences between the two groups are not significant.

a paucity of evidence about its influence on firm value. With a research design similar to that for investment efficiency, we examine how Reg FD has an impact on firm value and whether it is different between our treated and control firms. The findings are as follows: (1) firm value decreases significantly in the post-Reg FD period; (2) again, the negative effect is more pronounced for treated groups (i.e., firms with more analyst followings, positive R&D expenses, and more competitors in product market); (3) the negative impact diminishes over time for the full sample, while it appears to be persistent for the treated group. Thus, it appears that the negative real effect of Reg FD on firm value is amplified, resulted from the narrowing managerial learning channels.

There is ongoing debate as to whether Reg FD changes analysts' information environment, and the extant literature is inconclusive (Bailey et al., 2003; Heflin et al., 2003; Gintschel and Markov, 2004; Francis et al., 2006; Agrawal et al., 2006; Kross and Suk, 2012)[12]. We mainly examine the changes of analysts' information metrics (forecast dispersion and accuracy) over our fourteen-year sample period surrounding Reg FD's adoption. Consistent with Agrawal et al. (2006), we show that earnings forecasts become less accurate post-Reg FD and the dispersion increases. More importantly, the declining quality of analyst forecasts has continued and become worse with the passage of time following Reg FD's adoption, which is in contrast to previous results that the reduced price informativeness and investment efficiency has recovered in late Reg-FD period. Overall, we interpret the results to indicate that managers are able to gradually alleviate the downsides brought

[12] Some early evidence by Heflin et al. (2003) and Bailey et al. (2003) shows a significant increase in forecast errors and forecast dispersion, based on univariate tests. However, Francis et al. (2006) replicate the analysis by adding ADR firms, and find that differences in analyst accuracy and dispersion after Reg FD are similar for both U.S. and ADR firms, indicating that those changes in analyst forecasts are attributed to contemporaneous economic events, since ADR firms are not subject to Reg FD. Nonetheless, this study supports Gintschel and Markov (2004) about the decrease in informativeness of analysts reports can be uniquely associated with Reg FD. A more comprehensive study by Agrawal et al. (2006) documents that individual and consensus forecasts become less accurate and forecast dispersion increases post-Reg FD, particularly forecasts made early in the quarter, for smaller firms and firms in industries where guidance is less likely. Differing from earlier works in that this study uses a longer sample period, a larger sample, partition results by firm size and industry, and employ a fixed effects panel regression to control for the analyst, company, and seasonality associated with a particular time. A recent study by Kross and Suk (2012) shows that analysts respond to public disclosure (e.g., earnings announcement, management forecasts and conference calls) more quickly, more frequently and with larger forecast revisions after FD.

about by Reg FD, while analysts may find it more difficult and restricted to regain their advantages in acquiring private information.

This study contributes to the literature in several ways. We believe that our study is the first to empirically examine whether a disclosure regime that emphasizes a level playing field could harm the quality of investment decision making. Our study is closely tied to the theoretical framework of Gao and Liang (2013) that suggests that Reg FD could have two opposing effects on firm value: (1) Liquidity could improve due to the reduced informational gap between informed and uninformed investors. (2) At the same time, elimination of privileged access to management could weaken some speculators' incentives to acquire private information. As a result, stock price informativeness could decline and adversely affect investment decisions where market learning plays a material role.

Broadly, we contribute to the growing literature on the real effects of secondary financial markets. In our analysis, we appeal to the research that indicates that learning from prices can be crucial for corporate investment decisions.[13] There is abundant evidence from both theoretical and empirical studies showing that informative prices enable superior decision-making: price efficiency promotes real efficiency (Bond et al., 2012), investment decisions are guided by private information embedded in the stock price (Luo, 2005; Chen et al., 2007; Bakke and Whited, 2010), and disclosure plays a role in helping price discovery and improving informational efficiency (e.g., Diamond and Verrecchia, 1991; Verrecchia, 2001; Easley and O'Hara, 2004). The literature recognizes the potential interaction between public disclosure and information acquisition by market participants. For example, it is argued that more public disclosure may mitigate private information search incentives (Gao and Liang, 2013), attract noise traders (Han et al., 2014, 2016), deter speculators from trading on private information (Bond and Goldstein, 2015), or crowd out other types of information (Tang, 2014; Goldstein and Yang, 2015; Edmans et al., 2016; Han et

[13] In addition, Bond and Goldstein (2015) documents that government always benefits from some reliance on market prices when making policy to intervene in firms, but too much transparency could hurt the government as it might reduce trading incentives.

al., 2016).[14] As a result, the information quality in stock prices from which decision makers can learn is reduced. Hence, the overall effect of disclosing information could be counter-productive since real efficiency suffers. We find evidence supportive of this prediction. Our tests are based on the change in disclosure regime, which reduces endogeneity concerns. Further, the differential impact of the regulation across sample firms is consistent with the negative effect of Reg FD on real efficiency being related reduced price informativeness.

We also contribute to a substantial literature on the effects of Reg FD. As noted earlier, extant studies primarily assess the impact of Reg FD on firms' information asymmetry. Some common findings from this line of research are that Reg FD increases the quantity of voluntary public disclosures and difference in opinion (Bailey et al., 2003; Heflin et al., 2003; Irani et al., 2003;), and reduces private information flows to equity analysts (Jorion et al., 2005; Francis et al., 2006). The evidence is mixed on other relevant aspects such as return volatility around earnings announcement (Bailey et al., 2003; Eleswarapu et al.,2004; Francis et al., 2006), cost of capital (Gomes et al., 2007; Duarte et al., 2008; Chen et al., 2010), information quality and informed trading (Eleswarapu et al., 2004; Chiyachantana et al., 2004; Ahmed and Schneible, 2007).

A survey by Koch et al. (2013) reports that the collective evidence from prior research is that Reg FD is effective in reducing selective disclosures, thereby partially achieved its goal by mitigating information advantage enjoyed by a privileged few (e.g., institutional investors and analysts) and providing a more level playing field, while the aggregate information available in the market seems to be unaffected or improved. On the flip side, the unintended consequences include chilling effects (e.g., firms replace selective disclosure with nondisclosure) and the deterioration of the information environment, particularly for small or high-technology firms.[15] Focusing on the unequally distributed adverse effects of

[14]There are some differences between the arguments made in these papers. In Edmans et al., (2016) disclosure can increase financial efficiency and lead to a reduction in real efficiency; while other studies are of the view that "disclosure is costly"in terms of both financial and real efficiency, consistent with the belief that financial efficiency increases real efficiency.

[15]Gomes et al. (2007) shows that Reg FD triggers a significant shift in analyst attention, which results in a welfare loss (higher cost of capital) for small firms. Wang (2007) notes that the effects of Reg FD are not

Reg FD, we find support for the view that the negative impact on price informativeness and investment decisions is more pronounced for firms with a preference for selective disclosure prior to enactment. Finally, we provide evidence that the effects of Reg FD weaken with the passage of time, in terms of affecting price informativeness and investment decisions. However, the negative effects of Reg FD on analyst forecast quality remain and even worsen over a longer horizon,[16] although there is evidence that analysts exert more effort producing their forecasts in the post-Reg FD period.[17].

The remainder of the paper is structured as follows. Section 2.2 outlines research design, describes the data and variable definitions. Section 2.3 presents our empirical results. Section 2.4 provides some additional analyses. Section 2.5 concludes.

2.2 Research design and sample selection

2.2.1 Identifying firms affected more by Reg FD

Based on prior research, we recognize that the information effects of Reg FD are unlikely to be uniform across firms and that it is affected by a firm's reliance on selective disclosure, or proprietary costs of public disclosure. As we noted earlier, although Reg FD covers all U.S. public firms, research such as Wang (2007) and Chen (2010) argue that one can provide sharper tests by focusing on cross-sectional differences in how Reg FD affects its target firms, e.g., it mostly impacts firms that relied on selective disclosure pre-Reg FD.

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uniform across firms, and finds that cross-sectional differences in firms' post-FD disclosure policies lead to cross-sectional differences in the changes in their information environments.

[16]This evidence is consistent with Agrawal et al. (2006), documenting that analysts' earnings estimates become less accurate and more dispersed in the post-Reg FD period, and dispersion continues to increase.

[17]For example, Mohanram and Sunder (2006) find there is a decline in the number of firms covered per analyst, and a shift to less-followed firms and to firms with less competition for information; Janakiraman et al. (2007) find that the timeliness of the first forecast made by analysts is reduced; Yang (2008) show that analysts decrease herding (i.e., the tendency to issue a forecast similar to prior forecasts); Kross and Suk (2012) suggest that analysts speed up their responses to public discourse, make larger forecast revisions per unit of surprise, but with lower dispersion and forecast errors following the disclosure. Collectively, these studies indicate that analysts work harder searching their private information to produce forecasts in post-Reg FD period.

Specifically, it is believed that selective disclosure as a strategic fashion to release firm-specific information is particularly important for firms with positive R&D expenditures, high litigation risk, more institutional informed trading, more analyst followings, or firms where public disclosure is associated with higher proprietary costs, such as it is likely to adversely influence a firm's competitive position in the product market or increase the probability of litigation. The economic framework by King et al. (1990) hypothesizes that a manager tends to disclose information privately to analysts if the firm faces higher proprietary information costs. The interpretation is that analysts can process and summarize the disaggregated private information into a summary report such as earnings forecasts without revealing the details, thereby having no negative effect on the firm's competitive position. We use two empirical measures for proprietary information costs: (1) *RND* is an indicator of 1 if firms have positive R&D expenditures; (2) measures of competitiveness in the firm's product market. As noted in Wang (2007) that some prior studies are refrained from using traditional measures such as Herfindahl-Hirschman Index to measure proprietary information costs, as the index is in industry-level, which seems to be lack of validity to capture intra-industry variation in proprietary information costs. To address this concern, we take advantage of a novel time-varying measure borrowed from Hoberg and Philips (2015)[18]: firm-by-firm pairwise similarity scores (*TNICsim*). It is constructed to capture the relatedness between firms. The authors use the method[19] of web crawling and text parsing algorithms to build firm pairwise product similarity based on how firms describe themselves in the product description section in their 10-K filings with the SEC, and thus allow each firm to have its own potentially unique set of competitors. Beside the novel measure for competitiveness, we also use the Herfindahl-Hirschman Index (*HHI*) for robustness checks, measured as the sum of squared market shares of all firms in an industry, where market shares are computed based on firm sales.

[18]Hoberg and Phillips generously provide access to their data at http://cwis.usc.edu/projects/industrydata/.

[19]The new method is termed as "text-based network industry classifications"(*TNIC*), which is analogous to a social network where each individual can have a distinct set of friends, or to geographic networks where the distance from a firm determines whether or not it is a competitor.

Selective disclosure does not necessarily mean high public disclosure costs (Hermalin and Weisbach, 2012), and two main reasons given by the SEC for the adoption of Reg FD include small investors fear that insiders profit at their expense (e.g., institutional investors are likely to receive favored private disclosure if they have larger shareholdings) and, managers use information to bribe analysts in exchange for a quid pro quo. Hence, our third measure for capturing a firm's preference for selective disclosure is the number of analyst followings (*Analyst*)[20], which takes on a value of 1 if a firm is above sample median[21]in most years preceding to Reg FD. However, we choose not to use investor base as an identifier due to the following two reasons. First, given the facts found in recent studies (Bushee et al., 2014; Solomon and Soltes, 2015) that some investors (particularly large shareholders) continue to meet privately with executives in spite of the passage of Reg FD, and this phenomenon tends to be more prevalent in recent years, we extrapolate that firms with more institutional investors, especially investors are classified as being dedicated[22], might not lose their stock price informativeness since this managerial learning channel has not been set back as it is supposed to be. Second, for short-horizon (or transient) investors, it is important to consider them as being endowed with heterogeneous private information with different precision levels (Chen et al., 2014). Thus, the association between short-horizon investors and price informativeness is not clear. In summary, we classify firms as having more reliance on selective disclosure if they have: more competitors in product market, more analyst followings, and positive research and development expenditures.

2.2.2 Measures of private information

Due to different benefits and costs of private information production (Grossman and Stiglitz, 1980), different stocks possibly have different amount of private information in their prices,

[20]Gomes et al. (2007) point out that analysts are typically characterized as key recipients of information through the selective disclosure channel.
[21]We also use different cutoffs to verify the results for robustness checks, such as the top tercile.
[22]The classification methodology refers to Bushee (1998).

in equilibrium. While it is difficult to directly measure such benefits and costs, prior literature has come up with measures relying on consequent asset returns and trading behaviors to estimate the equilibrium level of private information in price (Glosten and Harris, 1988; Hasbrouck, 1991; Easley et al., 1996; Llorente et al., 2002). Following Chen et al. (2007), we employ two measures in our analysis. The first measure is price nonsynchronicity, which is first proposed by Roll (1988) about firm-specific return variation is correlated with private information, and further developed by Morck et al. (2000), Durnev et al. (2003), and Durnev et al. (2004). It is derived from an expanded market model, computed as $1 - R^2$, where R^2 is the measure of goodness-of-fit of the following regression:

$$r_{i,j,t} = \alpha_{i,0} + \theta_i + \beta_{i,m} \times r_{m,t} + \beta_{i,j} \times r_{j,t} + \varepsilon_{i,t} \tag{2.1}$$

Where $r_{i,j,t}$ is the return of firm i in industry j at time t, $r_{m,t}$ is the CRSP value-weighted market return at time t, and $r_{j,t}$ is the return of industry j (3-digit SIC codes) at time t. This model implies there are three contributors to variation of a stock return: a market-related variation, an industry-related variation, and a firm-specific variation. The first two contributors are from systematic variations, and the last one captures price nonsynchronicity, here is measured by $1 - R^2$. Roll (1988) argue that prices incorporate new information by two ways: the revaluation of stock fundamental values following public information release, and the trading behaviors of speculators who glean and process private information. Roll (1988) point out that firm-specific stock price movements are not associated with information release that is already anticipated by the market, thus the private information is essential in the capitalization of firm-specific information. There has been a long debate about information environment and firm-specific return variation. In the country level, prior literature consistently shows that a well-developed economy or more transparent environment is associated with more firm-specific information and lower return synchronicity (Morck et al., 2000; Jin and Myers, 2006). While the results are mixed when carry over to the firm level. On the one hand, Durnev et al. (2003) found that higher stock price nonsynchronicity is related to superior information content about future earnings (i.e., stock prices' ability to predict firms' future earnings is high), implying that price nonsynchronicity reflects more private information than noise. Durnev et al. (2004) and Wurgler (2000) document that industries with higher firm-specific return variation tend to allocate capital more efficiently, according to high values of marginal Tobin's Qs. On the other hand, there is evidence suggesting that an improved firm-level transparency is positively correlated with return synchronicity, for example, Barberis et al. (2005) find that inclusion in the S&P 500 index increases stock return synchronicity, and Piotroski and Roulstone (2004) show that return synchronicity increases with the number of analyst followings. Dasgupta et al. (2010) highlight the importance of controlling for the β effect[23] in firm-level studies of R^2 when information environment needs to be taken into account. We alleviate this concern by conducting subsample analysis or directly including the factors that are tied in with β effect in the model.

The second measure is probability of informed trading, the PIN measure, which has been widely studied in both theoretical and empirical research with regard to its ability of capturing private information in stock price. I use the adjusted probability of information-based trading (AdjPIN) developed by Duarte and Young (2009). It is based on the market microstructure model by Easley et al. (1996), in which trades can come from noise traders or from informed traders. Conceptually, informed traders will trade on their information only if they think it is not yet publicly known. Duarte and Young (2009) decompose the original PIN into an information asymmetry component (adjusted PIN) and a non-information asymmetry component, so that the adjusted PIN more precisely capture

[23] The arguments of Dasgupta et al. (2010) are as follows. A simple version of R^2 from a regression of firm return on market return is $R^2 = SSR/SST = \beta^2 S_{xx}/(\beta^2 S_{xx} + SSE)$, hence an increase in return synchronicity comes from 3 sources, ceteris paribus: 1) an increase in marketwide return variation (S_{xx}); 2) a decrease in idiosyncratic return variation (SSE); and 3) an increase in beta (β), which is the stock's comovement with the market. At the country level, the aggregated β is exactly 1 by definition, whereas this is not the case at the firm level, such as more analyst coverage or S&P inclusions result in greater comovement with the market and the β as well. Therefore, Dasgupta et al. (2010) claim that the mixed results on R^2 at the firm level can be reconciled by this β effect.

the adverse selection component of the spread, or the amount of private information trading. Chen et al. (2007) argue that since PIN directly estimates the probability of informed trading, it could be a sound measure for private information reflected in stock price, in this sense, they are the first to relate the PIN to real investment, or use a market-microstructure measure in a corporate finance context.

2.2.3 Research design

We acknowledge that there are several confounding economic events surrounding the implementation of Reg FD, such as the Global Research Analyst Settlement, decimalization of stock exchanges, the Internet bubble, disclosure of accounting scandals, and the economic recession, which raising the concern that the economic environment of our sample firms and their investment decisions could be impacted by confounding events over the sample period. However, instead of drawing broad conclusions about the impact of Reg FD per se, we focus on the differential consequence of Reg FD by interacting it with firm attributes. In other words, we valuate narrower predictions and relate the interaction between the shift in disclosure regime and firm attributes (i.e., indicative of selective disclosure and "targeted"by Reg FD) to price informativeness, firm investment decisions and firm value.

An appropriate empirical setting for our analysis is the difference-in-difference (DID) methodology. The enactment of Reg FD provides a natural experimental setting, which appears to impose differential impacts across the affected firms. Hence, we could distinguish between a treatment group and a control group on the basis of a firm's tendency to choose selective disclosure, which rests with firms' proprietary information costs of public disclosure and potentially targeted professional groups by this regulation (i.e., financial analysts and large institutional investors). We discussed detailed criterion for identifying the treatment group firms in the previous section. Our contention is that the elimination of selective disclosure channels reduces advantages and incentives of speculators to acquire private information, leading to a reduction in firm-specific information that is impounded in stock price, especially for the treated firms. We base our analysis on the argument by Roll (1988) that private information is crucial in the capitalization of firm-specific information, as he initially found that firm-specific return variation does not seem to be associated with identifiable news release.

Thus, the first part of our analysis is to examine the impact of Reg FD on firm-specific information and how it is varying between the treatment group versus the control group. Our first baseline model specification is as below:

$$Info_{i,t} = \alpha_t + \theta_i + \beta_1 Post4_t + \beta_2 Post4_t \times Treat_{i,t} + \beta_3 Controls_{i,t} + \varepsilon_{i,t} \qquad (2.2)$$

Where $Info_{i,t}$ represents price informativeness, the amount of private information impounded in stock price. The two measures employed in this study are discussed in Section 2.2.2, denoted as $1 - R^2$ and $AdjPIN$. α_t is year fixed effects and θ_i is firm fixed effects. *Post4* is an indicator variable equal to 1 if the observation is from the first four years after the passage of Reg FD, i.e., fiscal years between 2001 and 2004, and 0 otherwise. This dummy variable identifies a shift in the disclosure regime that prohibited selective disclosure. For this regression, sample years begin with 1996 and exclude the event year 2000. This selection of event window is consistent with some recent studies related to Reg FD (Wang, 2007; Petacchi, 2015; Albring et al., 2016). *Treat* is a set of indicator variables determining our treatment group firms-those are most affected by Reg FD-that we have discussed in section 2.2.1. Note that all the indicators for treatment group are constructed in pre-Reg FD period. As model includes both firm and year fixed effects, it is not necessary to include the main effects of *Treat*. These variables are time invariant, which will be absorbed by the firm fixed effects. For all regressions, we correct standard errors to allow for clustering of errors at the firm level. The coefficient β_1 captures the average effect of the enactment of Reg FD on price informativeness in subsequent four years, and the coefficient of interest β_2 captures the differential changes in price informativeness between the treated and control firms in post-Reg FD. The hypothesis first predicts that Reg FD is intended to level playing field, the average effect is expected to be negative; besides, the cross-sectional variation in firms' reliance on selective disclosure is negatively related to changes in stocks' price informativeness; hence, both β_1 and β_2 are predicted to be negative.

Next, we estimate a model of the following form to test whether the impacts of Reg FD have lasted for a longer horizon, in which the event window is extended to seven years before and after Reg FD's adoption year, and we split the extended post-Reg FD period into two sub-periods, respectively:

$$\begin{aligned} Info_{i,t} = &\alpha_t + \theta_i + \beta_1 Post4_t + \beta_2 Post7_t + \beta_3 Post4_t \times Treat_{i,t} \\ &+ \beta_4 Post7_t \times Treat_{i,t} + \beta_5 Controls_{i,t} + \varepsilon_{i,t} \end{aligned} \tag{2.3}$$

In this test, we only use $(1 - R^2)$ to measure private information subject to the data availability. The difference of this model from Eq. (1) is the time indicators. *Post4* has the same definition as in Eq. (1), here it is referred as the first period of post-Reg FD (i.e., fiscal year between 2001 and 2004), and *Post7* indicates the second period of post-Reg FD, i.e., fiscal years between 2005 and 2007. Accordingly, we begin the sample period in 1993 so that the pre- and post-periods have an equal number of years, and again, we exclude the event year of 2000. Given prior findings that are suggestive of an overall increase in voluntary disclosure in the post-Reg FD period, and the continuing private access to management, we predict that the chilling effect on price informativeness will fade out over time. Therefore, we speculate that β_4 is less negative than β_3 or may become positive, and it is likely to be significantly different between the two coefficients.

Next, we conduct analysis to investigate the real effect of Reg FD on firms investment decision. The second baseline model specification is as follows:

$$I_{i,t+1} = \alpha_t + \theta_i + \beta_1 Post_t + \beta_2 Q_{i,t} + \beta_3 Q_{i,t} \times Post_t + \beta_4 Controls_{i,t} + \varepsilon_{i,t} \tag{2.4}$$

Where $I_{i,t+1}$ is firm $i's$ investment in year $t+1$, and β_t and θ_i represent year and firm-fixed effects. We use two different measures for the dependent variable ($I_{i,t+1}$): 1) $CAPX_{i,t+1}$, measured as capital expenditures scaled by beginning-of-year book assets ($A_{i,t}$); 2) $CAPXRND_{i,t+1}$, computed as the sum of capital expenditure and R&D expenses, scaled by $A_{i,t}$. Those are direct measures of firms' ongoing investment and R&D activities. $Q_{i,t}$ is the (normalized) price for firm i, which is calculated as the market value of equity (price multiplies shares outstanding from CRSP) plus the book value of assets minus the book value of equity, scaled by book assets[24]. The time dummy $Post_t$ is set to 1 if the observation is from the seven years after the passage of Reg FD, i.e., fiscal years between 2001 and 2007, and 0 otherwise. Combined the established linkage between private information in stock price and corporate investment decisions (Chen et al., 2007)[25] with the implications from our preceding hypothesis–Reg FD reduces the amount of private information in price, we predict a negative coefficient on the interaction term: $\beta_3 < 0$.

On top of the baseline test, again, we employ the methodology of DID to examine one of our main research questions, that is, how the impact of Reg FD on firms' sensitivity of investment to price varies with treated and control firms. To do this, we regress the model above respectively for our partitioned samples (i.e., treated and control firms), and compare the estimate coefficient of interest, β_3. A more negative and significant β_3 for the subsample of treated firms than that of control group will support our hypothesis.

Similarly, in order to separate the short-term and long-term effects for further inferences, we split the post-Reg FD 7-year sample into two shorter-window periods, and con-

[24] We define book value of equity as total shareholders' equity plus deferred taxes and investment tax credit (Compustat item TXDITCQ) minus the book value of preferred stock (Compustat item PSTKQ). We prefer the shareholders' equity numbers as reported by Compustat (Compustat item SEQQ). In case this data is not available, it is calculated as sum of common and preferred equity (Compustat items CEQQ and PSTKQ). If neither of the two are available, shareholders' equity is defined as the differences of total assets and total liabilities (Compustat items ATQ and LTQ).

[25] The study shows the learning channel is an important contributor to the sensitivity of investment-to-price.

duct the subsample tests as follows:

$$\begin{aligned} I_{i,t+1} =& \alpha_t + \theta_i + \beta_1 Post4_t + \beta_2 Post7_t + \beta_3 Q_{i,t} + \beta_4 Q_{i,t} \times Post4_t \\ &+ \beta_5 Q_{i,t} \times Post7_t + \beta_6 Controls_{i,t} + \varepsilon_{i,t} \end{aligned} \tag{2.5}$$

The variable definitions are the same as aforementioned. We predict the differential effects of Reg FD on investment-to-price sensitivity are larger between the two subsamples over a shorter-term horizon, i.e., the difference of β_4 between treated and control firms is more pronounced than that in the previous test.

Lastly, we apply the same methodology to examine Reg FD's influence on firm value and the quality of analyst forecasts:

$$\begin{aligned} Q_{i,t+1}/Dispersion_{i,t}/Error_{i,t} =& \alpha_t + \theta_i + \beta_1 Post_t + \beta_2 Post_t \times Treat_{i,t} \\ &+ \beta_3 Controls_{i,t} + \varepsilon_{i,t} \end{aligned} \tag{2.6}$$

$$\begin{aligned} Q_{i,t+1}/Dispersion_{i,t}/Error_{i,t} =& \alpha_t + \theta_i + \beta_1 Post4_t + \beta_2 Post7_t + \beta_3 Post4_t \times Treat_{i,t} \\ &+ \beta_4 Post7_t \times Treat_{i,t} + \beta_5 Controls_{i,t} + \varepsilon_{i,t} \end{aligned} \tag{2.7}$$

Where $Q_{i,t+1}$ is the Tobin's Q in year $t+1$; $Dispersion_{i,t}$ is the measure for analyst forecast dispersion in year t, computed as annual mean of standard deviation of monthly analyst earnings forecast, and scaled by mean monthly price; $Error_{i,t}$ is the measure for analyst forecast accuracy in year t, calculated as the annual mean of the difference between announced earnings as reported by I/B/E/S and the median of forecasts from individual analysts from the I/B/E/S detail data, and the difference is normalized by announced earnings. The causality between investment and firm value is not clear according to prior literature, but the association between the two is mostly shown to be positive. Thus, we predict the estimate coefficients on interactions (β_2 in Eq.2.6 and β_3 in Eq.2.5) are negative in the Tobin's Q model. As to extant research on how Reg FD changes analyst forecasts, the majority finds a deterioration in the forecast quality, in terms of increased dispersion and error. Therefore, we predict a positive sign on the interactions of analyst model. Regarding to the Q model with split post-Reg FD periods, we cannot directly refute alternative explanations that help us hypothesize whether the firm value exhibits the similar pattern of recovery as Investment-Q sensitivity does, since it is possible that the effect on firm value can not be fully captured by the changes in investment. Whereas a closely related study by Agrawal et al. (2006) show that forecast dispersion continues to increase over time after the passage of Reg FD, and some indirect evidence from Kross and Suk (2012) suggests that the changes in analysts' reliance on firms' public disclosure are caused by Reg FD are maintained one decade after Reg FD. Hence, we predict that the effects on the quality of analyst forecasts tend to persist, i.e., β_4 in Eq.2.5 is significantly positive.

2.2.4 Sample selection

We obtain firms' stock price and return information from Center for Research in Security Prices (CRSP), investment and other financial data from Compustat, analyst following and forecasts from I/B/E/S, institutional ownership from Thomson Financial 13F Database, the measure for firm-specific product similarity from Hoberg and Phillips Data Library. Consistent with prior research, we exclude financial firms (SIC codes 6000-6999) and utilities (SIC codes 4900-4999). For price informativeness tests, given the data availability of adjusted PIN from Duarte and Young (2009), we use eight-year sample period from 1996 to 2004, and specify 1996 to 1999 as the pre-Reg FD period[26] and 2001 to 2004 as the

[26]This setting is following Wang (2007) and Petacchi (2016). Wang (2007) starts the sample period in 1996 for reasons that the Private Securities Litigation Reform Act, which became effective in December 1995, provides broader safe harbor provisions for voluntary disclosures, and prior research suggests that it become more pervasive for managers to provide earnings guidance in the mid-nineties.

post-Reg FD period[27], omitting the regulatory change year of 2000. To investigate how the impacts have evolved with the passage of time relative to the effective date of Reg FD, we extend post-Reg FD period to 2007 and split it in two shorter windows to compare the results in different post-Reg FD periods (i.e., 2001-2004 versus 2005-2007). Accordingly, we extent pre-period to 1993 such that the pre- and post-periods have an equal number of years. We further exclude any observation that does not have data available to construct our control variables and the measures indicative of selective disclosure, which leaves 24,511 firm-year observations for eight-year sample period[28] and 44,800 for fourteen-year sample period meeting our requirements. All noncategorical variables are winsorized to the 1st and 99th percentiles of their distributions annually to mitigate the impact of outliers.

2.2.5 Descriptive statistics

Table 2.10 presents the summary statistics for all the variables used in our empirical analysis for the full sample of 57,112 firm-year observations. In the empirical analysis, all standard errors are adjusted for arbitrary heteroskadasticity and for error correlations clustered by firm. Also, as a standard procedure in the literature, we winsorize all unbounded variables at the 1st and 99th percentiles to mitigate the influences of outliers. The mean (standard deviation) of $(1-R^2)$ (denoted as *INFO*) is 0.809 (0.249), implying that the market and industry returns together explain about 19% of firms return variations, on average. This number is well in line with those documented in Roll (1988) and Chen et al.(2007), our number is slightly smaller though, which could attribute to the general decreasing return nonsynchronicity over time. Still, it is a sufficiently large number to suggest stock price movements are mostly driven by firm-specific information (Roll, 1988). The correlation of this private information measure and firm size, institutional ownership, the number of analyst followings are all significantly negative, consistent with those reported in Chen

[27]Petacchi (2016) ends the sample period in 2004 so that the pre- and post-periods have an equal number of years.

[28]Due to the data limitation, we have 13,152 observations when $AdjPIN$ measure is applied.

et al. (2007) and Dasgupta et al. (2010). We control for analyst following and institutional ownership in most of our tests for reasons discussed in section 2.2.3.

[Insert Table 2.10 Here]

2.3 Empirical Results

2.3.1 Price informativeness and Reg FD

Table 2.11 shows the estimated coefficients of our first baseline model Eq.2.2. The first two columns present the results by having price nonsynchronicity $(1 - R^2)$ as the dependent variable, while the last two columns have the adjusted PIN measure $(AdjPIN)$ as the dependent variable. Column (1) and (3) show the univariate analysis, and column (2) and (4) include the bunch of control variables and fixed effects. The estimate coefficients on $Post4$ are significantly negative through all the four models, suggesting that the passage of Reg FD reduces the amount of private information capitalized in stock prices on average. Then, we explore the cross-sectional difference among firms by interacting the dummy variables that are indicative of firms' severe reliance on selective disclosure with the time indicator ($Post4$).

[Insert Table 2.11 Here]

Table 2.12 present the results of the cross-sectional differences between treated and control firms caused by Reg FD. The dependent variable is price nonsynchronicity $(1 - R^2)$ for the first four columns and that is the adjusted PIN measure $(AdjPIN)$ for the last four columns. The coefficients of interest are those on interaction terms, which are shown to be negative for all regressions. The rationale is that firms who are identified as private disclosers prior to the implementation of Reg FD are most likely to be targeted by the regulation, thus the impact on target firms tends to be larger. The estimate coefficients in models (column (3) and (7)) with R&D as a criteria to differentiate treated firms from controls are insignificant, we interpret it as a result of quick responses/adjustments by innovative companies in their strategic disclosure policies after Reg FD. As it is indicated in Figure 2.3b that the price informativeness (measured by $1 - R^2$) of subsample firms with positive R&D expenses (blue line) declines more rapidly than their counterparts (red line), i.e., firms without R&D expenditures; however, the treated firms recover very quickly after the event year of 2000, where the intersection of the two lines occurs just one to two years afterwards. Similarly, Figure 2.3a show that blue line (represents firms with high analyst coverage) is apparently steeper than red line (represents firms with low analyst coverage) around the event year. Noticeably, the overall price nonsynchronicity is lower for the group of high analyst coverage, which is supportive of the argument by Dasgupta et al. (2010) that it is important to control for β effect since it can account for some confounded results in the literature. Figure 2.2a and Figure 2.2b show the average levels of price informativeness for subsamples that are partitioned by product similarity scores (Hoberg and Philips, 2015) and Herfindahl-Hirschman Index (HHI), respectively. The blue line represents the group of firms facing more competition from product market or industries they belong to, and both graphs show a greater decline for firms in more competitive environment.

[Insert Table 2.12 Here]

In Table 2.13, we report the results for examining Equation 2.3 where we extend sample period to seven years before and after the effective year of the regulation (i.e., from 1993 to 2007 and exclude the year of 2000) and split post-Reg FD period to two shorter windows which are indicated by time dummies Post4 (2001-2004) and Post7 (2005-2007). First of all, column (1) shows the overall effect of Reg FD in this longer sample period is negative[29]. In Column (2) to (4), where we respectively interact the sub-period dummies $Post4$ and $Post7$ with each of the three indicators for treated firms that are classified as private disclosers preceding to Reg FD, the estimate coefficients on interactions of $Post4 * Treat$

[29]Note that in this column, Post7 is set to 1 if observation is from year 2001 to 2007, which is different from its definition in other models.

are all significantly negative, while those on $Post7 * Treat$ are insignificantly negative or even become positive, and the differences between the coefficients (β_3 and β_4) within each model are all significant at 1% level (i.e., *P-value* is 0). The results support our hypothesis that firms are able to recover from the negative influence of the mandatory shift of disclosure regime that restrains firms from making selective disclosures. However, we do not note a significant difference between the estimate coefficients (β_3 and β_4) for column (5), where we include the measure for the extent to which a firm is geographically dispersed (as measured by Garcia and Norli [2012]). Since the variable $Treat$ is set to 1 if a firm falls into the most dispersed group, this finding suggests that more dispersed firms encountered greater difficulty in mitigating Reg FD's negative impact on information acquisition. In other words, more concentrated firms may promote communication with local investors as a response to the regulation.

[Insert Table 2.13 Here]

2.3.2 Investment-Q sensitivity and Reg FD

We present the results of investigating the impacts of Reg FD on investment-to-price sensitivity in Tables 2.14 to 2.17. First, we examine Equation 2.4 for the overall effect of Reg FD and report the results in Table 2.14, where the dependent variable is capital expenditure (*CAPX*) for first two models and that is capital expenditure plus R&D expenses for model (3) and (4). We include the sensitivity of investment to non-price measures of investment opportunities (e.g., cash flow) to rule out the possibility that the changes on the sensitivity of investment-Q could be washed out by other factors. The estimate coefficients on the interaction of $Q * Post$ are significantly negative for all models, and the magnitudes are larger for Model (3) and (4), suggesting that the effect appears to be more pronounced for firms with innovation activities.

[Insert Table 2.14 Here]

Built on the baseline test, we next partition our sample by the three indicators (i.e., the number of analyst followings, R&D expenses, and the degree of competition in product market) to capture the extent to which firms rely on selective disclosure, such that they get affected differently by Reg FD. The columns denoted by odd numbers estimate subsamples of treated firms, while the others are tests for control firms. By comparing the estimate coefficients on $Q * Post$ between treated and control groups, we show that only treated groups are significant and negative. This is consistent with our hypothesis that the prohibition of selective disclosure harms corporate investment decisions through narrowing channels of managerial learning, as only targeted firms who formerly have more reliance on private information in their stock prices experience deterioration in price nonsynchronicity as well as investment-Q sensitivity. The results echo the findings by Chen et al. (2007) that there is a strong positive correlation between the amount of private information in price and the investment-to-price sensitivity. Hence, we provide empirical evidence on how disclosure regime that underscores "level playing field"can have opposing effects on real efficiency. We acknowledge that there is somewhat a consensus about Reg FD has improved informational efficiency, for example, early study by Heflin et al. (2003) find that stock prices can better and more timely anticipate earnings announcements, and there is an increase in the volume of voluntary and forward-looking earnings guidance. Bushee et al. (2004) argue that although Reg FD had negative impacts on managers' decisions to continue hosting conference calls, for those voluntarily switched from close to open calls, information disclosed during the call period was not being cut down. Combined with the evidence that the amount of individual investor trading increased, the authors conclude that opening up previously closed conference calls prompts more information flows to the market. Ke et al. (2008) indicate that transient investors lose informational advantage[30]. Nevertheless, despite the fact that market efficiency is often considered as a good proxy for real efficiency, recent theoretical study by Goldstein and Yang (2014) indicates that this argument is only condi-

[30]Based on the finding that speculative trading behavior before bad news breaks was reduced after the implementation of Reg-FD.

tionally valid since the link between market efficiency and real efficiency is demonstrated to be delicate, especially when real decision makers learn information from the market to guide their actions. They point out that if regulation policies are not carefully designed, it is plausible that real efficiency deteriorates although market efficiency improves.

[Insert Table 2.15 Here]

Then, we split post-Reg FD period into two shorter windows with the same method as above. In Table 2.16, we partition the sample into treated and control firms by two of the firm characteristics indicative of selective disclosure preceding to the rule change, namely, the number of analyst followings and R&D expenses. Table 2.17 present the results with the similar model design but having the sample divided by the two competition measures: product similarity scores and Herfindahl-Hirschman Index, respectively. Columns denoted by odd numbers are for treated firms and the others are for controls. Both tables show that coefficients on interactions of $Post4 * Q$ are significantly negative for models with treated firms while those are insignificant for control groups. Also consistent with the results in Table 2.13, the coefficients on $Q * Post7$ are insignificant for nearly all columns, indicating that the negative effects of Reg FD on investment-Q sensitivity are diminished with the passage of time following the adoption of Reg FD.

Based on the established link between price nonsynchronicity and investment-Q sensitivity in Chen et al. (2007), the rationale of our study is that Reg FD prohibits the disclosure of material nonpublic information to selective groups or individuals, which is presumed to inhibit the aggregation and production of private information, leading to a decline in price informativeness; consequently, the feedback from the stock market to real decision makers was weakened, thus the shift of disclosure regime inadvertently caused a deterioration in corporate investment decisions. The recovery pattern for both price informativeness and investmnet-Q sensitivity could be ascribed to the following reasons. From the regulation side, the final regulation did not explicitly prohibit private meetings with investors[31], and

[31]Reg FD only requires that contents of any private conversation between investors and management must

very limited guidance has been offered as to what types of questions and responses could be counted as violation, so as to managers can have considerable latitude to interpret what information constitutes as material. Since we noted earlier that there is anecdotal evidence about private meetings between executives and some investors continue to occur regularly (Soltes, 2014; Solomon and Soltes, 2015) regardless of Reg FD, even without revealing material information during meetings, any non-material information might be used in a ‘mosaic”, or having private signals confirmed. Our results suggest that the possible private meetings help firms recover from losing private information in their stock prices and therefore improve investment decisions.

[Insert Table 2.16 and Table 2.17 Here]

2.3.3 Other unintended consequences of Reg FD

We lastly explore if there were other real effects caused by Reg FD’s adoption. Table 2.18 report the results by examining the effect on firm value, proxied by Tobin’s Q. In columns (1) to (3), we compare the firm value for periods of 7 years pre- and post-Reg FD between our treated and control firms. The coefficient of interest is $Post * Treat$, which are significantly negative for all the three models. We split post-Reg FD period into two sub-periods as above, and the results are presented in columns (4) to (6). Interestingly, the estimate coefficients on $Post4 * Treat$ and $Post7 * Treat$ are all significantly negative, implying that firm value did not recover (or at least took longer to recover) as the trend we show in tests for price informativeness and investment-Q sensitivity. Hence, we find robust empirical evidence in support of the hypothesis proposed by Gao and Liang (2013) in their theoretical framework that reduce private information acquisition can have negative effects on the firm value, when informational feedback from the stock market to real decisions is taken into account.

Table 2.19 present results of how Reg FD has an impact on analyst forecast quality,

comply with Reg FD (i.e. no new material information).

measured by two analysts' information metrics: forecast dispersion and error. In column (1) and (2), we simply examine the overall effect from the passage of Reg FD, and show that the estimate coefficients on the time dummy $Post$ (i.e., it is set to 1 if observation is from a year after 2000) are significantly positive for both metrics, implying that on average, analyst forecast dispersion and error increase following the rule change. Then we interact $Post$ with the product similarity score ($TNICSim$), to examine if the regulation causes differential impacts across firms. The estimate coefficients on the interactions are significantly positive, implying that firms with more competitors in product market are likely to experience more deterioration in the analyst forecast quality in post-Reg FD. Similar to previous tests, we conduct the analysis by splitting the post-Reg FD period into two sub-periods, the results show that the deterioration in analyst report quality becomes more severe in the second post-period, consistent with arguments in prior literature (Agrawal et al., 2006; Kross and Suk, 2012) that the changes in analysts' behavior caused by Reg FD are maintained one decade after the regulation. Despite the fact that in the new disclosure regime, more effort demanded from analysts as to search for more information in order to distinguish themselves (Bailey et al.,2003; Mohanram and Sunder, 2006; Mensah and Yang, 2008), the continuing deterioration can be partially explained by the nature of analyst forecast functioning, e.g., analysts place larger than efficient weights on their private information when they forecast corporate earnings (Chen and Jiang, 2006). As the channels for them to glean private information has been restricted in a long-run, it is relatively difficult for them to regain their information advantage.

[Insert Table 2.18 and Table 2.19 Here]

2.4 Additional analysis and other robustness checks

We next carry out a range of additional analyses to evaluate the robustness of our findings and to raise the confidence for any conclusions drawn. First, to rule out the possibility that our results are driven by the Internet bubble, we rerun the analysis excluding firms in high-tech industries[32] and find the similar results. Another concern is that the changes on investment-Q sensitivity are driven by industry-wide variations, thus we decompose Tobin's Q measure into industry-Q and residual-Q (firm-specific) to examine whether the investment is sensitive to firm-specific price information. The results show that only the residual-Q plays a significant role.

2.5 Conclusion

We exploit the undesirable effects of Reg FD by examining links between mandatory disclosure, private information acquisition, and investment efficiency. We document that the prohibited selective disclosure after Reg FD reduces price informativeness, investment-to-price sensitivity, reporting quality of analyst forecasts, and firm value. Notably, the impacts are more striking for firms who tend to rely more on private communication with outsiders. Further, we show that most of these adverse effects, however, are reversed to some extent over time, except for analyst forecast quality. While a fully recovery for firm value is likely to take longer, especially for geographically dispersed firms. Hence, the results highlight a bright side of allowing privileged access to management, as which benefits the informational feedback from the stock market to real decisions.

[32]Following Bushee et al.(2003), SIC codes classified as high-tech industries include: Drugs (2833-2836); Electric Distribution Equipment (3612-3613); Electrical Industrial Apparatus (3621-3629); Household Audio & Video Equipment (3651-3652); Communications Equipment (3661-3669);Electron Tubes (3671); Printed Circuit Boards (3672); Semiconductors & Related Devices (3674); Magnetic and Optical Recording Media(3695); Telephone Communications (4812-4822); Radio & TV Broadcasting(4832-4899); and Computer and Data Processing Services (7370-7379).

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Appendix

Appendix A: Variable Definitions for Chapter 1

A.1. Dependent variables

- *CAR* is the average monthly cumulative abnormal return over various windows.
- *DisAccrual* is the discretionary accruals, measured using modified Jones Model (Dechow, Sloan and Sweeney(1995)).
- *Guidance* is the logarithm of one plus the number of earnings forecasts provided by the management in the fiscal quarter.
- *Forecast News* (*NEWS*) is computed as the difference between the point estimate (or the midpoint estimate of the range forecast) and the consensus analyst forecast, scaled by beginning-of-quarter stock price. The consensus analyst forecast is the median of analyst forecasts at the time of the management earnings forecast. Based on the sign of forecast news, each earnings forecast is classified as conveying good news (*NEWS* > 0) or bad news (*NEWS* < 0).
- *Forecast Precision* (*PRECISION*) is the negative of earnings forecast width. For range estimate, forecast width is the difference between the upper- and the lower-end estimates, scaled by beginning-of- quarter stock price; while for point estimate, forecast width is assigned as zero.
- *Forecast Error* (*ERROR*)is defined as the absolute difference between earnings forecast and actual earnings, scaled by beginning-of-quarter stock price.

A.2. Main Independent Variables

- *MFFlow* is the hypothetical mutual fund outflow provided by Edmans, Goldstein, and Jiang (2012).
- MF_D is a binary variable that equals one if a firm's quarterly *MFFlow* is below the 10th percentile, and zero otherwise.
- *Analyst Coverage* is the average number of analysts following the stock for the fiscal quarter. It is coded as 0 if there is no coverage from I/B/E/S.
- *Analyst Dispersion* is defined as standard deviation of analyst earnings forecasts in the current quarter, scaled by beginning-of-quarter share price.
- *Amihud* is defined as $ln(1 + AvgILLIQ \times 10^6)$, where $AvgILLIQ$ is an average illiquidity for stock i in quarter t from daily data. Specifically, *AvgILLIQ* is calculated as the absolute return divided by dollar trading volume:$AvgILLIQ_{i,t} = \frac{1}{Days_{i,t}} \sum_{d=1}^{Days_{i,t}} \frac{|R_{i,t,d}|}{DolVol_{i,t,d}}$, where $Days_{i,t}$ is the number of valid observation days for stock i in fiscal year t, and $R_{i,t,d}$ and $DolVol_{i,t,d}$ are the return and dollar trading volume of stock i on day d in the current quarter t.
- $Bid - Ask\ Spread_{i,t} = \frac{1}{Days_{i,t}} \sum_{d=1}^{Days_{i,t}} \frac{Ask_{i,t,d} - Bid_{i,t,d}}{(Ask_{i,t,d} + Bid_{i,t,d})/2}$ where $Days_{i,t}$ is the number of observations for stock i in fiscal year t, and $Ask_{i,t,d}$ and $Bid_{i,t,d}$ are the closing ask and bid prices of the stock i on day d of quarter t.
- *Blockholder* is institutional investor that holds more than 5% of a firm's stock.

- *Institutional Ownership* is the ratio of the number of shares held by institutional investors to the total number of shares outstanding for a firm.
- *Investor Turnover* is measured based on the average of investors' churn rate following (Gaspar, Massa, and Matos, 2005).

A.3. Control Variables

- *SIGMA* is standard deviation of a firm's daily stock returns over the current quarter.
- *Turnover* is calculated as the mean of daily trading volume divided by the total number of shares outstanding over the current quarter.
- *MRET* is the mean of daily returns over the current quarter.
- *Tobin's Q* is computed as market value of equity plus book value of assets minus book value of equity, scaled by book value of assets.
- *ROA* is income before extraordinary items divided by lagged total assets.
- *SIZE* is log of the market value of equity.
- *Leverage* is the sum of long-term debt and debt in current liabilities divided by total assets.

Appendix B: Variable Definitions for Chapter 2

B.1. Dependent variables: Price informativeness and Investment

- *INFO* is a measure of the private information in stock price, defined as 1-R^2, where R^2 is the goodness-of-fit from a regression of firm i's daily return on the value-weighted market return and the return of the three-digit SIC industry portfolio over year t.
- *PIN* is the probability of informed trading, another measure for price informativeness. We use both PIN measure per Easley et al. (1996) and its extended version, adjusted PIN by Duarte and Young (2009).
- *CAPX* is capital expenditure scaled by beginning-of-year assets.
- *CAPXRND* is capital expenditure plus R&D expenses scaled by beginning-of-year assets.

B.2. Main Independent Variables

- *Q* is computed as market value of equity plus book value of assets minus book value of equity, scaled by book value of assets.
- *Analyst* is the average number of analysts following firm i for the fiscal year. It is coded as 0 if there is no coverage from I/B/E/S. In subsample tests, the corresponding dummy variable is equal to 1 if the number of analyst followings of firm i' is in the top tercile among sample firms at least twice in four years preceding to Reg FD, and 0 otherwise.
- *Dispersion* is defined as annual mean of standard deviation of monthly analyst earnings forecast (scaled by mean monthly price).
- *Error* is defined as the annual mean of the difference between announced earnings as reported by I/B/E/S and the median of forecasts from individual analysts from the I/B/E/S detail data, where the difference is normalized by announced earnings.
- *TNICSim* is the text-based product similarity scores, following Hoberg and Phillips (2015). The access to the data is provided at http://cwis.usc.edu/projects/industrydata/. It is an indicator variable equal to 1 if a firm with the score above sample median at least twice in four years preceding to Reg FD, and 0 otherwise.
- *HHI* is the sales-based HirschmannHerfindahl index calculated at the four-digit SIC code level: $HHI_{jt} = \sum_{i=1}^{N_j} S_{ijt}^2$, where S_{ijt} is the market share of firm i in industry j in year t. The same way to define its indicator variable as it does to the previous variable *TNICSim*.

- *R&D* is an indicator of 1 if firms have positive R&D expenditure in consecutive two years prior to Reg FD.
- *GEO* is a textual-based measure for geographic dispersion, categorizing firms into truely local and dispersed firms. Garcia and Norli (2012) provides the access to this data at http://leeds-faculty.colorado.edu/garcia/page3.htm

B.3. Control Variables

- *InstOwn* is ownership of institutional investors, constructed as the ratio of the number of shares held by institutional investors to the total number of shares outstanding for a firm.
- *DRET* is the standard deviation of monthly returns over the fiscal-year period.
- *ROA* is income before extraordinary items divided by lagged total assets.
- *SIZE* is the natural logarithm of total assets.
- *BLEV* is the sum of long term debt and debt in current liabilities divided by total assets.
- *CFO* is cash flow from operations deflated by average total assets.
- *RND* is research and development intensity, calculated as R&D expenses scaled by average total assets.
- *Loss* is a binary variable that equals one if a firm reports a loss in current year,zero otherwise.
- *1/Asset* is the inverse of total asset.
- *SaleGrowth* is the difference between current net sales and lagged net sales divided by lagged net sales.

Tables and Figures

Table 2.1: Descriptive Statistics

This table presents the summary statistics and correlation matrix of the main variables used in our analyses. We winsorize all firm characteristics at the 1st and 99th percentiles. Definitions of the variables are provided in the Appendix.

Panel A: Summary Statistics

	N	Mean	S.D.	25%	Median	75%
Price Impact Variables:						
CAR[0,0]	211516	0.003	0.099	-0.051	-0.002	0.048
CAR[1,4]	211201	0.004	0.053	-0.023	0.002	0.028
CAR[1,8]	211225	0.005	0.042	-0.014	0.004	0.024
Managerial Responses:						
Guidance	176393	0.162	0.370	0.000	0.000	0.000
DisAccrual	126129	0.004	0.118	-0.042	0.000	0.044
Guidance Precision	18904	-0.002	0.004	-0.003	-0.001	0.000
Guidance Error	18904	0.022	0.037	0.001	0.005	0.029
Main Independent Variables:						
MFFlow	211516	-0.238	0.692	-0.131	0.000	0.000
Amihud	211516	3.754	2.918	1.626	3.680	5.824
Ln(Bid_Ask Spread)	198577	-4.319	1.307	-5.115	-4.104	-3.368
Analyst Coverage	211516	1.160	0.840	0.511	1.099	1.792
Forecast Dispersion	135866	0.010	0.025	0.001	0.003	0.008
ROA	211516	-0.029	0.304	-0.031	0.038	0.089
Inst. Ownership	211516	0.346	0.290	0.069	0.305	0.582
Block5	174686	1.572	1.469	0.000	1.000	2.000
Control Variables:						
Size	211516	5.492	1.829	4.162	5.305	6.652
Tobin's Q	211516	2.264	2.517	1.123	1.562	2.476
Leverage	211516	0.214	0.224	0.016	0.168	0.340
Turnover	211516	0.007	0.007	0.002	0.004	0.008
MRet	211516	0.001	0.005	-0.001	0.001	0.003
Sigma	211516	0.037	0.022	0.022	0.032	0.046

Panel B: Correlation Matrix

Variables	$CAR[0,0]_{VW}$	$CAR[1,4]_{VW}$	MF_D	$Guidance$	$DisAccrual$	$Analyst$	$Amihud$	ROA	*Inst. Ownership*	$Size$
$CAR[0,0]_{VW}$	1.000									
$CAR[1,4]_{VW}$	0.014	1.000								
MF_D	-0.042	0.015	1.000							
$Guidance$	0.006	-0.003	0.076	1.000						
$DisAccrual$	0.036	-0.016	0.015	0.013	1.000					
$Analyst$	-0.030	-0.047	0.009	0.291	-0.012	1.000				
$Amihud$	0.034	0.088	-0.021	-0.301	-0.017	-0.713	1.000			
ROA	0.043	-0.002	0.066	0.111	0.146	0.138	-0.177	1.000		
$Inst.Ownership$	-0.005	-0.027	0.222	0.203	0.005	0.416	-0.493	0.125	1.000	
$Size$	-0.012	-0.033	0.052	0.215	-0.046	0.595	-0.722	0.254	0.376	1.000

Table 2.2: The Price Impact of Mutual Fund Fire Sales

Panel A presents estimates of the impact of mutual fund fire sales on stock return over various horizons. The dependent variables are cumulative abnormal returns (CAR) during or after mutual fund fire sales. CAR is average monthly adjusted by equal-weighted or value-weighted market returns and over different event windows: the current quarter ($CAR[0,0]$), the subsequent four quarters ($CAR[1,4]$), and the subsequent eight quarters ($CAR[1,8]$). The independent variable of interest is MF_D, a binary variable that equals one for firm-quarter observations with *MFFlow* in the bottom decile. We control for firm characteristics, including *Size*, *Tobin's Q*, *Leverage*, *ROA*, *Turnover*, *MRet*, *Sigma*, *Analyst Coverage*, *Amihud*, and *Inst. Ownership*, measured in quarter $t-1$. We require each firm-quarter observation to have non-missing value for the variables and winsorize the variables at the $1st$ and $99th$ percentiles. We also include firm fixed effects and fiscal and calendar year-quarter fixed effects in the regressions. t-statistics with firm-clustered standard errors are in parentheses. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Price Impact over Various Horizons

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	$CAR[0,0]_{EW}$	$CAR[0,0]_{VW}$	$CAR[1,4]_{EW}$	$CAR[1,4]_{VW}$	$CAR[1,8]_{EW}$	$CAR[1,8]_{VW}$
MF_D_t	-0.015***	-0.014***	-0.002***	-0.002***	0.000	0.000
	(-21.84)	(-21.72)	(-4.06)	(-3.75)	(1.31)	(1.25)
Analyst Coverage	-0.003***	-0.003***	-0.000	-0.000	0.000	0.000
	(-5.68)	(-5.55)	(-0.14)	(-0.10)	(0.13)	(0.18)
Amihud	0.009***	0.009***	0.008***	0.008***	0.006***	0.006***
	(25.31)	(25.67)	(28.44)	(28.30)	(27.88)	(27.68)
Size	-0.015***	-0.015***	-0.012***	-0.012***	-0.008***	-0.008***
	(-19.17)	(-19.08)	(-17.94)	(-17.97)	(-13.14)	(-13.00)
Tobin's Q	-0.001***	-0.001***	-0.003***	-0.003***	-0.002***	-0.002***
	(-4.27)	(-4.19)	(-13.81)	(-13.79)	(-14.04)	(-13.96)
Leverage	-0.006**	-0.006**	-0.002	-0.001	-0.003*	-0.004*
	(-2.37)	(-2.50)	(-0.79)	(-0.73)	(-1.87)	(-1.91)
ROA	0.025***	0.025***	-0.003***	-0.003***	-0.002***	-0.002***
	(15.27)	(15.38)	(-3.04)	(-2.96)	(-2.95)	(-2.72)
Turnover	0.107	0.109	-0.117**	-0.115**	-0.045	-0.045
	(1.50)	(1.53)	(-2.39)	(-2.33)	(-1.11)	(-1.10)
MRet	-0.839***	-0.892***	-0.415***	-0.413***	-0.501***	-0.497***
	(-11.66)	(-12.37)	(-11.82)	(-11.74)	(-20.02)	(-19.75)
Sigma	0.146***	0.143***	0.110***	0.108***	0.057***	0.054***
	(5.68)	(5.57)	(6.60)	(6.48)	(4.24)	(3.99)
Inst. Ownership	-0.008***	-0.007***	-0.001	-0.001	-0.000	-0.000
	(-3.46)	(-3.24)	(-0.67)	(-0.69)	(-0.09)	(-0.04)
Adjusted R^2	0.054	0.096	0.233	0.265	0.383	0.408
Observations	211516	211516	211256	211256	211323	211323
Firm Fixed Effects	YES	YES	YES	YES	YES	YES
Time Fixed Effect	YES	YES	YES	YES	YES	YES

Panel B presents estimates of the relation between the quality of information environment and the price impact of mutual fund fire sales. The dependent variables are average monthly cumulative abnormal returns (CAR) in quarter t. The independent variable of interest is the interaction between MF_D, a binary variable that equals one for firm-quarter observations with *MFFlow* in the bottom decile, and binary variables indicating firms with high/medium/low quality of information environment. We sort the sample into terciles based on analyst coverage, and the negative value of analyst dispersion, Amihud's illiquidity, and bid-ask spread. We control for firm characteristics, including *Size*, *Tobin's Q*, *Leverage*, *ROA*, *Turnover*, *MRet*, *Sigma*, *Analyst Coverage*, *Amihud*, and *Inst. Ownership*, measured in quarter $t-1$. We require each firm-quarter observation to have non-missing value for the variables and winsorize the variables at the $1st$ and $99th$ percentiles. We also include firm fixed effects and fiscal and calendar year-quarter fixed effects in the regressions. t-statistics with firm-clustered standard errors are in parentheses. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel B: Information Environment and Price Impact

Information Measure:	*Analyst Coverage*		*-Forecast Dispersion*		*-(Amihud's Illiquidity)*		*-(Bid-Ask Spread)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variables	$CAR[0,0]_{EW}$	$CAR[0,0]_{VW}$	$CAR[0,0]_{EW}$	$CAR[0,0]_{VW}$	$CAR[0,0]_{EW}$	$CAR[0,0]_{VW}$	$CAR[0,0]_{EW}$	$CAR[0,0]_{VW}$
MF_D_t × Info_High$_{t-1}$	-0.010***	-0.010***	-0.006***	-0.006***	-0.008***	-0.008***	-0.008***	-0.008***
	(-10.39)	(-10.15)	(-6.02)	(-5.90)	(-10.00)	(-9.73)	(-10.20)	(-9.95)
MF_D_t × Info_Median$_{t-1}$	-0.014***	-0.014***	-0.014***	-0.014***	-0.019***	-0.019***	-0.019***	-0.019***
	(-14.29)	(-14.30)	(-12.00)	(-11.97)	(-17.29)	(-17.23)	(-16.41)	(-16.31)
MF_D_t × Info_Low$_{t-1}$	-0.023***	-0.023***	-0.023***	-0.022***	-0.023***	-0.024***	-0.024***	-0.024***
	(-17.18)	(-17.28)	(-13.94)	(-13.78)	(-13.38)	(-13.63)	(-12.69)	(-12.86)
Info_High$_{t-1}$	-0.001	-0.001	-0.003***	-0.003***	-0.010***	-0.010***	-0.006***	-0.007***
	(-1.03)	(-1.04)	(-3.66)	(-3.79)	(-11.27)	(-11.41)	(-7.15)	(-7.40)
Info_Low$_{t-1}$	0.005***	0.005***	0.004***	0.004***	0.015***	0.015***	0.008***	0.008***
	(6.60)	(6.61)	(4.80)	(4.75)	(13.87)	(14.18)	(7.89)	(8.13)
Adjusted R^2	0.054	0.096	0.074	0.106	0.051	0.093	0.050	0.092
Observations	211516	211516	135866	135866	211516	211516	198577	198577
P-value(MF_D×High=MF_D×Low)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Control Variables	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES

Table 2.3: Mutual Fund Fire Sales and Earnings Guidance

This table presents estimates of the effect of mutual fund fire sales on management earnings guidance. The dependent variable is $Earnings\ Guidance_{t+1}$, the natural logarithm of one plus the frequency of management earnings forecasts in quarter $t+1$. The independent variable of interest is MF_D_t, a binary variable that equals one for firm-quarter observations with *MFFlow* in the bottom decile. We control for firm characteristics, including *Size*, *Tobin's Q*, *Leverage*, *ROA*, *Turnover*, *MRet*, *Sigma*, *Analyst Coverage*, *Amihud*, and *Inst. Ownership*, measured in quarter $t-1$. We require each firm-quarter observation to have non-missing value for the variables and winsorize the variables at the $1st$ and $99th$ percentiles. We also include firm fixed effects and fiscal and calendar year-quarter fixed effects in the regressions. t-statistics with firm-clustered standard errors are in parentheses. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:		$Earnings\ Guidance_{t+1}$	
	(1)	(2)	(3)
MF_D_t	0.028***	0.020***	0.022***
	(10.01)	(6.78)	(6.31)
Size		0.045***	0.021***
		(14.52)	(4.34)
Tobin's Q		0.003***	0.000
		(7.25)	(0.38)
Leverage		-0.028***	0.004
		(-3.50)	(0.30)
ROA		0.011***	0.027***
		(5.44)	(7.39)
Turnover		0.976***	-1.312***
		(6.19)	(-4.53)
MRet		0.087	0.717***
		(0.76)	(4.07)
Sigma		-0.192***	-0.052
		(-4.74)	(-0.62)
Inst. Ownership		0.115***	0.028*
		(9.41)	(1.88)
Analyst Coverage			0.036***
			(11.53)
Amihud			-0.021***
			(-12.37)
Adjusted R^2	0.387	0.392	0.394
Observations	354830	252812	176553
Firm Fixed Effects	YES	YES	YES
Time Fixed Effects	YES	YES	YES

Table 2.4: Managerial Decisions on Forecast Characteristics

Columns 1 and 2 present estimates of a multinomial logit model. The dependent variable in column 1 (2) is a binary variable indicating firms that issue more good news than bad news (more bad news than good news) in their earnings forecasts, with the baseline case of firms not issuing forecasts. Column 3 presents estimates of an ordered logit model where the dependent variable equals zero if there is no management earnings forecast, one if there are more range forecasts, and two if there are more point forecasts. We control for firm characteristics, including *Size*, *Tobin's Q*, *Leverage*, *ROA*, *Turnover*, *MRet*, *Sigma*, *Analyst Coverage*, *Amihud*, and *Inst. Ownership*, measured in quarter $t-1$. We require each firm-quarter observation to have non-missing value for the variables and winsorize the variables at the $1st$ and $99th$ percentiles. We also include Fama-French 48 industry fixed effects and fiscal and calendar year-quarter fixed effects in the regressions. t-statistics with firm-clustered standard errors are in parentheses. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Multinomial Logit		*Ordered Logit*
	(1)	(2)	(3)
Dependent Variable	$Good\_News > Bad\_News$	$Good\_News < Bad\_News$	$Point > Range$
MF_D_t	0.283***	0.285***	0.254***
	(6.55)	(7.53)	(9.18)
Analyst Coverage	0.526***	0.493***	0.342***
	(11.90)	(13.93)	(13.08)
Amihud	-0.288***	-0.239***	-0.209***
	(-8.78)	(-10.53)	(-10.87)
Size	-0.226***	-0.174***	-0.156***
	(-5.72)	(-5.98)	(-6.75)
Tobin's Q	-0.100***	-0.067***	-0.061***
	(-5.33)	(-5.84)	(-6.69)
Leverage	0.246*	0.323***	0.229***
	(1.72)	(3.06)	(2.76)
ROA	2.098***	1.256***	1.421***
	(13.07)	(12.20)	(15.31)
Turnover	-7.276*	-13.457***	-10.785***
	(-1.69)	(-4.31)	(-4.32)
MRet	36.335***	-13.139***	8.132***
	(9.97)	(-4.46)	(3.39)
Sigma	-16.473***	0.769	-4.755***
	(-9.41)	(0.67)	(-4.96)
Inst. Ownership	0.466***	0.572***	0.328***
	(4.03)	(6.52)	(5.51)
Pseudo R^2	0.199	0.199	0.212
Observations	162081	162081	173616
Control Variables	YES	YES	YES
Industry Fixed Effects	YES	YES	YES
Time Fixed Effect	YES	YES	YES

Table 2.5: Earnings Guidance and the Price Impact of Mutual Fund Fire Sales

In Panel A, we use the full sample and estimate the differential effects of mutual fund fire sales on stock mispricing, depending on firms' policies on management forecasts. The dependent variables are average monthly cumulative abnormal returns (CAR) adjusted by value-weighted market returns over different event windows: from current quarter to four quarters after ($CAR[0,4]$), or to eight quarters after ($CAR[0,8]$). The independent variable of interest is the interaction between MF_D_t and a binary variable indicating firms issuing earnings guidance in quarter $t+1$. In Panel B, we use firm-quarter observations that experience mutual fund fire sales (*MFFlow* below the 10th percentile) and examine the effect of earnings guidance on the speed of price recovery after mutual fund fire sales. The dependent variables are $CAR[1,4]$ and $CAR[1,8]$, and the independent variable of interest is the interaction between $CAR[0,0]$ and a binary variable indicating firms issuing earnings guidance in quarter $t+1$. In Panel C, we use firm-quarter observations that experience mutual fund fire sales and firms that issue earnings guidance after the fire sales. The dependent variables are $CAR[1,4]$ and $CAR[1,8]$, and the independent variable of interest is the interaction between $CAR[0,0]$ and binary variables indicating firms issuing guidance with high precision or low error. We control for firm characteristics, including *Size*, *Tobin's Q*, *Leverage*, *ROA*, *Turnover*, *MRet*, *Sigma*, *Analyst Coverage*, *Amihud*, and *Inst. Ownership*, measured in quarter $t-1$. We require each firm-quarter observation to have non-missing value for the variables and winsorize the variables at the $1st$ and $99th$ percentiles. We also include firm fixed effects and fiscal and calendar year-quarter fixed effects in the regressions. t-statistics (in parentheses) are computed using robust, firm-clustered standard errors. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Earnings Guidance and Price Impact (Full Sample)

Dependent Variable	(1) $CAR[0,4]_{VW}$	(2) $CAR[0,8]_{VW}$
MF_D_t	-0.005***	-0.002***
	(-11.10)	(-6.72)
$MF\_D_t \times Guidance_{t+1}$	0.001*	0.002***
	(1.94)	(3.13)
$Guidance_{t+1}$	-0.002***	-0.001**
	(-5.06)	(-2.56)
Adjusted R^2	0.343	0.485
Observations	176443	176485
Control Variables	YES	YES
Firm Fixed Effects	YES	YES
Time Fixed Effects	YES	YES

Panel B: Earnings Guidance and Price Recovery (Fire Sales Sample)

Dependent Variable:	(1) $CAR[1,4]_{VW}$	(2) $CAR[1,8]_{VW}$
$CAR[0,0]_{VW}$	-0.075***	-0.065***
	(-12.25)	(-16.96)
$CAR[0,0]_{VW} \times Guidance_{t+1}$	-0.019*	0.002
	(-1.75)	(0.30)
$Guidance_{t+1}$	-0.005***	-0.002***
	(-4.48)	(-2.93)
Adjusted R^2	0.442	0.604
Observations	22730	22732
Control Variables	YES	YES
Firm Fixed Effects	YES	YES
Time Fixed Effects	YES	YES

Panel C: Earnings Guidance Quality and Price Recovery (Fire Sales Sample)

Dependent Variable	(1) $CAR[1,4]_{VW}$	(2) $CAR[1,8]_{VW}$	(3) $CAR[1,4]_{VW}$	(4) $CAR[1,8]_{VW}$
$CAR[0,0]$	-0.111***	-0.075***	-0.171***	-0.091***
	(-6.13)	(-5.91)	(-8.69)	(-7.00)
$CAR[0,0]_{VW}$ × Precision_High	-0.051**	-0.008		
	(-1.96)	(-0.47)		
$CAR[0,0]_{VW}$ × Error_Low			-0.073***	-0.026
			(-2.59)	(-1.52)
Precision_High/Error_Low	0.000	-0.001	-0.002	-0.001
	(0.22)	(-0.45)	(-1.12)	(-1.18)
Adjusted R^2	0.567	0.716	0.569	0.717
Observations	3501	3501	3501	3501
Control Variables	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES

Table 2.6: Firm Performance and Managerial Response to Mutual Fund Fire Sales

This table shows that the effect of mutual fund fire sales on disclosure policies varies across operating performance. The dependent variables are $Earnings\ Guidance$ and $DisAccrual$ in quarter $t+1$, and the independent variables of interest are the interactions between MF_D_t and binary variables indicating firms with lagged *ROA* in the top/medium/bottom tercile. We control for firm characteristics, including *Size*, *Tobin's Q*, *Leverage*, *Turnover*, *MRet*, *Sigma*, *Analyst Coverage*, *Amihud*, and *Inst. Ownership*, measured in quarter $t-1$. We require each firm-quarter observation to have non-missing value for the variables and winsorize the variables at the $1st$ and $99th$ percentiles. We also include firm fixed effects and fiscal and calendar year-quarter fixed effects in the regressions. t-statistics with firm-clustered standard errors are in parentheses. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Dependent Variables	$Earnings\ Guidance_{t+1}$	$DisAccrual_{t+1}$
$MF\_D_t \times$ ROA_High$_{t-1}$	0.027***	-0.001
	(5.14)	(-0.65)
$MF\_D_t \times$ ROA_Medium$_{t-1}$	0.027***	0.002*
	(5.38)	(1.78)
$MF\_D_t \times$ ROA_Low$_{t-1}$	-0.002	0.008***
	(-0.42)	(3.00)
ROA_High$_{t-1}$	0.012***	0.020***
	(3.97)	(17.12)
ROA_Low$_{t-1}$	-0.034***	-0.030***
	(-10.76)	(-18.59)
Analyst Coverage	0.036***	-0.002**
	(11.70)	(-2.00)
Amihud	-0.019***	-0.008***
	(-11.24)	(-10.48)
Size	0.025***	-0.020***
	(5.17)	(-9.69)
Tobin's Q	0.000	-0.000
	(0.13)	(-0.13)
Leverage	0.005	0.022***
	(0.41)	(3.88)
Turnover	-1.279***	0.382**
	(-4.43)	(2.13)
MRet	0.503***	0.949***
	(2.87)	(8.08)
Sigma	-0.004	-0.303***
	(-0.05)	(-6.78)
Inst. Ownership	0.025*	-0.004
	(1.68)	(-0.93)
Adjusted R^2	0.395	0.202
Observations	176553	126230
P-value(MF_D*ROA_High=MF_D*ROA_Low)	0.000	0.003
Control Variables	YES	YES
Firm Fixed Effects	YES	YES
Quarter Fixed Effect	YES	YES

Table 2.7: Investor Horizon and Earnings Management

This table shows that the effect of mutual fund fire sales on earnings management varies across investor horizons. The dependent variable is $DisAccrual$ in quarter $t+1$, and the independent variables of interest are the interactions between MF_D_t and binary variables indicating firms with lagged *ROA* in the top/medium/bottom tercile. We divide the sample based on investor horizon, measured by institutional ownership, presence of blockholder, and investor turnover rate. We control for firm characteristics, including *Size*, *Tobin's Q*, *Leverage*, *Turnover*, *MRet*, *Sigma*, *Analyst Coverage*, *Amihud*, and *Inst. Ownership*, measured in quarter $t-1$. We require each firm-quarter observation to have non-missing value for the variables and winsorize the variables at the $1st$ and $99th$ percentiles. We also include firm fixed effects and fiscal and calendar year-quarter fixed effects in the regressions. t-statistics with firm-clustered standard errors are in parentheses. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	$DisAccrual_{t+1}$					
Sample:	Institutional Ownership		Presence of Blockholder		Investor Turnover	
	Low	High	No	Yes	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
$MF\_D_t \times ROA\_High_{t-1}$	-0.002	0.002	0.001	0.000	-0.001	0.001
	(-0.39)	(1.54)	(0.31)	(0.04)	(-0.52)	(0.72)
$MF\_D_t \times ROA\_Medium_{t-1}$	0.002	0.001	-0.000	0.002	0.001	0.003**
	(0.78)	(0.85)	(-0.08)	(1.50)	(0.58)	(2.02)
$MF\_D_t \times ROA\_Low_{t-1}$	0.012**	0.003	0.027***	0.002	0.010**	0.004
	(2.12)	(0.91)	(3.56)	(0.67)	(2.51)	(0.92)
ROA_High$_{t-1}$	0.025***	0.013***	0.023***	0.016***	0.021***	0.014***
	(13.54)	(9.22)	(8.02)	(11.72)	(10.15)	(9.66)
ROA_Low$_{t-1}$	-0.034***	-0.020***	-0.034***	-0.025***	-0.030***	-0.024***
	(-14.74)	(-9.87)	(-9.04)	(-13.51)	(-11.11)	(-11.74)
Adjusted R^2	0.227	0.227	0.286	0.226	0.242	0.243
Observations	63113	63117	28680	75345	51326	51325
P-value(High=Low)	0.044	0.883	0.002	0.552	0.015	0.539
Control Variables	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES	YES	YES

Table 2.8: Earnings Management and the Price Impact of Mutual Fund Fire Sales

In Panel A, we use the full sample and estimate the differential effects of mutual fund fire sales on stock mispricing, depending on firms' discretionary accruals. The dependent variables are average monthly cumulative abnormal returns (CAR) adjusted by value-weighted market returns over different event windows: from current quarter to four quarters after ($CAR[0,4]$), or to eight quarters after ($CAR[0,8]$). The independent variable of interest is the interaction between MF_D_t and a binary variable indicating firms with $t+1 DisAccrual_{t+1}$ above sample median. In Panel B, we use firm-quarter observations that experience mutual fund fire sales (*MFFlow* below the 10th percentile) and examine the effect of earnings management on the speed of price recovery after fire sales. The dependent variables are $CAR[1,4]$ and $CAR[1,8]$, and the independent variable of interest is the interaction between $CAR[0,0]$ and a binary variable indicating firms with $t+1 DisAccrual_{t+1}$ above sample median. We control for firm characteristics, including *Size*, *Tobin's Q*, *Leverage*, *ROA*, *Turnover*, *MRet*, *Sigma*, *Analyst Coverage*, *Amihud*, and *Inst. Ownership*, measured in quarter $t-1$. We require each firm-quarter observation to have non-missing value for the variables and winsorize the variables at the $1st$ and $99th$ percentiles. We also include firm fixed effects and fiscal and calendar year-quarter fixed effects in the regressions. t-statistics (in parentheses) are computed using robust, firm-clustered standard errors. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Earnings Management and Price Impact (Full Sample)

	(1)	(2)
Dependent Variable	$CAR[0,4]_{VW}$	$CAR[0,8]_{VW}$
MF_D_t	-0.004***	-0.002***
	(-7.25)	(-3.75)
$MF\_D_t \times DisAccrual_{t+1}$	-0.000	0.000
	(-0.11)	(0.70)
$DisAccrual_{t+1}$	-0.000	-0.001**
	(-1.21)	(-2.54)
Adjusted R^2	0.338	0.488
Observations	126163	126189
Control Variables	YES	YES
Firm Fixed Effects	YES	YES
Time Fixed Effects	YES	YES

Panel B: Earnings Management and Price Recovery (Fire Sales Sample)

	(1)	(2)
Dependent Variable:	$CAR[1,4]_{VW}$	$CAR[1,8]_{VW}$
$CAR[0,0]_{VW}$	-0.063***	-0.058***
	(-6.74)	(-10.06)
$CAR[0,0]_{VW} \times DisAccrual_{t+1}$	-0.005	0.001
	(-0.37)	(0.14)
$DisAccrual_{t+1}$	-0.001	0.000
	(-0.84)	(0.76)
Adjusted R^2	0.345	0.523
Observations	16171	16173
Control Variables	YES	YES
Firm Fixed Effects	YES	YES
Time Fixed Effects	YES	YES

Table 2.9: The Sarbanes-Oxley Act and Managerial Responses to Mutual Fund Fire Sales

This table presents estimates of Model (1.7) and Model (1.8) in Panels A and B, respectively. In Panel A, the dependent variables are $Guidance_{t+1}$ and $DisAccrual_{t+1}$, and the independent variable of interest is the interaction between MF_D_t and a binary variable $Post\_SOX$ that indicates firm-quarter observations after the passage of Sarbanes-Oxley Act. In Panel B, the sample only includes firm-quarter observations under mutual fund fire sales ($MFFlow$ in the bottom decile). The dependent variables are average monthly cumulative abnormal returns (CAR) adjusted by value-weighted market returns over different event windows: four quarters after fire sales ($CAR[1,4]$), and eight quarters after fire sales ($CAR[1,8]$). The independent variable of interest is the interaction between $CAR[0,0]$ and $Post\_SOX$. We control for firm characteristics, including *Size*, *Tobin's Q*, *Leverage*, *ROA*, *Turnover*, *MRet*, *Sigma*, *Analyst Coverage*, *Amihud*, and *Inst. Ownership*, measured in quarter $t-1$. For both panels, columns 3 and 4 (5 and 6) include firms that were compliant (not compliant) with the key requirements of SOX prior to its passage. We also include firm fixed effects and fiscal and calendar year-quarter fixed effects in the regressions. t-statistics (in parentheses) are computed using robust, firm-clustered standard errors. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

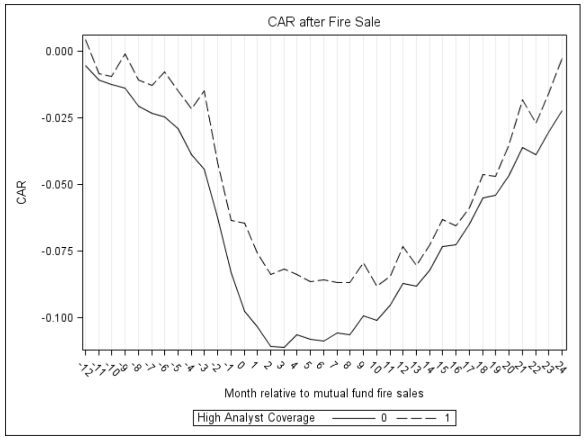
Panel A: The Impacts of RegFD/SOX on Responses

Sample:	*Full*		*Compliant*		*Non-compliant*	
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	$Guidance_{t+1}$	$DisAccrual_{t+1}$	$Guidance_{t+1}$	$DisAccrual_{t+1}$	$Guidance_{t+1}$	$DisAccrual_{t+1}$
MF_D_t	0.006	0.003**	0.021	0.002	0.008	0.008***
	(1.42)	(2.52)	(1.28)	(0.63)	(0.66)	(2.75)
$MF\_D_t \times Post\_SOX$	0.040***	-0.004**	-0.003	0.002	0.030*	-0.009**
	(5.03)	(-2.01)	(-0.10)	(0.39)	(1.82)	(-2.22)
$Post\_SOX$	-0.077***	0.025***	-0.129	0.011	-0.082	0.030**
	(-3.04)	(3.22)	(-1.34)	(0.93)	(-1.63)	(2.41)
Adjusted R^2	0.394	0.202	0.410	0.140	0.496	0.205
Observations	176553	126230	14151	10674	33633	22536
Control Variables	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES	YES	YES

Panel B: The Effects of SOX on Price Recovery

Sample:	*Full*		*Compliant*		*Non-compliant*	
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	$CAR[1,4]_{VW}$	$CAR[1,8]_{VW}$	$CAR[1,4]_{VW}$	$CAR[1,8]_{VW}$	$CAR[1,4]_{VW}$	$CAR[1,8]_{VW}$
$CAR[0,0]_{VW}$	-0.062***	-0.058***	-0.089***	-0.059***	-0.067***	-0.053***
	(-10.40)	(-15.29)	(-5.37)	(-6.70)	(-4.68)	(-5.68)
$CAR[0,0]_{VW} \times Post\_SOX$	-0.038***	-0.006	-0.012	0.006	-0.053***	-0.025*
	(-3.99)	(-0.89)	(-0.52)	(0.34)	(-2.67)	(-1.91)
$Post\_SOX$	-0.135*	-0.094	-0.064	-0.069***	-0.054	0.011
	(-1.92)	(-1.53)	(-1.46)	(-2.65)	(-0.45)	(0.13)
Adjusted R^2	0.409	0.572	0.446	0.562	0.447	0.597
Observations	26004	26006	2428	2428	5424	5424
Control Variables	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES	YES	YES

(a) CAR around mutual fund fire sales: subsamples by analyst coverage



(b) CAR around mutual fund fire sales: subsamples by stock illiquidity

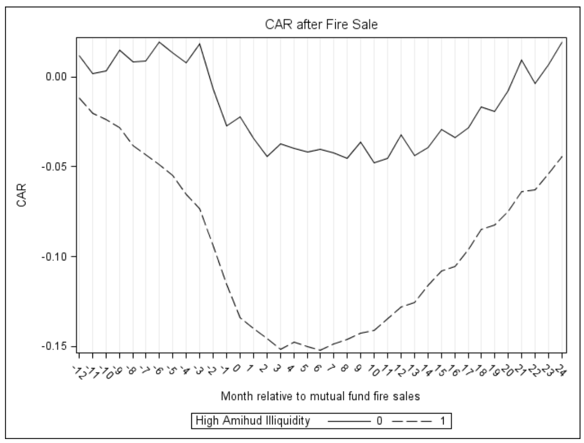


Figure 2.1: Cumulative abnormal return surround mutual fund fire sale

Table 2.10: Descriptive Statistics

This table presents the summary statistics of the main variables used in our analyses. We winsorize all firm characteristics at the 1st and 99th percentiles. Definitions of the variables are provided in the Appendix.

	N	Mean	S.D.	5%	25%	Median	75%	95%
CAPX	47749	0.073	0.093	0.006	0.022	0.044	0.087	0.241
CAPXRND	47560	0.127	0.130	0.011	0.041	0.086	0.166	0.390
INFO	51708	0.807	0.249	0.174	0.741	0.913	0.976	0.997
AdjPIN	16925	0.165	0.078	0.075	0.112	0.148	0.198	0.323
Q	48799	2.069	1.663	0.799	1.109	1.521	2.355	5.249
Resi_Q	48480	0.280	1.432	-1.570	-0.427	0.085	0.703	2.975
Indus_Q	49007	1.792	1.051	0.717	1.106	1.519	2.166	3.630
Analyst	44625	1.292	0.795	0.000	0.693	1.229	1.859	2.686
Dispersion	34980	0.008	0.021	0.000	0.001	0.003	0.007	0.029
Error	41149	0.029	0.085	0.001	0.003	0.008	0.023	0.106
InstOwn	51348	0.447	0.270	0.052	0.213	0.431	0.667	0.896
HHI_4	51708	0.217	0.166	0.055	0.095	0.169	0.283	0.564
TNICSim	40644	3.943	5.036	1.030	1.269	1.986	4.260	14.111
GEO	29522	8.507	8.307	1.000	3.000	6.000	10.000	26.000
Sensitivity	36208	0.015	0.314	-0.347	-0.025	0.003	0.072	0.413
CFO	51616	0.099	0.174	-0.247	0.055	0.125	0.190	0.315
SIZE	51708	5.600	1.755	3.161	4.256	5.364	6.712	8.858
BLEV	51708	0.198	0.195	0.000	0.015	0.158	0.323	0.566
LOSS	51708	0.300	0.458	0.000	0.000	0.000	1.000	1.000
DRET	51708	0.001	0.003	-0.003	-0.000	0.001	0.002	0.005
RND	51613	0.066	0.141	0.000	0.000	0.000	0.074	0.310
SaleGrowth	51394	0.316	0.994	-0.243	0.009	0.117	0.306	1.246
1/Asset	51708	1.144	0.794	0.152	0.599	1.008	1.484	2.666

Table 2.11: The Impacts of Reg FD on Price Informativeness: Baseline Test

This table presents the estimation of whether the passage of Reg FD has a impact on price informativeness, on average. All variables are defined in the Appendix. The two dependent variables are indicated under column headings: $1 - R^2$, price nonsynchronicity derived from an expanded market model (Equation 2.1); $AdjPIN$, an adjusted measure for probability of informed trading, following Duarte and Young (2009). All the variables are calculated during the same fiscal year t. The sample period ranges from 1996 to 2004 but excludes the regulation effective year 2000. $Post4$=1 if the observation is from the post-Reg FD period (2001-2004), and 0 otherwise; $Size$ is the log of assets; $BLEV$ is the debt-to-asset ratio; CFO is cash flow from operations scaled by assets; $DRET$ is the standard deviation of monthly returns over the fiscal-year t; $Loss$=1 if a firm reports a loss in current year, and 0 otherwise; RND is R&D expenses scaled by average total assets; $SaleGrowth$ is difference between current net sales and lagged net sales divided by lagged net sales; $Analyst$ is the average number of analysts following firm i for the fiscal year t; $InstOwn$ is the ratio of the number of shares held by institutional investors to the total number of shares outstanding. We require each firm-year observation to have non-missing values for the variables and winsorize the variables at both $1st$ and $99th$ percentiles. Firm fixed effect is included. The standard deviations (in parentheses) are computed using robust, firm-clustered method. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent variables	(1) $1 - R^2$	(2) $AdjPIN$
Post4	-0.070***	-0.002
	(0.004)	(0.001)
SIZE	-0.047***	-0.026***
	(0.004)	(0.002)
BLEV	0.083***	0.022***
	(0.014)	(0.006)
CFO	-0.053***	-0.032***
	(0.013)	(0.010)
DRET	2.205***	1.237***
	(0.341)	(0.261)
LOSS	0.007**	0.003*
	(0.003)	(0.002)
RND	-0.023**	0.014
	(0.012)	(0.028)
SaleGrowth	-0.004***	0.003**
	(0.001)	(0.001)
Analyst	-0.026***	-0.009***
	(0.003)	(0.001)
InstOwn	-0.104***	-0.053***
	(0.012)	(0.005)
Adjusted R^2	0.822	0.668
Observations	24511	13151
Firm Fixed Effect	YES	YES
Year Fixed Effect	No	No

Table 2.12: Differential Effects of Reg FD on Price Informativeness

This table presents regression analysis of the different cross-sectional effects of Reg FD on price informativeness. All variables are defined in the Appendix. The two dependent variables are indicated above column headings: $1 - R^2$, price nonsynchronicity derived from an expanded market model (Equation 2.1); $AdjPIN$, an adjusted measure for probability of informed trading, following Duarte and Young (2009). All the variables are calculated during the same fiscal year t. The sample period ranges from 1996 to 2004 but excludes the regulation effective year 2000. All the second variables in the interaction terms are binary variables that are equal to 1 if the observation is from treatment groups, which are constructed in Section 2.2.1, and 0 if it is from control groups. $Post4$=1 if the observation is from the post-Reg FD period (2001-2004), and 0 otherwise; $Size$ is the log of assets; $BLEV$ is the debt-to-asset ratio; CFO is cash flow from operations scaled by assets; $DRET$ is the standard deviation of monthly returns over the fiscal-year t; $Loss$=1 if a firm reports a loss in current year, and 0 otherwise; RND is R&D expenses scaled by average total assets; $SaleGrowth$ is difference between current net sales and lagged net sales divided by lagged net sales; $Analyst$ is the average number of analysts following firm i for the fiscal year t; $InstOwn$ is the ratio of the number of shares held by institutional investors to the total number of shares outstanding. We require each firm-year observation to have non-missing values for the variables and winsorize the variables at both $1st$ and $99th$ percentiles. Firm fixed effect is included. The standard deviations (in parentheses) are computed using robust, firm-clustered method. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent variables	$1 - R^2$				$AdjPIN$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment variables	$Analyst$	$TNICSim$	$R\&D$	GEO	$Analyst$	$TNICSim$	$R\&D$	GEO
Post4*Analyst	-0.053***				-0.015***			
	(0.007)				(0.003)			
Post4*TNICsim		-0.020***				-0.008***		
		(0.007)				(0.002)		
Post4*R&D			-0.003				-0.002	
			(0.007)				(0.002)	
Post4*GEO				-0.020***				-0.001
				(0.007)				(0.002)
Adjusted R^2	0.829	0.827	0.826	0.827	0.680	0.678	0.678	0.678
Observations	24511	24511	24511	24511	13152	13152	13152	13152
Control Variables	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effect	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effect	YES	YES	YES	YES	YES	YES	YES	YES

Table 2.13: Price Informativeness and Reg FD: Long-term Effects

This table presents regression analysis of the differential effects of Reg FD on price informativeness across firms and the evolution of changes over time. All variables are defined in the Appendix. The dependent variable is $1 - R^2$, price nonsynchronicity derived from an expanded market model (Equation 2.1). All the variables are calculated during the same fiscal year t. The sample period ranges from 1993 to 2007 but excludes the regulation effective year 2000. $Treat$=1 if the observation is from one of the four treatment groups, and 0 otherwise. Variables used for constructing treated and control groups are discussed in Section 2.2.1 and indicated under the column headings. $Post$=1 if the observation is from 2001 to 2007, and 0 otherwise; $Post4$=1 if the observation is from the first period of post-Reg FD (2001-2004), and 0 otherwise; $Post7$=1 if the observation is from the second period of post-Reg FD(2005-2007), and 0 otherwise. We require each firm-year observation to have non-missing values for the variables and winsorize the variables at both $1st$ and $99th$ percentiles. Firm fixed effect is included in column (1), and both firm and year fixed effects are included in other models. The standard deviations (in parentheses) are computed using robust, firm-clustered method. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Treatment variables	(1) *No treatment*	(2) $Analyst$	(3) $TNICsim$	(4) $R\&D$	(5) GEO
Post	-0.051***				
	(0.003)				
Post4*Treat		-0.057***	-0.018**	-0.013*	-0.023***
		(0.007)	(0.008)	(0.008)	(0.008)
Post7*Treat		-0.013	0.027***	0.038***	-0.025***
		(0.009)	(0.009)	(0.009)	(0.009)
Adjusted R^2	0.797	0.799	0.798	0.798	0.797
Observations	40967	40967	40967	40967	40967
Control Variables	YES	YES	YES	YES	YES
Firm Fixed Effect	YES	YES	YES	YES	YES
Year Fixed Effect	No	YES	YES	YES	YES

Table 2.14: The Impact of Reg FD on Investment to Price Sensitivity: Baseline

This table presents the estimation of how the passage of Reg FD has an impact on investment-to-price sensitivity. All variables are defined in the Appendix. The two dependent variables are indicated above column headings: $CAPX$ is the capital expenditure scaled by beginning-of-year assets; $CAPRND$ the sum of capital expenditure and R&D expenses, then scaled by beginning-of-year assets. The dependent variables are calculated in fiscal year $t+1$, while other variables are calculated in current year t. The sample period ranges from 1993 to 2007 but excludes the regulation effective year 2000. Q represents Tobin's Q, computed as market value of equity plus book value of assets minus book value of equity, scaled by book value of assets. $Post$=1 if the observation is from 2001 to 2007, and 0 otherwise. We control for the investment to cash flow sensitivity in column (2) and (4). We require each firm-year observation to have non-missing values for the variables and winsorize the variables at both $1st$ and $99th$ percentiles. Both firm and year fixed effects are included for all models. The standard deviations (in parentheses) are computed using robust, firm-clustered method. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent variables	$CAPX$		$CAPRND$	
	(1)	(2)	(3)	(4)
Q	0.008***	0.008***	0.015***	0.015***
	(0.001)	(0.001)	(0.001)	(0.001)
Q*Post	-0.002***	-0.002**	-0.005***	-0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
CFO*Post		-0.050***		0.012
		(0.007)		(0.015)
CFO	0.064***	0.091***	-0.012	-0.018
	(0.006)	(0.008)	(0.010)	(0.014)
SIZE	-0.020***	-0.019***	-0.039***	-0.039***
	(0.002)	(0.002)	(0.002)	(0.002)
BLEV	-0.052***	-0.052***	-0.053***	-0.053***
	(0.005)	(0.005)	(0.006)	(0.006)
LOSS	-0.008***	-0.007***	-0.014***	-0.014***
	(0.001)	(0.001)	(0.001)	(0.001)
MRET	1.455***	1.457***	1.425***	1.425***
	(0.210)	(0.209)	(0.270)	(0.270)
RND	0.004	0.005	0.098***	0.098***
	(0.005)	(0.005)	(0.013)	(0.013)
SaleGrowth	0.004***	0.004***	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
1/Asset	0.000	0.000	0.010***	0.010***
	(0.002)	(0.002)	(0.003)	(0.003)
Analyst	0.004***	0.004***	0.006***	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)
InstOwn	0.011**	0.011**	0.011**	0.011**
	(0.004)	(0.004)	(0.005)	(0.005)
Adjusted R^2	0.623	0.624	0.706	0.706
Observations	44800	44800	44800	44800
Firm Fixed Effect	YES	YES	YES	YES
Year Fixed Effect	YES	YES	YES	YES

Table 2.15: Investment to Price Sensitivity and Reg FD: Subsamples

This table presents the regression analysis of the differential impacts of Reg FD's adoption on investment-to-price sensitivity across firms. All variables are defined in the Appendix. The dependent variables are indicated under column headings: $CAPX$ is the capital expenditure scaled by beginning-of-year assets; $CAPRND$ the sum of capital expenditure and R&D expenses, then scaled by beginning-of-year assets. The dependent variables are calculated in fiscal year $t+1$, while other variables are estimated in current year t. The sample period ranges from 1993 to 2007 but excludes the regulation effective year 2000. Q represents Tobin's Q, computed as market value of equity plus book value of assets minus book value of equity, scaled by book value of assets. $Post$=1 if the observation is from 2001 to 2007, and 0 otherwise. We partition the sample into treated and control groups by three firm characteristics indicative of selective disclosure preferences, respectively. The coefficients of interest are those on interaction terms, thereby we compare between treated and control models with each treatment variable and for both investment measures. The three variables used to distinguish treated firms from controls are denoted above column headings, i.e., the number of analyst followings, R&D expenses, and product similarity score. We require each firm-year observation to have non-missing values for the variables and winsorize the variables at both $1st$ and $99th$ percentiles. Both firm and year fixed effects are included for all models. The standard deviations (in parentheses) are computed using robust, firm-clustered method. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Treatment variables	*Analyst*				*R&D*				*Competition*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dependent variables	$CAPX$	$CAPX$	$CAPRND$	$CAPRND$	$CAPX$	$CAPX$	$CAPRND$	$CAPRND$	$CAPX$	$CAPX$	$CAPRND$	$CAPRND$
Subsample	$Treat$	$Control$	$Treat$	$Control$	$Treat$	$Control$	$Treat$	$Control$	$Treat$	$Control$	$Treat$	$Control$
Q	0.008***	0.007***	0.014***	0.014***	0.006***	0.012***	0.014***	0.014***	0.008***	0.008***	0.017***	0.010***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Q*Post	-0.003**	-0.001	-0.007***	-0.002	-0.002***	-0.001	-0.004***	-0.001	-0.003***	-0.001	-0.006***	-0.002
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Adjusted R^2	0.626	0.620	0.687	0.720	0.507	0.637	0.709	0.693	0.619	0.628	0.689	0.709
Observations	19927	24873	19927	24873	17390	27410	17390	27410	20972	23828	20972	23828
Control Variables	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effect	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effect	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Table 2.16: Long-term Effects of Reg FD on Investment to Price Sensitivity: Subsample by Analyst and R&D

This table presents the regression analysis of the differential impacts of Reg FD's adoption on investment-to-price sensitivity across firms, over a long-term period. All variables are defined in the Appendix. The dependent variables are indicated under column headings: $CAPX$ is the capital expenditure scaled by beginning-of-year assets; $CAPRND$ the sum of capital expenditure and R&D expenses, then scaled by beginning-of-year assets. The dependent variables are calculated in fiscal year $t+1$, while other variables are estimated in current year t. The sample period ranges from 1993 to 2007 but excludes the regulation effective year 2000. The full sample period is further split into two shorter periods: $Post4$=1 if the observation is from the first period of post-Reg FD (2001-2004), and 0 otherwise; $Post7$=1 if the observation is from the second period of post-Reg FD(2005-2007), and 0 otherwise. Q represents Tobin's Q, computed as market value of equity plus book value of assets minus book value of equity, scaled by book value of assets. We partition the sample into treated and control groups by two firm characteristics indicative of selective disclosure preferences, respectively. We primarily compare the estimate coefficients on $Q * Post4$ between treated and control models under each treatment variable and each investment measure. Besides, we compare $Q * Post4$ and $Q * Post7$ within each model. The two variables used to distinguish treated firms from controls are denoted above column headings, i.e., the number of analyst followings and R&D expenses. We require each firm-year observation to have non-missing values for the variables and winsorize the variables at both $1st$ and $99th$ percentiles. Both firm and year fixed effects are included for all models. The standard deviations (in parentheses) are computed using robust, firm-clustered method. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Treatment variables	*Analyst*				*R&D*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variables	$CAPX$	$CAPX$	$CAPXRND$	$CAPXRND$	$CAPX$	$CAPX$	$CAPXRND$	$CAPXRND$
Subsample	$Treat$	$Control$	$Treat$	$Control$	$Treat$	$Control$	$Treat$	$Control$
Q	0.008***	0.007***	0.015***	0.014***	0.006***	0.012***	0.014***	0.014***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Q*Post4	-0.003***	-0.001	-0.008***	-0.003	-0.002***	-0.001	-0.006***	-0.001
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Q*Post7	-0.001	-0.001	-0.001	-0.001	-0.002	-0.001	0.001	-0.000
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Adjusted R^2	0.626	0.620	0.688	0.720	0.507	0.637	0.709	0.693
Observations	19927	24873	19927	24873	17390	27410	17390	27410
Control Variables	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effect	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effect	YES	YES	YES	YES	YES	YES	YES	YES

Table 2.17: Long-term Effects of Reg FD on Investment to Price Sensitivity: Subsample by Competitiveness

This table presents the regression analysis of the differential impacts of Reg FD's adoption on investment-to-price sensitivity across firms, over a long-term period. All variables are defined in the Appendix. The dependent variables are indicated under column headings: $CAPX$ is the capital expenditure scaled by beginning-of-year assets; $CAPRND$ the sum of capital expenditure and R&D expenses, then scaled by beginning-of-year assets. The dependent variables are calculated in fiscal year $t+1$, while other variables are estimated in current year t. The sample period ranges from 1993 to 2007 but excludes the regulation effective year 2000. The full sample period is further split into two shorter periods: $Post4$=1 if the observation is from the first period of post-Reg FD (2001-2004), and 0 otherwise; $Post7$=1 if the observation is from the second period of post-Reg FD(2005-2007), and 0 otherwise. Q represents Tobin's Q, computed as market value of equity plus book value of assets minus book value of equity, scaled by book value of assets. We partition the sample into treated and control groups by two firm characteristics indicative of selective disclosure preferences, respectively. We primarily compare the estimate coefficients on $Q * Post4$ between treated and control models under each treatment variable and each investment measure. Besides, we compare $Q * Post4$ and $Q * Post7$ within each model. The two variables used to distinguish treated firms from controls are denoted above column headings, i.e., product similarity score from Hoberg and Phillips (2015) and sales-based HirschmannHerfindahl index calculated at the four-digit SIC code level. We require each firm-year observation to have non-missing values for the variables and winsorize the variables at both $1st$ and $99th$ percentiles. Both firm and year fixed effects are included for all models. The standard deviations (in parentheses) are computed using robust, firm-clustered method. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Treatment groups	$TNICSim$				HHI			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variables	$CAPX$	$CAPX$	$CAPXRND$	$CAPXRND$	$CAPX$	$CAPX$	$CAPXRND$	$CAPXRND$
Subsample	$Treat$	$Control$	$Treat$	$Control$	$Treat$	$Control$	$Treat$	$Control$
Q	0.008***	0.008***	0.017***	0.010***	0.008***	0.008***	0.016***	0.012***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)
Q*Post4	-0.003***	-0.002	-0.007***	-0.002	-0.003***	-0.002	-0.007***	-0.003
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Q*Post7	-0.002*	-0.001	-0.001	-0.001	-0.001	-0.002	-0.003	0.000
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Adjusted R^2	0.619	0.628	0.690	0.709	0.620	0.628	0.689	0.728
Observations	20972	23828	20972	23828	23423	21377	23423	21377
Control Variables	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effect	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effect	YES	YES	YES	YES	YES	YES	YES	YES

Table 2.18: Firm Value and Reg FD

This table presents the regression analysis of the differential impacts of Reg FD's adoption on firm value across firms. All variables are defined in the Appendix. The dependent variable is firm value, measured by Tobin's Q in fiscal year $t+1$. Other variables are calculated in current year t. The sample period ranges from 1993 to 2007 but excludes the regulation effective year 2000. $Post$=1 if the observation is from the post-Reg FD period (2001-2007). The post-Reg FD period is further split into two shorter periods: $Post4$=1 if the observation is from the first period of post-Reg FD (2001-2004), and 0 otherwise; $Post7$=1 if the observation is from the second period of post-Reg FD(2005-2007), and 0 otherwise. $Treat$=1 if the observation is from one of the three treatment groups, and 0 otherwise. Variables used for differentiating treated from control groups are discussed in Section 2.2.1 and are denoted under the column headings. Column (1)-(3) report the results by interacting $Post$ with one of the three $Treat$ variables. Column (4)-(6) use the dummies of sub-periods to interact with $Treat$, respectively. We require each firm-year observation to have non-missing values for the variables and winsorize the variables at both $1st$ and $99th$ percentiles. Both firm and year fixed effects are included for all models. The standard deviations (in parentheses) are computed using robust, firm-clustered method. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

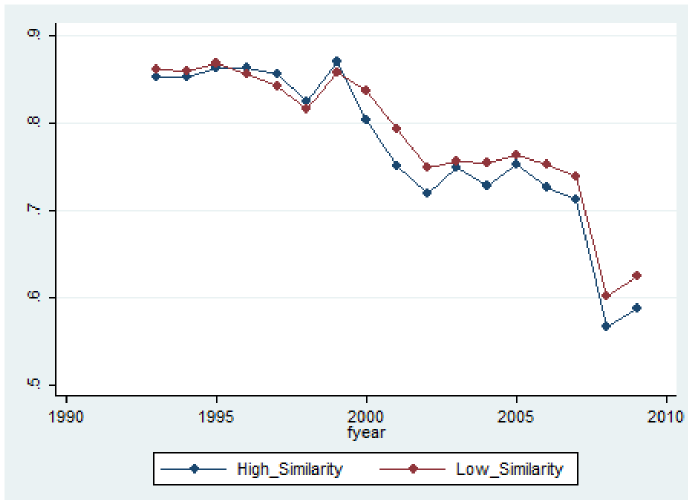
Sample period	*Full sample period*			*Sub-periods*		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment variables	$Analyst$	$TNICSim$	$R\&D$	$Analyst$	$TNICSim$	$R\&D$
Post*Treat	-0.220***	-0.106**	-0.260***			
	(0.046)	(0.046)	(0.047)			
Post4*Treat				-0.171***	-0.077*	-0.234***
				(0.045)	(0.045)	(0.046)
Post7*Treat				-0.304***	-0.156***	-0.306***
				(0.055)	(0.055)	(0.056)
Adjusted R^2	0.606	0.605	0.606	0.606	0.605	0.606
Observations	44559	44559	44559	44559	44559	44559
Control Variables	YES	YES	YES	YES	YES	YES
Firm Fixed Effect	YES	YES	YES	YES	YES	YES
Year Fixed Effect	YES	YES	YES	YES	YES	YES

Table 2.19: Analyst forecast quality and Reg FD

This table presents the regression analysis of the differential impacts of Reg FD's adoption on analyst forecast quality across firms. All variables are defined in the Appendix. The dependent variables are analyst forecast dispersion and forecast error, denoted under the column headings: $Dispersion$ is computed as annual mean of standard deviation of monthly analyst earnings forecast (scaled by mean monthly price); $Error$ is the annual mean of the difference between announced earnings as reported by I/B/E/S and the median of forecasts from individual analysts from the I/B/E/S detail data, where the difference is normalized by announced earnings. All the variables are calculated in current fiscal year t. The sample period ranges from 1993 to 2007 but excludes the regulation effective year 2000. $Post$=1 if the observation is from the post-Reg FD period (2001-2007). The post-Reg FD period is further split into two shorter periods: $Post4$=1 if the observation is from the first period of post-Reg FD (2001-2004), and 0 otherwise; $Post7$=1 if the observation is from the second period of post-Reg FD(2005-2007), and 0 otherwise. $TNICSim$=1 if the observation is from the treatment group characterized by high product similarity score, and 0 otherwise. The methodology is discussed in Section 2.2.1. Column (1) and (2) report the results by interacting $Post$ with $TNICSim$. Column (3) and (4) use the dummies of sub-periods, $Post4$ and $Post7$, to interact with $TNICSim$, respectively. We require each firm-year observation to have non-missing values for the variables and winsorize the variables at both $1st$ and $99th$ percentiles. Both firm and year fixed effects are included for all models. The standard deviations (in parentheses) are computed using robust, firm-clustered method. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Sample period	*Full sample period*		*Sub-periods*	
	(1)	(2)	(3)	(4)
Dependent variables	$Dispersion$	$Error$	$Dispersion$	$Error$
Post*TNICSim	0.001*	0.003*		
	(0.000)	(0.001)		
Post4*TNICsim			0.000	0.002
			(0.001)	(0.002)
Post7*TNICsim			0.001***	0.004**
			(0.001)	(0.002)
Adjusted R^2	0.591	0.648	0.591	0.648
Observations	44821	53997	44821	53997
Control	YES	YES	YES	YES
Firm Fixed Effect	YES	YES	YES	YES
Year Fixed Effect	YES	YES	YES	YES

(a) Price informativeness and Reg FD: subsamples by product similarity scores



(b) Price informativeness and Reg FD: subsamples by HHI of 4-digit SIC industry

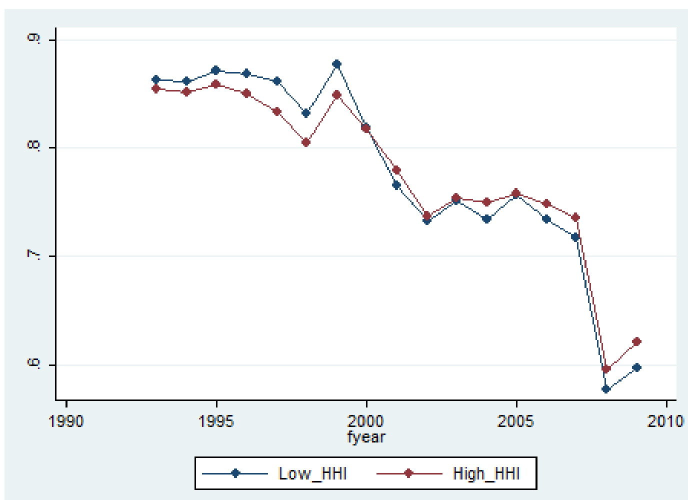
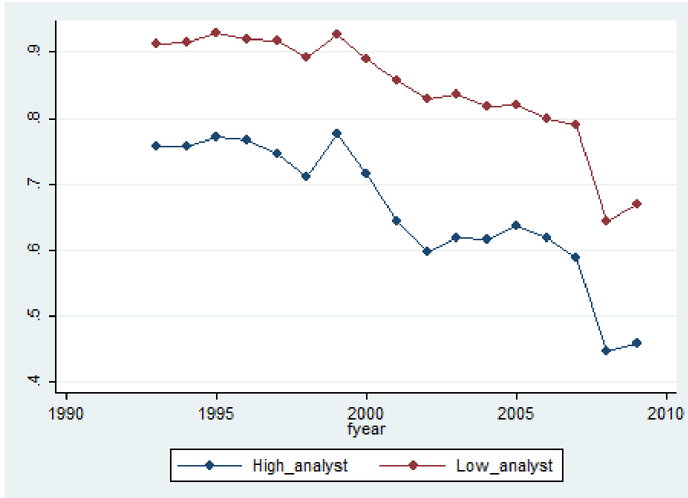


Figure 2.2: Price informativeness from 1993 to 2009: subsamples by competitiveness

(a) Price informativeness and Reg FD: subsamples by the number of analyst followings



(b) Price informativeness and Reg FD: subsamples by R&D expenditures

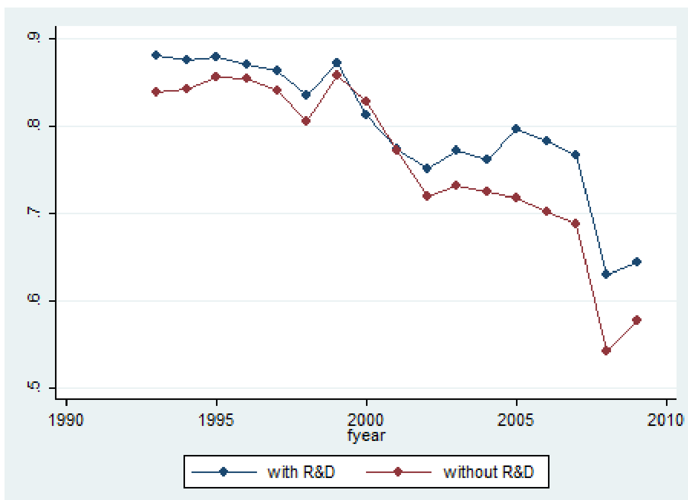


Figure 2.3: Price informativeness from 1993 to 2009: subsamples by proprietary cost