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The Effect of Mandatory IFRS Adoption on Financial Analysts' Information Environment

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ABSTRACT

This paper examines the effect of the mandatory adoption of International Financial Reporting Standards (IFRS) by the European Union on financial analysts' information environment. To control for the effect of confounding concurrent events, we use a control sample of firms that had already voluntarily adopted IFRS at least two years prior to the mandatory adoption date. We find that analysts' absolute forecast errors and forecast dispersion decrease relative to this control sample only for those mandatory IFRS adopters domiciled in countries with both strong enforcement regimes and domestic accounting standards that differ significantly from IFRS. Furthermore, for mandatory adopters domiciled in countries with both weak enforcement regimes and domestic accounting standards that differ significantly from IFRS, we find

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that forecast errors and dispersion decrease more for firms with stronger incentives for transparent financial reporting. These results highlight the important roles of enforcement regimes and firm-level reporting incentives in determining the impact of mandatory IFRS adoption.

1. Introduction

The adoption of International Financial Reporting Standards (IFRS) in Europe and around the world represents perhaps the most important accounting regulatory change in recent years. In 2002, the European Union (EU) required all firms listed in its 27 member countries to switch to IFRS for financial reporting purposes for fiscal years beginning on or after January 1, 2005. This move forced over 7,000 public companies to replace various domestic accounting standards with IFRS. Despite the widespread mandatory adoption of IFRS, there is relatively little evidence as to the related economic consequences (see Ball [2006], Daske et al. [2008]).

This study examines the effect of the EU-wide mandatory adoption of IFRS on financial analysts' information environment. Ex ante, it is unclear how mandatory IFRS adoption would affect analysts' information environment. On the one hand, IFRS adoption may improve analysts' information environment by enhancing disclosure and transparency or by increasing the comparability of financial reports (e.g., Bae, Tan, and Welker [2008], Barth, Landsman, and Lang [2008]). On the other hand, if the "one size fits all" IFRS are suboptimal relative to firms' domestic accounting standards in reflecting firm performance, IFRS adoption may render financial reporting less informative, thereby reducing the quality of analysts' information. In addition, earnings may become more volatile and thus more difficult for analysts to forecast under IFRS (Ball [2006]). However, it is also possible that mandatory IFRS adoption may have little impact on analysts' information environment. If mandatory adopters already optimize their financial reporting strategies under their domestic standards, they may lack incentives to rigorously implement IFRS. This lack of incentive can be further exacerbated by the weak enforcement regimes in many EU countries. As a result, firms may adopt IFRS in name only and make little change to their financial reporting practices. Further, even if IFRS are properly implemented, the adoption effect should be small in EU countries with domestic accounting standards similar to IFRS (Ashbaugh and Pincus [2001]).

We employ a difference-in-difference research design to examine the impact of mandatory IFRS adoption. Our test sample includes 1,168 mandatory IFRS adopters in Europe. To control for the effect of concurrent confounding events such as the EU's ongoing efforts to strengthen the enforcement of accounting standards (Hail and Leuz [2007]), we identify a control sample of 250 European firms that had already voluntarily adopted IFRS at least two years prior to the mandatory adoption date. Because these control firms use IFRS both before and after mandatory adoption, changes for these firms likely reflect the impact of concurrent economic and regulatory

changes, but not mandatory IFRS adoption. To test the impact of mandatory IFRS adoption, we focus on the difference between the test and control firms in the change in analysts' information environment from the two-year *pre*-mandatory-adoption period to the two-year *post*-mandatory-adoption period. We capture the properties of analysts' information environment using absolute forecast errors, forecast dispersion, and analyst following, where smaller absolute forecast errors, smaller forecast dispersion, or higher analyst following generally indicates a richer information environment.

We perform three levels of analysis: (1) an examination of the average effect of mandatory IFRS adoption, (2) an examination conditioned on country-level institutional characteristics, and (3) an examination conditioned on firm-level reporting incentives. In the first analysis, we find that mandatory adopters exhibit no statistically significant change relative to the control firms in analysts' forecast errors, forecast dispersion, or analyst following, thus providing no evidence that mandatory IFRS adoption changes analysts' information environment on average. In the second analysis, we partition mandatory adopters into groups based on the strength of country-level enforcement regimes and the extent to which their domestic accounting standards differ from IFRS. For mandatory adopters domiciled in countries with both strong enforcement regimes and domestic accounting standards that differ substantially from IFRS, we find statistically and economically significant decreases relative to the control firms in both forecast errors and dispersion. In contrast, we find no statistically significant differences between the control firms and the mandatory adopters based in other countries. In the third analysis, we explore how firm-level incentives for transparent financial reporting affect the adoption effect. To increase the power of our test, we focus on mandatory adopters domiciled in countries with both weak enforcement environments and domestic accounting standards that differ significantly from IFRS. For such firms, the changes required by IFRS adoption are substantial, and weak country-level enforcement allows firm-level reporting incentives to play a relatively greater role in shaping reporting outcomes (Daske et al. [2008], Holthausen [2009]). We find larger decreases in analysts' forecast errors and dispersion for firms with stronger reporting incentives (i.e., more growth opportunities, a smaller proportion of closely held shares, or higher-quality auditors). In addition to these three analyses, further tests suggest that mandatory IFRS adoption improves analysts' information environment by increasing both analysts' public and private information. Taken together, our results suggest that the adoption effect is uneven across countries and firms, and that analysts' information environment improves only when the changes mandated by IFRS are both substantial and rigorously enforced.

Our findings highlight that simply mandating IFRS is not sufficient to ensure actual changes to firms' financial reporting practices. Our study may be of interest to policy makers in evaluating the costs and benefits of mandatory IFRS adoption and to financial statement users who wish to understand the effect of mandatory IFRS adoption.

Two caveats are in order. First, our control sample has some limitations. Our results would understate the adoption effect if the changes for the control firms are attributable to either concurrent changes in IFRS requirements or positive externalities arising from mandatory IFRS adoption. On the other hand, if the control sample does not fully control for contemporaneous changes unrelated to mandatory IFRS adoption (e.g., due to some fundamental differences between the test and control samples), our results would overstate the adoption effect. Second, we examine only two years following the adoption, so our results may not shed light on the long-term adoption effect. It is possible that in the long run, IFRS adoption may result in an across-the-board improvement for all firms.

2. *Hypotheses*

Our first hypothesis, stated in the null form, is:

> *H1*: On average, the EU-wide mandatory adoption of IFRS is not associated with a change in the quality of analysts' information environment.

We next exploit cross-country differences to identify countries where the adoption effect is likely to be greater. The adoption effect, if any, is likely to be more pronounced for mandatory adopters domiciled in countries with both strong enforcement regimes and domestic accounting standards that differ substantially from IFRS.[1] For these firms, mandatory IFRS adoption requires substantial accounting changes, while strong enforcement forces firms to comply with these changes. Our second hypothesis, stated in the null form, is thus:

> *H2*: In countries with both strong enforcement regimes and domestic accounting standards that differ significantly from IFRS, the EU-wide mandatory adoption of IFRS is not associated with a change in the quality of analysts' information environment.

Holding country-level enforcement constant, the adoption effect likely varies with firm-level incentives for transparent financial reporting. If IFRS adoption improves financial reporting, we expect firms with stronger reporting incentives to implement IFRS more rigorously, leading to a greater improvement in analysts' information environment for these firms. Conversely, if IFRS adoption does not improve financial reporting, we expect no relation between firm-level reporting incentives and the change in analysts' information environment. Therefore, our third hypothesis examines how the adoption effect varies with firm-level reporting incentives. Because firms' reporting incentives likely play a more prominent role in determining reporting outcomes when country-level enforcement is weak and the

[1] However, as long as there is discretion in financial reporting and firms have differing reporting incentives, strong enforcement will not eliminate all variation in reporting quality across firms (Leuz [2006]).

changes required by IFRS are larger, we focus on firms domiciled in countries with both weak enforcement and domestic accounting standards that differ significantly from IFRS. Our third hypothesis, stated in the null form, is thus:

> *H3*: For firms domiciled in countries with both weak enforcement and domestic accounting standards that differ significantly from IFRS, the impact of the EU-wide mandatory adoption of IFRS on analysts' information environment is not related to firm-level reporting incentives.

3. Sample Selection and Study Design

3.1. SAMPLE SELECTION

We obtain analysts' forecast data from the IBES international (split unadjusted) database and accounting and market data from the Global Compustat or Datastream/Worldscope database. European firms subject to mandatory IFRS adoption were required to switch to IFRS for financial reporting purposes for fiscal years beginning on or after January 1, 2005. Since we study the change in analysts' information environment after mandatory IFRS adoption, we divide the sample period into two reporting regimes: the *pre*-mandatory-adoption window is the last two fiscal years a firm reported using its domestic accounting standards, while the *post*-mandatory-adoption window is the first two fiscal years a firm reported using IFRS. For example, for a December year-end firm, the *pre*-mandatory-adoption window is 2003 and 2004, while the *post*-mandatory-adoption window is 2005 and 2006.

We begin our sample-selection process by selecting from I/B/E/S all public companies domiciled in Europe with analysts' annual earnings forecasts for our four-year sample period.[2] We exclude firms that were cross-listed in the United States as American Deposit Receipts (ADRs) during our sample period based on the ADR databases of the Bank of New York and Citibank. We also exclude firms that changed their country of domicile or primary exchange listing during our sample period. We require that annual earnings announcements be made no later than 240 calendar days after the fiscal year-end. For each firm-year, we retrieve all annual earnings forecasts issued during the 12-month period prior to the year-end from each analyst covering the firm. If an analyst issues more than one annual forecast for a firm-year, we retain only the last forecast. We further require each analyst to issue at least one annual forecast for a firm within both the *pre*- and *post*-mandatory-adoption windows. Thus, our sample is composed of a constant set of analyst-firm pairs matched across the *pre*- and *post*-mandatory-adoption windows. This alleviates potential concerns that any changes in analysts' information environment are driven by changes in sample

[2] Our sample includes two non-EU-member countries (Norway and Switzerland) that also mandated IFRS adoption. Our results are robust to excluding these two countries.

composition, as both firm and analyst characteristics can affect properties of analysts' forecasts.

Next, to identify mandatory IFRS adopters, we obtain information on firms' accounting standards from both Global Compustat and Datastream/Worldscope, and retain firms that reported under their domestic accounting standards during the two-year *pre*-mandatory-adoption window. When Global Compustat and Datastream/Worldscope provide conflicting information as to which accounting standards a firm used, we verify data from the firm's annual reports. Finally, we exclude firms with missing accounting or market data from both Global Compustat and Datastream/Worldscope that are needed to construct the variables we use in our analyses (discussed below). Our final test sample contains 1,168 firms.

To control for the impact of potentially confounding concurrent events, we select a control sample of European firms that voluntarily adopted IFRS at least two years before the mandatory adoption deadline. We also require the control sample to have a constant analyst-firm composition over the *pre*- and *post*-mandatory-adoption windows. For each voluntary adopter, we define the *pre*-mandatory-adoption window as the last two fiscal years ending before mandatory adoption, and the *post*-mandatory-adoption window as the first two fiscal years beginning on or after January 1, 2005, the mandatory adoption date. The two windows for the control sample are thus concurrent with the windows for the mandatory adopters. All other data requirements for the control sample are the same as those for the test sample. The control sample contains 250 firms.[3]

The control firms use IFRS during both windows, while the test firms switch accounting standards between the two windows. To the extent that IFRS requirements are unchanged around the mandatory adoption, the change for the control firms likely reflects the impact of concurrent economic and regulatory changes, but not the impact of mandatory IFRS adoption. To identify the impact of mandatory IFRS adoption, we therefore focus on the difference in the change from the *pre*- to *post*-mandatory-adoption window between the test and control firms.

The first two columns of table 1, panel A, provide the distributions of our test and control samples by country. The test (control) sample represents firms from 20 (15) different countries, with heavy concentrations in the United Kingdom and France (Germany and Switzerland).[4] We note that the country distributions of our test and control samples differ substantially, so we later explore the robustness of our results to this feature of the data.

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[3] December year-end firms account for 75% (88%) of the test (control) sample.

[4] Our control sample includes firms domiciled in European countries (i.e., Hungary, France, Poland, and Sweden) that did not allow voluntary IFRS adoption for domestic listed firms. We inspected the annual reports of these firms to confirm that these firms voluntarily released IFRS financial statements in addition to the required domestic GAAP financial statements in the *pre*-mandatory-adoption period. Our results are, however, robust to removing these firms from our control sample. In addition, our inferences remain unchanged if we exclude UK firms from both the test and control samples.

TABLE 1
Sample Description

Panel A: Distributions of the Test and Control Firms by Country

Country	Test Sample (no. of firms) (1)	Control Sample (no. of firms) (2)	Law Enforcement (*LAW*) (3)	Difference b/w Domestic Accounting Standards and IFRS (*IFRSdiff*) (4)
Austria	1	15	1.8	12
Belgium	36	10	1.4	13
Czech Republic	1	2	0.7	14
Denmark	34	9	1.9	11
Finland	51	6	1.9	15
France	176	21	1.3	12
Germany	21	99	1.7	11
Greece	34	3	0.7	17
Hungary	1	7	0.7	13
Ireland	24		1.6	1
Italy	96		0.5	12
Luxembourg	1	4	1.9	18
Netherlands	54		1.7	4
Norway	59		1.9	7
Poland	7	4	0.3	12
Portugal	13	1	1.1	13
Spain	69		1.1	16
Sweden	70	1	1.8	10
Switzerland	21	67	2.0	12
United Kingdom (Includes Channel Islands)	399	1	1.6	1
Total Number of Firms:	1,168	250	Median: 1.6	11

Panel B: Distributions of the Test and Control Firms by Country-Level Institutional Characteristics: Partitioning on the Median Values of *LAW* and *IFRSdiff*

	High LAW (i.e., *LAW* $\geq$ 1.6)	*Low LAW* (i.e., *LAW* $<$ 1.6)
High IFRSdiff (i.e., *IFRSdiff* $\geq$ 11)	Austria, Denmark, Finland, Germany, Luxembourg, and Switzerland No. of Test Firms: 129 No. of Control Firms: 200	Belgium, Czech Republic, France, Greece, Hungary, Italy, Poland, Portugal, and Spain No. of Test Firms: 433 No. of Control Firms: 48
Low IFRSdiff (i.e., *IFRSdiff* $<$ 11)	Ireland, the Netherlands, Norway, Sweden, and the United Kingdom No. of Test Firms: 606 No. of Control Firms: 2	None

Panel A shows the distributions of the test and control firms across countries. The test sample consists of 1,168 European firms that switched from their domestic accounting standards to IFRS as a result of the European Union (EU) -wide mandatory adoption of IFRS effective for fiscal years beginning on or after January 1, 2005. The control sample consists of 250 European firms that voluntarily adopted IFRS at least two years prior to the mandatory adoption deadline. We identify our test and control firms based on firms' accounting standards retrieved from the Global Compustat and Datastream/Worldscope databases. When these two databases provide conflicting information on accounting standards for a firm, we hand-check the firm's annual reports.

(*Continued*)

TABLE 1 — *Continued*

Panel B shows the partition of our sample firms into groups based upon two country-level institutional variables: *LAW* and *IFRSdiff*. *LAW* is the measure of the quality of a country's legal and enforcement environment for 2005 from Kaufmann, Kraay, and Mastuzzi [2007]. Higher values of *LAW* represent stronger legal and enforcement regimes. *IFRSdiff* is the GAAP difference measure from Bae, Tan, and Welker [2008, table 1]. *IFRSdiff* measures differences between countries' domestic accounting standards and IFRS along 21 key accounting items; higher values of *IFRSdiff* indicate greater differences. We partition countries based on the median values of *IFRSdiff* and *LAW*, which are 11 and 1.6, respectively. This partitioning scheme results in three country groups: (1) a *High LAW-High IFRSdiff* group; (2) a *Low LAW-High IFRSdiff* group; and (3) a *High LAW-Low IFRSdiff* group. Note that no country has below-median values of both *LAW* and *IFRSdiff*.

3.2. MEASURING THE QUALITY OF ANALYSTS' INFORMATION ENVIRONMENT

We use three measures to capture analysts' information environment for a firm: absolute forecast errors, forecast dispersion, and the number of analysts following a firm. We focus on these three proxies because they are more direct measures of the impact of mandatory IFRS adoption on analysts' information environment than other analyst-related measures (e.g., stock recommendations, forecast revisions, and forecast frequency). For example, analysts' stock recommendations reflect both accounting and nonaccounting information, and there is no clear benchmark to assess the accuracy of stock recommendations. Price reactions to stock recommendations or forecast revisions can be difficult to interpret, because if IFRS adoption enhances firm disclosures, this could result in an increase or a decrease in market reactions, depending on whether more disclosures primarily crowd out analysts' private information or help analysts develop more unique insights. Similarly, because forecast timeliness reflects the amount of analysts' private information and forecast frequency captures analysts' effort to generate private information, it is debatable whether an increase in forecast frequency or timeliness indicates an improvement or a deterioration in analysts' information environment.

For each firm, we calculate the changes in the three proxies for analysts' information environment between the *pre-* and *post*-mandatory-adoption windows. We obtain the change in analysts' absolute forecast error (ΔAFE) by first calculating analysts' absolute forecast error (AFE_{it}) for each firm-year as: $|Actual\ Earnings_{it} - Median\ Forecast_{it}|/Stock\ Price_{it}$, where $Actual\ Earnings_{it}$ is the actual I/B/E/S annual EPS for firm i in year t, $Median\ Forecast_{it}$ is the median of forecasts made by analysts in our sample during the 12-month period prior to the fiscal year-end for firm i and year t, and $Stock\ Price_{it}$ is the stock price of firm i at the beginning of year t.[5] We then compute each firm's average *AFE* for the *pre-* and *post*-mandatory-adoption windows, respectively, and calculate the difference as ΔAFE. We similarly compute the change in forecast dispersion ($\Delta DISP$). For each firm-year, we first calculate forecast dispersion as: *Standard Deviation of Forecasts/Stock*

[5] The results are similar using the mean forecast rather than the median forecast. We winsorize continuous variables at the 1% and 99% levels to mitigate the influence of outliers.

Price. We then calculate $\Delta DISP$ as the difference between the averages of *DISP* for the *pre*-and *post*-mandatory-adoption windows. For each firm-year, we measure analyst following (*FOLLOW*) as the total number of analysts who issue at least one annual forecast for that firm-year. Note that we include all analysts following a firm, not just the analysts included in our constant analyst-firm sample. For each firm, we then calculate the change in analyst following, $\Delta LogFOLLOW$, as the difference in the log of average analyst following between the *pre*- and *post*-mandatory-adoption windows.[6] Because a minimum of two analysts' forecasts in each firm-year is needed to measure dispersion, the sample size is slightly smaller for $\Delta DISP$ than for ΔAFE and $\Delta LogFOLLOW$. For ease of exposition, we multiply both ΔAFE and $\Delta DISP$ by 100.

3.3. TESTING THE AVERAGE EFFECT OF MANDATORY IFRS ADOPTION (H1)

To test H1, we estimate the following model[7]

$$\Delta Info.\ Environment_i = \alpha_0 + a_1 D_{MANDi} + \alpha_j \Delta Controls_i + \varepsilon_i, \qquad (1)$$

where the subscript i refers to firm i. The dummy variable D_{MAND} equals one (zero) for the test (control) firms, and $\Delta Info.\ Environment$ refers to ΔAFE, $\Delta DISP$, or $\Delta LogFOLLOW$. The intercept, α_0, captures the change around mandatory IFRS adoption for the control firms. The key coefficient of interest, α_1, captures the difference in the change in analysts' information environment between the test and control firms and, thus, the impact of mandatory IFRS adoption.

We control for changes in firm size, analyst following, and forecast horizon (i.e., the number of days between the forecast issuing date and the earnings announcement date) in the ΔAFE and $\Delta DISP$ regressions.[8] Larger firms and firms with greater analyst following tend to have smaller forecast errors and forecast dispersion (Bhushan [1989]); forecast errors and dispersion also decrease as forecast horizon shortens. We control for the change in firm size in the $\Delta logFOLLOW$ regression, because larger firms tend to attract a larger analyst following (Bhushan [1989]). To measure the change in firm size ($\Delta logSIZE$), we first calculate the beginning-of-the-year market capitalization in U.S. dollars for each firm-year using the exchange rate in effect at the beginning of the year from I/B/E/S. We then compute

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[6] We use a log transformation to reduce the skewness in *FOLLOW*. Our results are robust to reestimating the change in analyst following using an ordered-probit model.

[7] The results are robust to allowing the coefficients on the control variables to vary between the test and control firms.

[8] Our results are robust to including three additional control variables used by Lang and Lundholm [1996]: the standard deviation of return on assets, the return-earnings correlation, and earnings surprise based on a random-walk model. We do not include these variables in our reported results because the first two require a long time-series of data to compute, and earnings surprise constructed based on actual earnings is mechanically associated with forecast errors (Hope[2003]). In addition, our results are also robust to controlling for the change in firm risk, measured by the standard deviation of monthly stock returns.

the difference in the log of average market capitalization between the *pre*- and the *post*-mandatory-adoption windows.[9] Since multiple forecasts may exist for a firm-year, we first compute the average horizon (i.e., the number of days between the forecast issuing date and the earnings announcement date) across all forecasts for that firm-year. Then for each firm, we compute $\Delta logHORIZON$ as the change in the log of average horizon between the *pre*- and *post*-mandatory-adoption windows.

3.4. TESTS CONDITIONAL ON COUNTRY-LEVEL INSTITUTIONAL CHARACTERISTICS (H2)

We use the "Rule of Law" (*LAW*) variable for 2005 from Kaufmann, Kraay, and Mastuzzi [2007] as a proxy for the quality of countries' legal and enforcement regimes. Higher values of *LAW* indicate stronger enforcement. We base our measure of the difference between a country's accounting standards and IFRS on the *gaapdiff1* measure of Bae, Tan, and Welker[2008, table 1]. This is a comprehensive measure that captures differences between domestic accounting standards and IFRS along 21 key accounting items. We relabel this measure *IFRSdiff* to emphasize that it measures differences between domestic accounting standards and IFRS. Higher values of *IFRSdiff* indicate greater difference. Columns (3) and (4) of table 1, panel A, show the *LAW* and *IFRSdiff* scores for each country in our sample.

We classify sample firms into country groups based on the median values of *LAW* (1.6) and *IFRSdiff* (11) as follows:

(1) a *High LAW-High IFRSdiff* group (*LAW* $\geq$ 1.6 and *IFRSdiff* $\geq$ 11),
(2) a *Low LAW-High IFRSdiff* group (*LAW* $\leq$ 1.6 and *IFRSdiff* $\geq$ 11), and
(3) a *High LAW-Low IFRSdiff* group (*LAW* $\geq$ 1.6 and *IFRSdiff* $\leq$ 11).

No country is below the median values of both *LAW* and *IFRSdiff*. Panel B of table 1 shows the distributions of the test and control firms across these three country groups. The numbers of test and control firms are highly unbalanced within the *High LAW-Low IFRSdiff* and the *Low LAW-High IFRSdiff* groups: in the former (latter) group, there are only 2 (48) control firms, far fewer than the 606 (433) test firms. Hence, the distribution of the data restricts meaningful comparisons within each group. As a result, we compare mandatory adopters in each of the three country groups with the control firms pooled across all countries using the following equation:

$$\begin{aligned}\Delta Info.\ Environment_i = {} & \beta_0 + \beta_1 D_{MANDi} * D_{HLAW-HDIFFi} \\ & + \beta_2 D_{MANDi} * D_{LLAW-HDIFFi} + \beta_3 D_{MANDi} * D_{HLAW-LDIFFi} \\ & + \beta_j \Delta Controls_i + \varepsilon_i, \end{aligned} \quad (2)$$

[9] We measure firm size by market value of equity because financial analysts mainly serve equity investors. However, the results are robust to measuring firm size by enterprise value (defined as the sum of market value of equity and book value of debt).

where the subscript i refers to firm i. Δ*Info. Environment* refers to Δ*AFE,* Δ*DISP,* or Δ*LogFOLLOW*. $D_{HLAW-HDIFF}$ is a dummy variable coded one if a test or control firm is from the *High LAW-High IFRSdiff* country group; similarly, $D_{LLAW-HDIFF}$ and $D_{HLAW-LDIFF}$ are dummy variables indicating test or control firms from the *Low LAW-High IFRSdiff* and *High LAW-Low IFRSdiff* groups. The controls are the same as in equation (1). The intercept, β_0, reflects the change in analysts' information environment for the control firms. The coefficient of interest, β_1, reflects the difference between the test firms in the *High LAW-High IFRSdiff* group and the control firms. Similarly, β_2 (β_3) capture the difference between the test firms in the *Low LAW-High IFRSdiff* (*High LAW-Low IFRSdiff*) group and the control firms.

3.5. TESTS OF FIRM-LEVEL REPORTING INCENTIVES (H3)

Prior research (e.g., Ashbaugh [2001], Pagano et al. [2002], Leuz [2006], Christensen, Lee, and Walker [2007], Daske et al.[2007], Barth, Landsman, and Lang [2008]) suggests that firms that: (1) are more profitable, (2) have more growth opportunities, (3) are more highly leveraged, (4) have a less concentrated ownership structure, (5) are more international, and (6) have higher-quality auditors have stronger incentives to provide high-quality financial reporting. We measure these firm characteristics using accounting and market data from Global Compustat or Datastream/Worldscope. We measure profitability by return on assets (*ROA*), defined as net income/total assets; growth opportunities by Tobin's q (*TOBIN_Q*), defined as market value of assets/book value of assets, where the numerator is measured as (book value of assets + market value of common equity – book value of common equity – balance sheet deferred taxes). Financial leverage (*LEVERAGE*) is measured as total liabilities/total assets, ownership concentration is the percentage of closely held shares (*CLOSEHELD*) as reported by Datastream/Worldscope, and international diversification is the percentage of foreign sales (*FOREIGNSALES*) reported by Datastream/Worldscope. These variables are all measured as the mean values for the two-year *pre*-mandatory-adoption period. Finally, we use a dummy variable to indicate the presence of a big four auditor (*BIG4*) in the year immediately preceding mandatory IFRS adoption.

To test *H3*, we estimate the following model using mandatory adopters domiciled in countries with both weak enforcement regimes and domestic accounting standards that differ significantly from IFRS (i.e., the *Low LAW-High IFRSdiff* group)

$$\begin{aligned}\Delta Info.\ Environment_i = {} & \delta_0 + \delta_1 ROA_i + \delta_2 TOBIN\_Q_i + \delta_3 LEVERAGE_i \\ & + \delta_4 CLOSEHELD_i + \delta_5 FOREIGNSALES_i + \delta_6 BIG4_i \\ & + \delta_j \Delta Controls_i + \varepsilon_i, \end{aligned} \tag{3}$$

where the subscript i refers to mandatory adopter i. Δ*Info. Environment* refers to Δ*AFE,* Δ*DISP,* or Δ*LogFOLLOW*; the control variables are the same as in equations (1) and (2). We estimate this equation using only

test firms because our focus is on the cross-sectional variation in reporting incentives within the sample of mandatory adopters. As long as concurrent confounding events have the same effect on all mandatory adopters, this effect is cancelled out in this within-mandatory-sample comparison. If firms with stronger reporting incentives experience a greater improvement in analysts' information environment, we expect the coefficients on *ROA*, *TOBIN_Q*, *LEVERAGE*, *FOREIGNSALE*, and *BIG4* to be negative for the ΔAFE and $\Delta DISP$ regressions, and positive for the $\Delta LogFOLLOW$ regression; additionally, we expect the coefficient on *CLOSEHELD* to be positive for the ΔAFE and $\Delta DISP$ regressions, and negative for the $\Delta LogFOLLOW$ regression.

4. Results

4.1. DESCRIPTIVE STATISTICS

Table 2 provides descriptive statistics for the test and control samples, and presents univariate comparisons across the two samples to explore potential differences. For the test sample, the means (medians) of ΔAFE, $\Delta DISP$, and $\Delta LogFOLLOW$ are -0.79 (-0.43), -0.28 (-0.18), and 0.13 (0.09), respectively; these changes are all statistically significant (two-tailed $p \leq 0.01$). The reductions in analysts' forecast errors and dispersion and increase in analyst following suggest an improvement in analysts' information environment. However, we also observe a similar improvement for the control sample: the means (medians) of ΔAFE, $\Delta DISP$, and $\Delta LogFOLLOW$ are -0.87 (-0.57), -0.26 (-0.15), and 0.10 (0.00), respectively, for the control firms; again, these changes are all statistically significant (two-tailed $p \leq 0.01$). More importantly, Column (9) shows that ΔAFE, $\Delta DISP$, and $\Delta LogFOLLOW$ are not statistically different between the test and control firms, except for the median $\Delta LogFOLLOW$, which is higher for the test sample (two-tailed $p = 0.03$). Thus, table 2 provides little evidence of an incremental change in analysts' information environment for the test firms relative to the control firms. However, because these univariate comparisons do not control for correlated factors that potentially influence the change in analysts' information environment (e.g., changes in firm size), the results only provide preliminary evidence on H1 and should be interpreted with caution.

The improvement in analysts' information environment for the control firms could reflect: (1) concurrent economic or regulatory shocks such as the EU's efforts to improve the enforcement of accounting standards, (2) IFRS changes between the *pre*- and *post*-mandatory-adoption windows, and/or (3) some positive externality—such as increased comparability—arising from mandatory IFRS adoption. It is difficult to distinguish among these effects in our setting. To the extent that the improvement for the control firms is attributable to either IFRS changes around mandatory adoption or to some positive externality arising from the adoption, examining the difference between the test and control firms may understate the adoption effect.

TABLE 2

Descriptive Statistics for the Test and Control Firms: Pre- and Post-Mandatory-Adoption Windows

	Test Firms: Mandatory IFRS Adopters (1,168 Firms)				*Control Firms: Voluntary IFRS Adopters (250 Firms)*				*p*-value of testing (3)–(7)	*p*-value of testing (1)–(5)
	Preperiod (1) Mean (Median) [STD]	Postperiod (2) Mean (Median) [STD]	Change (3) Mean (Median) [STD]	Sig. of Change (4) Two-tailed *p*-values: *t*-test Signrank	Preperiod (5) Mean (Median) [STD]	Postperiod (6) Mean (Median) [STD]	Change (7) Mean (Median) [STD]	Sig. of Change (8) Two-tailed *p*-values: *t*-test Signrank	(9) Two-tailed *p*-values: *t*-test Ranksum	(10) Two-tailed *p*-values: *t*-test Ranksum
AFE	3.00	2.24	−0.79	<0.01	2.97	2.10	−0.87	<0.01	0.55	0.92
	(1.49)	(0.75)	(−0.43)	<0.01	(1.68)	(0.72)	(−0.57)	<0.01	0.28	0.45
	[4.82]	[4.94]	[4.57]		[3.71]	[4.16]	[3.87]			
DISP	1.52	1.25	−0.28	<0.01	1.43	1.17	−0.26	0.01	0.85	0.52
	(0.93)	(0.64)	(−0.18)	<0.01	(0.84)	(0.61)	(−0.15)	<0.01	0.61	0.39
	[1.81]	[1.95]	[1.70]		[1.64]	[1.96]	[1.44]			
LogFOLLOW	1.92	2.05	0.13	<0.01	2.05	2.15	0.10	<0.01	0.39	<0.01
	(2.02)	(2.14)	(0.09)	<0.01	(2.14)	(2.20)	(0.00)	<0.01	0.03	0.10
	[0.93]	[0.89]	[0.47]		[1.05]	[0.91]	[0.42]			
LogSIZE	6.09	6.40	0.31	<0.01	6.36	6.83	0.48	<0.01	<0.01	<0.01
	(5.95)	(6.21)	(0.40)	<0.01	(6.13)	(6.57)	(0.47)	<0.01	<0.01	0.12
	[1.73]	[1.70]	[0.79]		[1.99]	[1.93]	[0.63]			
LogHORIZON	5.06	5.12	0.05	<0.01	5.06	5.14	0.08	<0.01	0.33	0.61
	(5.09)	(5.13)	(0.08)	<0.01	(5.05)	(5.14)	(0.11)	<0.01	0.35	0.92
	[0.31]	[0.30]	[0.39]		[0.27]	[0.23]	[0.32]			
ROA	0.03	0.04	0.02	<0.01	0.03	0.05	0.02	<0.01	0.25	0.62
	(0.04)	(0.05)	(0.01)	<0.01	(0.03)	(0.05)	(0.01)	<0.01	0.02	0.56
	[0.10]	[0.09]	[0.08]		[0.08]	[0.07]	[0.07]			
SALES GROWTH	0.08	0.11	0.03	<0.01	0.08	0.12	0.04	<0.01	0.31	0.78
	(0.05)	(0.08)	(0.04)	<0.01	(0.05)	(0.08)	(0.05)	<0.01	0.17	0.65
	[0.20]	[0.20]	[0.25]		[0.18]	[0.18]	[0.21]			
LEVERAGE	0.58	0.59	0.01	0.07	0.56	0.55	−0.01	0.11	0.05	0.10
	(0.59)	(0.61)	(0.00)	0.08	(0.57)	(0.56)	(−0.01)	0.02	0.01	0.07
	[0.19]	[0.18]	[0.11]		[0.17]	[0.17]	[0.08]			

This table provides summary statistics for the test and control firms. The test sample consists of 1,168 European firms that switched from their domestic accounting standards to IFRS as a result of the mandatory European Union (EU) -wide adoption of IFRS effective for fiscal years beginning on or after January 1, 2005. The control sample consists of 250 European firms that voluntarily adopted IFRS at least two years prior to the mandatory adoption deadline. Because a minimum of two analysts' forecasts is needed to calculate forecast dispersion (*DISP*), the sample for forecast dispersion is smaller, and consists of 879 (197) test (control) firms. For each (test or control) firm, the *pre*-mandatory-adoption window is the last two fiscal years prior to the mandatory adoption of IFRS, and the *post*-mandatory-adoption window is the first two years after the mandatory adoption of IFRS. *AFE* is the absolute forecast error, (*Actual Earnings-Median Forecast* | / *Stock Price*). *DISP* is the dispersion of analysts' forecasts, *(Standard Deviation of Forecasts / Stock Price)*. For ease of exposition, we multiply both *AFE* and *DISP* by 100. *FOLLOW* is the total number of analysts following a firm. *SIZE* is market capitalization (in U.S. $). *HORIZON* is the number of days between the forecast issuance date and the earnings announcement date. *ROA* is net income divided by total assets. *SALES GROWTH* is $(Sales_t - Sales_{t-1}) / Sales_{t-1}$. *LEVERAGE* is total liabilities divided by total assets. The summary statistics reported are for the average value of each variable in either the *pre*- or the *post*-mandatory-adoption window. For example, we first calculate *AFE* for each firm-year, and then compute each firm's average *AFE* for the *pre*- and *post*-mandatory-adoption windows, respectively, and then report summary statistics for the averages. We use a constant sample of matched individual analysts across the *pre*- and *post*-mandatory-adoption windows to calculate *AFE*, *DISP*, and *HORIZON*, but include all analysts following a firm when calculating *FOLLOW*. Analysts' forecast data are obtained from I/B/E/S, while financial and price data are from Global Compustat or Datastream/Worldscope. All variables are winsorized at the 1% and 99% levels to mitigate the influence of outliers.

In table 2, we also compare other financial and analyst variables across the test and control samples to gauge whether and how the two samples differ. We find a statistically significant increase in firm size and analysts' forecast horizon for both the test and control samples. Although the increase in analysts' forecast horizon is not statistically different across the two samples, the increase in firm size is statistically greater for the control firms. Both return on assets and sales growth increase for the test and control samples, but the increases are not statistically different across the two samples. We find that leverage increases for the test sample, but there is weak evidence that it decreases for the control sample. Column (10) compares the *pre*-mandatory-adoption levels of all these variables across the test and control samples: control firms tend to be larger and have higher analyst following and lower leverage, but differences between the two samples in forecast errors, dispersion, analysts' forecast horizons, sales growth, or firm performance are statistically insignificant.

Overall, the results in tables 1 and 2 indicate that our test and control firms differ in important ways, most prominently country of origin and firm size. Although our results are robust to comparing test and control firms within country groups and to controlling for various firm characteristics, we cannot rule out the possibility that some unknown difference between the test and control firms may cause them to react differently to confounding concurrent events, and, consequently, our control firms may not fully capture the impact of these events on our test firms.

4.2. THE AVERAGE EFFECT OF MANDATORY IFRS ADOPTION (H1)

Table 3 reports the results of estimating equation (1), which regresses the change in analysts' information environment (measured by ΔAFE, $\Delta DISP$, or $\Delta LogFOLLOW$) on a dummy variable indicating mandatory IFRS adopters (D_{MAND}) and the control variables. The estimated coefficients on D_{MAND} are: -0.00 (t-stat $= -0.01$), -0.05 (t-stat $= -0.42$), and 0.06 (t-stat $= 1.24$) for the ΔAFE, $\Delta DISP$, and $\Delta logFOLLOW$ regressions, respectively. These results indicate that on average mandatory adopters exhibit no incremental change in analysts' information environment relative to the control firms. Thus, we are unable to reject H1.

The sum of the intercept and the coefficient on D_{MAND} is negative and statistically significant for both the ΔAFE and $\Delta DISP$ regressions (untabulated), indicating that mandatory adopters experience decreases in both forecast errors and dispersion. The intercept, which captures the change for the control firms, is negative and statistically significant for both the ΔAFE and $\Delta DISP$ regressions, indicating an improvement in analysts' information environment for the control firms (see section 4.1 for a discussion of possible reasons). Overall, the regression results in table 3 confirm the univariate results in table 2.

TABLE 3

Testing the Average Effect of Mandatory IFRS Adoption on Analysts' Information Environment

$$\Delta Info.\ Environment_i = \alpha_0 + \alpha_1 D_{MANDi} + \alpha_j \Delta Controls_i + \varepsilon_i$$

Independent Variables	Dependent Variables		
	ΔAFE	$\Delta DISP$	$\Delta logFOLLOW$
Intercept	−0.50*	−0.20*	0.01
	(0.25)	(0.10)	(0.04)
	[−2.05]	[−1.91]	[0.11]
D_{MAND}	−0.00	−0.05	0.06
	(0.24)	(0.13)	(0.05)
	[−0.01]	[−0.42]	[1.24]
$\Delta LogSIZE$	−1.19***	−0.25***	0.21***
	(0.22)	(0.08)	(0.04)
	[−5.45]	[−3.08]	[5.73]
$\Delta LogFOLLOW$	0.22	0.05	
	(0.31)	(0.19)	
	[0.72]	[0.27]	
$\Delta LogHORIZON$	0.94**	0.51***	
	(0.39)	(0.13)	
	[2.41]	[4.02]	
Number of Firms	1,418	1,076	1,418
Adjusted R^2	4.3%	1.4%	11.4%

This table provides the results of testing the average effect of mandatory IFRS adoption on the change in analysts' information environment. The subscript *i* in the regression model refers to firm *i*. For the ΔAFE and $\Delta LogFOLLOW$ regressions, the test sample consists of 1,168 European firms subject to mandatory IFRS adoption, and the control sample consists of 250 European firms that voluntarily adopted IFRS at least two years before mandatory IFRS adoption. Because a minimum of two analysts' forecasts is needed to calculate forecast dispersion, the sample for the $\Delta DISP$ regression consists of 879 (197) test (control) firms. For each (test or control) firm, the *pre*-mandatory-adoption window is the last two fiscal years prior to the mandatory adoption of IFRS, and the *post*-mandatory-adoption window is the first two years after the mandatory adoption of IFRS. Analysts' forecast data are obtained from I/B/E/S, while financial and price data are from Global Compustat or Datastream/Worldscope. All continuous variables are winsorized at the 1% and 99% levels to mitigate the influence of outliers. We cluster on country to correct for the inflation in standard errors due to multiple observations from the same country. Estimated coefficients are followed by standard errors () and *t*-statistics []. Significance levels at 10%, 5%, and 1%, two-tailed, are indicated by *, **, and ***, respectively.

Variable Definitions:

ΔAFE = the change in average absolute forecast error (*AFE*) between the *pre*- and *post*-mandatory-adoption windows, where *AFE* is (|*Actual Earnings-Median Forecast*| / *Stock Price*). For ease of exposition, we multiply ΔAFE by 100.

$\Delta DISP$ = the change in average forecast dispersion (*DISP*) between the *pre*- and *post*-mandatory-adoption windows, where *DISP* is *(Standard Deviation of Forecasts / Stock Price)*. For ease of exposition, we multiply $\Delta DISP$ by 100.

$\Delta LogFOLLOW$ = the change in the log of average analyst following (i.e., the number of analysts following the firm) between the *pre*- and *post*-mandatory-adoption windows.

D_{MAND} = a dummy variable equal to one for firms that switched from their domestic accounting standards to IFRS as a result of the mandatory EU-wide adoption of IFRS effective for fiscal years beginning on or after January 1, 2005, and zero for European-based firms that voluntarily adopted IFRS at least two years prior to the mandatory adoption deadline.

$\Delta LogSIZE$ = the change in the log of average market capitalization between the *pre*- and *post*-mandatory-adoption windows.

$\Delta LogHORIZON$ = the change in the log of average forecast horizon (the number of days between the forecast issue date and the earnings announcement date) between the *pre*- and *post*-mandatory-adoption windows.

4.3. RESULTS CONDITIONAL ON COUNTRY-LEVEL INSTITUTIONAL CHARACTERISTICS (H2)

4.3.1. Primary Analyses. Table 4 reports the results of estimating equation (2), which regresses the change in analysts' information environment (measured by ΔAFE, $\Delta DISP$, or $\Delta LogFOLLOW$) on the dummy variable indicating mandatory IFRS adopters (D_{MAND}) interacted with three dummy variables — $D_{HLAW-HDIFF}$, $D_{LLAW-HDIFF}$, and $D_{HLAW-LDIFF}$ — that identify the three different country-groups, in addition to the control variables. The coefficient of interest is on $D_{MAND} * D_{HLAW-HDIFF}$, which captures the difference between the test firms domiciled in *High Law-High IFRSdiff* countries and all the control firms. The estimated coefficients on this interaction term are: −0.56 (t-stat = −2.30), −0.31 (t-stat = −2.00), and 0.05 (t-stat = 1.33) for the ΔAFE, $\Delta DISP$, and $\Delta logFOLLOW$ regressions, respectively. Thus, we reject H2 in favor of the alternative hypothesis that, following mandatory IFRS adoption, analysts' information environment improves for firms in *High Law-High IFRSdiff* countries.

The estimated effects of mandatory IFRS adoption on analysts' forecast errors and dispersions also appear to be economically significant. For test firms domiciled in *High Law-High IFRSdiff* countries, the means of *AFE* and *DISP* in the *pre*-mandatory-IFRS-adoption window are 2.86 and 1.53, respectively (untabulated). Thus, the results in table 4 indicate that mandatory IFRS adoption is associated with a 22% (i.e., −0.56/2.86) reduction in analysts' forecast error and a 20% (i.e., −0.31/1.53) reduction in analysts' forecast dispersion for mandatory adopters domiciled in *High Law-High IFRSdiff* countries.

For mandatory adopters domiciled in the other two country groups, we find no evidence that the change in analysts' information environment differs from that for the control firms: the coefficients on $D_{MAND} * D_{LLAW-HDIFF}$ and $D_{MAND} * D_{HLAW-LDIFF}$ are statistically insignificant in any regression. Comparing across the three groups of mandatory adopters, we find that in the ΔAFE regression the coefficient on $D_{MAND} * D_{HLAW-HDIFF}$ is statistically more negative than $D_{MAND} * D_{LLAW-HDIFF}$ or $D_{MAND} * D_{HLAW-LDIFF}$; in the $\Delta DISP$ regression the coefficient on $D_{MAND} * D_{HLAW-HDIFF}$ is statistically more negative than $D_{MAND} * D_{HLAW-LDIFF}$. Thus, the adoption effect is greater for mandatory adopters domiciled in the *High Law-High IFRSdiff* countries than in other countries.

The above results are based on a simultaneous partition of the test sample using the median values of both *LAW* and *IFRSdiff*. To evaluate the relative importance of these two partitioning variables, we split the test sample by the median value of each variable, one at a time. We find some improvement in analysts' information environment for mandatory adopters in either high *LAW* or high *IFRSdiff* countries, but the magnitudes of the improvement (untabulated) are smaller and the statistical significance levels are lower than our reported results. These results suggest that both high *IFRSdiff* and high *LAW* contribute to our main findings reported in table 4.

TABLE 4

Testing the Impact of Mandatory IFRS Adoption on Analysts' Information Environment Conditional on Country-Level Institutional Characteristics

$$\Delta Info.\ Environment_i = \beta_0 + \beta_1 D_{MAND_i} * D_{HLAW-HDIFF_i} + \beta_2 D_{MAND_i} * D_{LLAW-HDIFF_i} + \beta_3 D_{MAND_i} * D_{HLAW-LDIFF_i} + \beta_j \Delta Controls_i + \varepsilon_i$$

Independent Variables	Dependent Variables		
	ΔAFE	$\Delta DISP$	$\Delta logFOLLOW$
Intercept	−0.51*	−0.20*	0.01
	(0.24)	(0.10)	(0.04)
	[−2.08]	[−1.93]	[0.11]
$D_{MAND}*D_{HLAW-HDIFF}$	−0.56**	−0.31*	0.05
	(0.24)	(0.16)	(0.04)
	[−2.30]	[−2.00]	[1.33]
$D_{MAND}*D_{LLAW-HDIFF}$	0.16	−0.03	0.03
	(0.35)	(0.18)	(0.05)
	[0.46]	[−0.18]	[0.60]
$D_{MAND}*D_{HLAW-LDIFF}$	−0.00	−0.01	0.09
	(0.35)	(0.12)	(0.07)
	[0.00]	[−0.10]	[1.34]
$\Delta LogSIZE$	−1.19***	−0.24***	0.21***
	(0.22)	(0.08)	(0.04)
	[−5.38]	[−3.05]	[5.67]
$\Delta LogFOLLOW$	0.23	0.04	
	(0.32)	(0.18)	
	[0.72]	[0.21]	
$\Delta LogHORIZON$	0.94**	0.49***	
	(0.40)	(0.12)	
	[2.35]	[3.99]	
Number of Firms	1,418	1,076	1,418
Adjusted R^2	4.4%	1.5%	11.6%
Two-Tailed p-values for Tests of Differences Across Country Groups			
$D_{MAND} * D_{HLAW-HDIFF} = D_{MAND} * D_{LLAW-HDIFF}$	0.05	0.15	0.73
$D_{MAND} * D_{HLAW-HDIFF} = D_{MAND} * D_{HLAW-LDIFF}$	0.01	0.02	0.55
$D_{MAND} * D_{LLAW-HDIFF} = D_{MAND} * D_{HLAW-LDIFF}$	0.68	0.90	0.40

This table reports the results of testing the effect of mandatory IFRS adoption on the change in analysts' information environment conditional on country-level enforcement and the degree of difference between domestic accounting standards and IFRS. The subscript *i* in the regression model refers to firm *i*. For the ΔAFE and $\Delta LogFOLLOW$ regressions, the test sample consists of 1,168 European firms subject to mandatory IFRS adoption, and the control sample consists of 250 European firms that voluntarily adopted IFRS at least two years before mandatory IFRS adoption. Because a minimum of two analysts' forecasts is needed to calculate forecast dispersion, the sample for the $\Delta DISP$ regression consists of 879 (197) test (control) firms. For each (test or control) firm, the *pre*-mandatory-adoption window is the last two fiscal years prior to the mandatory adoption of IFRS, and the *post*-mandatory-adoption window is the first two years after the mandatory adoption of IFRS. We partition our sample firms based on the median values of *LAW* and *IFRSdiff*. *LAW* is the "rule of law" variable for 2005 from Kaufmann, Kraay, Mastuzzi [2007]; higher values of *LAW* represent stronger enforcement. *IFRSdiff* measures differences between national accounting standards and IFRS based on Bae, Tan, Welker [2008, table 1]; higher values of *IFRSdiff* indicate greater differences. Partitioning on these two variables divides our sample firms into three country groups (see table 1, panel B for details): (1) a *High LAW-High IFRSdiff* group, (2) a *Low LAW-High IFRSdiff* group, and (3) a *High LAW-Low IFRSdiff* group. Note that no country has below-median values of both *LAW* and *IFRSdiff*. Analysts' forecast data are obtained from I/B/E/S, while financial and price data are from Global Compustat or Datastream/Worldscope. All continuous variables are winsorized at the 1% and 99% levels to mitigate the influence of outliers. We cluster on country to correct for the inflation in standard errors due to multiple observations from the same country. Estimated coefficients are followed by standard errors () and *t*-statistics []. Significance levels at 10%, 5%, and 1%, two-tailed, are indicated by *, **, and ***, respectively.

(*Continued*)

TABLE 4 — *Continued*

Variable Definitions:

ΔAFE = the change in average absolute forecast error (*AFE*) between the *pre-* and *post-*mandatory-adoption windows, where *AFE* is (|*Actual Earnings-Median Forecast*| / *Stock Price*). For ease of exposition, we multiply ΔAFE by 100.

$\Delta DISP$ = the change in average forecast dispersion (*DISP*) between the *pre-* and *post-*mandatory-adoption windows, where *DISP* is (*Standard Deviation of Forecasts* / *Stock Price*). For ease of exposition, we multiply $\Delta DISP$ by 100.

$\Delta LogFOLLOW$ = the change in the log of average analyst following (i.e., the number of analysts following the firm) between the *pre-* and *post-*mandatory-adoption windows.

D_{MAND} = a dummy variable equal to one for firms that switched from their domestic accounting standards to IFRS as a result of the mandatory EU-wide adoption of IFRS effective for fiscal years beginning on or after January 1, 2005, and zero for European-based firms that voluntarily adopted IFRS at least two years prior to the mandatory adoption deadline.

$D_{HLAW-HDIFF}$ = a dummy variable equal to one if a firm (test or control) is domiciled in a *High LAW-High IFRSdiff* country, and zero otherwise.

$D_{LLAW-HDIFF}$ = a dummy variable equal to one if a firm (test or control) is domiciled in a *Low LAW-High IFRSdiff* country, and zero otherwise.

$D_{HLAW-LDIFF}$ = a dummy variable equal to one if a firm (test or control) is domiciled in a *High LAW-Low IFRSdiff* country, and zero otherwise.

$\Delta LogSIZE$ = the change in the log of average market capitalization between the *pre-* and *post-*mandatory-adoption windows.

$\Delta LogHORIZON$ = the change in the log of average forecast horizon (the number of days between the forecast issue date and the earnings announcement date) between the *pre-* and *post-*mandatory-adoption windows.

4.3.2. Robustness Tests. Equation (2) compares test firms from three country groups with all of the control firms. A cleaner approach would be to compare test and control firms within each country group. However, as discussed earlier, we lack sufficient observations to conduct a meaningful within-group comparison for each country group. To overcome this limitation of the data, we combine the *Low LAW-High IFRSdiff* and *High LAW-Low IFRSdiff* groups, and estimate the following model:

$$\Delta Info.\ Environment_i = \chi_1 D_{HLAW-HDIFFi} + \chi_2 D_{MANDi} * D_{HLAW-HDIFFi} + \chi_3 D_{OTHERi} + \chi_4 D_{MANDi} * D_{OTHERi} + \chi_j \Delta Controls_i + \varepsilon_i, \quad (4)$$

where the subscript i refers to firm i. $\Delta Info.\ Environment$ refers to ΔAFE, $\Delta DISP$, or $\Delta LogFOLLOW$; the controls are the same as in equations (1) to (3). D_{OTHER} is a dummy variable equal to one for firms in either the *Low LAW-High IFRSdiff* or *High LAW-Low IFRSdiff* groups, and zero otherwise. The intercept is omitted to avoid perfect collinearity, as the sum of $D_{HLAW-HDIFF}$ and D_{OTHER} equals one. The coefficients on $D_{HLAW-HDIFF}$ and D_{OTHER} capture the changes for the control firms located in these two country groups. Similarly, the coefficients on the two interaction terms capture the difference between the test and control firms within each of these two country groups. Of the two interaction terms, we are primarily interested in the coefficient on $D_{MAND} * D_{HLAW-HDIFF}$.

The results of estimating equation (4) are as follows (untabulated). Confirming our findings in table 4, the coefficients on $D_{MAND} * D_{HLAW-HDIFF}$ are: -0.62 (t-stat $= -2.29$), -0.34 (t-stat $= -1.87$), and 0.06 (t-stat $= 1.60$) for the ΔAFE, $\Delta DISP$ and $\Delta logFOLLOW$ regressions, respectively. The

coefficients on $D_{MAND} * D_{OTHER}$ are statistically insignificant in all regressions, indicating that the change in analysts' information environment does not differ between the test and control firms domiciled in *Other* countries. We also compare the adoption effect across the two country groups. Consistent with the results in table 4, the coefficients on $D_{MAND} * D_{HLAW-HDIFF}$ are statistically more negative than $D_{MAND} * D_{OTHER}$ in the ΔAFE and $\Delta DISP$ regressions.[10]

To further test the robustness of our results, table 5 reestimates equation (2) using *IFRSdiff* and an alternative measure of *LAW* used by Leuz et al. [2003] and DeFond and Hung [2004]— the mean of the three legal variables from La Porta et al. [1998]: the efficiency of the judicial system, the rule of law, and the corruption index. Note table 5 now has four (rather than three) country groups using this alternative measure of *LAW*, with Ireland being the only country included in the *Low LAW-Low IFRSdiff* group. The results using this alternative partition are similar to our main results in table 4.

Our results are also robust to using the following alternative proxies for the quality of country-level enforcement environment (untabulated): (1) Kaufmann, Kraay, and Mastuzzi [2007] "rule of law" score for 2004, 2005, or 2006, as well as the average of the three years, and (2) the "governance effectiveness" score, the "regulatory quality" score, or the average of the six governance scores for 2005 from Kaufmann, Kraay, and Mastuzzi [2007]. In addition, when we partition the sample firms using mean (rather than median) values of both *LAW* and *IFRSdiff*, we find similar results (untabulated).[11]

4.4. RESULTS FOR FIRM-LEVEL REPORTING INCENTIVES (H3)

Table 6 reports the results of estimating equation (3), which regresses the change in analysts' information environment (measured by ΔAFE, $\Delta DISP$, or $\Delta LogFOLLOW$) on our proxies for firm-level reporting incentives and the control variables, for mandatory adopters domiciled in the *Low LAW-High IFRSdiff* countries. We find that *TOBIN_Q* is negatively related to both

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[10] We undertake a one-to-one matching of test and control firms within each country (e.g., on *pre*-mandatory-adoption firm size). The maximum number of within-country matched pairs is only 102. Using this approach, we find that the key coefficients of interest are statistically insignificant.

[11] In untabulated analyses, we also test the effect of the transition year (i.e., the first year under IFRS reporting) on our results. On the one hand, a number of firms, particularly large ones, provided significant interim guidance in the transition year. This may have temporarily reduced analysts' forecast errors and dispersion. On the other hand, the transition year may bias against finding our results, because the "one-time" adjustments coinciding with the first-time adoption of IFRS may temporarily increase the difficulty of forecasting earnings. We find that the results are similar when we remove the transition year from the *post*-mandatory-adoption window. We also find no statistically significant change in forecast errors, dispersion, or analyst following across the two years in the *post*-mandatory-year window. Thus, our results are unlikely to be confounded by the effect of the transition year.

TABLE 5

Testing the Adoption Effect Conditional on Country-Level Institutional Characteristics: Using an Alternative Measure of LAW Based on La Porta et al. [1998]

$$\Delta Info.\ Environmenti = \beta_0 + \beta_1 D_{MANDi} * D_{HLAW-HDIFFi} + \beta_2 D_{MANDi} * D_{LLAW-HDIFFi} + \beta_3 D_{MANDi} * D_{HLAW-LDIFFi} + \beta_4 D_{MANDi} * D_{LLAW-LDIFFi} + \beta_j \Delta Controls_i + \varepsilon_i$$

Independent Variables	Dependent Variables		
	ΔAFE	$\Delta DISP$	$\Delta logFOLLOW$
Intercept	−0.52*	−0.24**	−0.01
	(0.27)	(0.10)	(0.04)
	[−1.92]	[−2.39]	[−0.18]
$D_{MAND} * D_{HLAW-HDIFF}$	−0.58*	−0.44**	0.09
	(0.28)	(0.19)	(0.07)
	[−2.07]	[−2.28]	[1.36]
$D_{MAND} * D_{LLAW-HDIFF}$	0.23	0.02	0.05
	(0.39)	(0.19)	(0.05)
	[0.60]	[0.08]	[0.89]
$D_{MAND} * D_{HLAW-LDIFF}$	0.04	0.04	0.05
	(0.30)	(0.12)	(0.06)
	[0.13]	[−0.30]	[0.86]
$D_{MAND} * D_{LLAW-LDIFF}$	−0.11	−0.05	0.04
	(0.25)	(0.12)	(0.04)
	[−0.44]	[−0.41]	[1.10]
$\Delta LogSIZE$	−1.12***	−0.23**	0.21***
	(0.24)	(0.08)	(0.04)
	[−4.62]	[−2.74]	[5.85]
$\Delta LogFOLLOW$	−0.11	−0.02	
	(0.32)	(0.19)	
	[−0.36]	[−0.09]	
$\Delta LogHORIZON$	1.00**	0.53***	
	(0.43)	(0.10)	
	[2.34]	[4.33]	
Number of Firms	1,391	1,058	1,391
Adjusted R^2	4.4%	2.0%	10.3%
Two-Tailed p-values for Tests of Differences Across Country Groups			
$D_{MAND} * D_{HLAW-HDIFF} = D_{MAND} * D_{LLAW-HDIFF}$	0.04	0.08	0.57
$D_{MAND} * D_{HLAW-HDIFF} = D_{MAND} * D_{HLAW-LDIFF}$	0.01	0.02	0.60
$D_{MAND} * D_{HLAW-HDIFF} = D_{MAND} * D_{LLAW-LDIFF}$	0.00	0.04	0.47
$D_{MAND} * D_{LLAW-HDIFF} = D_{MAND} * D_{HLAW-LDIFF}$	0.65	0.91	0.97
$D_{MAND} * D_{LLAW-HDIFF} = D_{MAND} * D_{LLAW-LDIFF}$	0.38	0.72	0.92
$D_{MAND} * D_{HLAW-LDIFF} = D_{MAND} * D_{LLAW-LDIFF}$	0.39	0.23	0.88

This table repeats the analysis of table 4 using an alternative definition of *LAW*. The subscript *i* in the regression model refers to firm *i*. *IFRSdiff* is the same measure as in table 4: the Bae, Tan, and Welker [2008, table 1] measure of the differences between national accounting standards and IFRS. Higher values of *IFRSdiff* indicate greater differences. We measure *LAW* as the mean of the three enforcement measures from La Porta et al.[1998]: (1) the efficiency of the judicial system, (2) the rule of law, and (3) the corruption index. Partitioning sample firms by the median values of *IFRSdiff* and this new measure of *LAW* divides our sample firms into four (rather than three) country groups as follows:

(1) a *High LAW-High IFRSdiff* group (i.e., *LAW* ≥ 9.2 and *IFRSdiff* ≥ 11) including Austria, Belgium, Denmark, Finland, and Switzerland;

(2) a *Low LAW-High IFRSdiff* group (i.e., *LAW* < 9.2 and *IFRSdiff* ≥ 11) including France, Germany, Greece, Italy, Portugal, and Spain;

(3) a *High LAW-Low IFRSdiff* group (i.e., *LAW* ≥ 9.2 and *IFRSdiff* < 11) including the Netherlands, Norway, Sweden, and the UK; and

(4) a *Low LAW-Low IFRSdiff* group (i.e., with *LAW* < 9.2 and *IFRSdiff* ≤ 11) including only Ireland.

(*Continued*)

TABLE 5 — *Continued*

This alternative measure of *LAW* is not available for four countries: the Czech Republic, Hungary, Luxembourg, and Poland. As a result, the sample size is reduced by 27 firms for the ΔAEF and $\Delta LogFOLLOW$ regressions, and by 18 firms for the $\Delta DISP$ regression compared with the sample used in table 4. For the ΔAFE and $\Delta LogFOLLOW$ regressions, the total number of sample firms (test and control) is 1,391; for the $\Delta DISP$ regression, the total number of sample firms (test and control) is 1,058. Analysts' forecast data are from I/B/E/S, while financial and price data are from Global Compustat or Datastream/Worldscope. For each (test or control) firm, the *pre*-mandatory-adoption window is the last two fiscal years prior to mandatory IFRS adoption, and the *post*-mandatory-adoption window is the first two years after mandatory IFRS adoption. All continuous variables are winsorized at the 1% and 99% levels to mitigate the influence of outliers. We cluster on country to correct for the inflation in standard errors due to multiple observations from the same country. Estimated coefficients are followed by standard errors () and *t*-statistics []. Significance levels at 10%, 5%, and 1%, two-tailed, are indicated by *, **, and ***, respectively.

Variable Definitions:

ΔAFE = the change in average absolute forecast error (*AFE*) between the *pre*- and *post*-mandatory-adoption windows, where *AFE* is (|*Actual Earnings-Median Forecast*| / *Stock Price*). For ease of exposition, we multiply ΔAFE by 100.

$\Delta DISP$ = the change in average forecast dispersion (*DISP*) between the *pre*- and *post*-mandatory-adoption windows, where *DISP* is (*Standard Deviation of Forecasts* / *Stock Price*). For ease of exposition, we multiply $\Delta DISP$ by 100.

$\Delta LogFOLLOW$ = the change in the log of average analyst following (i.e., the number of analysts following the firm) between the *pre*- and *post*-mandatory-adoption windows.

D_{MAND} = a dummy variable equal to one for firms that switched from their domestic accounting standards to IFRS as a result of the mandatory EU-wide adoption of IFRS effective for fiscal years beginning on or after January 1, 2005, and zero for European-based firms that voluntarily adopted IFRS at least two years prior to the mandatory adoption deadline.

$D_{HLAW-HDIFF}$ = a dummy variable equal to one if a firm (test or control) is domiciled in a *High LAW-High IFRSdiff* country, and zero otherwise.

$D_{LLAW-HDIFF}$ = a dummy variable equal to one if a firm (test or control) is domiciled in a *Low LAW-High IFRSdiff* country, and zero otherwise.

$D_{HLAW-LDIFF}$ = a dummy variable equal to one if a firm (test or control) is domiciled in a *High LAW-Low IFRSdiff* country, and zero otherwise.

$D_{LLAW-LDIFF}$ = a dummy variable equal to one if a firm (test or control) is domiciled in a *Low LAW-Low IFRSdiff* country, and zero otherwise.

$\Delta LogSIZE$ = the change in the log of average market capitalization between the *pre*- and *post*-mandatory-adoption windows.

$\Delta LogHORIZON$ = the change in the log of average forecast horizon (the number of days between the forecast issue date and the earnings announcement date) between the *pre*- and *post*-mandatory-adoption windows.

ΔAFE and $\Delta DISP$ (t-stat = −4.01 and −2.91, respectively), *CLOSEHELD* is positively related to both ΔAFE and $\Delta DISP$ (t-stat = 2.36 and 2.95, respectively), and *BIG4* is negatively related to ΔAFE (t-stat = −2.17). These results indicate that firms with stronger reporting incentives (i.e., firms with more growth opportunities, a smaller proportion of closely held shares, or higher-quality auditors) exhibit larger decreases in analysts' forecast errors and dispersion around mandatory IFRS adoption. These results are consistent with mandatory adopters with stronger reporting incentives implementing the changes mandated by IFRS more rigorously and, thus, exhibiting a greater improvement in analysts' information environment.

For completeness, we also estimate equation (3) for mandatory adopters in the other two country groups (untabulated). As expected, the results are much weaker. To compare regression coefficients across the three groups, we estimate equation (3) for the three country groups pooled in a Seemingly Unrelated Regression (SUR) model. Untabulated results show that

TABLE 6

Testing the Impact of Firm-Level Reporting Incentives: Analysis of Mandatory Adopters Domiciled in Low LAW-High IFRSdiff Countries

$$\Delta Info.\ Environment_i = \delta_0 + \delta_1 ROA_i + \delta_2 TOBIN\_Q_i + \delta_3 LEVERAGE_i + \delta_4 CLOSEHELD_i + \delta_5 FOREIGNSALES_i + \delta_6 BIG4_i + \delta_j \Delta Controls_i + \varepsilon_i$$

	Dependent Variable		
Independent Variables	ΔAFE	$\Delta DISP$	$\Delta logFOLLOW$
Intercept	0.70	−0.19	0.10
	(1.04)	(0.56)	(0.12)
	[0.67]	[−0.35]	[0.85]
ROA	6.12	3.14	−0.20
	(4.35)	(3.02)	(0.47)
	[1.41]	[1.04]	[−0.43]
TOBIN_Q	−1.07***	−0.36**	0.02
	(0.27)	(0.12)	(0.04)
	[−4.01]	[−2.91]	[0.52]
LEVERAGE	0.21	−0.55	−0.11
	(0.62)	(0.50)	(0.11)
	[0.35]	[−1.11]	[−1.05]
CLOSEHELD	1.44**	1.09**	0.04
	(0.61)	(0.37)	(0.13)
	[2.36]	[2.95]	[0.29]
FOREIGNSALES	0.98	0.63	−0.11
	(1.06)	(0.39)	(0.07)
	[0.92]	[1.59]	[−1.59]
BIG4	−0.74*	−0.00	0.02
	(0.34)	(0.11)	(0.06)
	[−2.17]	[−0.02]	[0.43]
$\Delta LogSIZE$	−2.57***	−0.29	0.16*
	(0.72)	(0.19)	(0.08)
	[−3.58]	[−1.48]	[1.98]
$\Delta LogFOLLOW$	1.30**	−0.30	
	(0.43)	(0.50)	
	[3.03]	[−0.59]	
$\Delta LogHORIZON$	−0.06	0.43**	
	(0.69)	(0.16)	
	[−0.09]	[2.64]	
Number of Firms	385	299	385
Adjusted R^2	6.3%	2.3%	4.6%

This table reports results from testing how firm-level reporting incentives influence the change in analysts' information environment. The subscript *i* in the regression model refers to firm *i*. The sample only includes European firms subject to mandatory IFRS adoption that are domiciled in *Low LAW-High IFRSdiff* countries. *LAW* is the "rule of law" variable for 2005 from Kaufmann, Kraay, and Mastuzzi [2007]; higher values of *LAW* represent stronger enforcement. *IFRSdiff* measures differences between national accounting standards and IFRS based on Bae, Tan, and Welker [2008, table 1]; higher values of *IFRSdiff* indicate greater differences. We partition countries based on median values of *LAW* and *IFRSdiff*, which are 1.6 and 11, respectively, and select mandatory adopters from *Low LAW-High IFRSdiff* countries (i.e., *LAW* ≤ 1.6 and *IFRSdiff* ≥ 11) for the test in this table. For the ΔAFE and $\Delta LogFOLLOW$ regressions, the sample consists of 385 firms. Because a minimum of two analysts' forecasts is needed to calculate forecast dispersion, the sample for the $\Delta DISP$ regression consists of 299 firms. For each firm, the *pre*-mandatory-adoption window is the last two fiscal years prior to mandatory IFRS adoption, and the *post*-mandatory-adoption window is the first two years after mandatory IFRS adoption. Analysts' forecast data are from I/B/E/S, and accounting and price data are from Global Compustat or Datastream/Worldscope. All continuous variables are winsorized at the 1% and 99% levels to mitigate the influence of outliers. We cluster on country to correct for the inflation in standard errors due to multiple observations from the same country. Estimated coefficients are followed by standard errors () and *t*-statistics []. Significance levels at 10%, 5%, and 1%, two-tailed, are indicated by *, **, and ***, respectively. (*Continued*)

TABLE 6 — *Continued*

Variable Definitions:

ΔAFE = the change in average absolute forecast error (*AFE*) between the *pre*- and *post*-mandatory-adoption windows, where *AFE* is (|*Actual Earnings-Median Forecast*| / *Stock Price*). For ease of exposition, we multiply ΔAFE by 100.

$\Delta DISP$ = the change in average forecast dispersion (*DISP*) between the *pre*- and *post*-mandatory-adoption windows, where *DISP* is (*Standard Deviation of Forecasts* / *Stock Price*). For ease of exposition, we multiply $\Delta DISP$ both by 100.

$\Delta LogFOLLOW$ = the change in the log of average analyst following (i.e., the number of analysts following the firm) between the *pre*- and *post*-mandatory-adoption windows.

ROA = average return on assets (net income / total assets) for the *pre*-mandatory-adoption window.

TOBIN_Q = average Tobin's *q* for the *pre*-mandatory-adoption window. Tobin's *q* is market value of assets / book value of assets, where the numerator is measured as (book value of assets + market value of common equity – book value of common equity – balance sheet deferred taxes).

LEVERAGE = average leverage (total liabilities / total assets) for the *pre*-mandatory-adoption window.

CLOSEHELD = average percentage of closely hold shares (number of closely held shares / common shares outstanding) as reported by Datastream/Worldscope in the *pre*-mandatory-adoption window.

FOREIGNSALES = average percentage of foreign sales (foreign sales / total sales) for the *pre*-mandatory-adoption window as reported by Datastream/Worldscope.

BIG4 = a dummy variable indicating the presence of a big-four auditor in the year immediately before switching to IFRS.

$\Delta LogSIZE$ = the change in the log of average market capitalization between the *pre*- and *post*-mandatory-adoption windows.

$\Delta LogHORIZON$ = the change in the log of average forecast horizon (the number of days between the forecast issue date and the earnings announcement date) between the *pre*- and *post*-mandatory-adoption windows.

the effects of *TOBIN_Q*, *CLOSEHELD,* and *BIG4* are statistically larger for the *Low LAW-High IFRSdiff* group than the other two groups. These results confirm our expectation that in countries with weak enforcement regimes, firm-level reporting incentives play a relatively greater role in determining the effect of mandatory IFRS adoption.

4.5. ANALYSIS OF ANALYSTS' PUBLIC AND PRIVATE INFORMATION

In this subsection, we examine if the improvement in analysts' information environment documented above is attributable to an increase in analysts' public information, private information, or both. We expect enhanced disclosures and transparency under IFRS to increase analysts' public information. However, it is an open question how mandatory IFRS adoption would change analysts' private information. Theory (e.g., Kim and Verrecchia [1991, 1994]) suggests that more firm disclosures could either substitute for analysts' private information, or complement analysts' private knowledge, allowing analysts to develop more private information.

We use the Barron et al. [1998] measures to capture the average precision of analysts' public (*PUBLIC*) and private (*PRIVATE*) information, and analysts' consensus (*CONSENSUS*). *PUBLIC* and *PRIVATE* are measured using the variance of analysts' forecasts (*D*), the squared error in the mean forecast (*SE*), and the number of forecasts (*N*) as follows (see Barron et al.

[1998], Corollory 1, p. 428):

$$PUBLIC = \frac{SE - D/N}{[(1 - 1/N)\,D + SE]^2} \quad \text{and} \quad PRIVATE = \frac{D}{[(1 - 1/N)\,D + SE]^2}.$$

CONSENSUS is the average proportion of analysts' total information that is public (i.e., $CONSENSUS = PUBLIC/(PUBLIC + PRIVATE)$). We first compute *PUBLIC*, *PRIVATE*, and *CONSENSUS* for each firm-year; then, for each firm, we calculate $\Delta PUBLIC$, $\Delta PRIVATE$, and $\Delta CONSENSUS$ as the difference in the average *PUBLIC*, *PRIVATE*, and *CONSENSUS*, respectively, between the *pre*- and *post*-mandatory-adoption windows. We scale $\Delta PUBLIC$ and $\Delta PRIVATE$ by the average *pre*-mandatory-adoption levels for comparability across firms.

To examine the effect of mandatory IFRS adoption on analysts' public and private information and analysts' consensus, we estimate equation (2) using $\Delta PUBLIC$, $\Delta PRIVATE$, and $\Delta CONSENSUS$ as alternative dependent variables

$$\begin{aligned}\Delta Info.\,Environment_i = {} & \beta_0 + \beta_1 D_{MANDi} * D_{HLAW-HDIFFi} \\ & + \beta_2 D_{MANDi} * D_{LLAW-HDIFFi} + \beta_3 D_{MANDi} * D_{HLAW-LDIFFi} \\ & + \beta_j \Delta Controls_i + \varepsilon_i, \qquad (2)\end{aligned}$$

where the subscript i refers to firm i. $\Delta Info.\,Environment$ now refers to $\Delta PUBLIC$, $\Delta PRIVATE$, and $\Delta CONSENSUS$. We use rank regressions to reduce the influence of outliers and skewness. Specifically, we first transform $\Delta PUBLIC$, $\Delta PRIVATE$, and $\Delta CONSENSUS$ into [0, 1] decile ranks (i.e., we rank each raw variable into deciles of 0–9 and then divide the decile ranks by 9). Then, to facilitate the interpretation of the results, we subtract from the ranked values the rank corresponding to a zero change in the raw variable, so a positive (negative) adjusted rank corresponds to an increase (decrease) in the raw variable.[12] We also control for $\Delta logSIZE$.[13]

The results are reported in table 7. The results are similar if we use the equation (4) specification (untabulated). In table 7, the intercept is statistically insignificant for all of the $\Delta PUBLIC$, $\Delta PRIVATE$, and $\Delta CONSENSUS$ regressions, indicating no change in analysts' public or private information or analyst consensus for the control firms. In both the $\Delta PUBLIC$ and $\Delta PRIVATE$ regressions, the coefficients on $D_{MAND} * D_{HLAW-HDIFF}$ are positive and statistically significant, indicating an improvement relative to the control firms in both analysts' public and private information for mandatory adopters domiciled in the *High LAW-High IFRSdiff* countries. The increase in analysts' private information is consistent with prior findings that

[12] Take $\Delta PUBLIC$ as an example. The original rank of firms with $\Delta PUBLIC = 0$ is 4/9. After subtracting 4/9 from the original ranks of $\Delta PUBLIC$ for all firms, firms with $\Delta PUBLIC = 0$ have an adjusted rank of 0, and firms with positive (negative) $\Delta PUBLIC$ now also have positive (negative) adjusted ranks.

[13] Our inferences are unchanged if we include $\Delta logHORIZON$ and $\Delta logFOLLOW$ as additional controls.

more firm disclosures increase analysts' private information (e.g., Frankel, Kothari, and Weber [2006], Barron et al. [1998]). The coefficient on $D_{MAND} * D_{HLAW-HDIFF}$ is not statistically significant for the $\Delta CONSENSUS$ regression, indicating that analysts' public and private information increase

TABLE 7

Testing the Impact of Mandatory IFRS Adoption on Analysts' Public and Private Information

$$\Delta Info.\ Environment_i = \beta_0 + \beta_1 D_{MANDi} * D_{HLAW-HDIFFi} + \beta_2 D_{MANDi} * D_{LLAW-HDIFFi} + \beta_3 D_{MANDi} * D_{HLAW-LDIFFi} + \beta_j \Delta Controls_i + \varepsilon_i$$

Independent Variables	Dependent Variables		
	$\Delta PUBLIC$	$\Delta PRIVATE$	$\Delta CONSENSUS$
Intercept	0.04	0.07	−0.04
	(0.03)	(0.04)	(0.04)
	[1.21]	[1.61]	[−1.16]
$D_{MAND} * D_{HLAW-HDIFF}$	0.09**	0.08***	0.02
	(0.04)	(0.03)	(0.04)
	[2.22]	[3.03]	[0.66]
$D_{MAND} * D_{LLAW-HDIFF}$	0.01	0.01	0.01
	(0.03)	(0.05)	(0.05)
	[0.22]	[0.18]	[0.27]
$D_{MAND} * D_{HLAW-LDIFF}$	0.03	−0.00	0.03
	(0.02)	(0.41)	(0.04)
	[1.34]	[−0.01]	[0.86]
$\Delta LogSIZE$	−0.01	−0.04	−0.06**
	(0.03)	(0.03)	(0.03)
	[−0.41]	[−1.23]	[−2.57]
Number of Firms	1,076	1,076	1,076
Adjusted R^2	0.3%	0.2%	0.3%
Two-Tailed p-values for Tests of Differences Across Country Groups			
$D_{MAND} * D_{HLAW-HDIFF} = D_{MAND} * D_{LLAW-HDIFF}$	0.06	0.02	0.77
$D_{MAND} * D_{HLAW-HDIFF} = D_{MAND} * D_{HLAW-LDIFF}$	0.12	0.01	0.66
$D_{MAND} * D_{LLAW-HDIFF} = D_{MAND} * D_{HLAW-LDIFF}$	0.30	0.86	0.60

This table reports results using variables from Barron et al. [1998]—$\Delta PUBLIC$, $\Delta PRIVATE$, and $\Delta CONSENSUS$—as dependent variables. The subscript i in the regression model refers to firm i. We use a rank regression specification to reduce the influence of outliers and skewness. We first transform $\Delta PUBLIC$, $\Delta PRIVATE$, and $\Delta CONSENSUS$ into [0, 1] decile ranks (i.e., we rank each raw variable into deciles of 0–9 and then divide the decile ranks by 9). Then, to facilitate interpretation, we subtract from these ranked values the rank corresponding to a zero change in the raw variable, so a positive (negative) resulting rank corresponds to an increase (decrease) in the raw variable. The test sample consists of 879 European firms subject to mandatory IFRS adoption, and the control sample consists of 197 European firms that voluntarily adopted IFRS at least two years before mandatory IFRS adoption. We partition countries based upon median values of *LAW* and *IFRSdiff*. *LAW* is the "rule of law" variable for 2005 from Kaufmann, Kraay, and Mastuzzi [2007]; higher values of *LAW* represent stronger enforcement. *IFRSdiff* measures differences between national accounting standards and IFRS based on Bae, Tan, and Welker [2008, table 1]; higher values of *IFRSdiff* indicate greater differences. This partitioning divides our sample firms in three country groups (see table 1, panel B for details): (1) a *High LAW-High IFRSdiff* group, (2) a *Low LAW-High IFRSdiff* group, and (3) a *High LAW-Low IFRSdiff* group. Note that no country has below-median values of both *LAW* and *IFRSdiff*. For each (test or control) firm, the *pre*-mandatory-adoption window is the last two fiscal years prior to the mandatory adoption of IFRS, and the *post*-mandatory-adoption window is the first two years after the mandatory adoption of IFRS. Analysts' forecast data are from I/B/E/S, while accounting and price data are from Global Compustat or Datastream/Worldscope. All continuous variables are winsorized at the 1% and 99% levels to mitigate the influence of outliers. We cluster on country to correct for the inflation in standard errors due to multiple observations from the same country. Estimated coefficients are followed by standard errors () and *t*-statistics []. Significance levels at 10%, 5%, and 1%, two-tailed, are indicated by *, **, and ***, respectively. (*Continued*)

TABLE 7 — *Continued*

Variable Definitions:

$\Delta PUBLIC$ = the percentage change in the average quality of analysts' public information (*PUBLIC*) between the *pre-* and *post-*mandatory-adoption windows. *PUBLIC* is measured using the variance of the forecasts (D), the squared error in the mean forecast (SE), and the number of forecasts (N) as follows (for details, see Barron et al. [1998], Corollory 1, p. 428):

$$PUBLIC = \frac{SE - D/N}{[(1 - 1/N)D + SE]^2}$$

.

$\Delta PRIVATE$ = the percentage change in the average quality of analysts' private information (*PRIVATE*) between the *pre-* and *post-*mandatory-adoption windows. *PRIVATE* is measured using the variance of the forecasts (D), the squared error in the mean forecast (SE), and the number of forecasts (N) as follows (see Barron et al. [1998], Corollory 1, p. 428):

$$PRIVATE = \frac{D}{[(1 - 1/N)D + SE]^2}$$

.

$\Delta CONSENSUS$ = the change in the average analysts' consensus (*CONSENSUS*) between the *pre-* and *post-*mandatory-adoption windows. *CONSENSUS* is the ratio of analysts' public information to their total information, i.e., $CONSENSUS = \frac{PUBLIC}{(PUBLIC+PRIVATE)}$ (see Barron et al. [1998], Proposition 3, p. 427).

D_{MAND} = a dummy variable equal to one for firms that switched from their domestic accounting standards to IFRS as a result of the mandatory EU-wide adoption of IFRS effective for fiscal years beginning on or after January 1, 2005, and zero for European-based firms that voluntarily adopted IFRS at least two years prior to the mandatory adoption deadline.

$D_{HLAW-HDIFF}$ = a dummy variable equal to one if a firm (test or control) is domiciled in a *High LAW-High IFRSdiff* country, and zero otherwise.

$D_{LLAW-HDIFF}$ = a dummy variable equal to one if a firm (test or control) is domiciled in a *Low LAW-High IFRSdiff* country, and zero otherwise.

$D_{HLAW-LDIFF}$ = a dummy variable equal to one if a firm (test or control) is domiciled in a *High LAW-Low IFRSdiff* country, and zero otherwise.

$\Delta LogSIZE$ = the change in the log of average market capitalization between the *pre-* and *post-*mandatory-adoption windows.

by a similar degree for mandatory adopters domiciled in the *High LAW-High IFRSdiff* countries.

The results in table 7 also provide additional insight into our main result of a decrease in *AFE* (i.e., the error in analysts' average forecast) reported in table 4. Barron et al. [1998, equation (14)] show that the accuracy of analysts' average forecast can improve for three reasons: (1) an increase in the number of analysts' forecasts, (2) an increase in individual analysts' forecast accuracy (i.e., an increase in *PUBLIC* or *PRIVATE*), or (3) an increase in the degree to which individual forecasts are based on private information (i.e., a decrease in *CONSENSUS*). Because we use a matched sample of analysts, and find no change in analysts' consensus, the improvement in the accuracy of analysts' average forecast is attributable to reason (2). To corroborate this explanation, we calculate *AFE* as the average of individual analysts' absolute forecast errors, and find evidence very similar to our reported results (untabulated).

We also estimate equation (3) using $\Delta PUBLIC$, $\Delta PRIVATE$, and $\Delta CONSENSUS$ as dependent variables (untabulated). For the mandatory

adopters in the *Low LAW-High IFRSdiff* group, *TOBIN_Q* is positively related to both $\Delta PUBLIC$ and $\Delta PRIVATE$ (two-tailed $p \leq 0.01$ for both), and *CLOSEHELD* is negatively related to $\Delta PRIVATE$ (two-tailed $p = 0.04$). These results indicate a greater improvement in both analysts' public and private information for mandatory adopters with stronger reporting incentives.

5. *Conclusion*

We examine the impact of the 2005 EU-wide mandatory adoption of IFRS on analysts' information environment, as measured by forecast errors, forecast dispersion, and analyst following. To control for the effect of confounding concurrent events, we use a control sample of firms that had already voluntarily adopted IFRS at least two years prior to the mandatory adoption date. We find that analysts' absolute forecast errors and forecast dispersion decrease relative to this control sample only for mandatory adopters domiciled in countries with both strong enforcement regimes and domestic accounting standards that differ significantly from IFRS. Furthermore, for mandatory adopters located in countries with both weak enforcement regimes and domestic accounting standards that differ significantly from IFRS, analysts' forecast errors and dispersion decrease more for firms with stronger reporting incentives. Overall, these results suggest that mandatory IFRS adoption improves analysts' information environment only when the changes mandated by IFRS are substantial and rigorously enforced.

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