Re-examining the Effects of Regulation Fair Disclosure
Using Foreign Listed Firms to Control for Concurrent Shocks

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We re-examine the effects of Regulation Fair Disclosure (Reg FD) using ADRs (who are exempt from Reg FD) to control for confounding events which affected all traded firms. Tests based on public information metrics (returns volatility, informational efficiency and trading volume) and on analyst information metrics (forecast dispersion and accuracy) suggest that Reg FD did not uniquely affect the US information environment. However, analyst report informativeness declined for US firms relative to ADR firms, providing evidence consistent with Reg FD achieving one of its objectives -- reducing private information flows to analysts.

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1. Introduction and Motivation

We examine the unique effects of Regulation Fair Disclosure (“Reg FD”) by comparing US firms, explicitly subject to Reg FD, and foreign firms listed on US exchanges with American Depository Receipts (ADRs), explicitly exempt from Reg FD (Rule 243.101(b)). Because both US firms and ADRs are subject to confounding events affecting all firms traded in the US, any information effects uniquely attributable to Reg FD will appear as post-Reg FD differences between US firms and ADRs.

Our analyses are motivated by the conflicting claims made for the likely effects of Reg FD and by the findings of prior research. With regard to the former, Reg FD, implemented on October 23, 2000 by the Securities and Exchange Commission (SEC), prohibits the selective communication of material information unless the same information is publicly disclosed within 24 hours. Proponents claimed Reg FD would eliminate favored access to information for a subset of investment professionals and improve the flow of information to financial markets by reducing analysts’ reliance on management-provided information and increasing their amount and quality of independent research. Opponents countered that Reg FD would reduce the flow of information, by eliminating the benefit that managers derive from selective disclosure, and replace continual information flows between firms and analysts with discrete blocks of public disclosure. Opponents concluded that Reg FD would likely result in deteriorations of information efficiency and accuracy; in particular, they argued that returns volatility would increase, informational efficiency and trading volume would decrease, and analyst guidance would be less accurate.

Thus, Reg FD is an example of a rule whose intent is to shift an informational advantage (in this case, from analysts to investors generally), but whose unintended effects may extend to the amount of information in the marketplace.

Previous research on the effects of Reg FD reports that US firms experienced declines in returns volatility, and increases in both trading volume and informational efficiency, consistent with a shift toward a richer public information environment (see, for example, Heflin et al. [2002] and Bailey et al. [2003]). Prior studies also report either an increase or no change in analyst forecast accuracy and
dispersion, and a deterioration in the informativeness of analysts’ reports (see, for example, Heflin et al. [2002]; Agrawal and Chadha [2002]; Bailey et al. [2003]; Irani and Karamanou [2003]; Mohanram and Sunder [2001]; Topaloglu [2002]; Shane et al. [2001] and Gintschel and Markov [2004]).

Because the period that contains the passage and implementation of Reg FD (2000-2002) also contains other events that would be expected to affect these metrics, research that attempts to discern the unique effects of Reg FD must control for these confounding events. Some prior Reg FD studies control for confounding events by including industry or macroeconomic variables (Heflin et al. [2003]), while others focus on one alternative explanation and exploit variation in it to parse out the effects of Reg FD (Bailey et al.’s [2003] analysis of the effects of decimalization). These approaches do not address whether other explanations drive the results and their power to detect effects is limited by the availability of control variables (e.g., observations for both decimalized and undecimalized stocks are available for only one quarter). A third approach exploits cross-sectional variation in the degree to which firms are affected by Reg FD (e.g., Bushee et al.’s [2004] analysis of the relative effects of Reg FD on firms which had previously held open versus closed conference calls; Gintschel and Markov’s [2004] investigation of variation in pre- versus post-differences in price responses to analyst reports depending on brokerage and stock characteristics; and Mohanram and Sunder’s [2001] analysis of Reg FD effects on the forecasting ability of All-Star versus non-All-Star analysts). The power of the cross-sectional approach hinges on the differential Reg FD response of the two groups (e.g., closed versus open firms in Bushee et al.). We believe that our cross-sectional approach, which compares US firms with ADRs, provides for maximal differences in the response to Reg FD because ADRs are exempt from the regulation, yet face similar capital market influences.

Following previous research, we investigate both public information effects (captured by changes in returns volatility, trading volume, and informational efficiency) and analyst information effects

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1 Confounding events during the 2000-2002 post-Reg FD period include: the US economic recession; the crash of the Internet bubble; the decimalization of the US stock exchanges; the demise of Enron, Worldcom’s acknowledgement of accounting errors and bankruptcy filing, and the demise of Arthur Andersen.
(captured by analyst forecast dispersion, analyst forecast accuracy and the informativeness of analyst reports). Our inferences are based on the view that if US firms and ADR firms experience similar public information and analyst information effects, then changes in the US information environment that coincided with Reg FD cannot be uniquely attributed to that regulatory change.\(^2\) With regard to public information effects, we find that pre-Reg FD versus post-Reg FD changes in returns volatility, trading volume and informational efficiency for 4,773 US firms (49,356 firm-quarters) are indistinguishable from changes found for 392 ADR firms (2,802 firm-quarters); these results are consistent with no unique Reg FD effect on public information. With regard to analyst information effects, we find no difference between US and ADR firms pre- and post-Reg FD in terms of analyst forecast accuracy or dispersion; however, report informativeness declined for US firms relative to ADRs. We interpret the latter as indicating a post-Reg FD decline in analysts’ access to private information. Because one stated goal of Reg FD was to shift information flows away from private channels, the results for analyst report informativeness support the view that Reg FD had at least one of its intended effects.

We recognize that there are other explanations for some of our results. Specifically, it is possible that (1) public information effects exist but our tests do not have sufficient power to detect them; or (2) Reg FD caused market participants to increase (decrease) their analysis of US firms (ADR firms), resulting in no net difference in public information between the two types of firms; or (3) ADR firms voluntarily complied with Reg FD (despite an explicit exemption).\(^3\) Concerning the first explanation, we

\(^2\) In concurrent work, Gomes et al. [2004] and Mathew et al. [2002] also consider the effects of Reg FD on ADRs. Gomes et al. summarize the results of a robustness test showing that large (small) ADRs had similar responses to Reg FD as large (small) US firms. They do not report sample sizes or details of their tests. Mathew et al. re-examine prior studies’ finding of reduced returns volatility post Reg FD using a non-matched sample of 30-46 ADRs and 117-430 US firms. They report a significant immediate increase in returns volatility for US firms in the first post Reg FD quarter; no similar increase is found for ADR firms. Relative to these studies, we believe our study is more comprehensive: we use a longer series of pre- and post Reg FD quarters, have a larger sample of ADRs, and examine a broader set of information metrics.

\(^3\) Some ADRs indicated they would not follow Reg FD; others believed investors would lose confidence if they did not respond. For example, Alcatel management was quoted as saying “the company will continue to hold one-to-one meetings with US analysts…We want to avoid that extremely conservative approach but also recognize that investor confidence is built through openness” (Rosenbaum [2001]). On the other extreme, Nokia management claimed they planned to review and adhere to Reg FD (Rosenbaum [2001]). DaimlerChrysler AG and TV Azteca SA indicated their intent to adapt to the behaviors of their US counterparts (Remond [2000]).
note that our tests focusing solely on US firms (and not using ADRs as a benchmark) show the same
effects found in prior studies. Further, our tests that focus solely on ADRs also show effects similar in
magnitude (and statistical significance) to those documented for US firms. Since we estimate essentially
the same regressions as prior studies, we believe that our tests suffer no more from power concerns than
do prior studies’ tests.

The second explanation is a variant of Mohanram and Sunder’s [2003] substitution argument,
positing that, post-Reg FD, market participants would shift time and effort away from gathering and
processing information about ADR firms and toward analyzing US firms. Although such substitution is
consistent with our finding of no relative difference in the changes in most of the information metrics
between US firms and ADRs, it is not easily reconciled with the results for report informativeness. We
(and Gintschel and Markov) find that analyst report informativeness decreased for US firms (as would be
expected if Reg FD shifted information away from private channels for US firms) but increased for ADR
firms (as would be expected if ADR’s exemption from Reg FD allowed their private information flows to
continue). These findings are at odds with a substitution argument which would not predict either a
decline in report informativeness for US firms (because if analysts fully compensated for the loss in
private information, report informativeness would not change), or an increase in report informativeness
for ADR firms (if anything, substitution predicts a decrease in informativeness because analysts would
expend less, not more, effort on ADRs).

Concerning the third explanation, if ADR firms fully complied with Reg FD despite their explicit
exemption, there would be no differences in Reg FD effects between US firms and ADR firms; partial
compliance would be expected to be associated with some differences (of uncertain magnitude and
significance). We cannot rule out this explanation completely; however, there are several reasons for
believing that ADR firms did not systematically comply with Reg FD.5 First, we view our finding of a

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4 We thank both Stan Markov and an anonymous reviewer for bringing this possibility to our attention.
5 Anecdotal evidence of some ADR firms’ compliance with Reg FD is provided in Citigroup’s Depository Receipts
relative increase in analyst report informativeness for ADR firms as evidence that those firms relied on their exemption from Reg FD to continue to share private information with analysts. Second, foreign firms lobbied for exemption from Reg FD, which would not be consistent with subsequent voluntary compliance with its provisions. Third, non-US firms would generally have less incentive to avoid selective disclosure because their home country disclosure regulations and/or enforcement were not as stringent as the US post-Reg FD environment. Regardless of the rules, in no case did the actual disclosure practices in these countries result in the absence of selective disclosure (Eisinger, Hagerty and Kueppers [2001]; Pottinger [2001]).

In short, while we cannot completely rule out all alternative explanations for our findings, we believe that the weight of the evidence supports two conclusions. The first is that changes in the US public information environment that occurred concurrently with Reg FD cannot be uniquely attributed to Reg FD. The second is that Reg FD did cause a shift away from information dissemination through the channel of analyst reports, consistent with one of the stated goals of the regulation.

Recent survey evidence from PriceWaterhouse Coopers (PwC), the National Investor Relations Institute (NIRI), and Janvrin and Kurtenbach [2002] suggests that US firms themselves did not respond to the provisions of RegFD. All three surveys conclude that most US executives report that Reg FD had little impact on their disclosure practices. As a comparison, we conducted a survey of ADRs’ responses to Reg FD. Of the 43 firm-respondents, 36 said their overall disclosure practices were unchanged as a result of Reg FD, six reported an increase in overall disclosure, and one reported a decrease. Our survey

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6 Of the 164 respondents to the PwC survey, 51% reported that Reg FD did not affect their disclosure practices; those who saw an impact were split between Reg FD increasing disclosures (48%) and decreasing disclosures (52%); about 75% of respondents indicated that Reg FD had no effect on their share price or their share price volatility. The NIRI study showed similar results concerning overall disclosure practices (i.e., roughly half reporting no change, with the other half split between increases and decreases); it also revealed that 74% of respondents reported the same number of one-on-one meetings with analysts as they did pre-Reg FD. The surveys are discussed in Barbash [2001] and in the CPA Journal [2001]).
results are similar to other, larger, surveys in that most respondents (36 of 43, or 84%) indicated no change in disclosure practices in response to Reg FD.\textsuperscript{7}

The rest of the paper is organized as follows. Section 2 describes the sample and data, section 3 presents the empirical tests and results, and section 4 concludes.

2. Sample Selection and Descriptive Statistics

We identify 4,788 US firms and 417 ADRs (Level II and Level III ADRs and Canadian firms) with stock price data available for at least 18 months prior to the effective quarter of Reg FD (i.e., April 1999 through October 2000) and with annual Compustat data on assets and sales for 1999, 2000 and 2001. This initial sample is further reduced by requirements of the public information metrics and analyst information metrics, described next.

Public information metrics: Returns volatility, information efficiency, and trading volume are calculated relative to earnings announcement dates. We assign each fiscal quarter earnings announcement to a calendar quarter based on the announcement date. We use calendar quarters rather than fiscal quarters to include firms with and without December fiscal year-ends. (It is inappropriate to compare market measures linked to fiscal quarters because the events that occurred concurrent with Reg FD are calendar-time specific, not fiscal-time specific.) We exclude the third and fourth calendar quarters of 2000 (III.2000 and IV.2000) because these quarters cover Reg FD discussion and implementation periods. Each of our pre- and post-periods consists of six calendar quarters: I.1999 to II.2000 for the pre-Reg FD period and I.2001 to II.2002 for the post-Reg FD period.

Our public information tests require that each US or ADR firm have earnings announcements in the same calendar quarters of both the pre-period and the post-period. The quarters are matched as follows: I.1999 with I.2001; II.1999 with II.2001; III.1999 with III.2001; IV.1999 with IV.2001; I.2000\textsuperscript{7}

\textsuperscript{7} Barnett [2001] suggests that US firms may not have responded to Reg FD because it did not materially change the “rules of conduct” between companies, analysts and shareholders, but rather made the existing rules easier to enforce; the key question then becomes the enforcement of Reg FD. On this point, the popular press is replete with examples of managers’ confusion with respect to how the SEC would interpret the materiality of a disclosure (e.g., Appin [2001]; Cowan [2001]; \textit{The Economist} [February 10, 2001]).
with I.2002; and II.2000 with II.2002. The matched calendar quarter requirement reduces the sample to 4,773 US firms (49,356 quarters) and 392 ADRs (2,802 quarters). For the most part, sample size for these tests is not affected by other data requirements. The exception is the trading volume tests, where Bailey et al [2003] include the dispersion in analysts' forecasts as a control variable. The dispersion of analysts' forecasts for firm $i$ in quarter $q$ is measured as the standard deviation of all earnings forecasts for quarter $q$ made in quarter $q-1$ as reported on the Zacks Investment research database, scaled by stock price. Requiring data on dispersion reduces the sample to 2,272 US firms (19,382 matched quarters) and 69 ADR firms (474 matched quarters). Because of this control variable's significant effect on sample size, we test for trading volume effects with and without a control for forecast dispersion.

**Analyst information metrics:** We focus on the dispersion, accuracy, and newsworthiness of analysts' reports in the pre- versus post- Reg FD periods. Tests of dispersion use the sample described above for trading volume, requiring data on dispersion. For tests of accuracy, we apply the same test procedures as described for the public information metrics within the set of firms each analyst follows; that is, we require that analyst $j$ follow both US firms and at least one ADR firm in the same industry. We further require that these same-analyst pairings have earnings forecasts for quarter $q+1$ (made in quarter $q$) in the matched pre- and post-Reg FD quarters (e.g., the pairing is available for quarter $q=I.1999$ and $q=I.2001$). The latter ensures that the pre- versus post-Reg FD comparison of forecast accuracy holds constant, for each firm, the fiscal quarter of earnings being forecast. The forecast accuracy sample consists of 4,312 pairings (836 ADRs and 3,476 US), representing 120 analysts and 296 US firms (57 ADR firms).

Limiting the sample to firms with same-analyst forecasts eliminates information effects arising from pre- and post-Reg FD changes in analyst following, and controls for analyst-specific effects (e.g., experience of the analyst, employer resources, and the definition of earnings being forecast) on forecast accuracy (Mikhail, Walther and Willis [1997]; Clement [1999]; Jacob, Lys and Neale [1999]; Abarbanell
and Lehavy [2002]). While the same-analyst design reduces statistical power if information about ADRs and US firms is perfectly complementary (so that analyst $j$ reduces analysis of an ADR if its US counterpart restricts information), it increases power if analysts view US firms and ADR firms as substitutes (so that analysts reduce effort expended on ADRs and increase effort on US firms, or vice versa). Because we believe our results are more sensitive to substitution (see section 1) than to complementarity, and because of the analyst-specific controls, we believe the same-analyst design is preferred to one that does not require forecasts to be issued by the same analyst. However, tests of analyst forecast accuracy that rely on a consensus measure would be potentially sensitive to shifts in either the number of analysts or their behavior; for example, if the number of analysts drops or each analyst decreases the number of firms followed, the consensus will become less accurate even if each analyst’s individual accuracy is unchanged.

For tests of newsworthiness, we use Gintschel and Markov’s procedures to calculate the effect of Reg FD on forecast announcement volatility. The tests are based on forecasts made for 2,927 US firms and 126 ADRs between January 1, 1999 and June 30, 2002. Because analysts often issue several forecasts of different horizons on the same date, we treat such multiple forecasts as a single forecast event.

Table 1 reports descriptive information about the US and ADR firms for the samples used to calculate public information metrics; results (not reported) are similar for the analyst sample. Panel A shows that the Canadian and UK firms have the largest concentrations of the 392 ADRs (Panel A). Panel B reports selected financial information about the ADRs. We measure all variables as of the end of 1999, and we winsorize them at the 1% and 99% values. As is evident from these data, ADR firms are larger than US firms: the mean value of total assets is $2.0 billion and $15.2 billion for the US and ADR firms, respectively; for sales, the comparison figures are $1.2 billion and $5.9 billion. Based on prior research, we expect that the substantially larger size of ADRs versus US firms in our samples will, if anything, bias

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8 We control for the definitional consistency of earnings because the different accounting rules ADRs follow make it difficult to identify exactly what earnings number analysts are forecasting. In addition, analysts following a given firm (US or ADR) may forecast different earnings numbers. Our use of same-analyst forecasts assumes that analysts exhibit over-time consistency in their definitions of earnings.
our tests towards finding larger Reg FD effects for our sample of US firms. In particular, prior studies show that Reg FD had a more pronounced effect on smaller firms (Gomes et al., 2004; Bailey et al., 2003), and size has been associated in prior research with characteristics of firms’ information environments (e.g., El-Gazzar, 1998; Bamber, 1987; Lang and Lundholm, 1996.) Given that our US sample contains, on average, smaller firms than the ADR sample, we expect that US firms will show larger effects. To address some of the effects of size difference between the samples, our multivariate tests control for size. As an additional check for possible sensitivities of our results to size and industry effects, we repeat our tests on a subset of US and ADRs, matched on industry and size. Results (discussed in section 4) are similar in all respects to those documented for the full sample.

We also examine the correlation in stock returns of the ADR and US samples. Correlation coefficients based on average monthly stock returns over 1999-2001 are 0.91 (Pearson) and 0.92 (Spearman), significant at the 0.0001 level. These high correlations indicate that the implicit assumption that ADRs faced similar economic shocks as their US counterparts is descriptively valid for our sample.

3. Empirical Tests and Results

Our tests examine changes in public information metrics and in analyst information metrics before and after Reg FD. If Reg FD improved the public information environment (by replacing private information flows with public information flows), we predict declines in returns volatility and increases in trading volume and informational efficiency. If Reg FD shifted information dissemination so that analysts obtained and released relatively less private information in their reports, we predict a decrease in analyst report newsworthiness. We make no predictions about analyst forecast accuracy and dispersion. On the one hand, if analysts had been using private information to prepare their forecasts, the public dissemination of this information should not affect either accuracy or dispersion, since analysts would still have access to the information; however, public dissemination would reduce the informativeness of their reports. On the other hand, if firms’ responses to Reg FD were to publicly disclose less information
than what they previously disclosed privately to analysts, we expect forecast accuracy to decline and forecast dispersion to increase. As noted in section 1, prior evidence shows improvements in public information metrics, and either no change or an increase in analyst forecast dispersion and forecast accuracy. Our tests evaluate the null hypothesis of no change in any metric against an alternative two-sided hypothesis (all tests of significance are two-sided).

Table 2 reports results for our main test variables; we do not tabulate results for the control variables. For each metric, we summarize the results found in prior research (Panel A) and document that our sample of US firms exhibits similar results (Panel B). To be consistent with prior research, we report results that compare US firms’ pre-Reg FD values of a given metric with their post-Reg FD values, controlling for other factors affecting this difference.\(^9\) The test variable for Panel B tests is \(\alpha\) estimated from the following regression (the test of significance for report informativeness is based on Newey-West [1987] t-statistics):

\[
Metric_{i,q} = \alpha_0 + \alpha_1 PostRegFD_q + \sum \lambda_c Control(c)_{i,q} + \epsilon_{i,q}
\]

where \(Metric_{i,q}\) = the dependent variable of interest; \(PostRegFD_q = 1\) if quarter \(q\) is after IV.2000, 0 otherwise; \(Control(c)_{i,q}\) = vector of \(c\) control variables.

Documenting similar effects for our US sample provides confidence that differences between US firms and their ADR counterparts (found in our third set of tests) cannot be attributed to low power tests or differences in US firm sample composition.

Our main test, summarized by equation (2), examines the relative effect of Reg FD on US firms, using ADRs as the benchmark (Panel C):

\[
Metric_{i,q} = \beta_0 + \beta_1 PostRegFD_q + \beta_2 US_i + \beta_3 PostRegFD_q * US_i + \sum \lambda_c Control(c)_{i,q} + \epsilon_{i,q}
\]

where \(US_i = 1\) firm \(i\) is a US firm, 0 if firm \(i\) is an ADR.

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\(^9\) Our multivariate tests use control variables that are similar, but not always identical, to those used in prior research. In some cases, we include additional control variables; in other cases, we use a measure that is highly correlated with a measure used in prior research; in still other cases, the inclusion of a particular control variable changes the sample significantly due to the data requirements needed to calculate the variable.
In equation (2), the coefficient on $PostRegFD_q \cdot \beta_i$, captures the difference in the metric between the pre- and post-Reg FD periods. Because both US and ADR firms are included in estimating (2), $\beta_i$ captures the change in the metric that is not uniquely associated with Reg FD; that is, it captures the average change in the metric observed for all firms. We include $US_i$ as a separate variable to control for levels-differences between US and ADR firms; for example, if US firms have lower returns volatility than ADR firms in both the pre- and post-Reg FD periods, then $\beta > 0$. Including $US_i$ as a main effect avoids concerns that any documented effects are merely the result of US firms differing systematically from ADR firms along dimensions not explicitly controlled for in our tests. The focus of our hypotheses tests is the coefficient on the interaction term, $PostRegFD_q \cdot US_i \cdot (\beta_i)$, which captures the change in the metric that is uniquely associated with Reg FD.

The appendix contains details of the tests for each metric, where we report the full regression models estimated for each metric and describe the measurement of the dependent, test and control variables used in each regression.

3.1. Evidence concerning pre- versus post-Reg FD changes in public information metrics

Event returns volatility (around earnings announcements): Following Heflin et al. [2002], we use the sum of daily squared abnormal returns as our measure of event returns volatility around firm $i$’s quarter $q$ earnings announcement, $SqrCAR(-1,+1)_{i,q} = \sum_{t=-1}^{+1} AR_{i,t}^2$, where $AR_{i,t}^2 = \text{firm } i \text{'s squared abnormal return on day } t$, and abnormal returns are calculated using the Fama and French [1993] 3-factor regression. Heflin et al. [2002] estimate the 3-factor model over the one year period ending the day before the start of the pre-Reg FD quarter; our estimation period of days (-266,-66) is roughly similar. Table 2, Panel A shows that Heflin et al. [2002] find that US firms’ $SqrCAR(-1,+1)$ declined by -0.0017 (p-value = 0.00), controlling for other factors affecting $SqrCAR(-1,+1)$. Replication of their tests on our sample of US firms (Panel B) shows a similar decline in event returns volatility of -0.0013 (p-value = 0.000). In unreported tests, we also find a similar decline using Bailey et al’s [2003] measure of event returns.
volatility – the absolute value of daily CAPM abnormal returns summed over days (-1,+1). These findings indicate that our US sample exhibits similar declines in event returns volatility following Reg FD as documented by prior research.

Our main tests focus on the results of estimating (2) using the combined sample of US and ADR firms. The results (Panel C) show no evidence (as indicated by insignificant values of \( \beta_3 \)) that US firms’ event returns volatility shifted relative to ADR firms’ volatility. We also repeat these tests using Bailey et al.’s measure of event returns volatility and find the same results. Based on this evidence, we conclude that Reg FD had no effect on the volatility of returns around US firms’ earnings announcements that is incremental to the concurrent change in event returns volatility experienced by ADR firms.

*General returns volatility:* Our measure of general returns volatility follows Heflin et al. [2002], who calculate the squared value of daily 3-factor abnormal returns for the interval beginning three days after the announcement of quarter \( q-1 \) earnings and ending two days after the announcement of quarter \( q \) earnings, \( SqrCAR(\tau +3,+2)_{t,q} = \sum_{t=\tau+3}^{\tau+2} AR_{t,q}^2 \), where \( \tau = q-1 \) earnings announcement date. (This interval corresponds, roughly, to days -65 to +2, measured relative to the quarter \( q \) earnings announcement on day 0.) Their comparison of this measure for US firms between pre- and post-Reg FD quarters shows a decline of -0.0109 (p-value of 0.00), controlling for other factors known to affect volatility. Replication of their tests on our US sample (Panel B) shows a decline of -0.0281 (p-value of 0.000).

Our main tests of the relative change in general returns volatility (Panel C) indicate a general decline in general returns volatility between the pre- and post-Reg FD periods (\( \beta_1 = -0.0229 \), p-value = 0.000). The estimate of \( \beta_3 \) is not reliably different from zero indicating that Reg FD had no incremental effect on the general volatility of US firms’ returns, as measured relative to the general returns volatility effects experienced by ADR firms. These results (like those for event returns volatility) suggest that shifts in returns volatility experienced by US firms following Reg FD cannot be attributed to Reg FD, and are more likely to be attributable to events affecting all traded firms.
Informational efficiency: Informational efficiency measures the gap between the full-information stock price and a pre-event price. We follow Heflin et al’s [2003] calculation of this construct which is the absolute cumulative abnormal return over \( h \) days prior to the earnings announcement:

\[
ACAR(-h_2+2)_{t,q} = \prod_{t=-h}^{2} [1 + (AR_{t,q})] - 1,
\]

where abnormal returns \((AR)\) are calculated using the CAPM estimated over days \((-266,-66)\) and \( h \in [-1, -2, -5, -10, -30] \). As shown in Panel A, Heflin et al. [2003] report smaller values of this measure for post-Reg FD quarters, conditional on other factors expected to affect informational efficiency. They interpret this result as indicating that US firms’ information environments became more efficient after Reg FD. Our US sample also shows smaller values of the measure in post-Reg FD quarters relative to pre-Reg FD quarters (significant at the 0.000 level). We note that for all values of \( h \) (Panel B) the magnitude of this effect is somewhat smaller for our US sample than the information efficiency effects documented by Heflin et al. [2003].

Results in Panel C suggest that the improvement in informational efficiency documented for our US firms is not driven by compliance with Reg FD: we find an overall increase in informational efficiency for both US and ADR firms \((\beta_1 < 0 \text{ for all values of } h, \text{ significant at the 0.014 level or better})\); however, there is no evidence of an incremental decline in the measure for US firms \((\beta_2 \text{ is statistically indistinguishable from zero})\). These results suggest that the post-Reg FD increase in informational efficiency is likely to be attributable to factor(s) other than Reg FD itself.

Trading volume: We follow Bailey et al [2003] and define abnormal trading volume as the difference between the average trading volume over days \((-1,+1)\) relative to the earnings announcement date and the average volume for that stock over days \((-200, -11)\), divided by the latter:

\[
ATV(-1,+1)_{t,q} = \frac{\text{avg}[TV(-1,+1)_{t,q}] - \text{avg}[TV(-200,-11)_{t,q}]}{\text{avg}[TV(-200,-11)_{t,q}]}, \text{ where trading volume } (TV) \text{ on day } t \text{ is the percentage of outstanding shares traded that day. Bailey et al’s multivariate tests show a significant}
\]
increase in abnormal trading volume (the average coefficient estimate is 0.3104, and is significant in all six quarters), conditional on other factors affecting trading volume. We repeat Bailey et al.’s tests (excluding dispersion as a control variable) on our US sample (Panel B) and, consistent with their findings, we document an increase in abnormal trading volume following Reg FD of 0.1767, significant at the 0.000 level (the increase is 0.1431 if dispersion is included, significant at the 0.000 level).

Our main tests (Panel C) show no evidence of a bigger change in abnormal trading volume (either upward or downward) for US firms; in all specifications, the coefficient on PostRegFD*US is indistinguishable from zero. In contrast, we continue to find (for the specification that excludes dispersion as a control variable) that $\beta > 0$, consistent with a general post-Reg FD increase in abnormal trading volume.

3.2. Evidence concerning pre- versus post-Reg FD changes in analyst information metrics

Forecast dispersion: At least two prior studies (Heflin et al. [2003] and Bailey et al. [2003]) examine the change in forecast dispersion between pre- and post-Reg FD periods. In a univariate comparison, Bailey et al. report an increase in mean dispersion of 0.19, significant at the 0.00 level; they do not report a multivariate test. While Heflin et al.’s [2003] univariate comparison also shows an increase (of 0.243, significant at the 0.00 level); their multivariate tests indicate dispersion is unchanged. We repeat their tests on the sample of US firms with data on forecast dispersion and the control variables, and obtain similar results (Panel B); specifically, we document a significant increase in dispersion in univariate tests and no change in multivariate tests. Our main tests (Panel C) show no evidence that US firms experienced any larger (or smaller) change in dispersion than ADRs.

Forecast accuracy: Our tests of forecast accuracy use the 4,312 analyst-firm-quarter observations (836 ADR observations and 3,476 US observations) with data on analyst j’s forecast accuracy for both US firms and ADRs, and the control variables. We define forecast accuracy as the absolute value of the difference between firm i’s actual earnings for quarter q and the value of analyst j’s forecasts (made in the prior calendar quarter), scaled by the stock price ten trading days before the forecast date. We average the
measure across all forecasts made by a given analyst for quarter \( q \) to obtain an analyst-quarter-specific measure of forecast accuracy. Results using the last forecast made by analyst \( j \) during the calendar quarter produce similar results and are not reported.

Both Bailey et al. [2003] and Heflin et al. [2003] find a significant (at the 0.00 level) increase in absolute forecast errors using univariate comparisons. When control variables are included, Heflin et al. show this increase is no longer significant at conventional levels. We find similar results for our US sample (Panel B): a significant increase in absolute forecast errors based on univariate tests, and no change based on multivariate tests. Tests in Panel C of the relative pre- versus post-Reg FD change in forecast accuracy between US and ADR firms provide no evidence of any differential change in forecast accuracy between pre- and post-Reg FD periods for US versus ADR firms.

**Newsworthiness:** Gintschel and Markov [2004] investigate whether the newsworthiness of analysts’ reports changed after implementation of Reg FD. Following their study, we first estimate the daily coefficients on the event of earnings forecast announcements from a regression of absolute standardized daily returns on an indicator variable that equals 1 for the interval \( t \) through \( t+5 \) relative to the earnings forecast date, and equals 0 otherwise. Next, the estimated daily coefficients are regressed on a dummy variable equal to 1 if the forecast date is after October 23, 2000, Reg FD’s effective date, and 0 otherwise. The coefficient on the Reg FD variable captures changes in the newsworthiness of analysts’ forecasts. As shown in Panel A, Gintschel and Markov find a decline in the market reaction to analyst reports in the post-Reg FD period of about -0.0172 (significant at the 0.05 level). For our US sample, Panel B also shows a decline in newsworthiness, of -0.0251 (significant at the 0.010 level). Our main tests (Panel C) indicate that this decline understates the effect attributable to Reg FD; specifically, we find a significant positive value of \( \beta_1 = 0.0446 \) (p-value of 0.048), indicating a substantial increase in the general newsworthiness of analyst reports. Relative to this positive on-average effect, the newsworthiness of reports for US firms declined significantly, as indicated by \( \beta_3 = -0.0697 \) (p-value = 0.005). We interpret
this result as indicating that Reg FD had a discernible, unique effect on the newsworthiness of analysts’ reports.

3.3. Sensitivity tests

We perform several sensitivity tests. Because of size differences between US firms and ADR firms, and based on previous research that associates size differences with differences in information environments generally and differences in responses to Reg FD specifically (Gomes et al., 2004; Bailey et al., 2003), we repeat our tests after matching each ADR firm with a US firm based on industry and firm size. We match on industry and size because: 1) both have been shown to be related to the information variables that we examine and 2) the confounding events that ADRs control for are also related to industry and size. In terms of the first point, we note that prior research shows that larger firms have smaller stock return volatility (El-Gazzar, 1998), smaller abnormal trading volume around earnings announcements (Bamber, 1987), more accurate and less dispersed analyst earnings forecasts (Lang and Lundholm, 1996), and smaller market reactions to analysts reports (Asquith, Mikhail and Au, 2005). In terms of the second point, we note that the crash of internet bubble clearly had a differential impact on technology firms versus non-technology firms, indicating the importance of industry controls. As another example, the decimalization of US stock exchanges significantly reduced bid-ask spreads and return volatility (Ronen and Weaver, 2001; He and Wu [2005]; Bessembinder, 2003) but its effect was smaller for small and less actively traded firms (Li and Parker, 2005).

As discussed by Kothari, Leone and Wasley [2005], matching is superior to the control variable approach because it does not impose a specific functional form on the relation linking the variable of interest to the control variables. Matches are identified by an algorithm similar to Lo [2003]: for each US firm $i$ in the same industry as ADR firm $k$, we calculate the absolute percentage differences in both assets and sales, measured in 1999. The sum of the two differences yields a matching score for each US firm $i$ that is in the same industry as ADR firm $k$. From the set of matching scores that are less than two (values of one and three produce similar results), we choose the US firm with the smallest matching score for each ADR firm; we then remove the matched pair (the ADR and its US counterpart) from the lists of
ADR and US firms. When one US firm is the best match for several ADRs, we control for the order that we match by calculating all possible matching scores, and then assigning the US firm \( i \) to the ADR firm \( k \) with the smallest score. For the remaining ADRs, we repeat the steps using the remaining US firms. This process yields 370 matched ADR and US firms (2,602 matched firm-quarters). Repeating our tests on this matched sample produces the results shown in Table 3. Panel A shows the results for our matched US firms, and Panel B shows the relative effects between the matched US and ADR firms. Overall, our inferences are similar to those reported for the pooled sample, except that the effects for US firms are generally less pronounced for the matched US sample (Panel A, Table 3) than for the non-matched US sample (Panel B, Table 2). Importantly, the matched US-ADR samples show the same patterns in relative effects (Panel B, Table 3) as shown in Panel C, Table 2.

A second concern is that our sample of ADRs has low power and any difference between US firms and ADRs is due to our inability to document significant effects in the sample of ADRs. We address this power concern by repeating our tests, based on equation (1), for just the sample of ADRs. If the ADR sample has low power, we expect to document insignificant coefficients for the ADR sample; if, however, the coefficients for the ADR sample are comparable in magnitude and significance to those found for the US sample, such a finding increases our confidence that the absence of any difference between US and ADR firms is not due to lower power of the ADR sample. The results of these tests, shown in Panel A of Table 3,\(^{10}\) indicate that for most metrics, the effect observed for the ADR sample is comparable to the effect found for the matched US sample (shown in Panel A, Table 3) and the full US sample (shown in Panel B, Table 2). In particular, where we find a significant effect for US firms, we also generally find a significant effect for ADR firms; further, there is no pattern indicating that the effect for ADR firms is consistently smaller than the effect for US firms. The exception is the measure of report informativeness, where we find an increase of 0.0549 for ADR firms and a decrease for US firms (of -0.0329 for the matched US firms and -0.0251 for all US firms). The latter difference is consistent with

\(^{10}\) For consistency with the other information shown in Table 3, we report the results of tests that use the ADR firms in the matched sample (\( n=370 \) ADRs). In unreported tests, we verify that we draw similar conclusions using the full sample of ADR firms (\( n=392 \)).
our tests based on equation (3) which show a significant post-Reg FD decline in relative analyst report informativeness for US firms, consistent with finding documented by Gintschel and Markov [2004].

A third concern is that our tests are confounded by an exchange effect. Here, we repeat our tests including an exchange indicator variable \((\text{NYSE}=1 \text{ if the firm (ADR or US) is listed on the New York Stock Exchange, 0 otherwise})\) as a main effect and interacted with PostRegFD. Results of these tests (not tabulated) are similar in all respects to those documented.

A fourth issue is that some unspecified feature of the information environments of ADR issuers’ home countries affect ADR firms’ metrics in systematic ways that offset (true) Reg FD effects. We repeat our tests adding indicator variables for the five issuer countries with the largest concentrations of ADRs in our sample (Canada, England, Japan, France and Mexico). While the country indicators are significantly different from zero in some tests, in no case do they alter inferences about the relative effects of Reg FD on US firms versus ADRs. We also examine whether our results are driven by non-Canadian ADRs. This analysis is motivated by the fact that Canadian firms are exempt from US reporting requirements because of the comparability of disclosure requirements between the US and Canada (Leuz [2003]). We re-estimate all regressions excluding Canadian firms; results are similar to those reported.

Our fifth sensitivity check examines the claim that smaller firms were more affected by Reg FD because they generally had fewer public disclosures and smaller analyst following than larger firms (Gomes, Gorton and Madureira [2004]). While our pooled tests include size as a control variable and the matched samples control for size by construction, we further probe the effects of size by repeating our tests on size quintiles, where the quintile break points are based on the total asset values (in 1999) for all Compustat firms. We find similar results to those reported for the full sample; in particular, there is no evidence that our findings are driven by the smallest firms (or, for that matter, by the largest firms).

As a final sensitivity check, we examine whether the results for the public information metrics and for forecast dispersion are robust to regressions which use a single observation for each firm for each
of the pre- and post-Reg FD periods.\textsuperscript{11} For each firm we calculate the average value of each variable over the six pre-Reg FD quarters (I.1999 to II.2000) and the six post-Reg FD quarters (I.2001 to II.2002); we repeat our tests substituting these means for the firm-quarter values. Results are similar to those reported.

4. Conclusion

We re-examine the effects of Reg FD on measures of public information (returns volatility, information efficiency and trading volume) and analyst information (forecast dispersion and accuracy and the informativeness of analysts’ reports) using a design that compares information measures before and after Reg FD for US firms relative to ADR firms that are traded in the US but specifically exempt from Reg FD. This research design, which benchmarks the changes experienced by US firms against the changes experienced by ADR firms and thereby controls for changes in the information environment that coincided with Reg FD, allows us to detect which effects, if any, can be attributed uniquely to Reg FD. Our results are consistent with contemporaneous events, and not Reg FD, affecting the public information environment, and with unique effects of Reg FD on the analyst information environment (the informativeness of analysts’ reports declined for US firms, both absolutely and measured relative to ADR firms). That is, of changes in the US information environment attributed by prior research to effects of Reg FD, our results show that only the decrease in analyst report informativeness can be uniquely associated with Reg FD.

While we believe the weight of the evidence supports a conclusion that effects documented by previous research are attributable to other forces, we cannot rule out all alternative explanations (as discussed in section 1). Some of these alternative explanations raise other questions. For example, it is possible that the finding of no differences between US firms and ADR firms is due to ADR firms’ voluntary compliance with Reg FD (after lobbying for exemption); if so, the question arises as to what market or institutional forces would force this change in ADR firms’ behavior. It is also possible that Reg

\textsuperscript{11} It is not possible to use firm-level tests to examine forecast accuracy because our research design for this metric is analyst-specific. Tests of report informativeness are already conducted at the firm-level.
FD simply had no effects on disclosures, either because there was little selective disclosure before 2000 (if so, the question arises as to why the regulation was implemented) or because Reg FD is not meaningfully enforced (if so, the question arises as to why the SEC would promulgate a controversial rule and then not enforce it).

Our results do not imply that Reg FD had no influence at all on either the types of information provided by management and sought by analysts, or the mechanisms used to communicate such information. There is anecdotal evidence that Reg FD increased the amount and timing of company-initiated earnings pre-announcements (Opdyke [2001]); increased the use of webcasts of earnings disclosures (Vinzant [2001]); and altered how analysts and other investment professionals acquire and process information (Bodow [2001]; Opdyke [2000]; Opdyke and Nelson [2000]; Clifford [2000]). Our large sample tests provide systematic evidence on the net effect of these changes on commonly used measures of firms' information environments.
Appendix: Details of Tests Conducted

The main test variables are the same across the metrics examined: \( \text{PostRegFD}_q = 1 \) if quarter \( q \) is after IV.2000; 0 otherwise; \( \text{US}_i = 1 \) if firm \( i \) is a US firm, 0 if firm \( i \) is an ADR.

A.1. Tests of event returns volatility, general returns volatility, and information efficiency\(^\text{12}\)

For US sample only:

\[
\text{Metric}_{i,q} = \alpha_0 + \alpha_1 \text{PostRegFD}_q + \lambda_{c1} \text{RETVAR}_{i,q} + \lambda_{c2} \text{ABSCAR}_{i,q} + \lambda_{c3} \text{NEGCAR}_{i,q} + \lambda_{c4} \text{LOSS}_{i,q} \\
+ \lambda_{c5} \text{EPRatio}_{i,q} + \lambda_{c6} \text{SIZE}_{i,q} + \lambda_{c7} \text{ABSINDX}_{q} + \varepsilon_{i,q}
\]

For combined US and ADR sample:

\[
\text{Metric}_{i,q} = \beta_0 + \beta_1 \text{PostRegFD}_q + \beta_2 \text{US}_i + \beta_3 \text{PostRegFD}_q * \text{US}_i + \lambda_{c1} \text{RETVAR}_{i,q} + \lambda_{c2} \text{ABSCAR}_{i,q} \\
+ \lambda_{c3} \text{NEGCAR}_{i,q} + \lambda_{c4} \text{LOSS}_{i,q} + \lambda_{c5} \text{EPRatio}_{i,q} + \lambda_{c6} \text{SIZE}_{i,q} + \lambda_{c7} \text{ABSINDX}_{q} + \varepsilon_{i,q}
\]

Dependent variable: \( \text{Metric}_{i,q} \in \{ \text{Event returns volatility, general returns volatility, information efficiency} \} \).

Event returns volatility = \( \text{SqrCAR}(-1,+1)_{i,q} = \sum_{t=-1}^{+1} AR_{i,t}^2 \), where \( AR_{i,t}^2 \) = firm \( i \)'s squared abnormal return on day \( t \), and 3-factor abnormal returns where the 3-factor model is estimated over days (-266,-65) relative to the earnings announcement date; General returns volatility = squared daily 3-factor abnormal returns over days (-65,+2) relative to the earnings announcement on day 0; Informational efficiency = absolute cumulative abnormal returns over \( h \) days prior to the earnings announcement:

\[
\text{ACAR}(-h,+2)_{i,q} = \left[ \prod_{t=-h}^{+2} \left[ 1 + (AR_{i,t}) \right] - 1 \right], \text{where abnormal returns are calculated using the CAPM and}
\]

\( h \in [-1,-2,-5,-10,-30] \).

\(^\text{12}\) There are two differences in our calculations versus HSZ [2002]. First, we estimate the 3-factor model over days (-266,-66) relative to the earnings announcement date; this period roughly corresponds to HSZ’s [2003] estimation period. For consistency, we measure \( \text{RETVAR} \) over the same (-266,-66) window. Second, we include as additional controls, firm size (\( \text{SIZE} \), as proxied by the firm’s total assets at the end of quarter \( q \)) and whether the firm announced a loss in quarter \( q \) \( \text{LOSS}=1 \) if quarter \( q \) earnings are less than zero, 0 otherwise). These differences have no substantive effect on the results.
Control variables: RETVAR = standard deviation of the firm’s returns over days (-266,-65); ABS CAR = absolute value of the firm’s cumulative abnormal return during quarter $q$; NEGRET = 1 if firm’s cumulative abnormal return in quarter $q$ is negative, 0 otherwise; ABSINDX = absolute value of value-weighted return on the market index in quarter $q$; EPRATIO = firm’s earnings-price ratio equal to quarter $q$ earnings divided by quarter-end price; LOSS = 1 if firm reported negative earnings in quarter $q$, 0 otherwise; SIZE = firm’s total assets at the end of quarter $q$.

A.2. Tests of abnormal trading volume

For US sample only:

$$ATV_{i,q} = \alpha_0 + \alpha_1 \text{PostRegFD}_{q} + \lambda_{c1} \ln\text{(SIZE)}_{i,q} + \lambda_{c2} \text{ARV}_{i,q} + \lambda_{c3} \text{ARV}_{i,q} \times \text{PostRegFD}_{q} + \lambda_{c4} \text{DISP}_{i,q} + \epsilon_{i,q}$$

For combined US and ADR sample:

$$ATV_{i,q} = \beta_0 + \beta_1 \text{PostRegFD}_{q} + \beta_2 \text{US}_{i} + \beta_3 \text{PostRegFD}_{q} \times \text{US}_{i} + \lambda_{c1} \ln\text{(SIZE)}_{i,q} + \lambda_{c2} \text{ARV}_{i,q}$$

$$+ \lambda_{c3} \text{ARV}_{i,q} \times \text{PostRegFD}_{q} + \lambda_{c4} \text{DISP}_{i,q} + \epsilon_{i,q}$$

Dependent variable: $ATV$ = difference between the average daily trading volume over days (-1,+1) relative to earnings announcement date 0 and the average daily volume for that stock over days (-200, -11), normalized by average volume.

Control variables: $\ln\text{(SIZE)}$ = log of total assets for quarter $q$; $\text{ARV}$ = absolute value of daily abnormal returns summed over days (-1,+1); $\text{DISP}_{i,q}$ = the dispersion of analysts’ forecasts for firm $i$ in quarter $q$, scaled by stock price at the end of quarter $q$-1.

A.3. Tests of analyst forecast dispersion and forecast accuracy

For US sample only:

$$\text{Metric}_{i,q} = \alpha_0 + \alpha_1 \text{PostRegFD}_{q} + \lambda_{c1} \text{ABSUE}_{i,q} + \lambda_{c2} \text{NEGUE}_{i,q} + \lambda_{c3} \text{LOSS}_{i,q} + \lambda_{c4} \text{SIZE}_{i,q}$$

$$+ \lambda_{c5} \text{DAYS}_{i,q} + \lambda_{c6} \text{EPRATIO}_{i,q} + \epsilon_{i,q}$$

For combined US and ADR sample:

$$\text{Metric}_{i,q} = \beta_0 + \beta_1 \text{PostRegFD}_{q} + \beta_2 \text{US}_{i} + \beta_3 \text{PostRegFD}_{q} \times \text{US}_{i} + \lambda_{c1} \text{ABSUE}_{i,q}$$

$$+ \lambda_{c2} \text{NEGUE}_{i,q} + \lambda_{c3} \text{LOSS}_{i,q} + \lambda_{c4} \text{SIZE}_{i,q} + \lambda_{c5} \text{DAYS}_{i,q} + \lambda_{c6} \text{EPRATIO}_{i,q} + \epsilon_{i,q}$$
Dependent variable: Metric_{i,q} \in (analyst forecast dispersion, analyst forecast accuracy). Analyst forecast dispersion ($DISP_{i,q}$) = standard deviation of all earnings forecasts for firm $i$ of fiscal quarter $q$ earnings, that are made in calendar quarter, $q-1$, scaled by stock price at the end of quarter $q-1$; analyst forecast accuracy ($AFE_{i,k,q}$) = absolute value of the difference between firm $i$’s actual earnings for quarter $q$ and the mean value of analyst $k$’s forecasts (made in the prior calendar quarter), scaled by the stock price ten trading days before forecast releases;

Control variables: $ABSUE_{i,q}$ = absolute value of firm $i$’s unexpected earnings in quarter $q$; $NEGUE_{i,q} = 1$ if the firm’s unexpected earnings for quarter $q$ are less than zero, 0 otherwise; $LOSS_{i,q} = 1$ if firm $i$ reports loss in quarter $q$, 0 otherwise; $SIZE_{i,q}$ = total assets for quarter $q$; $DAYS_{i,q}$ = the average number of days that the forecasts precede the earnings announcements; $EPRATIO_{i,q}$ = the firm’s earnings-price ratio measured as the firm’s quarter $q$ earnings divided by end of quarter $q$ price.

A.4. Tests of the newsworthiness of analyst reports

Following GM, we estimate daily cross-sectional regressions for each trading day in the pre-Reg FD period (01/01/1999-06/30/2000) and the post-Reg FD period (01/01/2001-06/30/2002):

$$|r_{i,t}| = \alpha_t + \beta_t D_{i,t} + \varepsilon_{i,t},$$

where $|r_{i,t}|$ is the absolute value of firm $i$’s standardized return on day $t$ and $D_{i,t} = 1$ for firm $i$ on day $t$ if an analyst report is announced in the interval of day $t$ to $t+5$, and 0 otherwise.

We standardize firm returns to mean zero and standard deviation one, based on the firm’s daily stock returns over the entire, pre- and post-, period. The slope coefficient, $\beta_t$, captures the increase in volatility around analyst reports. We exclude forecasts announced in the 2-day period following a firm’s earnings announcement. The daily slope coefficients are estimated separately for US firms ($\beta_{t,US}$) and ADR firms ($\beta_{t,ADR}$). These estimated coefficients are the dependent variables in the following equations:

For US firms: $\beta_{t,US} = \phi_0 + \phi_1 PostRegFD_t + \varepsilon_t$

For combined US and ADR sample: $\beta_{t,m} = \eta_0 + \eta_1 PostRegFD_t + \eta_2 US + \eta_3 US * PostRegFD_t + \varepsilon_t$

where $m \in (US, ADR)$; $US = 1$ if $\beta_t$ for US firms, 0 otherwise.
Table 1
Descriptive Statistics For US and ADR Sample Firms

Panel A: Distribution of US and ADR sample observations

<table>
<thead>
<tr>
<th></th>
<th># firms</th>
<th>% firms</th>
<th># qtr.obs.</th>
<th>% obs.</th>
</tr>
</thead>
<tbody>
<tr>
<td>US Sample</td>
<td>4,773</td>
<td>100%</td>
<td>49,356</td>
<td>100%</td>
</tr>
<tr>
<td>ADR Sample:</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Canada</td>
<td>120</td>
<td>30.6%</td>
<td>1,190</td>
<td>42.5%</td>
</tr>
<tr>
<td>England</td>
<td>54</td>
<td>13.8%</td>
<td>324</td>
<td>11.6%</td>
</tr>
<tr>
<td>Japan</td>
<td>23</td>
<td>5.9%</td>
<td>118</td>
<td>4.2%</td>
</tr>
<tr>
<td>France</td>
<td>19</td>
<td>4.8%</td>
<td>112</td>
<td>4.0%</td>
</tr>
<tr>
<td>Mexico</td>
<td>18</td>
<td>4.6%</td>
<td>90</td>
<td>3.2%</td>
</tr>
<tr>
<td>Chile</td>
<td>14</td>
<td>3.6%</td>
<td>72</td>
<td>2.6%</td>
</tr>
<tr>
<td>Australia</td>
<td>12</td>
<td>3.1%</td>
<td>44</td>
<td>1.6%</td>
</tr>
<tr>
<td>Netherlands</td>
<td>11</td>
<td>2.8%</td>
<td>74</td>
<td>2.6%</td>
</tr>
<tr>
<td>Ireland</td>
<td>11</td>
<td>2.8%</td>
<td>86</td>
<td>3.1%</td>
</tr>
<tr>
<td>Other</td>
<td>110</td>
<td>28.1%</td>
<td>692</td>
<td>24.7%</td>
</tr>
<tr>
<td>Total</td>
<td>392</td>
<td>100%</td>
<td>2,802</td>
<td>100%</td>
</tr>
</tbody>
</table>

Panel B: Comparison of selected financial data for US and ADR firms

<table>
<thead>
<tr>
<th>Variable</th>
<th>US Sample</th>
<th>ADR Sample</th>
<th>p-value for diff.</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>Median</td>
<td>Std Dev</td>
</tr>
<tr>
<td>Total assets ($ mil)</td>
<td>2,040</td>
<td>240</td>
<td>6,111</td>
</tr>
<tr>
<td>Sales ($mil)</td>
<td>1,205</td>
<td>143</td>
<td>3,555</td>
</tr>
<tr>
<td>Earnings-price ratio</td>
<td>-0.002</td>
<td>0.045</td>
<td>0.187</td>
</tr>
<tr>
<td>Leverage</td>
<td>0.183</td>
<td>0.122</td>
<td>0.196</td>
</tr>
<tr>
<td>Return on assets</td>
<td>-0.026</td>
<td>0.021</td>
<td>0.218</td>
</tr>
<tr>
<td>Return on equity</td>
<td>-0.005</td>
<td>0.090</td>
<td>0.532</td>
</tr>
</tbody>
</table>

Sample description and variable definitions: We report descriptive evidence for 4,773 US firms (49,356 firm-quarters) and 392 ADRs (2,802 firm-quarters). The financial variables (Panel B) are measured at the end of fiscal year 1999. Leverage is defined as long-term debt to total assets; the remaining variables are self-explanatory. We winsorize the financial variables to 1% and 99% values.

We report the mean, median and standard deviation of each variable for each of the US firms and ADR samples. The far right columns of Panel B report the p-values of tests of differences in mean and median values between the US and ADR samples. P-values for means are from two sample t-tests; p-values for medians are from Wilcoxon two-sample tests.
Table 2
Comparison of Changes in Public Information and Analyst-Information Metrics Around Reg FD
For Full Samples of US and ADR Firms

Panel A: Results found in prior research

<table>
<thead>
<tr>
<th>PostRegFD</th>
<th>coef. est.</th>
<th>p-value</th>
<th>Source</th>
</tr>
</thead>
<tbody>
<tr>
<td>Event returns volatility&lt;sup&gt;b&lt;/sup&gt;</td>
<td>-0.0017</td>
<td>0.00</td>
<td>HSZ[2002, Table 3]</td>
</tr>
<tr>
<td>General returns volatility&lt;sup&gt;b&lt;/sup&gt;</td>
<td>-0.0109</td>
<td>0.00</td>
<td>HSZ[2002, Table 3]</td>
</tr>
<tr>
<td>Informational efficiency:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>h=-1</td>
<td>-0.012</td>
<td>0.00</td>
<td>HSZ[2003, Table 2]</td>
</tr>
<tr>
<td>h=-2</td>
<td>-0.012</td>
<td>0.00</td>
<td>HSZ[2003, Table 2]</td>
</tr>
<tr>
<td>h=-5</td>
<td>-0.011</td>
<td>0.00</td>
<td>HSZ[2003, Table 2]</td>
</tr>
<tr>
<td>h=-10</td>
<td>-0.016</td>
<td>0.00</td>
<td>HSZ[2003, Table 2]</td>
</tr>
<tr>
<td>h=-30</td>
<td>-0.053</td>
<td>0.00</td>
<td>HSZ[2003, Table 2]</td>
</tr>
<tr>
<td>Abnormal trading volume</td>
<td>0.3104</td>
<td>6/6</td>
<td>BLMZ[2003, Table III]</td>
</tr>
</tbody>
</table>

Panel B: Results found for our sample of US firms

<table>
<thead>
<tr>
<th>PostRegFD</th>
<th>coef. est.</th>
<th>p-value</th>
<th>Source</th>
</tr>
</thead>
<tbody>
<tr>
<td>Event returns volatility</td>
<td>-0.0013</td>
<td>0.000</td>
<td></td>
</tr>
<tr>
<td>General returns volatility</td>
<td>-0.0289</td>
<td>0.000</td>
<td></td>
</tr>
<tr>
<td>Informational efficiency:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>h=-1</td>
<td>-0.0053</td>
<td>0.000</td>
<td></td>
</tr>
<tr>
<td>h=-2</td>
<td>-0.0062</td>
<td>0.000</td>
<td></td>
</tr>
<tr>
<td>h=-5</td>
<td>-0.0072</td>
<td>0.000</td>
<td></td>
</tr>
<tr>
<td>h=-10</td>
<td>-0.0086</td>
<td>0.000</td>
<td></td>
</tr>
<tr>
<td>h=-30</td>
<td>-0.0149</td>
<td>0.000</td>
<td></td>
</tr>
<tr>
<td>Abnormal trading volume</td>
<td>0.1767</td>
<td>0.000</td>
<td></td>
</tr>
</tbody>
</table>

Analyst information metrics:

<table>
<thead>
<tr>
<th>PostRegFD</th>
<th>coef. est.</th>
<th>p-value</th>
<th>Source</th>
</tr>
</thead>
<tbody>
<tr>
<td>Forecast dispersion (univariate test)</td>
<td>0.243</td>
<td>0.000</td>
<td></td>
</tr>
<tr>
<td>Forecast dispersion (multivariate test)</td>
<td>-0.068</td>
<td>0.24</td>
<td></td>
</tr>
<tr>
<td>Forecast accuracy (univariate test)&lt;sup&gt;b&lt;/sup&gt;</td>
<td>0.0009</td>
<td>0.000</td>
<td></td>
</tr>
<tr>
<td>Forecast accuracy (multivariate test)&lt;sup&gt;b&lt;/sup&gt;</td>
<td>0.0002</td>
<td>0.12</td>
<td></td>
</tr>
<tr>
<td>Report newsworthiness</td>
<td>-0.0172</td>
<td>0.05</td>
<td>GM[2004, Table 5]</td>
</tr>
</tbody>
</table>
Panel C: Results comparing changes in pre- versus post Reg FD effects for US versus ADR firms

<table>
<thead>
<tr>
<th>Public information metrics:</th>
<th>PostRegFD</th>
<th>US</th>
<th>PostRegFD*US</th>
</tr>
</thead>
<tbody>
<tr>
<td>Event returns volatility</td>
<td>-0.0007</td>
<td>0.220</td>
<td>0.0016</td>
</tr>
<tr>
<td>General returns volatility</td>
<td>-0.0229</td>
<td>0.000</td>
<td>0.0000</td>
</tr>
<tr>
<td>Informational efficiency</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$h=-1$</td>
<td>-0.0062</td>
<td>0.014</td>
<td>-0.0008</td>
</tr>
<tr>
<td>$h=-2$</td>
<td>-0.0076</td>
<td>0.004</td>
<td>-0.0019</td>
</tr>
<tr>
<td>$h=-5$</td>
<td>-0.0079</td>
<td>0.008</td>
<td>-0.0020</td>
</tr>
<tr>
<td>$h=-10$</td>
<td>-0.0090</td>
<td>0.010</td>
<td>-0.0018</td>
</tr>
<tr>
<td>$h=-30$</td>
<td>-0.0213</td>
<td>0.000</td>
<td>-0.0016</td>
</tr>
<tr>
<td>Abnormal trading volume</td>
<td>0.1338</td>
<td>0.011</td>
<td>-0.0243</td>
</tr>
</tbody>
</table>

| Analyst information metrics: |           |     |           |       |
| Forecast dispersion (univariate test) | 0.6517    | 0.022 | -0.1421  | 0.488 | -0.3301 | 0.251 |
| Forecast dispersion (multivariate test) | 0.1783    | 0.498 | -0.0456  | 0.810 | -0.2180 | 0.413 |
| Forecast accuracy (univariate test) | 0.0076    | 0.000 | -0.0015  | 0.335 | 0.0002  | 0.937 |
| Forecast accuracy (multivariate test) | 0.0007    | 0.519 | 0.0006   | 0.533 | -0.0015 | 0.235 |
| Report newsworthiness        | 0.0446    | 0.048 | 0.0519   | 0.004 | -0.0697 | 0.005 |

Table 1 describes the sample for the public information metrics. For the analyst information metrics, the samples are: Forecast dispersion sample: 2,272 US firms (19,382 quarters) and 69 ADR firms (474 quarters); Forecast accuracy sample: 4,312 analyst-firm-quarter observations (836 ADR and 3,476 US); Report newsworthiness sample: 2,927 US firms and 126 ADR firms. PostRegFD = 1 if quarter $q$ follows the implementation date of Reg FD, 0 otherwise; US = 1 if firm $i$ is a US firm, 0 if it is an ADR. Event returns volatility = $\sum_{t=1}^{n} AR_{t,j}^2$, where $AR_{t,j}^2$ = firm $i$’s squared abnormal return on day $t$. General returns volatility = the sum of the squared value of daily abnormal returns over the interval beginning three days after the announcement of quarter $q-1$ earnings and ending two days after the announcement of quarter $q$ earnings. Informational efficiency = the absolute cumulative abnormal returns over $h$ days prior to the earnings announcement, $h \in [-1,-2,-5,-10,-30]$. Abnormal trading volume = the difference between the average daily trading volume over days (-1,+1) relative to the announcement date and the average daily volume for that stock over days (-200,-11), normalized by average volume. Forecast dispersion = the standard deviation of all earnings-per-share forecasts, related to next fiscal quarter earnings, that are made in calendar quarter, $q-1$, scaled by the firm’s stock price at the end of calendar quarter $q-1$. Forecast accuracy = the absolute value of the difference between firm $i$’s actual earnings for quarter $q$ and the mean value of analyst $k$’s forecasts (made in the prior calendar quarter), scaled by the stock price ten trading days before forecast releases. Report newsworthiness = daily slope coefficient on analyst forecast dates, where the slope coefficient is based on a regression of the absolute value of firm $i$’s standardized return on day $t$ on an indicator variable that equals one for firm $i$ on day $t$ if an analyst report is announced in the interval of day $t$ to $t+5$, and zero otherwise.

Panel A describes results from prior studies, Panel B describes results from our sample of US firms, and Panel C describes results from our sample of US and ADR firms.

HSZ [2002,2003] multiply their metrics by 1000. For comparison with our calculations, we divide their reported results by 1000.
Table 3
Comparison of Changes in Public Information and Analyst Metrics Around Reg FD
For Matched Samples of US and ADR Firms

Panel A: Results found for our sample of US firms

<table>
<thead>
<tr>
<th>Capital market metrics:</th>
<th>PostRegFD (US firms)</th>
<th>PostRegFD (ADR firms)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>coef. est.</td>
<td>p-value.</td>
</tr>
<tr>
<td>Event returns volatility</td>
<td>-0.0017</td>
<td>0.000</td>
</tr>
<tr>
<td>General returns volatility</td>
<td>-0.0258</td>
<td>0.000</td>
</tr>
<tr>
<td>Informational efficiency</td>
<td></td>
<td></td>
</tr>
<tr>
<td>$h=-1$</td>
<td>-0.0029</td>
<td>0.242</td>
</tr>
<tr>
<td>$h=-2$</td>
<td>-0.0035</td>
<td>0.169</td>
</tr>
<tr>
<td>$h=-5$</td>
<td>-0.0062</td>
<td>0.062</td>
</tr>
<tr>
<td>$h=-10$</td>
<td>-0.0068</td>
<td>0.081</td>
</tr>
<tr>
<td>$h=-30$</td>
<td>-0.0155</td>
<td>0.000</td>
</tr>
<tr>
<td>Abnormal trading volume</td>
<td>0.0821</td>
<td>0.221</td>
</tr>
</tbody>
</table>

Panel B: Results comparing changes in pre- versus post Reg FD effects for US versus ADR firms

<table>
<thead>
<tr>
<th>Capital market metrics:</th>
<th>PostRegFD</th>
<th>US</th>
<th>PostRegFD*US</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>coef. est.</td>
<td>p-value.</td>
<td>coef. est.</td>
</tr>
<tr>
<td>Event returns volatility</td>
<td>-0.0007</td>
<td>0.075</td>
<td>0.2612</td>
</tr>
<tr>
<td>General returns volatility</td>
<td>-0.0194</td>
<td>0.000</td>
<td>-0.0023</td>
</tr>
<tr>
<td>Informational efficiency</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$h=-1$</td>
<td>-0.0059</td>
<td>0.009</td>
<td>-0.0037</td>
</tr>
<tr>
<td>$h=-2$</td>
<td>-0.0068</td>
<td>0.005</td>
<td>-0.0044</td>
</tr>
<tr>
<td>$h=-5$</td>
<td>-0.0034</td>
<td>0.355</td>
<td>-0.0033</td>
</tr>
<tr>
<td>$h=-10$</td>
<td>-0.0065</td>
<td>0.139</td>
<td>-0.0047</td>
</tr>
<tr>
<td>$h=-30$</td>
<td>-0.0227</td>
<td>0.000</td>
<td>-0.0063</td>
</tr>
<tr>
<td>Abnormal trading volume</td>
<td>0.0610</td>
<td>0.341</td>
<td>-0.0444</td>
</tr>
</tbody>
</table>

For the public information metrics, the sample consists of 370 pairs of US-ADR firms with 2,602 quarters. The forecast dispersion sample has 34 US firms (232 quarters) and 34 ADR firms (232 quarters). Forecast accuracy sample has 944 analyst-firm-quarters (102 analyst-pairings). Report newsworthiness sample includes 95 matched US-ADR firm pairs. All variables are as defined in Table 2.

*a Panel A describes results for our sample of US firms and ADR firms (separately), and Panel B describes results for the combined sample of matched US and ADR firms.
References


