

The Impact of Regulation Fair Disclosure: Trading Costs and Information Asymmetry

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Abstract

In October 2000, the Securities and Exchange Commission (SEC) passed Regulation Fair Disclosure (FD) in an effort to reduce selective disclosure of material information by firms to analysts and other investment professionals. We find that the information asymmetry reflected in trading costs at earnings announcements has declined after Regulation FD, with the decrease more pronounced for smaller and less liquid stocks. Return volatility around mandatory announcements is also lower but overall information flow is unchanged when mandatory and voluntary announcements are combined. Thus, the SEC appears to have diminished the advantage of informed investors, without increasing volatility.

I. Introduction

Effective October 23, 2000, the Securities and Exchange Commission (SEC) passed Regulation Fair Disclosure (Regulation FD) that prohibits selective disclosure of material information to analysts and other investment professionals. Under the regulation, any intentional disclosure of material non-public information by firms to analysts or other parties must be simultaneously released to the general public. Unintentional disclosures must be disclosed publicly within 24 hours.¹ Both proponents and critics expect the rule to have far-reaching effects on the efficiency of financial markets and the structure of the financial services industry.

The intended objective of the regulation was to provide equal access to firm disclosures. If equal access is improved, then the amount of asymmetric information in the securities market should decline subsequent to the regulatory adoption.

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¹Details about what constitutes a violation of Regulation FD as well as remedies and penalties are summarized, for example, in Bellezza, Huang, and Spiess (2002).

Our investigation attempts to measure changes in the amount of asymmetric information, as reflected in the adverse selection component of trading costs, for a sample of NYSE firms that traded both before and after the regulation. To enhance the power of the investigation, we focus on trading days surrounding the release of earnings information, where information asymmetry is elevated. As an adjunct, we also examine the regulatory impact on total information flow through an investigation of stock return volatility.

Parallel research into the total impact of the regulation is building. For example, Heflin, Subramanyam, and Zhang (2003) look at return variability around earnings announcements and find an apparent reduction due to the regulation. Agarwal and Chadha (2002), Janakiraman, Radhakrishnan, and Szwejkowski (2002), and Zitzewitz (2002) look for changes in analyst forecast accuracy with mixed results. Topaloglu (2002) finds that institutional trading activity after earnings announcements is relatively higher after Regulation FD than before. Sundar (2002) finds evidence of a decrease in information asymmetry around conference calls for firms that employed restricted disclosure practices before the regulation. Straser (2002) finds mixed results for changes in the probability of informed trading. Bellezza, Huang, and Spiess (2002), using data from the period before the regulation, find no evidence of selective disclosure around voluntary earnings announcements, thus casting a vote against any impact of regulation.

Our tests for changes in the adverse selection component of trading costs indicate a decline after the adoption of Regulation FD. Thus, we conclude that the regulation appears to have reduced the degree of preferential access to material information around earnings announcements. In cross section, the results suggest that uninformed traders in less liquid firms obtain the greatest benefit from reductions in asymmetric information and trading costs. Our analysis of stock return volatility indicates no material change in total information released through announcements when both mandatory and voluntary earnings announcements are combined. This supports the SEC's conjecture that increased public disclosures along with recent technological advances in Web communications allow firms to effect the same information flow as before regulation.² In further corroboration, market model residual variance shows no significant change, either in non-announcement periods or across all trading days.

This paper is organized as follows. Section II provides a brief model of how asymmetric information costs due to Regulation FD can be isolated. Section III presents measures of trading costs and information asymmetry, while Section IV contains the sample description. Empirical results for trading costs are presented in Section V. Section VI describes results for stock return volatility and information flow, while Section VII concludes.

II. Modeling the Impact of Regulation FD

It was reportedly a common practice before Regulation FD for corporate officials to discuss the future outlook of their companies and provide guidance

²Recent surveys suggest that companies are now more frequently "Web casting" important information releases and analyst meetings as well as using an open conference call format (see Sundar (2002)).

on earnings forecasts to select groups of analysts and large shareholders through meetings, conference calls, and phone conversations. Specific examples of such selective disclosure are summarized in the final report of the regulation (SEC (1999)). Also, it was alleged that companies were providing material information to analysts as a reward for obtaining favorable ratings and recommendations. The analysts could trade on this information or exchange it to large clients for brokerage business. The trading advantages attendant to these selective disclosure processes, if accurately depicted in the claims, contribute to the asymmetric information costs faced by uninformed traders. Regulation FD was intended to reduce the extent of such informed trading by forcing firms to either disclose information to everyone or disclose less information.

In opposition, if the regulation causes less information disclosure as suggested in recent surveys by the Securities Industry Association (SIA) (2001) and the Association for Investment Management and Research (AIMR) (2001), then it can result in less informative prices and a greater trading advantage for those able to discover the information through other channels. For example, less disclosure might give a greater informational advantage to corporate insiders, managers of competitors, as well as the most resourceful analysts and investors. Since the asymmetric information component of trading costs captures the combined effects of the likelihood of encountering an informed trader and the extent of his or her informational advantage, the regulation could either increase or decrease trading costs. Our investigation is designed to differentiate between these alternatives.

Two principal features of the trading environment have influenced our experimental design. First, the impact of the regulation should be more pronounced on trading days where the influence of selective disclosure on information asymmetry was greatest before the regulation. Hence, we study trading days surrounding earnings-related announcements with special emphasis on anticipated announcements. Anecdotal evidence suggests that analysts put the most pressure on managers around these times to comment on the accuracy of their earnings forecasts. Formally, Kim and Verrecchia (1991), (1994) discuss how market makers widen spreads in anticipation of an earnings announcement to guard against leaks and the possibility that some traders have the opportunity to process earnings announcements before they are generally made public. Aharony and Swary (1980) and other studies on earnings announcements have found that substantial price adjustments begin approximately two days before the actual announcement. Lee, Mucklow, and Ready (1993) document a statistically significant decrease in liquidity in the two trading days prior to an earnings announcement. In addition, Frankel, Johnson, and Skinner (1999) find that conference calls, which were usually closed to the public before Regulation FD, are concentrated on earnings announcement dates, and can include material information and forward-looking statements that are not revealed in the earnings announcement.³

Second, the measures of transactions costs, discussed in detail in Section III, exhibit both time-series and cross-sectional variation for reasons unrelated to reg-

³During the period when the conference call is in progress, they document unusually large return volatility, trading volume, and large transactions—evidence consistent with trading in real time on material non-public information. Results in Bowen, Davis, and Matsumoto (2002) also support these findings.

ulatory changes. To isolate the impact of the regulation, we construct abnormal transactions cost measures over announcement periods by taking the difference between trading costs in announcement and non-announcement periods for each firm. This normalization reduces the cross-sectional variation in announcement period cost measures and nets out trading costs not linked to asymmetric information differences. It also controls for changes in market conditions during the sample period, including the allowable minimum price increment (i.e., tick size) for trading.

To give some structure to the problem, let A represent trading costs during announcement periods and N the costs during non-announcement periods. In non-announcement periods, define I as the transaction cost reflecting the normal background level of adverse selection risk in the absence of the regulation, and U as the transaction cost unrelated to this risk. Let Δ_A be the increase in trading costs due to heightened adverse selection risk in announcement periods. Define Δ_R as the effect of regulation, either positive or negative, on asymmetric information costs. We then have four different levels of transactions costs:

Costs in announcement periods before regulation:

$$A_{\text{pre}} = U_{\text{pre}} + I_{\text{pre}} + \Delta_A,$$

Costs in non-announcement periods before regulation:

$$N_{\text{pre}} = U_{\text{pre}} + I_{\text{pre}},$$

Costs in announcement periods after the regulation:

$$A_{\text{post}} = U_{\text{post}} + (I_{\text{post}} + \Delta_A)(1 + \Delta_R),$$

Costs in non-announcement periods after the regulation:

$$N_{\text{post}} = U_{\text{post}} + I_{\text{post}}(1 + \Delta_R).$$

Subtracting non-announcement period costs from announcement period costs eliminates U and I and any variation in U and I over time and across firms. It leaves $\Delta_A(1 + \Delta_R)$ for the period after regulation and Δ_A for the period before regulation. The difference yields $\Delta_A \Delta_R$, which is the impact of the regulation on the increase in asymmetric information costs in announcement periods. As the regulatory impact itself might vary across firms, we model this element of the regulatory impact by linking it formally to firm characteristics.

III. Measures of Information Asymmetry

Our goal is to construct measures of increased information asymmetry around earnings-related announcements and compare these increases before and after the adoption of the regulation. The first measure we use is based on bid-asked spreads. The spread measures the cost of a round-trip trade and includes both an adverse selection component and a pure trading cost component. The adverse selection component compensates market makers for the risk of inadvertently trading against superior information and is the component of interest to our investigation. Glosten and Milgrom (1985) argue that the adverse selection component

should be an increasing function of the fraction of traders who are informed and the quality of their superior information. The pure trading cost component compensates the market maker for inventory risk, order-processing costs, and for the provision of immediacy.

To account for price improvements within the stated specialist quotes at the NYSE, we calculate the percentage effective spreads as in Lee (1993), Huang and Stoll (1996), and Bessembinder and Kaufman (1997),

$$(1) \quad \text{Percentage Effective Spread} = 200 \times D_{it} \times (\text{Price}_{it} - \text{Mid}_{it}) / \text{Mid}_{it},$$

where Price_{it} is the transaction price for security i at time t , Mid_{it} is the midpoint of the quoted ask and bid prices, and D_{it} is a binary variable that equals 1 for market buy orders and -1 for market sell orders, determined by the algorithm suggested in Lee and Ready (1991).

Our second measure of costs due to informed trading is based on how informed traders are revealed to liquidity providers by order flow imbalance. To the market maker, buy orders tend to exceed sell orders during periods of good news while the opposite is true during periods of bad news. Market makers incorporate the information in order flow by making an adjustment to their quotes upward (downward) after a series of buy (sell) orders. These quote adjustments capture how market makers interpret order flow imbalance. Following Huang and Stoll (1996), we measure the degree of the information asymmetry reflected in price adjustments as the percentage price impact,

$$(2) \quad \text{Percentage Price Impact} = 200 \times D_{it} \times (V_{i,t+30} - \text{Mid}_{it}) / \text{Mid}_{it},$$

where $V_{i,t+30}$, a measure of the “true” economic value of the asset after the trade, is proxied by the midpoint of the first quote reported at least 30 minutes after the trade.⁴

IV. Sample Selection, Descriptive Statistics, and Event Windows

A. Stratified Sample Selection

We specify January 2000 to September 2000 as the sample period before regulation and November 2000 to May 2001 as the period after regulation omitting the regulatory change month of October. Our initial sample consists of all NYSE-listed common stocks in the Trade and Quote (TAQ) database in January 2000, with trading data until September 2000. To remain in the sample, the stock must i) not be listed as an ADR, closed-end investment fund, or an REIT, ii) not have a change in shares outstanding between January 2000 and September 2000 of more than 10%, iii) have a market price between \$5 and \$500 in October 2000, and iv) have a corresponding CUSIP match in the IBES database. The screens reduce the sample size to 1,153.

⁴To control for the arrival of additional information between t and $(t + 30)$ minutes, we weight the price impact by the inverse of the number of transactions between t and $(t + 30)$. The first transaction price reported at least 30 minutes after the trade is also used as a proxy. The results are similar and not reported.

Since the regulatory impact is likely to depend on the information environment of the firm, our sample selection procedure stratifies on firm size and the number of analysts following the firm. The idea is to select a sample of firms with wide variation in market liquidity and the level of competition for information. Analysts following of a stock is defined as the number of analysts contributing annual earnings forecasts to the December 2000 listings of the Institutional Brokers Estimate System (IBES).

Based on the market capitalization at the beginning of October 2000, the sample firms are sorted into size quintiles. Firms in quintile 5 are assigned to the Large Size group (230 firms), quintile 4, 3, and 2 are merged to form the Medium Size group (693 firms), and quintile 1 is called the Small Size group (230 firms). We sort each size group by the number of analysts following the firm. The 50 firms with the highest analyst following are classified as the High Analyst subsample and the 50 firms with the lowest analyst following are classified as the Low Analyst subsample. The final sample is the 300 firms that are classified into six [Firm Size, Analyst Following] groups, i.e., 50 firms each from the six groups. The subsample of 277 firms that survive until the end of the sample period yields results similar to the entire sample (not reported).

B. Descriptive Statistics

Table 1 shows descriptive statistics for the six groups of firms. The sample has firms in the extremes of both market capitalization and analyst following. At one extreme, the average firm in the [Large Size, High Analyst] group has a market capitalization of \$62.66 billion with 31 analysts following the firm. At the other extreme, the average firm in the [Small Size, Low Analyst] group has a market capitalization of \$106 million with no analyst following.

TABLE 1
Descriptive Statistics on the Sample

Variable	Firm Size					
	Large		Medium		Small	
	High Analyst Following	Low Analyst Following	High Analyst Following	Low Analyst Following	High Analyst Following	Low Analyst Following
Size (\$ millions)	62,655	11,407	3,179	749	186	106
No. of analysts	31	11	21	1	7	0
Price	49.07	51.92	31.51	26.67	11.51	11.03
Trade size (\$ thousands)	125.14	93.27	57.78	25.16	14.80	10.80
No. of daily trades	1,393	671	389	43	29	12
Quoted spread (%)	0.2506	0.3187	0.4797	1.0338	1.7047	2.3784
Return std. dev.	0.0331	0.0390	0.0313	0.0254	0.0303	0.0236

The average firm size (\$ million), number of analysts following the firm, stock price, trade size (\$ thousands), daily number of trades, quoted spreads (in %), and standard deviation of daily returns in October 2000 are reported for each [Firm Size, Analyst Following] group of firms. Based on the market capitalization in October 2000, the sample firms are sorted into Firm Size quintiles. We assign quintile 5 as the Large Size group (230 firms), quintiles 4, 3, and 2 are merged to form the Medium Size group (693 firms), and quintile 1 is the Small Size group (230 firms). For each group, the 50 firms with the highest analyst following form the High Analyst group and the 50 firms with the lowest analyst following form the Low Analyst group. The data source is IBES database for the number of analysts following the firm and the TAQ database for other variables.

The six groups differ on several measures of market liquidity. To measure trading costs, only trades and quotes that occurred on the NYSE during the normal

trading hours are analyzed. We use filters to delete trades and quotes that are non-standard or likely to contain errors.⁵ From Table 1, we see that the [Large Size, High Analyst] firms have an average trade size of \$125,000, an average of 1,393 daily trades, and a quoted bid-ask spread of 0.25%. In contrast, the [Small Size, Low Analyst] firms have an average trade size of \$10,800, an average of 12 daily trades, and a bid-ask spread of 2.38%. Also, within each size category, the firms with more analysts are more liquid, on average.

C. Earnings Announcement and Non-Announcement Windows

Precise earnings announcement times were hand collected from the Dow Jones News Retrieval Service (DJNS) for the 300 sample firms over the period January 2000 to May 2001: a total of 1,595 earnings-related announcements. As Table 2 shows, the sample consists of 870 mandatory earnings announcements before regulation and 591 after. Of the 134 voluntary announcements about forthcoming earnings that we identified, 66 occur before regulation and 68 after. We define the announcement window as days -2 through 0 around an announcement, and the non-announcement window as all days outside -2 to $+2$ surrounding any announcement. Days $+1$ to $+2$ are used as components of announcement period return variance measures in Section VI.

TABLE 2
Statistics on Earnings-Related Announcements

Period	Earnings-Related Information Event		
	Mandatory Announcement	Voluntary Disclosure	Total
Before regulation (Jan. 2000–Sept. 2000)	870	66	936
After regulation (Nov. 2000–May 2001)	591	68	659
Total	1,461	134	1,595

Reported are the number of earnings-related announcements of 300 sample firms during the sample period—January 2000 to May 2001 (omitting the month of October 2000). Earnings announcements are either classified as mandatory announcements or as voluntary disclosures on their forthcoming earnings. They are further classified as those made before (January 2000 to September 2000) and after (November 2000 to May 2001) the adoption of Regulation FD. The precise time of the announcements is hand-collected from the Dow Jones News Service (DJNS).

V. Empirical Results for Trading Costs

A. Preliminary Findings

Before aggregating all of the data occurring after Regulation FD, we first must acknowledge an important structural event: the switch in tick size from “teenies” (6.25¢) to “decimals” (1¢) for trade prices. This occurred on January 29,

⁵Trades are omitted if they are out of time sequence, are coded as an error or cancellation, involve a non-standard settlement, are exchange acquisitions or distributions, have negative trade prices, or involve a price change (since the prior trade) greater than 10% in absolute value. Quotes are deleted if the bid or ask is non-positive, the bid-ask spread is negative, the change in the bid or ask price is greater than 10% in absolute value, the bid or ask depth is non-positive, or the quotes are disseminated during a trading halt or a delayed opening.

2001 for most stocks in our sample. Bessembinder (2002) finds that various measures of transactions costs fall significantly after the switch to decimals. Therefore, in Table 3 we separate the period after regulation into the teenies and decimals regimes and report average trading cost measures for the different regimes during earnings-related announcement days (TC_{Ann}) and non-announcement days (TC_{Non}).

TABLE 3
Transactions Cost Measures for Various Trading Periods

Transaction Cost Measure	(1)	(2)	(3)
	Transactions Costs on Earnings-Related Announcement Days [-2, 0](TC_{Ann})	Transactions Costs on Non-Announcement Trading Days (TC_{Non})	Abnormal Transactions Costs (TC_{Ann}) - (TC_{Non})
<i>Effective Spreads (%)</i>			
Before Regulation FD	0.6981	0.6529	0.0437
After Regulation FD			
teenies	0.6780	0.6803	-0.0050
decimal	0.4916	0.4907	0.0189
<i>t</i> -stat. (teenies—before FD)	(-0.34)	(0.53)	(-1.97)**
<i>t</i> -stat. (decimal—before FD)	(-3.01)***	(-3.42)***	(-1.42)*
<i>t</i> -stat. (decimal—teenies)	(-2.97)***	(-3.62)***	(0.89)
<i>Price Impact (%)</i>			
Before Regulation FD	0.5334	0.4603	0.0780
After Regulation FD			
teenies	0.5233	0.4553	0.0674
decimal	0.3554	0.3668	0.0146
<i>t</i> -stat. (teenies—before FD)	(-0.16)	(-0.13)	(-0.26)
<i>t</i> -stat. (decimal—before FD)	(-4.04)***	(-2.55)***	(-2.32)**
<i>t</i> -stat. (decimal—teenies)	(-2.92)***	(-2.32)***	(-1.29)
<i>Null Hypothesis: Increase in Transactions Cost</i>			
Joint <i>p</i> -val. (teenies—before FD)	(0.721)	(0.741)	(0.055)
Joint <i>p</i> -val. (decimal—before FD)	(0.000)	(0.000)	(0.028)
Joint <i>p</i> -val. (decimal—teenies)	(0.000)	(0.000)	(0.203)

Reported are transactions cost measures for a sample of NYSE-listed firms on earnings-related announcement days (TC_{Ann}) and non-announcement days (TC_{Non}). Percentage effective spread is computed as $[200 \times \text{Dummy} \times (\text{Price} - \text{Mid}) / \text{Mid}]$, where the dummy equals 1 for a market buy and -1 for a market sell, price is the transaction price, and mid is the quote midpoint at the time of the trade. Percentage price impact is computed as $[200 \times \text{Dummy} \times (\text{QMid30} - \text{Mid}) / \text{Mid}]$, where QMid30 is the midpoint of the first quote observed after 30 minutes. Announcement window is defined as days -2 to 0 around the earnings-related announcement. All spread measures are cross sectional averages across sample firms in the period before the regulation, and after the regulation when the tick size in the NYSE is Teenies and Decimals. Also reported are the abnormal trading costs defined as the difference between TC_{Ann} and TC_{Non} . The *t*-statistic tests the null that the transactions cost measures are equal. Also reported are the *p*-values of the joint tests of the restriction that both the effective spreads and price impact measures have increased.

***, **, and * for *t*-statistic of difference: significant at the 1%, 5%, and 10% levels, respectively (one-tailed)

Consistent with Bessembinder (2002), Table 3, columns (1) and (2) show that the various measures of trading costs fall significantly after the switch to decimals. In the context of our model in Section II, U and I have fallen in the decimals regime. This clearly implies that the impact of Regulation FD should not be determined by directly comparing trading costs before regulation with the decimal regime after regulation. Comparing trading costs before regulation with those in the teenies regime after regulation shows a reduction in point estimates of effective spreads and price impact around earnings-related announcement days, but the differences are not significant at conventional levels. Abnormal trading costs in column (3), however, indicate stronger evidence in favor of a reduction in effective spreads (*t*-statistic = -1.97) and price impact (*t*-statistic = -0.26). Abnormal trading costs in the decimal regime support the same conclusion.

It is noteworthy that the differences between the decimal and teenies regimes for abnormal trading costs are not significant for either effective spreads or price impact. Further, the effective spread difference is positive while the price impact difference is negative. From this we conclude that our approach of constructing abnormal trading costs over announcement days does a good job of controlling for the effect of tick size and other economy-wide changes that are unrelated to the regulation.

As we have two measures of transactions costs, a proper statistical test of an increase or decrease in trading costs should involve both measures jointly. Focusing on single t -tests ignores the fact that two statistics have been calculated. A traditional χ^2 or F -test could be used, but these tests do not account for the direction of the parameter estimates since squared distances are taken without regard to sign, in essence testing the null hypothesis of no effect. We emphasize joint inequality tests in the remaining analysis because these tests take into account the probability that the statistics could have incorrect signs by chance when the hypothesis is true. To test joint inequality restrictions, we take the approach described in Wolak (1989) and applied by Boudoukh, Richardson, and Smith (1993). The test uses the Wald quadratic form underlying a χ^2 test but the significance level accounts for the direction of the parameter estimates. For our application, the Wald is defined as,

$$W = \gamma' \Sigma^{-1} \gamma,$$

where γ is the vector of distances between the cost estimates and the closest value consistent with the hypothesis being tested (e.g., for testing the hypothesis of a cost increase, negative cost estimates would have their magnitudes in γ , while positive estimates would have zero in γ). Σ is a consistent estimate of the covariance matrix of the estimates.⁶ Additional intuition and details underlying the test procedure are available in an appendix located on the JFQA Web site (<http://www.jfqa.org>). In Table 3, the joint tests indicate rejection of the hypothesis of a cost increase at the 0.055 level in the teenies regime and at the 0.028 level in the decimal regime.

B. Specifying a Regression Model of Changes in Asymmetric Information Costs

Table 3 does not effectively aggregate information across the two regimes after the regulation. To bring the most power to bear on the hypotheses of interest, we propose a regression format that folds all trading regimes into one model. The model has trading costs for announcement days on the left-hand side and includes non-announcement trading costs as an explanatory variable on a firm-by-firm, regime-by-regime basis. The impact of Regulation FD is captured through an intercept indicator. We also extend the model to include the influence of trading volume, firm size, and analyst following on trading cost measures. This extension is

⁶Throughout the tests, the covariance matrix uses the standard errors of the cost estimates along the diagonal, while the correlation between the ordered firm-level cost estimates form the off-diagonal. Where a model is fitted, the standard errors and correlation of the ordered residuals is used. Across all models, the average correlation in ordered cost estimates is about 0.35.

motivated by prior research showing that firms with large analyst following have lower earnings surprises (Dempsey (1989)) and adjust more quickly to macroeconomic (Brennan, Jegadeesh, and Swaminathan (1993)) and firm-specific (Hong, Lim, and Stein (2000)) announcements. Easley, Kiefer, O'Hara, and Paperman (1996) show that larger and more liquid firms have lower information asymmetry. The model has the form,

$$(3) \quad TC_{\text{Ann},i,\text{Regime}} = \alpha + \beta_1 \text{Post} + \beta_2 TC_{\text{Non},i,\text{Regime}} + \beta_3 \text{LNTRADVOL} \\ + \beta_4 \text{LNMKTSZ} + \beta_5 \text{ANALFOLL} + \varepsilon_{i,\text{Regime}},$$

where Regime denotes Before Regulation FD, After Regulation FD_{Teenies}, or After Regulation FD_{Decimals}. $TC_{\text{Ann},i,\text{Regime}}$ and $TC_{\text{Non},i,\text{Regime}}$ are the average transaction costs measures for stock i over announcement and non-announcement days in the specific regime, and Post equals one for announcements after the regulation and zero otherwise. The intercept captures the base increase in asymmetric information costs during announcement days. β_2 captures firm-specific aspects of trading costs in non-announcement days and should be close to unity. The influence on Δ_A of the three firm characteristics, log of trading volume (LNTRADVOL), log of market size (LNMKTSZ), and analyst following (ANALFOLL) enter through the coefficients β_3 , β_4 , and β_5 . For a specific firm type, Δ_A equals α plus the sum of these influences.

The coefficient on the Post dummy, β_1 , estimates $\Delta_A \Delta_R$ and measures the overall change in trading costs around announcements that we attribute to the impact of Regulation FD.⁷ The hypothesis that trading costs decreased predicts a negative β_1 , while the view that trading costs increased has the opposite prediction. The model is estimated with weighted least squares in which the weights equal the number of announcements for stock i in each regime.

Results for the announcement days -2 through 0 are shown in panel A of Table 4. The positive intercepts indicate that announcement period spreads and price impact exceed those in non-announcement days for a base firm. The slope coefficients on TC_{Non} are insignificantly different from unity, which suggests that the intercepts capture the cost increases. For the price impact measure, the increase during announcement periods is higher for firms with large analyst following (t -statistic of $\beta_5 = 2.04$) and for less liquid firms (t -statistic of $\beta_3 = -2.44$). The point estimates of the Post coefficient, β_1 , indicate a decline in effective spreads and price impact, by 3.25 basis points and 5.90 basis points, respectively, due to the introduction of Regulation FD. Both estimates have strong statistical significance, viewed individually, with t -ratios below -2.0 .

Panel B of Table 4 presents the Post coefficients from regression (3) for several additional trading windows around information events. Results for days -2 through 0 are reported first and correspond with panel A. The joint test that trading costs increase is shown in the last column, where the p -value of 0.02 indicates rejection. On days -2 through -1 , for all earnings-related announcements, the

⁷As decimalization affects both $TC_{\text{Ann},i,\text{Decimal}}$ and $TC_{\text{Non},i,\text{Decimal}}$, the regression specification controls for the change in tick size. We ran the specification shown in equation (3) including an additional dummy for the decimal regime. The decimal dummy is not significant in this specification and the joint tests on the Post dummy are similar to the results in Table 3 for the impact of the regulation during the teenies regime.

TABLE 4
Regression Analysis of Transactions Cost around Earnings-Related Announcements

Panel A. Regression Analysis of Transaction Cost Measures around Earnings-Related Announcement [Days -2 to 0]							
	Intercept	After Regulation FD Dummy Post	Non-Ann. Trading Costs TC _{Non}	Log of Trading Vol. LNTRADVOL	Log of Mkt. Cap LNMKTSZ	Analyst Following ANALFOLL	Adj. R ²
Effective Spreads	0.3815	-0.0325	0.9851	-0.0140	-0.0050	0.0022	89.37%
<i>t</i> -stat.	(2.52)**	(-2.01)**	(46.37)***	(-1.53)	(-0.47)	(1.75)*	
<i>p</i> -val. (diff. from 1)			(0.481)				
Price Impact	0.9357	-0.0590	0.9261	-0.0378	-0.0090	0.0043	62.47%
<i>t</i> -stat.	(3.73)***	(-2.13)**	(19.05)***	(-2.44)**	(-0.49)	(2.04)**	
<i>p</i> -val. (diff. from 1)			(0.124)				

Panel B. Impact of Regulation FD on Transactions Cost Measures					
	Effective Spreads		Price Impact		Joint <i>p</i> -Val.
	Coeff.	<i>p</i> -Val.	Coeff.	<i>p</i> -Val.	Null: Increase in Trading Cost
<i>All Earnings-Related Announcements</i>					
Days -2 to 0	-0.0325	(0.04)	-0.0590	(0.03)	(0.020)
Days -2 to -1	-0.0357	(0.05)	-0.0432	(0.19)	(0.055)
Day 0	-0.0360	(0.10)	-0.0916	(0.02)	(0.018)
<i>Mandatory Earnings Announcements</i>					
Days -2 to 0	-0.0283	(0.09)	-0.0500	(0.06)	(0.038)
Days -2 to -1	-0.0318	(0.09)	-0.0286	(0.37)	(0.121)
Day 0	-0.0360	(0.13)	-0.0990	(0.01)	(0.010)
<i>Voluntary Earnings Announcements</i>					
Days -2 to 0	-0.0410	(0.33)	-0.1460	(0.23)	(0.305)
Days -2 to -1	-0.0500	(0.28)	-0.1810	(0.20)	(0.263)
Day 0	0.0020	(0.98)	-0.1010	(0.60)	(0.550)

Reported in panel A are the coefficients from weighted least square regressions of transactions cost measures on earnings-related announcement days (TC_{Ann}) for each firm by Regulation FD regimes, on non-announcement transactions cost (TC_{Non}), a Post dummy, and firm characteristics specified as

$$(3) \quad TC_{Ann,i,Regime} = \alpha + \beta_1 Post + \beta_2 TC_{Non,i,Regime} + \beta_3 LNTRADVOL + \beta_4 LNMKTSZ + \beta_5 ANALFOLL + \epsilon_{i,Regime}$$

$$Regime = \text{Before Regulation FD, After Regulation FD}_{\text{Teenies}}, \text{ or After Regulation FD}_{\text{Decimals}}$$

The transactions cost measures are effective spreads and price impact, in percentage basis points. Announcement window is defined as days -2 to 0 around earnings-related announcement. The Post dummy equals 1 for earnings announcements after the adoption of the regulation and 0 otherwise. For each firm, LNMKTSZ is the log of market size at the end of October 2000, LNTRDVOL is the log of the trading volume in October 2000, and ANALFOLL is the number of analysts following the firm. The weight variable is the number of earnings-related announcements for stock *i* in each regime, where the regimes are period before regulation, and after regulation when the tick size in the NYSE is teenies and decimal. Reported in parentheses are the *t*-statistics. Reported in panel B are the coefficients of the Post dummy variable from the regression specification in panel A for different trading windows around each information event. Also reported are the *p*-values of the joint tests of the restriction that both the effective spreads and price impact measures have increased.

***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively (two-tailed).

regulation has reduced effective spreads by 3.57 basis points and price impact by 4.32 basis points. The joint restriction of a cost increase is rejected in this trading window at a *p*-value of 0.055. For day 0, the joint test indicates stronger evidence against trading cost increases with a *p*-value of 0.018.

Kim and Verrechia (1994) argue that spreads widen on public announcements to compensate for higher asymmetry caused by the superior ability of some market participants to interpret the information content of announcements. Based on their model, the reduced spread and price impact measures on day 0 suggest that earnings announcements after the regulation are made in an environment with more information available before the public announcement, thus reducing the processing asymmetry at the time of the announcement. This supports the notion that firms are finding other ways to communicate earnings information to the pub-

lic. Another interpretation builds on Frankel, Johnson, and Skinner (1999) who find that conference calls are concentrated on earnings announcement dates. The results on day 0 then indicate that selective disclosure in these calls has diminished after the regulation.

Panel B shows separate results for mandatory and voluntary earnings-related disclosures. As the majority of our announcements are mandatory (1,461 out of 1,595), it is not surprising that results for the mandatory announcements are similar to those obtained when all the announcements are combined. Joint p -values for days -2 through 0 and day 0 remain below 0.05 although for the -2 to -1 trading window, the p -value for the joint test increases to 0.12. For voluntary announcements, the negative point estimates again suggest a reduction in transaction costs after the regulation. The magnitudes of the point estimates are quite high, but the smaller sample size of only 60-odd announcements is insufficient to achieve statistical significance.

Notwithstanding the lack of significance for the voluntary disclosures, the results thus far indicate that Regulation FD has lowered trading costs and the risk of adverse selection across all firms and announcements combined. We now test for differential effects across firms of varying trading volume, market size, and analyst following. Specifically, we allow the Post coefficient in equation (3) to be a linear function of trading volume (LNTRDVOL), market size (LNMKTSZ), and analyst following (ANALFOLL). We define β_1 in equation (3) as

$$(4) \quad \beta_1 = \gamma_1 + \gamma_2 \text{LNTRDVOL} + \gamma_3 \text{LNMKTSZ} + \gamma_4 \text{ANALFOLL}$$

and estimate the modified regression (3) using sample data over days -2 through 0. Next, we measure the influence of the regulation on the six [Firm Size, Analyst Following] groups by evaluating β_1 of equation (4) at the group means of LNTRDVOL, LNMKTSZ, and ANALFOLL. Panel A of Table 5 reports the average fitted values of equation (4) for each group. Results suggest that the regulation has reduced effective spreads for the [Small Size, High Analyst] and [Small Size, Low Analyst] groups by 6.66 (p -value of 0.00) and 7.15 (p -value of 0.01) basis points, respectively. The analysis of price impact yields similar results. Joint tests of significance strongly reject the hypothesis of a cost increase for the two small firm groups and the medium firm with low analyst following group. However there is no significant impact for the other groups, suggesting that the impact of the regulation differs across firm groups.

To assess this more directly, we compute the *difference* between the impact of the regulation for each group and that for the full sample. In panel B of Table 5, the *difference* measures in effective spreads for the [Small Size, High Analyst] and [Small Size, Low Analyst] groups are -0.0353 (p -value of 0.04) and -0.0402 (p -value of 0.05), respectively. This implies that the Small size group had a larger decline in effective spreads of 3.5 to 4.0 basis points, relative to the average firm. To offset, the Large size group have positive *differences*. This general trend also pertains to the price impact measures. Recall from Table 1 that the level of liquidity declines monotonically as we move from the [Large Size, High Analyst] to [Small Size, Low Analyst] groups. Thus, our interpretation of the *difference* estimates is that Regulation FD has had a larger effect of reducing trading costs and information asymmetry around earnings announcements for smaller and less

TABLE 5
Regulation FD and Firm Characteristics

Transaction Cost Measure	Cumulative Impact of Firm Size, Trading Volume, and Analyst Following					
	Firm Size					
	Large		Medium		Small	
	High Analyst Following	Low Analyst Following	High Analyst Following	Low Analyst Following	High Analyst Following	Low Analyst Following
<i>Panel A. Impact of Regulation FD by Groups</i>						
Effective Spreads (%)	-0.0128	0.0252	-0.0313	-0.0308	-0.0666	-0.0715
<i>p</i> -val.	(0.71)	(0.41)	(0.20)	(0.23)	(0.00)	(0.01)
Price Impact (%)	0.0150	-0.0089	0.0066	-0.1272	-0.0971	-0.1447
<i>p</i> -val.	(0.80)	(0.86)	(0.88)	(0.00)	(0.01)	(0.00)
Joint <i>p</i> -val. (null: increase in trading cost)	(0.63)	(0.73)	(0.21)	(0.00)	(0.00)	(0.00)
<i>Panel B. Impact of Regulation FD relative to the Average Sample Firm</i>						
Effective Spreads (%)	0.0189	0.0565	-0.0000	0.0005	-0.0353	-0.0402
<i>p</i> -val.	(0.52)	(0.03)	(1.00)	(0.98)	(0.04)	(0.05)
Price Impact (%)	0.0706	0.0496	0.0651	-0.0687	-0.0386	-0.0862
<i>p</i> -val.	(0.17)	(0.26)	(0.04)	(0.05)	(0.19)	(0.01)

Reported are the estimates of the differential impact of Regulation FD on the six [Firm Size, Analyst Following] groups. First, we estimate the following weighted least square regression:

$$(3) \quad TC_{Ann,i,Regime} = \alpha + \beta_1 Post + \beta_2 TC_{Non,i,Regime} + \beta_3 LNTRADVOL + \beta_4 LNMKTSZ + \beta_5 ANALFOLL + \varepsilon_{i,Regime}$$

$$(4) \quad \text{where } \beta_1 = \gamma_1 + \gamma_2 LNTRDVOL + \gamma_3 LNMKTSZ + \gamma_4 ANALFOLL,$$

$$\text{Regime} = \text{Before Regulation FD, After Regulation FD}_{Teenies}, \text{ or After Regulation FD}_{Decimal}$$

where $TC_{Ann,i,Regime}$ and $TC_{Non,i,Regime}$ denote the average transaction costs measure for stock i on days -2 through day 0 around earnings-related announcements and during non-announcement periods in the corresponding regimes, Post dummy equals 1 for earnings announcements after the adoption of Regulation FD and 0 otherwise, and the weight variable is the number of earnings-related announcements for stock i in each regime. For each firm, LNMKTSZ is the log of market size at the end of October 2000, LNTRDVOL is the log of the trading volume in October 2000, and ANALFOLL is the number of analysts following the firm. For each group, we evaluate β_1 of equation (4) at the group means of LNTRDVOL, LNMKTSZ, and ANALFOLL. Reported in panel A are the average fitted values of each group. Also reported are the *p*-values of the joint tests of the restriction that both the effective spreads and price impact measures have increased. Panel B shows the *difference* between the impact of Regulation FD for each group and that for the full sample and the corresponding *p*-values.

liquid stocks. That this reflects the general tendency for less liquid stocks to have more informed trading before the regulation supports the arguments in Easley et al. (1996).

VI. Stock Return Volatility

The analysis thus far indicates a reduction in asymmetric information and the attendant trading costs around earnings-related announcements after Regulation FD. We now turn to the question of stock return volatility for additional perspective. Here we distinguish between the total amount of information flow and the amount of information asymmetry in that the latter measures only the cross-sectional advantage that some traders have over others. Our volatility investigation complements the work by Heflin et al. (2003) who investigate similar issues for a different sample of firms.

There is theoretical support for the notion that more informative prices should be more volatile. Ross (1989) shows, for example, that the variance of price changes should equal the rate of information flow because prices change in re-

sponse to information. If the regulation serves to concentrate information flow on earnings announcements and other public disclosures, as predicted by the critics of Regulation FD, then non-announcement volatility should fall and announcement volatility rise after regulation. On the other hand, no change in volatility around earnings announcements would be consistent with the predictions of the regulation's proponents that firms will adopt other forms of public disclosure to convey information previously released by selective disclosure.

We study the total information flow by looking at root mean-squared errors and average announcement prediction errors from a market model with two leads and lags where daily returns are based on quote midpoints, and the NYSE value-weighted index from CRSP is used as the market portfolio. For each trading regime, the logarithm of the root mean-squared error of the market model for all trading days and for non-announcement days was calculated.⁸ These showed no evidence of a change in volatility when the period before regulation is compared with the teenies regime after regulation. Thus, these comparisons provide no compelling evidence of a change in overall information flow, although the point estimates for non-announcement days indicate an insignificant increase in the teenies regime (from 0.94 to 0.97). The decimal regime shows a drop in volatility from both the period before regulation and the teenies regime after regulation for non-announcement days (0.73) and for all trading days (0.77). The drop across the two regimes after regulation suggests that the decline is unrelated to the regulation and is likely caused by reduced measurement (rounding) error in the midpoint of bid-ask quotes during the decimals regime.

To capture the aggregate information flow around earnings announcements, we use several cumulative information measures (CIM). Within each trading regime, market model coefficients are estimated with data over non-announcement days and then used to generate residuals in the non-announcement days and prediction errors in the announcement days. For each announcement, we define a ratio, $CIM_{i,a}$,

$$(5) \quad CIM_{i,a} = \left[\frac{\left(\sum_{t=\tau}^{\tau+T} \text{PREDERR}_{i,a,t} \right)^2}{T \times \text{MSE}_i} \right],$$

where $\text{PREDERR}_{i,a,t}$ is the market model prediction error for firm i in day t of announcement a and MSE_i is the mean-squared error of the residuals in non-announcement days from the same trading regime. In this measure, the prediction errors over several days are cumulated and then squared. Scaling by MSE_i accounts for firm-level heteroscedasticity and for changes in volatility over time due to decimalization and changing market conditions. The $CIM_{i,a}$ are averaged across announcements for each firm in the periods before and after the regulation,

⁸In this and subsequent tests involving mean-squared errors or residual variances, we work with the log of the variables because this monotonic transformation results in data more closely approximated by a normal distribution. In all cases, the untransformed data strongly reject normality, while the transformed data do not reject normality with a Kolmogorov-Smirnov goodness-of-fit. Thus t -tests of means and mean differences are better specified under the log transformation.

and then logs are taken. Intuitively, CIM measures the cumulative information flow during announcement periods relative to non-announcement periods. Note that the CIM measure equals one under the null hypothesis that announcement days have the same amount of information flow as non-announcement days.

Using (logged) $CIM_{i,a}$ measures for several trading windows around mandatory earnings announcements, we find no empirical support for an increase in price volatility after regulation. In fact, for days -2 through -1 , 0 , and 0 through $+2$, the evidence shows a marginal reduction in the total information flow around mandatory announcements (t -statistics of -1.99 , -1.70 , and -2.25 , respectively). Taken in isolation, the hint of a reduction in price reaction to mandatory announcements is puzzling in that one would expect an increase in information flow at the time of announcement if the primary effect of Regulation FD is to limit prior selective disclosure. However, the result is understandable if the regulation limits selective disclosure during conference calls on these specific days as suggested by Frankel et al. (1999) and Sundar (2002), or if the firms reveal more information through prior public disclosures.

To address the possibility that firms reveal more information through enhanced voluntary disclosures after regulation as a substitute for selective disclosure, we cumulate information flow by aggregating the CIM across both mandatory and any preceding voluntary announcements within a quarter. For each firm and quarter, define

$$(6) \quad CIMQ = \sum_{a=1}^A CIM_{i,a} - A,$$

where $CIMQ$ cumulates information across all of the A earnings-related announcements in the quarter for firm i , and $CIM_{i,a}$ is defined in (5) above. Since the number of voluntary announcements differs across quarters and firms, we subtract the expected $CIMQ$ under the null of no announcement effect, which is the number of announcements for the firm in the quarter. Next, the average $CIMQ$ for each firm across all quarters in the periods before and after regulation is computed, and then logged after adding a small constant.⁹ Although the point estimates remain generally negative, we find no significant change in the overall announcement-period information flow; the most negative t -statistic is for days 0 through $+2$ at -1.21 .

VII. Summary and Conclusions

Our study of a stratified sample of 300 NYSE firms finds that the level of information asymmetry as revealed in trading costs is lower after the introduction of Regulation FD. In the trading window of days -2 to 0 surrounding all earnings-related announcements, effective spreads and price impact decrease by 3.25 basis points and 5.90 basis points, respectively. In cross section, the results imply that Regulation FD has had the greatest impact on smaller and less liquid stocks; here the reductions are highly significant and as large as 14 basis points.

⁹The constant was chosen to provide a log transformation that approximates a normal distribution in cross section. We found that setting the constant equal to 1.1 times the $ABS(\text{MIN}_i(CIMQ_i))$ works quite well.

Analysis of return volatility suggests a reduction in average information flow around mandatory earnings announcements after the regulation. However, when mandatory and voluntary announcements are combined, any change in return volatility loses significance. Hence, our findings are more moderate than those of Heflin et al. (2003) who find rather dramatic decreases in squared prediction errors around mandatory announcements.

Given concerns of the investment community over possible increases in volatility around earnings announcements, the finding of a marginal reduction around these announcements is one of the more interesting results about the impact of Regulation FD. Much of the answer may rest in the fact that selective disclosure, before regulation, often occurred during announcement periods. But a lack of significance for changes in total information flow is consistent with firms finding other methods of public disclosure to offset the information flow provided by selective disclosure before regulation. The hint of an increase in overall information flow outside the days surrounding mandatory reporting is an intriguing area for future research.

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