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IFRS reporting, firm-specific information flows, and institutional environments: international evidence

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Abstract This study investigates whether and how a firm's voluntary adoption of International Financial Reporting Standards (IFRS) influences the extent to which firm-specific information is capitalized into stock prices measured by stock price synchronicity. We also study the role of analyst following and institutional environments in determining the relation between IFRS reporting and synchronicity. Using firm-level data from 34 countries, we find that synchronicity is significantly lower for IFRS adopters than for non-adopters across all regression specifications and that for IFRS adopters it decreases from the pre-adoption period to the post-adoption period. This finding supports the view that voluntary IFRS adoption facilitates the incorporation of firm-specific information into stock prices, thereby reducing synchronicity. We also find that the synchronicity-reducing effect of IFRS adoption is attenuated (accentuated) for firms with high (low) analyst following and is stronger (weaker) for firms in countries with poor (good) institutional environments.

Keywords International Financial Reporting Standards (IFRS) · Firm-specific information flows · Analyst coverage · Institutional environment

JEL Classification M16 · G14 · M48

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1 Introduction

This study investigates whether and how a firm's voluntary adoption of International Financial Reporting Standards (IFRS) influences firm-specific information flow into the market, particularly the extent to which firm-specific information is incorporated into stock prices relative to common (industry-wide and/or market-wide) information. While mandatory IFRS adoption is a country-level regulatory event that aims to enhance the quality of public disclosure, voluntary IFRS adoption can be viewed as an individual firm's strategic commitment to better reporting or enhanced disclosures (Leuz and Verrecchia 2000; Covrig et al. 2007). This commitment is costly and thus credible, because it is difficult for IFRS adopters to reverse the decision, once adopted, since IFRS adoption requires nontrivial efforts and resources on the part of preparers of financial statements and their auditors.[1]

Although some studies (for example, Ball 2001; Ball et al. 2003) cast doubt on the effect of a country-wide adoption of high-quality accounting standards without supporting institutional infrastructures, other recent studies claim that an individual firm's decision to voluntarily adopt IFRS leads to desirable economic consequences and provide evidence suggesting that public disclosures under IFRS are, on average, of higher quality than those under local accounting standards in most financial reporting regimes. Specifically, these studies find that voluntary IFRS adoption is associated with less accounting flexibility and fewer analyst forecast errors (Ashbaugh and Pincus 2001), lower costs of capital (Kim and Shi 2010; Daske et al. 2011), higher market liquidity and trading volume (Leuz and Verrecchia 2000), larger earnings response coefficients (Bartov et al. 2005), better accounting quality (Barth et al. 2008), a convergence of accounting numbers under IFRS with those under U.S. Generally Accepted Accounting Principles (GAAP; Leuz 2003), increased investment flows due to the attraction of more foreign mutual funds (Covrig et al. 2007), and more favorable price and non-price terms of loan contracts (Kim et al. 2011). Overall, the findings of these studies suggest that IFRS adoption leads to an increase in the quantity and/or quality of firm-specific information.

Enhanced disclosures via voluntary IFRS adoption could affect market participants' incentives to collect, process, and trade on firm-specific information. Voluntary IFRS adoption can thus improve a firm's information environment by facilitating the flow of firm-specific information into the market. In such a case, voluntary IFRS adoption causes stock prices to co-move more (less) closely with firm-specific (common) information, thereby decreasing stock price synchronicity.[2] However, the aforementioned studies are, in general, silent on the question of whether enhanced disclosures via voluntary IFRS adoption encourage or discourage the incorporation of firm-specific information into stock prices. Our analyses therefore focus on whether and how voluntary IFRS adoption facilitates the price

[1] For example, Kim et al. (2012) provide evidence that mandatory IFRS adoption increases audit fees.

[2] The large amount of firm-specific information capitalized into stock prices means that the stock prices are less synchronous with market and/or industry returns. We therefore use the terms *higher firm-specific information flow* and *lower stock price synchronicity* interchangeably.

formation process in which firm-specific information is capitalized into stock prices in an accurate and timely manner.

For our empirical tests, we first construct a large sample of firm-year observations from (non-U.S. and non-Canadian) firms from 34 countries that voluntarily adopted IFRS over the seven years from 1998 through 2004 (hereafter IFRS adopters) and those that did not adopt IFRS over the same period (hereafter non-adopters). Since IFRS adoption was not a requirement during our sample period, IFRS adopters in our sample could be viewed as having made a voluntary commitment to better reporting strategies by adopting higher-quality reporting standards, namely, IFRS. We then compare the level of stock price synchronicity between IFRS adopters and non-adopters after controlling for known determinants of synchronicity.

Briefly, our results reveal the following. First, we find that synchronicity is significantly lower for IFRS adopters than for non-adopters across all empirical specifications considered in the paper. We also find that, for IFRS adopters, synchronicity decreases significantly from the pre-adoption period to the post-adoption, and that the decrease is not due to the pre-existing differences in the synchronicity of IFRS adopters. These findings support the view that IFRS adoption improves the information environment by facilitating firm-specific information flows into the market. Second, we find that the synchronicity-reducing effect of IFRS adoption is more pronounced for firms with low analyst followings than for firms with high analyst followings. Finally, we provide evidence that synchronicity decreases with the strength of a country's institutions. More importantly, we find firm-level evidence that the synchronicity-reducing effect of IFRS adoption is greater for firms in countries with weak institutions than for those in countries with strong institutions. The finding supports the view that firm-level disclosure strategies such as voluntary IFRS adoption and country-level institutional factors act as *substitutes* for each other.

Our study adds to the literature in the following ways. First, to our knowledge, our study is the first to provide systematic evidence of the synchronicity-reducing role of voluntary IFRS reporting. Second, since our focus is on firm-level voluntary adoption, our study distinguishes itself from that of Beuselinck et al. (2009), which focuses on country-level mandatory adoption.[3] Third, our study extends and

[3] While the European Union (EU) mandated the use of IFRS in the preparation of consolidated financial statements starting in 2005, many other countries are still in the process of converging local GAAP with IFRS. Using a sample of firms from 14 EU countries that were mandated to adopt IFRS starting in 2005, Beuselinck et al. (2009) also find that synchronicity decreases in the year of mandatory adoption, compared with that in the pre-adoption period. However, studying the effect of IFRS adoption in a mandatory setting can create other problems: using a single year (2005) as the benchmark ignores other regulatory changes that can occur simultaneously with mandatory IFRS adoption. In this regard, our sample of both voluntary adopters and non-adopters is less likely to suffer from this problem, since firms decide to voluntarily adopt IFRS in different years. In addition, we find that some of the results provided by Beuselinck et al. (2009) are not easy to interpret. For example, in their Table 4, they find that synchronicity reduces more for the EU Mandatory group than for the EU Late Adopt group, but in all three periods of interest the former group is associated with greater synchronicity than the latter group. The evidence actually implies that mandatory IFRS adoption is not the reason for the decrease in synchronicity, since synchronicity decreases even for the group that has not yet been required to adopt IFRS.

complements that of Fernandes and Ferreira (2008) by providing evidence that synchronicity decreases with IFRS adoption, even after controlling for a firm's cross-listing and a country's legal institutions. Fernandes and Ferreira (2008) find that U.S. cross-listing improves the incorporation of firm-specific information into stock prices. Unlike IFRS adoption, the cross-listing of non-U.S. firms on U.S. exchanges causes cross-listed firms to voluntarily subject themselves to a more stringent legal liability and enforcement regime, in addition to a stronger disclosure regime (Doidge et al. 2004; Choi et al. 2009). Therefore it is not clear whether the decreased synchronicity observed for U.S. cross-listed firms is driven by an upward shift in legal regime or enhanced disclosures associated with cross-listing on U.S. exchanges. Non-U.S. firms' IFRS adoptions, which cause *no* shift in legal regime, provide us with a better controlled setting in which the effect of enhanced disclosures can be effectively isolated from the effect of an upward shift in legal regime. Finally, to our knowledge, our study is one of the few, if not the first, to evaluate the interaction of firm-level disclosure strategies and country-level governance mechanisms in influencing firm-specific information flow in the market. Given the scarcity of empirical evidence on the issue, our results help us better understand how firm-level disclosure strategies interplay with country-level institutional infrastructures in relation to IFRS adoption.

Section 2 explains background and research questions. Section 3 describes our sample and explains our variable measurement. Section 4 presents descriptive statistics and the results of univariate tests. Section 5 discusses empirical procedures. Section 6 presents the results of the main regressions, while Sect. 7 reports the results of a variety of robustness tests. Section 8 examines the conditioning effect of institutional infrastructure on our results. The final section concludes the paper.

2 Background and research questions

2.1 Does IFRS adoption increase firm-specific information flow?

Observed stock prices reflect both common information and firm-specific information. Prior studies show that the enhanced flow of firm-specific information into the market increases firm-specific return variation, which in turn lowers stock price synchronicity (Durnev et al. 2003; Piotroski and Roulstone 2004; Fernandes and Ferreira 2008). We therefore use stock price synchronicity to capture the extent of firm-specific information flow to stock prices. Roll (1988) argues that synchronicity is inversely associated with the intensity of informed trading based on firm-specific private information. Furthermore, Ferreira and Laux (2007) point out that the inverse of synchronicity is "a good summary measure of information inflow, especially for private information about firms" (p. 952).[4]

[4] Firm-specific information consists of both public and private information. However, the synchronicity measure used in previous studies cited above (as well as in the current study) does not isolate the private information-related part of synchronicity from the public information part. Stock price synchronicity can thus be viewed as an *indirect* measure of market participants' private information-gathering activities.

In a different context, Kim and Verrecchia (1994, 1997) develop a model in which low-frequency public disclosures of higher-quality public information (for example, via IFRS adoption) may encourage informed traders, or "elite information processors," to collect additional private information and/or process publicly available information into value-relevant private information. Recent research provides further evidence corroborating the above view. For example, Ferreira and Laux (2007), Fernandes and Ferreira (2008), Hutton et al. (2009), and Gul et al. (2010) show that enhanced disclosure resulting from cross-listing, better governance, less earnings management, and higher audit quality, respectively, leads to lower synchronicity.

Furthermore, Jin and Myers (2006) predict and find that information opacity causes greater stock price co-movement by forcing outside investors to rely more (less) on common information (firm-specific information). In their model, lack of firm-specific information "affects the division of risk bearing between inside managers and outside investors" (Jin and Myers 2006, p. 258) by allowing insiders to capture some part of cash flows that are higher than what outsiders perceive and forcing insiders (outside investors) to absorb more firm-specific variance (common variance), which in turn leads us to observe higher synchronicity.

Veldkamp's (2006a) model of information markets also implies that voluntary IFRS adoption could lead to a decrease in stock price co-movement. The author's model shows that co-movement can be driven by information markets: when higher-quality firm-specific information is not readily available, investors rely more on common information. Common information is cheaper to acquire than firm-specific information because it typically has a higher demand or broader user base in the information market.[5] To the extent that enhanced disclosures via IFRS adoption facilitate the flow of higher-quality firm-specific information into the market at no additional (or cheaper) cost, investors are likely to rely more on firm-specific information than on common information.

Given evidence that IFRS reporting improves the quality of a firm's financial reporting (for example, Barth et al. 2008), an important implication from the aforementioned studies is that voluntary IFRS adoption facilitates the flow of firm-specific information into the market and thus motivates outside investors to rely more (less) on firm-specific (common) information when making their trading decisions. As a result, the amount of firm-specific information incorporated into stock prices increases, or, equivalently, stock price synchronicity decreases. Similar in spirit to Ferreira and Laux (2007), we conveniently call this effect the *information encouragement* role of IFRS adoption.

On the other hand, an increase in the quantity and quality of public information associated with IFRS adoption may lower the profitability of acquiring firm-specific private information and thus discourage informed traders from collecting and trading on private information as more and better (firm-specific) information

[5] In Veldkamp's model, information production involves a large amount of fixed cost, and high-demand information has a lower per unit cost of production and thus is available to information users at a lower cost than low-demand information.

becomes publicly available (Kim and Verrecchia 2001). In such a case, stock prices become more synchronous or co-move more closely with common information (Dasgupta et al. 2010). We call this effect the *crowding-out* role of IFRS adoption.

Given the lack of evidence on which effect is supported or dominating, our first objective is to test whether and how voluntary IFRS adoption influences the incorporation of firm-specific information into stock prices or stock price synchronicity. A negative (positive) relation between IFRS adoption and synchronicity is consistent with the information encouragement (crowding out) effect of IFRS adoption.

2.2 Does analyst following matter?

Prior research provides evidence suggesting that analysts are involved primarily in the production of common information rather than the costly acquisition of firm-specific private information (Piotroski and Roulstone 2004; Chan and Hameed 2006; Ferreira and Laux 2007). Furthermore, Veldkamp (2006a, b) suggests that analysts have greater incentives to acquire and disseminate common (less expensive) information than firm-specific (more expensive) information for two reasons. First, in the information market, common information does, in general, have a larger demand or user base than firm-specific information. Second, the unit cost of producing common information is much smaller than that of firm-specific information, given the high fixed cost of information production. As a result, analysts are more likely to acquire, process, and disseminate common information than firm-specific information. This implies that a greater (smaller) amount of common information, relative to firm-specific information, is available for firms with higher (lower) analyst followings. As a result, a greater amount of common information (relative to firm-specific information) is capitalized into stock prices for firms with higher analyst followings, which leads us to observe a positive relation between analyst following and synchronicity.

Moreover, the synchronicity-reducing effect of IFRS adoption can be attenuated for firms with higher analyst followings: in the information market, firm-specific information produced via IFRS and common information produced by analysts compete with each other, all else being equal. Since analysts produce a greater amount of common information and make it available to the market at a relatively low cost, investors are likely to rely more heavily on analyst-produced common (and less expensive) information than on firm-specific (and more expensive) information. One may therefore expect that the synchronicity-reducing effect of enhanced disclosure, for example, via IFRS adoption is likely to be higher (lower) for firms with low (high) analyst followings.

Given the lack of empirical evidence on the above issue, we aim to provide evidence on whether and how the effect of IFRS adoption on synchronicity is conditioned upon the intensity of analyst following. In so doing, we first establish the relation between synchronicity and analyst following and then examine whether the relation between IFRS adoption and synchronicity differs systematically between firms with high analyst coverage and those with low coverage.

2.3 Does institutional environment matter?

Previous studies show that synchronicity decreases with the strength of a country's property right protection (Morck et al. 2000) and level of accounting transparency (Jin and Myers 2006). It is therefore interesting to examine whether and how the strength of a country's institutions matters in determining the IFRS adoption effect on synchronicity. One stream of research (for example, Ball 2001; Ball et al. 2003; Berkowitz et al. 2003) suggests that the mere adoption of higher-quality accounting standards such as IFRS is not sufficient to improve the quality of accounting information unless a country's enforcement mechanisms work effectively or firms have incentives to voluntarily communicate higher-quality information to the market. In particular, Ball (2001) argues that, in the absence of appropriate incentives or effective enforcement mechanisms, higher-quality standards themselves do not improve the quality of financial reporting. Recent studies by Durnev and Kim (2005), Francis et al. (2005), Hope et al. (2006), and Burgstahler et al. (2006) provide evidence corroborating the above argument. For example, Burgstahler et al. (2006) find that a firm's reporting incentives and a country's legal institutions reinforce each other in providing high-quality financial reporting. In this *reinforcement* scenario, one can expect the synchronicity-reducing effect of IFRS adoption to be more pronounced when IFRS adopters are from countries with strong institutions than when they are from countries with poor institutions.

The other stream of research predicts and finds that country- and firm-level governance mechanisms act as substitutes for each other. A major argument here is that strong country-level governance significantly ameliorates potential agency problems associated with poor firm-level governance and thus that the effect of country-level (firm-level) governance is of first-order (second-order) importance (Doidge et al. 2004; Lel and Miller 2008; Leuz et al. 2010). For example, Leuz et al. (2010) investigate whether country- or firm-level governance matters to U.S. investors' decision to invest in foreign stocks. Their study provides evidence that firm-level governance is less important in foreign equity investment by U.S. investors when country-level governance is strong. Similarly, Lel and Miller (2008) find that the effect of bonding by U.S. listing is greatest for firms domiciled in the countries with the weakest investor protections, which also implies a *substitution* effect between firm- and country-level governance mechanisms. In this *substitution* scenario, one can expect the synchronicity-reducing effect of IFRS adoption to be attenuated (accentuated) when IFRS adopters are from countries with strong (weak) institutions.

Given the mixed views and evidence on the interplay of firm-level disclosure strategy and country-level institutions, we aim to provide systematic evidence on which perspective—the reinforcement or the substitution scenario—is supported or dominating in the context of voluntary IFRS adoption. In so doing, we first establish the relation between synchronicity and the strength of institutions such as a country's governance and enforcement mechanisms. We then test whether and how the effect of IFRS adoption on synchronicity is conditioned upon institutional strength.

3 Data and variable measurement

3.1 Sample and data sources

The initial list of our sample consists of all non-U.S. and non-Canadian firms that are jointly included in the three databases Datastream, Worldscope, and IBES International for the sample period 1998–2004.[6] Our sample period ends in 2004 because all listed firms in EU countries were mandated to adopt IFRS starting in January 2005. We merged the three databases and excluded firms in the banking, insurance, and other financial industries[7] from the sample. All financial statement data, including a firm's adoption of particular accounting standards, are from Worldscope. All stock return data are from Datastream, while all data on analysts' earnings forecasts are from IBES International. When certain financial statement data were missing in Worldscope, we supplemented them with data, when available, from Compustat Global. We deleted firms if the data required to measure firm-specific control variables were not available from either Worldscope or Compustat Global. After applying the above selection criteria, we obtained 15,382 firm-years with IFRS adoption and non-adoption from 34 countries over the period 1998–2004.

3.2 Measurement of stock price synchronicity

Our dependent variable is stock price synchronicity for each firm–year, which captures the extent to which firm-specific information flows into stock prices. Similar to previous research (for example, Morck et al. 2000; Piotroski and Roulstone 2004; Gul et al. 2010), we measure stock price synchronicity using the R^2 statistics of the market model. Specifically, for each sample year in each country, we regress firm j's weekly returns (RET) on the current and prior week's value-weighted market return ($MKTRET$) and the current and prior week's value-weighted industry return ($INDRET$):

$$RET_{j,t} = a + b_1 MKTRET_{j,t-1} + b_2 MKTRET_{j,t} + b_3 INDRET_{j,t-1} + b_4 INDRET_{j,t} + \varepsilon_{i,t} \quad (1)$$

The industry return ($INDRET_{j,t}$) for a specific week t for the industry of firm j is created using all firms within the Worldscope general industry classification code. Here $INDRET_{j,t}$ is the value-weighted average of these firms' returns for week t (excluding firm j). Following Piotroski and Roulstone (2004), we include lagged return metrics to correct for potential autocorrelation problems. We require that a minimum of 40 weekly return observations be available for each firm in each year. As in other studies, synchronicity for firm j from each country in each sample year ($Synchronicity_j$) is defined as

[6] The year 1998 is chosen as the starting point because few IFRS adopters were identified before 1998.

[7] Worldscope data field 06010 describes the general industry classification of a specific firm. In our sample, firms in the financial service industries (06010 = 04, 05, or 06) were deleted.

$$Synchronicity_j = \log\left(\frac{R_j^2}{1 - R_j^2}\right) \quad (2)$$

We use the log transformation of R^2 to create a continuous variable from a variable that is bounded by zero and one, thus making our dependent variable more normally distributed. By construction, high values of *Synchronicity* indicate that individual firms' stock returns co-move closely with the market and/or industry returns, and thus the firm-specific return variation is low.[8] For the purpose of our study, an inverse relation between IFRS adoption and our synchronicity measure can be viewed as an indication that IFRS adoption facilitates the flow of firm-specific information into the market and its incorporation into stock prices or as evidence supporting the information encouragement role of IFRS adoption.[9]

3.3 Identification of IFRS adoption

The key independent variable of interest in this paper is the indicator variable representing a firm's decision to voluntarily adopt IFRS, denoted *DIFRS*. The *DIFRS* variable equals one for firm-year observations that adopt IFRS and zero otherwise. For example, during our sample period 1998–2004, if a firm voluntarily switches from local GAAP to IFRS in 2001 and follows IFRS afterward, *DIFRS* is coded as one for the firm over 2001–2004 and zero for the firm over 1998–2000.

We obtain information about IFRS adoption from Worldscope. Worldscope data field 07536 describes the accounting standards followed by a specific firm. "Appendix 1" provides a detailed description of the classification of accounting standards as recorded in Worldscope data field 07536. Worldscope identifies 23 different accounting standards adopted by non-U.S. firms, including (1) local accounting standards (07536 = 01), (2) International Accounting Standards (IAS) pronounced by IASC (07536 = 02), (3) IFRS pronounced by IASB (07536 = 23),[10] and (4) some hybrid-type accounting standards that partially adopt IAS or IFRS (07536 = 06, 08, 12, 16, 18, or 19) along with local standards. In our main analyses, a firm is identified as an IFRS adopter if it adopts a full set of IAS or IFRS

[8] We also measure synchronicity using a single-factor market model in which lagged terms and industry returns in Eq. (1) are excluded and repeat our regression estimations. Though not reported here for brevity (available upon request), we find that the results using this alternative measure are qualitatively identical to those reported in this paper.

[9] Note that the amount of (firm-specific versus common) information being incorporated into stock prices is measured in a relative sense. One can argue that IFRS adoption not only increases the informativeness of firm-specific information but also reduces that of common information. However, our inference on the synchronicity effect of voluntary IFRS adoption is unlikely to change, because in such a case a *relatively* large amount of firm-specific information than common information is capitalized into stock prices. Admittedly, however, a few studies raise questions about this information-based interpretation of synchronicity and provide evidence suggesting that synchronicity may reflect noises in stock returns that are not related to firm-specific information (Ashbaugh et al. 2006). As in many other studies, our tests are predicated upon the information-based interpretation of synchronicity, given that evidence in support of this interpretation is overwhelming and growing in the contemporary finance (and accounting) literature.

[10] The IASC is the abbreviation of International Accounting Standards Committee and IASB is the abbreviation of International Accounting Standards Board, which succeeded the IASC in 2001.

(07536 = 02 or 23) and as a non-adopter if it adopts local accounting standards or a partial set of IFRS in combination with local standards (07532 = 01, 06, 08, 12, 16, 18, or 19). Note that firms that adopt a partial set of IFRS are treated as non-adopters.[11]

Panel A of Table 1 presents the distribution of our sample by country. As shown, IFRS adopters are unevenly distributed across 34 countries: more than half of the total number of observations from Austria, the Czech Republic, Hungary, and Switzerland adopt IFRS, while 10 countries do not have any IFRS adopters.[12] The number of IFRS adopter observations is the highest in Germany and Switzerland (406 and 323, respectively). As shown in the last two columns of Panel A of Table 1, the mean R^2 statistics and our synchronicity measure are the highest in the Czech Republic and Russia, while they are the lowest in the United Kingdom and Australia, which is consistent, overall, with evidence reported in other cross-country studies on stock price synchronicity (Morck et al. 2000; Jin and Myers 2006).

Panel B of Table 1 presents the yearly distribution of IFRS adopters, R^2 statistics, and synchronicity. Both the number and percentage of IFRS adopters increase steadily over the years, reflecting a growing trend of adopting a full set of IFRS around the world during our sample period. We note a decreasing trend in both R^2 statistics and synchronicity during the first 3 years of our sample period, though we do not see a clear trend over the last 4 years.

4 Descriptive statistics and univariate comparison

Sections A–C of Table 2 present descriptive statistics of the variables included in our main regressions for the full sample ($N = 15{,}382$), the subsample of IFRS adopters ($N = 1{,}064$), and the subsample of non-adopters ($N = 14{,}318$), respectively, while section D reports test statistics for the mean and median differences between the IFRS adopter and non-adopter samples. "Appendix 2" provides the definitions of all variables used in our study. As shown in Panel A of Table 2, the mean and median of *Synchronicity* are −0.544 and −0.533, respectively, for our full sample, much higher than for U.S. firms.[13] This finding suggests that the U.S. market incorporates more firm-specific information into stock prices than non-U.S. markets. While the IFRS adopter sample has a lower level of synchronicity than the non-adopter sample, the difference between the two samples is insignificant, as

[11] This approach is conservative, compared with the approach under which firms are classified as non-adopters only if they adopt local standards (i.e., if 07532 = 01), in the sense that our approach is likely to introduce a conservative bias against the rejection of the null hypothesis that IFRS adoption has no effect on synchronicity. As a further analysis (see Sect. 7), we use alternative definitions of IFRS adopters to check whether partial IFRS adoption has an impact on synchronicity and find that the synchronicity impact of partial adoption is smaller than that of full adoption.

[12] The inclusion of observations from these 10 countries with no IFRS adopters into our sample is consistent with previous studies, such as those of Covrig et al. (2007) and Kim et al. (2011). As is further explained in Sect. 7, we re-estimate our main regressions after excluding observations from these 10 countries and find that their exclusion does not alter our statistical inferences on the variables of interest.

[13] For example, Piotroski and Roulstone (2004), who measure synchronicity in the same way as in this study, report a mean and median synchronicity of −1.742 and −1.754, respectively, for their U.S. sample.

Table 1 Sample distribution

	Number of adopters	Total no. of observations	% of adopters	R^2	*Synchronicity*
Panel A: Distribution of sample firms by country					
Country					
Argentina	0	45	0	48.90	−0.047
Australia	3	790	0.38	16.86	−0.817
Austria	61	99	61.62	27.59	−0.533
Belgium	34	169	20.12	25.69	−0.568
Brazil	0	91	0	34.37	−0.346
Czech Rep.	9	14	64.29	64.09	0.213
Denmark	25	229	10.92	21.80	−0.689
Finland	13	267	4.87	23.99	−0.615
France	17	987	1.72	24.33	−0.612
Germany	406	1,080	37.59	23.31	−0.641
Greece	8	127	6.30	46.61	−0.092
Hong Kong	38	625	6.08	31.26	−0.413
Hungary	40	46	86.96	44.75	−0.170
India	0	488	0	30.15	−0.435
Ireland	0	89	0	23.87	−0.614
Italy	2	327	0.61	29.58	−0.488
Japan	0	3,778	0	28.05	−0.492
Luxembourg	6	21	28.57	44.05	−0.142
Malaysia	1	545	0.18	38.38	−0.245
Mexico	0	122	0	36.63	−0.314
Netherlands	14	368	3.80	24.87	−0.596
New Zealand	0	133	0	23.62	−0.618
Norway	1	136	0.74	29.40	−0.472
Poland	14	56	25.00	42.31	−0.174
Portugal	3	64	4.69	34.82	−0.338
Russian Federation	6	15	40.00	62.10	0.228
Singapore	10	445	2.25	36.46	−0.294
South Africa	25	365	6.85	24.58	−0.585
Spain	0	14	0	27.65	−0.501
Sweden	4	320	1.25	27.97	−0.504
Switzerland	323	550	58.73	24.03	−0.614
Taiwan	0	594	0	43.28	−0.138
Thailand	0	356	0	33.91	−0.354
United Kingdom	1	2,027	0.05	15.93	−0.857
Total	1,064	15,382	6.92	26.86	−0.544
Panel B: Composition of samples with IFRS and local GAAP					
Year					
1998	53	1,467	3.61	34.08	−0.359
1999	77	1,507	5.11	22.93	−0.670

Table 1 continued

	Number of adopters	Total no. of observations	% of adopters	R^2	*Synchronicity*
2000	132	2,179	6.06	20.14	−0.739
2001	163	2,320	7.03	31.79	−0.401
2002	186	2,446	7.60	28.55	−0.496
2003	213	2,644	8.06	25.46	−0.566
2004	240	2,819	8.51	26.18	−0.562
Total	1,064	15,382	6.92	26.86	−0.544

Panel A reports the sample distribution by country. Panel B reports the sample distribution by year. Column 1 reports the number of IFRS adopters. Column 2 reports the total number of IFRS adopters and non-adopters. Column 3 reports the number of IFRS adopters as a percentage of the total number of observations. Column 4 reports the mean R^2 statistics for each sample, where R^2 is the coefficient of determination for the market model. Column 5 reports the mean *Synchronicity* for each sample, where *Synchronicity* is calculated as $\log(R^2/(1 - R^2))$

shown in section D of Panel A of Table 2. As is further explained below, however, both firm-specific and country-level controls are significantly different between IFRS adopters and non-adopters, except for *Leverage* and *Accr*. It is therefore necessary to control for these variables when assessing the effect of IFRS adoption on synchronicity.

The mean and median of analyst following, measured by the natural log of one plus the number of analysts following (*Foll*), are 1.859 and 1.792, respectively, with a standard deviation of 0.790. Both the mean and median of *Foll* are significantly greater for IFRS adopters than for non-adopters, suggesting that IFRS adopters attract more analysts than non-adopters.

As shown in Panels B and C of Table 2, the following is apparent with respect to firm-specific and country-level control variables, respectively. Compared with non-adopters, IFRS adopters are significantly larger (*Size*), have higher long-term earnings growth (*Growth*), are more exposed to foreign product markets (*ForSale*), are more likely to have their stocks cross-listed on foreign stock exchanges (*Cross*) and to appoint Big 4 auditors (*Big4*), have smaller numbers of firms in the industry (*Nind*), provide less frequent interim financial reports (*Freq*), and have greater earnings volatility (*StdROA*). We find, however, no significant differences in leverage (*Leverage*) or accounting accruals (*Accr*) between IFRS adopters and non-adopters. Descriptive statistics on country-level variables indicate that, on average, firms have a greater tendency to adopt IFRS in countries with larger differences between local GAAP and IFRS (*GDiff*), higher income (*GDP*), smaller stock markets in terms of the number of listed firms (*Nlist*), smaller geographical size (*CtySize*), and more stable GDP streams (*VarGDP*).

Table 3 presents the Pearson correlations between the variables included in our regressions. Several key relations are apparent. First, synchronicity is positively associated with analyst following (*Foll*), firm size (*Size*), leverage (*Leverage*), the relative percentage of foreign sales (*ForSale*), cross listing (*Cross*), and the choice of Big 4 auditors (*Big4*), while it is negatively correlated with the number of firms in

Table 2 Descriptive statistics and results of univariate tests for mean and median differences between the IFRS adopters and the non-adopters

	Section A Full sample ($N = 15{,}382$)			Section B IFRS adopters: *DIFRS* = 1 ($N = 1{,}064$)			Section C Non-adopters: *DIFRS* = 0 ($N = 14{,}318$)			Section D Test for diff. in mean and median	
	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD	*t* Value	*z* Value
Panel A: Dependent variable											
Synchronicity	−0.544	−0.533	0.492	−0.546	−0.570	0.544	−0.544	−0.529	0.488	0.10	−1.40
Panel B: Firm-specific variables											
Foll	1.859	1.792	0.790	2.137	2.197	0.851	1.838	1.792	0.781	11.12***	10.88***
Size	6.048	5.899	1.747	6.254	6.180	1.937	6.032	5.880	1.731	3.62***	3.61***
Leverage	0.232	0.218	0.180	0.227	0.230	0.186	0.233	0.218	0.179	−0.96	−0.16
Growth	−1.889	−1.897	1.195	−1.773	−1.827	1.016	−1.897	−1.904	1.207	3.81***	3.28***
ForSale	0.105	0	0.198	0.207	0.087	0.256	0.097	0	0.191	13.72***	15.52***
Cross	0.297	0	0.457	0.574	1	0.495	0.276	0	0.447	19.07***	20.52***
Accr	1.656	0.561	4.312	1.586	0.561	4.166	1.662	0.561	4.323	−0.55	−0.51
Big4	0.467	0	0.499	0.638	1	0.481	0.454	0	0.498	11.68***	11.63***
Nind	8.440	8.654	1.507	7.340	7.418	1.337	8.522	8.733	1.487	−27.59***	−27.91***
Freq	1.902	2	0.945	1.432	1	0.875	1.937	2	0.941	−18.07***	−20.42***
StdROA	0.045	0.022	0.098	0.054	0.030	0.080	0.044	0.022	0.100	3.80***	7.83***
Panel C: Country-level variables											
GDiff	7.258	9	4.305	10.945	12	2.969	6.984	9	4.263	40.52***	33.70***
GDP	2.274	2.318	0.127	2.310	2.317	0.066	2.271	2.319	0.130	16.80***	9.53***
Nlist	6.856	6.869	1.069	5.878	5.666	0.792	6.929	6.990	1.051	−40.71***	−30.51***
CtySize	12.343	12.786	1.955	11.587	11.441	1.552	12.399	12.842	1.970	−16.12***	−21.23***
VarGDP	4.537	1.476	8.718	2.197	1.145	4.970	4.711	1.476	8.910	−14.83***	−6.83***

The variables and data sources are described in "Appendix 2". Section A reports the descriptive statistics for the full sample. Section B reports the descriptive statistics for the subsample of IFRS adopters. Section C reports the descriptive statistics for the subsample of non-adopters. Section D tests the difference between the IFRS adopters and non-adopters. The *t* value tests the difference of the mean value between the two groups, and the *z* value tests the difference of the median value between the two groups. Here *, **, and *** indicate the 10, 5, and 1 % levels of significance, respectively, for a two-tailed test

Table 3 Pearson correlation matrix

Variable	*DIFRS*	*Synchro-nicity*	*Foll*	*Size*	*Leverage*	*Growth*	*For-Sale*	*Cross*	*Accr*	*Big4*	*Nind*	*Freq*	*StdROA*	*GDiff*	*GDP*	*Nlist*	*Cty-Size*
Synchro-nicity	−0.001																
	(.913)																
Foll	0.096	0.308															
	(<.001)	(<.001)															
Size	0.032	0.349	0.672														
	(<.001)	(<.001)	(<.001)														
Leverage	−0.008	0.076	0.043	0.077													
	(.321)	(<.001)	(<.001)	(<.001)													
Growth	0.027	0.006	−0.131	−0.183	0.070												
	(.001)	(.422)	(<.001)	(<.001)	(<.001)												
ForSale	0.141	0.069	0.113	0.161	0.034	−0.039											
	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)											
Cross	0.165	0.057	0.150	0.263	0.098	−0.006	0.079										
	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(.477)	(<.001)										
Accr	−0.004	0.004	−0.072	−0.117	0.054	0.108	−0.043	−0.022									
	(.582)	(.583)	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(.006)									
Big4	0.094	−0.031	0.111	−0.009	−0.005	−0.035	0.096	−0.181	−0.003								
	(<.001)	(<.001)	(<.001)	(.272)	(.572)	(<.001)	(<.001)	(<.001)	(.711)								
Nind	−0.199	−0.139	−0.147	0.029	−0.144	−0.005	0.060	0.163	−0.033	−0.331							
	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(.510)	(<.001)	(<.001)	(<.001)	(<.001)							
Freq	−0.136	−0.063	−0.101	0.007	0.026	−0.013	−0.018	−0.003	−0.008	−0.144	0.231						
	(<.001)	(<.001)	(<.001)	(.362)	(.001)	(.121)	(.029)	(.742)	(.298)	(<.001)	(<.001)						
StdROA	0.025	−0.050	−0.113	−0.172	−0.071	0.145	−0.009	−0.087	0.062	0.088	−0.058	−0.070					
	(.002)	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(.240)	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)					
GDiff	0.233	0.118	0.109	0.109	0.072	0.084	−0.164	0.253	0.053	−0.199	−0.115	−0.115	−0.096				
	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)				

Table 3 continued

Variable	*DIFRS*	*Synchro-nicity*	*Foll*	*Size*	*Leverage*	*Growth*	*For-Sale*	*Cross*	*Accr*	*Big4*	*Nind*	*Freq*	*StdROA*	*GDiff*	*GDP*	*Nlist*	*Cty-Size*
GDP	0.077	−0.159	−0.045	0.113	−0.034	−0.023	0.128	0.184	−0.002	0.015	0.252	0.114	−0.027	0.104			
	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(.004)	(<.001)	(<.001)	(.764)	(.071)	(<.001)	(<.001)	(<.001)	(<.001)			
Nlist	−0.249	−0.092	−0.117	0.118	−0.072	−0.054	−0.009	0.133	−0.072	−0.399	0.716	0.206	−0.075	−0.188	0.012		
	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(.241)	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(.131)		
Cty-Size	−0.105	−0.122	−0.020	0.038	0.028	−0.016	−0.183	0.076	−0.018	−0.095	0.148	−0.056	0.001	0.188	−0.279	0.320	
	(<.001)	(<.001)	(.015)	(<.001)	(<.001)	(.043)	(<.001)	(<.001)	(.028)	(<.001)	(<.001)	(<.001)	(.870)	(<.001)	(<.001)	(<.001)	
Var-GDP	−0.073	0.231	0.073	−0.061	0.047	0.049	0.030	−0.106	0.002	0.027	−0.033	0.109	−0.012	−0.199	−0.205	−0.149	−0.487
	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)	(.824)	(.001)	(<.001)	(<.001)	(.154)	(<.001)	(<.001)	(<.001)	(<.001)

The variables and data sources are described in "Appendix 2". The *p* values are presented in parentheses

the industry (*Nind*), reporting frequency (*Freq*), and earnings volatility (*StdROA*). Second, the correlations between firm-specific control variables are not very high, with a high correlation of −0.331 between *Nind* and *Big4*. Third, as expected, the country-level control variables are relatively highly correlated with each other. Finally, though only suggestive of the underlying relation, the correlation between *DIFRS* and *Synchronicity* is negative but insignificant.

5 Empirical model

As mentioned earlier, IFRS adoption by our sample firms is voluntary. For this reason, examining the effect of IFRS adoption on synchronicity in a single-equation regression context may create problems of self-selection bias. To alleviate concerns over self-selection bias, we employ a Heckman-type two-stage treatment effect approach. In the first stage, we estimate a probit IFRS adoption model in which the likelihood of IFRS adoption, denoted by *DIFRS**, is regressed on a set of firm-specific variables that are deemed to influence the demand for IFRS reporting:

$$\begin{aligned} DIFRS* = {} & \beta_0 + \beta_1 Size + \beta_2 Leverage + \beta_3 Growth + \beta_4 ForSale + \beta_5 Cross \\ & + (Year\,fixed\,effect) + (Industry\,fixed\,effect) + (Country\,fixed\,effect) \\ & + error\,term \end{aligned} \tag{3}$$

where *DIFRS** is ex post coded as one for IFRS adopters and zero otherwise. We include *Size*, *Leverage*, and *Growth* because larger, less leveraged, and growing firms are more likely to adopt IFRS (Dumontier and Raffournier 1998; Barth et al. 2008). We include the percentage of foreign sales (*ForSale*) because the demand for IFRS reporting is likely to increase as a firm receives more exposure to the foreign product market (Dumontier and Raffournier 1998; Kim et al. 2011). We include the cross-listing indicator variable (*Cross*) to capture the effect of a firm's exposure to foreign capital markets on IFRS adoption (Dumontier and Raffournier 1998; Cuijpers and Buijink 2005). In addition, *Country fixed effects* are included to control for cross-country differences in the demand for better reporting strategies. The variables *Year* and *Industry fixed effects* are included to control for year and industry fixed effects.

In the second stage, we estimate the following regression, which links synchronicity with the IFRS adoption indicator (*DIFRS*), the number of analysts following (*Foll*), the interaction term (*DIFRS*Foll*), firm-specific and country-level control variables, and industry and year fixed effects:

$$\begin{aligned} Synchronicity = {} & \gamma_0 + \gamma_1 DIFRS + \gamma_2 Foll + \gamma_3 DIFRS * Foll \\ & + \Sigma_k \gamma_k Firm\text{-}specific\ Controls_k \\ & + \Sigma_j \gamma_j Country\text{-}level\ Controls_j + \delta Lambda + (Year\,fixed\,effect) \\ & + (Industry\,fixed\,effect) + error\,term \end{aligned} \tag{4}$$

This equation includes a set of firm-level controls that are known from prior literature to influence the flow of firm-specific information in the market. First, Hutton et al. (2009) show that synchronicity is positively related to financial reporting

opacity associated with opportunistic earnings management. We thus include *Accr* to control for the potential effect of earnings management on synchronicity. Second, we control for auditor quality proxied by *Big4* because Gul et al. (2010) show a significantly negative relation between synchronicity and the appointment of a high-quality auditor, using a sample of Chinese listed firms. Third, following prior related research (Piotroski and Roulstone 2004; Chan and Hameed 2006; Gul et al. 2010), we also control for firm size (*Size*), industry size (*Nind*), the extent of debt financing (*Leverage*), and earnings volatility (*StdROA*). In addition, we control for a firm's reporting frequency (*Freq*), because more frequent financial reporting may facilitate firm-specific information flows into the market, which in turn lowers synchronicity. Finally, *Cross* is included in Eq. (4) to control for the possibility that cross-listed firms are likely to disclose multiple sets of financial statements using multiple accounting standards. By including *Cross* in Eq. (4), we intend to isolate the synchronicity effect of IFRS reporting from the same effect associated with cross-listing. The inverse Mills ratio, denoted by *Lambda*, is included to address potential self-selection bias.

We also control for a set of country-level factors that are deemed to affect synchronicity. The variable *GDiff* is included to control for differences in local GAAP and IFRS (Bae et al. 2008), and it controls for the overall quality of a country's accounting system. Prior studies on synchronicity (Morck et al. 2000; Jin and Myers 2006; Fernandes and Ferreira 2008) show that countries with different levels of economic and capital market developments have different levels of synchronicity. We therefore control for the level of a country's economic development (*GDP*), stock market development (*Nlist*), and volatility of economic growth (*VarGDP*). We include country size (*CtySize*) because Morck et al. (2000) suggest that the stock prices of firms in large countries are more likely to move independently than those in small countries.

In specifying Eqs. (3) and (4), we implicitly assume that the use of Heckman's two-stage treatment effect approach is appropriate to address concerns about self-selection bias. The first-stage probit model (Eq. 3) includes two firm-level variables, *ForSale* and *Growth*, that are excluded in the second-stage regression (Eq. 4), for the following reasons. First, by doing so, the model technically satisfies the requirement of exclusion restrictions (Wooldridge 2002; Lennox et al. 2012). Second, Eq. (3) includes these two variables because (1) The clients of firms with high exposure to foreign product markets are more likely to demand IFRS reporting, because foreign clients are more familiar with IFRS than local GAAP, and (2) evidence shows that firms with high growth potential are more likely to adopt IFRS (Barth et al. 2008). On the other hand, these two variables are excluded from Eq. (4) because there is no compelling reason to believe that a firm's exposure to foreign operations and growth potential systematically influence stock price synchronicity in a certain direction.[14]

[14] Note that the information about the percentage of foreign sales and long-term earnings growth forecasts is firm-specific but publicly available to market participants, including both domestic and foreign investors.

Admittedly, however, it is difficult to find valid instrumental variables in the first-stage regression that can be excluded from the second-stage regression with economic justifications (Lennox et al. 2012; Larcker and Rusticus 2010).[15] Therefore, one cannot completely rule out the possibility that the Heckman two-stage regression fails to address potential self-selection bias associated with voluntary IFRS adoption. To alleviate this concern, we further address the issue in Sect. 7.1, using different sampling methods and/or alternative econometric procedures.

6 Results of main regressions

6.1 Results of the baseline regression

Panel A of Table 4 presents the results of the probit regression in Eq. (3). As shown, the likelihood of IFRS adoption is significantly and positively associated with *Size*, *ForSale*, and *Cross*, while it is insignificantly associated with *Leverage* and *Growth.* The explanatory power of the model, as reflected in the pseudo-R^2 statistics, is 53 %, suggesting that our probit model explains the demand for IFRS reporting reasonably well.

In Panel B of Table 4, column 1 presents the results of our baseline regression in Eq. (4) without including *Foll* and *IFRS*Foll*. In Table 4, all reported t values for regression coefficients are on an adjusted basis, using standard errors corrected for firm-level clustering. As reported in column 1 of Panel B of Table 4, the coefficient of *DIFRS* is significantly negative (-0.087 with $t = -2.27$). This result is in line with the information encouragement role of IFRS adoption: voluntary IFRS reporting facilitates the incorporation of firm-specific information into stock prices, which, in turn, lowers stock price synchronicity.

6.2 The conditioning effect of analyst coverage

Evidence (Kim and Shi 2012) suggests that voluntary IFRS adoption leads to added analyst coverage, which may, in turn, affects synchronicity. Therefore, IFRS adoption may influence synchronicity directly, via its information encouragement role (direct effect), while it may also affect synchronicity indirectly by attracting more analysts (indirect effect). To separate the direct effect from the indirect effect, we first estimate Eq. (4) by including *Foll* only in column 2 of Panel B of Table 4; we then include *DIFRS*Foll*, along with *Foll*, in column 3.

As shown in column 2 of Panel B of Table 4, when *Foll* is added into our baseline regression, the coefficient of *Foll* is significantly positive, and the coefficient of *DIFRS* remains significantly negative (-0.093 with $t = -2.42$). The

[15] Lennox et al. (2012) point out that, when applying the Heckman two-stage regression, the test for selectivity bias (*Lambda*) and the endogenized regressor (*DIFRS*) may be highly correlated, thereby creating a serious multicollinearity problem. However, our test results are unlikely to be driven by this multicollinearity problem: Though not tabulated, we find the variance inflation factors (VIFs) for *Lambda* and *DIFRS* to be only 2.86 and 3.40, respectively.

Table 4 Results of two-stage regressions of stock price synchronicity on IFRS adoption and other variables

	Pred. sign	Coefficient	*t* value
Panel A: Results of the first-stage probit regression of IFRS adoption on its determinants			
Size	+	0.050***	3.61
Leverage	−	−0.078	−0.57
Growth	+	0.031	1.35
ForSale	+	1.161***	11.29
Cross	+	0.376***	6.96
Intercept	?	−6.301	−0.11
Year fixed effect		Yes	
Industry fixed effect		Yes	
Country fixed effect		Yes	
N		15,382	
Pseudo R^2		53.48 %	

	Pred. sign	(1)	(2)	(3)	(4) Within-country median adjustment	(5) Within-country median adjustment
Panel B: Results of the second-stage OLS regression of the synchronicity on IFRS adoption and other determinants						
DIFRS	−	−0.087**	−0.093**	−0.322***	−0.032*	−0.033*
		(−2.27)	(−2.42)	(−5.13)	(−1.76)	(−1.71)
Foll	+		0.025***	0.017**	0.017**	0.018**
			(3.09)	(2.15)	(2.00)	(2.08)
*DIFRS*Foll*	?			0.104***	0.103***	0.102***
				(4.09)	(4.05)	(4.04)
Accr	+	0.004***	0.004***	0.005***	0.004***	0.004***
		(5.57)	(5.61)	(5.56)	(5.27)	(5.17)

Table 4 continued

	Pred. sign	(1)	(2)	(3)	(4) Within-country median adjustment	(5) Within-country median adjustment
Big4	−	−0.008	−0.009	−0.009	0.007	−0.002
		(−0.78)	(−0.91)	(−0.88)	(0.88)	(−0.22)
Size	+	0.108***	0.100***	0.099***	0.098***	0.098***
		(33.15)	(23.99)	(23.96)	(21.02)	(21.00)
Nind	+	0.147***	0.149***	0.148***	0.079**	0.073**
		(11.44)	(11.55)	(11.56)	(2.23)	(2.07)
Leverage	?	−0.002	0.001	0.003	0.016	0.018
		(−0.09)	(0.05)	(0.11)	(0.69)	(0.76)
Freq	−	−0.023***	−0.021***	−0.021***	−0.010**	−0.011**
		(−5.08)	(−4.60)	(−4.56)	(−2.29)	(−2.41)
StdROA	?	0.152***	0.153***	0.157***	0.168***	0.163***
		(3.11)	(3.09)	(3.09)	(3.00)	(2.93)
Cross	−	−0.018	−0.018	−0.017	−0.007	−0.002
		(−1.51)	(−1.51)	(−1.42)	(−0.71)	(−0.21)
GDiff	+	0.018***	0.018***	0.018***		−0.002
		(12.78)	(12.70)	(12.82)		(−1.41)
GDP	−	−1.247***	−1.238***	−1.236***		−0.027
		(−20.76)	(−20.60)	(−20.68)		(−0.71)
Nlist	−	−0.150***	−0.149***	−0.149***		−0.011*
		(−12.16)	(−12.11)	(−12.19)		(−1.92)
CtySize	−	−0.043***	−0.043***	−0.043***		0.003
		(−11.94)	(−11.93)	(−11.96)		(1.02)

Table 4 continued

	Pred. sign	(1)	(2)	(3)	(4) Within-country median adjustment	(5) Within-country median adjustment
VarGDP	+	0.006***	0.005***	0.005***		−0.001
		(7.80)	(7.50)	(7.58)		(−0.61)
Lambda	?	0.019	0.021	0.029		
		(0.72)	(0.83)	(1.13)		
Intercept		1.947***	1.901***	1.922***	−0.042***	0.068
		(13.69)	(13.31)	(3.50)	(−3.97)	(0.64)
Year fixed effect		Yes	Yes	Yes	Yes	Yes
Industry fixed effect		Yes	Yes	Yes	Yes	Yes
N		15,382	15,382	15,382	15,382	15,382
R^2 (%)		34.13	34.20	34.41	20.42	20.49

The variables and data sources are described in "Appendix 2". Here *, **, and *** indicate the 10, 5, and 1 % levels of significance, respectively, for a two-tailed test. All reported *t* statistics are based on standard errors adjusted for clustering at the firm level. Columns 1–3 present the results using the Heckman (1979) approach, where the first-stage results are reported in Panel A. Column 1 includes *DIFRS* only in the model. In column 2, *Foll* is added to the model. In column 3, *DIFRS*Foll* is added. In columns 4 and 5, all the firm-level continuous variables are adjusted by country median values, with column 4 excluding all the country-level control variables and column 5 including all the country-level control variables. The results in columns 4 and 5 are presented using the ordinary least squares (OLS) approach

significantly negative coefficient of *DIFRS*, coupled with the significantly positive coefficient of *Foll*, indicates that the synchronicity-reducing effect of IFRS adoption (that is, direct effect) is not dominated by the synchronicity-increasing effect of analyst following associated with IFRS adoption (that is, indirect effect).[16] As shown in column 3 of Panel B, when both *Foll* and *DIFRS***Foll* are added into our baseline regression, the coefficient of *DIFRS* remains significantly negative, and the coefficient of the interaction term, *DIFRS***Foll*, is significantly positive. The significantly negative coefficient of *DIFRS* in column 3 of Panel B means that the synchronicity-reducing effect of IFRS adoption is unlikely to be driven by IFRS-induced analyst activities. The significantly positive coefficient of *DIFRS***Foll* indicates that the synchronicity-reducing effect of IFRS adoption is weakened for IFRS adopters with high analyst coverage, compared with the same effect for IFRS adopters with low coverage.

In short, our results suggest that the synchronicity-reducing effect of IFRS adoption is not dominated by the synchronicity-increasing effect of analyst coverage associated with IFRS adoption. The coefficients of our variables of interest are economically significant as well. For example, the results in column 3 of Panel B of Table 4 reveal that the adoption of a full set of IFRS decreases the logarithmic transformation of the relative co-movement of firm-specific returns, that is, *Synchronicity*, by 32.2 percentage points, or roughly 59 % of the average *Synchronicity* of our full sample. Furthermore, one unit increase in *Foll*, on average, increases our measure of synchronicity by 1.7 percentage points for non-adopters, while it increases *Synchronicity* by 12.1 ($\gamma_2 + \gamma_3$) percentage points for IFRS adopters.

6.3 Within-country median adjustment

One can argue that the firm-level relation observed between synchronicity and IFRS adoption could be unduly influenced by country-level factors that are not accounted for by our regression specifications. To ensure that the observed effect of IFRS adoption on synchronicity, as shown in columns 1–3 of Panel B of Table 4, is not driven by omitted country-level factors that are correlated with firm-specific factors included in Eq. (4), we also estimate our main regression after making a *within-country median* adjustment for firm-specific variables.[17] Columns 4 and 5 of Panel B of Table 4 present the results of the regressions using country median-adjusted values for all firm-specific continuous variables. Note that country-level controls are excluded in column 4, while they are included in column 5.[18]

As seen in both columns 4 and 5 of Panel B of Table 4, we find that our statistical inferences on the estimated coefficients of the variables of interest, *DIFRS* and

[16] Note that the significantly positive coefficient of *Foll* is consistent with evidence reported in the U.S. market (Piotroski and Roulstone 2004), in emerging markets (Chan and Hameed 2006), and around the world (Fernandes and Ferreira 2008).

[17] The within-country median (rather than the mean) adjustment is similar in spirit to the procedure used by Kim et al. (2011).

[18] We do not include the inverse Mills ratio (i.e., *Lambda*) in columns 4 and 5, because the coefficients of *Lambda* are insignificant across columns 1–3, and the use of the within-country median transformation alleviates concerns over possible endogeneity between *DIFRS* and *Synchronicity*.

*DIFRS*Foll*, remain unchanged: while the coefficients of *DIFRS* become a bit less significant, the coefficients of the interaction term, *DIFRS*Foll*, remain significant, with a positive sign in both columns 4 and 5. This suggests that our main regression results reported in columns 1–3 are unlikely to be driven by omitted country-level factors that are correlated with firm-specific controls included in Eq. (4).

6.4 Results on control variables

With respect to our control variables, the following are noteworthy. First, accounting opacity, which is proxied by the ratio of absolute accounting accruals to absolute operating cash flows (*Accr*), is positively related to synchronicity across all cases, a finding consistent with those of Ferreira and Laux (2007) and Hutton et al. (2009). Second, the coefficient of *Size* is significantly positive across all columns, indicating that returns on large stocks are more synchronized with the market relative to returns on small stocks (Fernandes and Ferreira 2008; Dasgupta et al. 2010). Third, the positive coefficient of *Nind* indicates that the stock prices of firms in a large industry tend to co-move more closely with the market than those in a small industry. Fourth, the coefficient of *Freq* is significantly negative, suggesting that frequent interim financial reporting fosters the production of firm-specific information and therefore leads to a decrease in synchronicity. Fifth, the coefficient of *StdROA* is significantly positive, suggesting that firms with volatile earnings have more synchronous stock prices. Sixth, the coefficient of *Cross* is, overall, negative, albeit insignificant at the conventional level, providing weak evidence that stock prices are less synchronous for cross-listed firms. Seventh, the coefficients of *Big4* and *Leverage* are insignificant. Finally, the coefficient of *Lambda* is insignificant, suggesting that self-selection bias may not be severe in our empirical model.

The coefficients of country-level control variables are, overall, highly significant, with expected signs across all columns of Panel B of Table 4, except column 5, where all firm-level continuous variables are adjusted for their country median values. Specifically, the significantly positive coefficient of *GDiff* suggests that large differences between IFRS and local GAAP lead to more synchronous stock prices. Moreover, stock prices are less synchronized with common factors in countries with high income (*GDP*), large stock markets (*Nlist*), and large geographical size (*CtySize*), and more synchronized in countries with less stable income streams (*VarGDP*). This finding is consistent with those of Morck et al. (2000) and Jin and Myers (2006).

7 Further analysis

7.1 Endogeneity

To the extent that firms with relatively low (or high) synchronicity are more likely to choose IFRS reporting, our analyses thus far may suffer from an endogeneity, or reverse causality, problem. To alleviate this potential endogeneity concern, we conduct additional analyses as explained below. First, if it is the IFRS adoption that

causes a decrease in synchronicity, one can expect synchronicity to decrease from the pre-adoption period to the post-adoption period, all else being equal. To test the above possibilities, we construct a reduced sample of IFRS adopter observations only (excluding firms that never adopt IFRS during the sample period) in the following ways. We first identify the year of adoption for IFRS adopters. We exclude observations in the year of adoption to more cleanly compare synchronicity between the pre- and post-adoption periods.[19] A specific IFRS adopter is included in the sample if it has more than 1 year of data both before and after the adoption year. After applying the above criteria, we obtain a reduced sample of 1,160 firm-year observations. Using this reduced sample of adopters only, we re-estimate Eq. (4), where *DIFRS* equals one for observations in the post-adoption period and zero for observations in the pre-adoption period. Column 1 of Table 5 presents the regression results using this reduced sample. As shown, we find that the coefficient of *DIFRS* (*DIRFS*FOLL*) remains significantly negative (positive), which is consistent with the results reported in Panel B of Table 4. This result is in line with the view that it is the change in accounting standards from local GAAP to IFRS that leads us to observe a decrease in synchronicity.

Second, we further examine whether the pre-adoption difference in synchronicity between IFRS adopters and non-adopters, rather than IFRS adoption itself, leads us to observe a decrease in synchronicity from the pre- to the post-adoption period. For this purpose, we construct another reduced sample by excluding all the observations for adopters in the post-adoption period (that is, observations with $DIFRS = 1$ in column 1 of Table 5). We then compare the level of synchronicity between "never-adopters" and "to-be adopters." Note that both never-adopters and to-be adopters (IFRS adopters that have not yet adopted IFRS in the pre-adoption period) are coded as $DIFRS = 0$ in the main test. If the pre-adoption difference in synchronicity between to-be adopters and never-adopters is a driving force for the observed difference in synchronicity between the pre- and post-adoption periods, one should observe a significant pre-adoption difference in synchronicity between these two distinct groups.

To test the above possibility, we first identify all IFRS adopters and their years of adoption. We then delete all the observations that belong to the post-adoption period. For these pre-adoption observations, we create an indicator variable, *Pre-adp*, that equals one for to-be adopters and zero for firms that never adopt IFRS during our sample period. We then re-estimate Eq. (4) by replacing *DIFRS* with *Pre-adp*. Column 2 of Table 5 presents the regression results. As shown, we find that the coefficient of *Pre-adp* (as well as the coefficient of *Pre-adp*Foll*) is insignificant, suggesting that there is no significant inter-group difference in synchronicity during the pre-adoption period. This result in column 2, coupled with that in column 1, lends support to the view that it is not the pre-adoption difference in synchronicity but, rather, IFRS adoption that leads us to observe a decrease in synchronicity from the pre-adoption to the post-adoption period.

Third, as shown in columns 1–3 of Panel B of Table 4, we include the inverse Mills ratio, denoted by *Lambda*, to address the self-selection problem associated

[19] Exclusion or inclusion of observations in the year of adoption produces statistically similar results.

Table 5 Results for addressing potential endogeneity concern

	Pred. sign	(1) Pre- versus post-adoption	(2) Never adopters versus to-be adopters	(3) 2SLS	(4) PSM sample	(5a) PSM sample, *GDiff* < Q1	(5b) PSM sample, Q1 ≤ *GDiff* < Q2	(5c) PSM sample, Q2 ≤ *GDiff* < Q3	(5d) PSM sample, Q3 ≤ *GDiff* < Q4
DIFRS or *Pre-adp*	−	−0.279**	−0.007	−0.545***	−0.251***	−0.073	−0.081	−0.392***	−0.508***
		(−2.20)	(−0.11)	(−6.96)	(−4.60)	(−0.50)	(−0.99)	(−4.22)	(−2.74)
Foll	+	0.009	0.024***	0.008	0.056***	0.014	0.040	0.139***	0.070
		(0.28)	(2.93)	(1.02)	(3.14)	(0.36)	(1.35)	(4.05)	(0.98)
*DIFRS*Foll* or *Pre-adp*Foll*	?	0.136***	−0.033	0.205***	0.106***	−0.020	0.004	0.170***	0.245***
		(3.46)	(−1.14)	(5.92)	(3.99)	(−0.32)	(0.11)	(3.94)	(3.50)
Accr	+	0.002	0.005***	0.004***	0.003*	0.003	0.004	0.001	0.008
		(0.74)	(5.81)	(5.51)	(1.88)	(1.13)	(1.38)	(0.42)	(1.57)
Big4	−	0.015	−0.008	−0.009	−0.002	0.074*	−0.012	−0.049*	0.019
		(0.48)	(−0.78)	(−0.92)	(−0.08)	(1.75)	(−0.36)	(−1.90)	(0.26)
Size	+	0.087***	0.097***	0.099***	0.078***	0.076***	0.083***	0.051***	0.130***
		(6.46)	(22.42)	(23.84)	(9.10)	(4.11)	(5.24)	(3.49)	(4.16)
Nind	+	0.030	0.148***	0.145***	0.099	0.287***	−0.736***	−0.448***	0.300**
		(0.34)	(11.69)	(11.33)	(1.57)	(3.15)	(−9.69)	(−3.78)	(2.55)
Leverage	?	−0.037	0.007	0.002	−0.060	0.014	−0.028	−0.094	−0.275
		(−0.52)	(0.29)	(0.10)	(−1.17)	(0.13)	(−0.36)	(−1.15)	(−1.45)
Freq	−	−0.022	−0.023***	−0.019***	−0.021*	0.023	−0.048***	−0.005	0.053**
		(−1.41)	(−4.97)	(−4.26)	(−1.91)	(0.88)	(−2.64)	(−0.28)	(2.14)
StdROA	?	0.502*	0.147***	0.152***	0.576***	0.362**	0.086	1.174***	1.083
		(1.98)	(2.92)	(3.04)	(3.86)	(2.22)	(0.37)	(3.67)	(1.09)
Cross	−	0.024	−0.014	−0.014	−0.009	−0.031	0.048	0.001	−0.026
		(0.64)	(−1.14)	(−1.17)	(−0.42)	(−0.67)	(1.10)	(0.03)	(−0.29)

Table 5 continued

	Pred. sign	(1) Pre- versus post-adoption	(2) Never adopters versus to-be adopters	(3) 2SLS	(4) PSM sample	(5a) PSM sample, *GDiff* < Q1	(5b) PSM sample, Q1 ≤ *GDiff* < Q2	(5c) PSM sample, Q2 ≤ *GDiff* < Q3	(5d) PSM sample, Q3 ≤ *GDiff* < Q4
GDiff	+	0.018***	0.018***	0.018	0.010***				
		(3.03)	(12.62)	(12.99)	(3.96)				
GDP	−	−2.469***	−1.210***	−1.236***	−2.494***	−2.735***	−23.487**	−2.654***	−3.931***
		(−5.12)	(−20.56)	(−20.72)	(−8.48)	(−6.00)	(−2.22)	(−4.68)	(−5.17)
Nlist	−	−0.082	−0.145***	−0.148***	−0.138**	−0.299***	0.165	0.545***	−0.203*
		(−0.85)	(−12.04)	(−12.11)	(−1.98)	(−2.80)	(0.70)	(3.63)	(−1.73)
CtySize	−	−0.027	−0.046***	−0.044***	−0.038***	−0.055***	0.257***	−0.144***	−0.092***
		(−1.37)	(−12.39)	(−12.08)	(−4.30)	(−4.94)	(2.75)	(−4.15)	(−2.94)
VarGDP	+	0.002	0.005***	0.005***	0.002	−0.004	−0.145*	−0.129***	0.004
		(0.48)	(7.54)	(7.61)	(0.78)	(−1.18)	(−1.83)	(−3.15)	(0.42)
Lambda	?	−0.015	0.029						
		(−0.27)	(0.84)						
Intercept	?	5.012***	1.871***	1.965***	5.114***	5.489***	54.985**	7.261***	7.728***
		(4.52)	(13.26)	(13.79)	(7.57)	(5.46)	(2.11)	(5.00)	(4.58)
Year fixed effect		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effect		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N		1,160	14,318	15,382	3,327	640	981	1,435	271

Table 5 continued

	Pred. sign	(1) Pre- versus post-adoption	(2) Never adopters versus to-be adopters	(3) 2SLS	(4) PSM sample	(5a) PSM sample, *GDiff* < Q1	(5b) PSM sample, Q1 ≤ *GDiff* < Q2	(5c) PSM sample, Q2 ≤ *GDiff* < Q3	(5d) PSM sample, Q3 ≤ *GDiff* < Q4
R^2 (%)		43.02	34.02	34.51	36.02	39.70	37.59	38.77	61.59

The variables and data sources are described in "Appendix 2". Here *, **, and *** indicate the 10, 5, and 1 % levels of significance, respectively, for a two-tailed test. All reported *t* statistics are based on standard errors adjusted for clustering at the firm level. Columns 1 and 2 present the results using the Heckman (1979) approach. In column 1 the change effect is tested using a pre- versus post-adoption sample. We first identify the year of adoption for each IFRS adopter and include the specific IFRS adopter into the sample if it has at least 1 year of data before and 1 year of data after the adoption, with the year of adoption excluded. In column 2, adopters during the pre-adoption period (to-be adopters) and firms that never adopt IFRS during the sample period (never-adopters) are compared. We first identify all the adopters and their year of adoption and then delete all the observations that belong to the post-adoption period. The to-be adopters are defined as adopters that have not yet adopted IFRS (*Pre-adp* equals one for to-be adopters and zero for firms that never adopt IFRS). In column 3, a two-stage least squares (2SLS) approach is estimated. A fitted value of *DIFRS* from the first-stage probit estimation in Eq. (3) is obtained and used as an instrument for *DIFRS* in the second-stage regression (Eq. 4). In column 4, a PSM sample is tested. The PSM sample is constructed by first finding a control sample that never adopts IFRS during our sample period. The control sample is matched to the firm-year observations that voluntarily adopt IFRS based on the predicted propensity score obtained from Eq. (3) within ±0.1 % of the range of the voluntary adopters. In addition, the never-adopters are required to be in the same country, industry, and year as the voluntary adopters. In columns 5a to 5d, the PSM sample is divided into four subsamples based on *GDiff*

with voluntary IFRS adoption and find that the coefficient of *Lambda* is insignificant, suggesting that self-selection bias may not be a serious problem. As a check, we also employ a two-stage least squares (2SLS) procedure to see whether our results are robust to different approaches of addressing the self-selection problem. For this purpose, we obtain the fitted value of *DIFRS* from the first-stage probit estimation in Eq. (3) and then use it in the second-stage regression in Eq. (4) in lieu of *DIFRS*. As reported in column 3 of Table 5, the coefficient of *DIFRS* is significantly negative and the coefficient of *DIFRS***Foll* is significantly positive, though the coefficient of *Foll* is insignificant. This finding suggests that our main results on the effect of IFRS adoption on synchronicity, as reported in Table 4, are robust to alternative treatments for potential self-selection bias.

Fourth, as an alternative approach to addressing the self-selection concern, we now employ the propensity score matching (PSM) approach.[20] Following previous research that uses the PSM approach (Morsfield and Tan 2006; Doyle et al. 2007; Lennox et al. 2012), we now construct a matched sample using the predicted likelihood (called the propensity score) of IFRS adoption. For this purpose, we first identify a control sample of firms that have never adopted IFRS during our sample period. We then match voluntary IFRS adopters to these never-adopters using the predicted likelihood (or propensity score) obtained from Eq. (3). In so doing, we use a maximum allowable range of propensity score of 0.1 %. In addition, we require that the never-adopters be in the same country, industry, and year as the voluntary adopters. The PSM process results in 3,327 firm-year observations with both IFRS adopters and never-adopters.[21] Using this PSM sample, we then re-estimate Eq. (4) after excluding *Lambda* from the regression model. As shown in column 4 of Table 5, the PSM results are qualitatively identical to our main regression results reported in Panel B of Table 4, suggesting that the latter are robust to alternative treatments of potential self-selection bias and/or endogeneity.

Finally, as the (pre-adoption) divergence between local GAAP and IFRS is the country-level characteristics, it is unlikely to be endogenous in the sense that a specific firm in our sample is unlikely to choose the country where it is located. To further alleviate concerns over possible endogeneity with respect to IFRS adoption, we investigate whether the synchronicity-reducing effect of IFRS adoption differs systematically between countries with large (pre-adoption) divergence between local GAPP and IFRS (that is, *GDiff*) and countries with small *GDiff*. If IFRS adoption is a driving force that causes a decrease in synchronicity, we predict that the synchronicity-reducing effect of IFRS adoption is greater for firms in countries with large *GDiff* than for firms in counties with small *GDiff*. The intuition behind the prediction is that if *GDiff* is small, then the synchronicity-reducing effects of IFRS adoption should also be small.

To test the above prediction, we utilize the PSM sample of observations with both IFRS adopters and never-adopters ($N = 3{,}327$). Specifically, we divided the

[20] The Heckman approach addresses concerns over selection based on unobservables, while the PSM approach addresses concerns over selection based on observables.

[21] Note here we adopt a 1-to-*N* (as opposed to 1-to-1) matching in the sense that all never-adopters that meet the maximum allowable range of propensity score of 0.1 % (and the same country, year, and industry requirements) are included into the PSM sample.

PSM sample into four groups based on the quartile cut-off points of *GDiff* (Q1 to Q4): that is, (1) firm-years with *GDiff* < Q1 ($N = 640$), (2) those with Q1 ≤ *GDiff* < Q2 ($N = 981$), (3) those with Q2 ≤ *GDiff* < Q3 ($N = 1{,}435$), and (4) those with Q3 ≤ *GDiff* ($N = 271$).[22] We then re-estimate Eq. (4) for each of these four groups after excluding *GDiff* and *Lambda*. As shown in columns 5a to 5d of Table 5, we find that the coefficient of *DIFRS* is not significantly different from zero in the first and second groups, while it is significantly negative in the third and fourth groups. More interestingly, the magnitude of the estimated coefficient (in absolute value) increases monotonically from the first group (with the lowest *GDiff*) to the fourth group (with the highest *GDiff*). The above results are consistent with the view that the inverse relation observed between synchronicity and IFRS adoption is unlikely to be driven by potential endogeneity associated with voluntary IFRS adoption. Stated another way, we alleviate concerns over potential endogeneity problems associated with a firm-level event by using the country-level exogenous variable (*GDiff*) as a benchmark for assessing the effect of the firm-level endogenous event, that is, voluntary IFRS adoption.

7.2 Full versus partial adopters

This section further tests whether the synchronicity-reducing effect of IFRS adoption is greater for firms that adopt a full set of IFRS (full adopters) than for those that adopt a partial set of IFRS (partial adopters). We hypothesize that the full adopters can convey a stronger and more credible signal to the market with respect to their commitment to enhanced disclosures than the partial adopters. To test this hypothesis, we construct two different samples: (1) the full-adopter sample, consisting of full IFRS adopters (07536 = 02 or 23) and non-adopters (07536 = 01), where partial adopters are excluded from the sample; and (2) the partial-adopter sample, consisting of full as well as partial IFRS adopters (07536 = 02, 06, 08, 12, 16, 18, 19 or 23) and non-adopters (07536 = 01), where both full and partial adopters are treated as IFRS adopters (*DIFRS* = 1). We then re-estimate our baseline regression, separately, for the full-adopter and partial-adopter samples. Columns 1a and 1b of Table 5 present the empirical results. We use the same benchmark for both the full-adopter and partial-adopter samples when defining the non-adopters—that is, *DIFRS* = 0—only when a firm adopts local standards (07356 = 01).[23]

As shown in columns 1a and 1b of Table 6, we find that the coefficients of *DIFRS* are significantly negative in both samples, indicating that both full and partial adoptions of IFRS facilitate the flow of firm-specific information into the market. Moreover, we find that the coefficient of *DIFRS* is more negative for the

[22] Specifically, the quartile cut-off points of *GDiff* are Q1 = 5.5, Q2 = 12, Q3 = 13. *GDiff* is a country-level variable and the number of firm-year observations is unequally distributed across our sample countries. As a result, sample size could differ across four subsamples that are constructed using the *GDiff* quartile cut-off points (i.e., Q1, Q2, and Q3).

[23] We also check the sensitivity of our results by deleting firms using IAS (07536 = 02). In this case, we define IFRS adopters as firms adopting IFRS (07536 = 23) only, because one can argue that IAS is older than IFRS and may be different. Though not reported here, for brevity, our results using this narrowly defined *DIFRS* remain qualitatively unchanged: The coefficient of DIFRS is −0.263 with $t = -3.83$.

Table 6 Results of alternative regressions for sensitivity tests

	Pred. sign	(1a) Full-adopter sample	(1b) Partial-adopter sample	(2) Excl. first adoption year	(3) WLS regression	(4) Firm fixed effects	(5) Fama–MacBeth	(6a) Year clustering	(6b) Year and firm double clustering
DIFRS	−	−0.097**	−0.075**	−0.323***	−0.362***	−0.159**	−0.338***	−0.322***	−0.322***
		(−2.44)	(−2.30)	(−4.52)	(−10.12)	(−2.01)	(−4.90)	(−4.77)	(−3.97)
Foll	+			0.019**	0.038***	0.033***	0.003	0.017	0.017
				(2.32)	(5.71)	(3.61)	(0.12)	(0.84)	(0.82)
*DIFRS*Foll*	?			0.101***	0.093***	0.087***	0.113***	0.104***	0.104***
				(3.37)	(4.59)	(3.76)	(5.97)	(6.46)	(4.14)
Accr	+	0.004***	0.004***	0.005***	0.002***	0.001	0.004***	0.005***	0.005***
		(5.37)	(5.54)	(5.69)	(3.71)	(1.43)	(4.49)	(5.66)	(5.28)
Big4	−	−0.005	−0.007	−0.008	−0.021***	−0.034**	−0.001	−0.009	−0.009
		(−0.45)	(−0.64)	(−0.78)	(−2.87)	(−2.35)	(−0.01)	(−0.51)	(−0.48)
Size	+	0.107***	0.108***	0.099***	0.077***	0.072***	0.105***	0.099***	0.099***
		(32.19)	(33.25)	(23.64)	(28.14)	(9.83)	(16.84)	(19.86)	(17.11)
Nind	+	0.146***	0.145***	0.149***	0.192***		0.150***	0.148***	0.148***
		(11.22)	(11.11)	(11.71)	(24.50)		(4.02)	(4.01)	(3.88)
Leverage	?	0.006	−0.001	0.007	0.002	0.017	0.018	0.003	0.003
		(0.23)	(−0.02)	(0.27)	(0.09)	(0.41)	(0.29)	(0.06)	(0.05)
Freq	−	−0.024***	−0.023***	−0.022***	−0.028***	−0.024***	−0.005	−0.021	−0.021*
		(−5.28)	(−5.12)	(−4.92)	(−7.59)	(−5.29)	(−1.05)	(−1.84)	(−1.80)
StdROA	?	0.146***	0.148***	0.155***	0.133***	0.117	0.130***	0.157***	0.157***
		(3.05)	(3.07)	(3.04)	(4.78)	(1.24)	(2.93)	(4.85)	(3.21)
Cross	−	−0.018	−0.020*	−0.018	0.024		−0.010	−0.017	−0.017
		(−1.43)	(−1.70)	(−1.48)	(0.91)		(0.31)	(−0.54)	(−0.52)

Table 6 continued

	Pred. sign	(1a) Full-adopter sample	(1b) Partial-adopter sample	(2) Excl. first adoption year	(3) WLS regression	(4) Firm fixed effects	(5) Fama–MacBeth	(6a) Year clustering	(6b) Year and firm double clustering
GDiff	+	0.018***	0.018***	0.018***	0.020***		0.015***	0.018***	0.018***
		(12.53)	(12.54)	(12.70)	(22.01)		(4.45)	(7.14)	(6.63)
GDP	−	−1.240***	−1.237***	−1.231***	−1.378***	−3.058***	−1.312***	−1.236***	−1.236***
		(−20.41)	(−20.29)	(−20.72)	(−35.40)	(−7.98)	(−7.26)	(−6.81)	(−6.63)
Nlist	−	−0.149***	−0.149***	−0.149***	−0.159***	0.208***	−0.178***	−0.149***	−0.149***
		(−12.06)	(−12.09)	(−12.23)	(−19.86)	(6.74)	(−5.29)	(−4.30)	(−4.17)
CtySize	−	−0.043***	−0.043***	−0.044***	−0.058***	−5.491***	−0.040***	−0.043***	−0.043***
		(−11.85)	(−11.91)	(−12.10)	(−24.47)	(−2.89)	(−3.93)	(−4.12)	(−4.00)
VarGDP	+	0.006***	0.006***	0.005***	0.005***	0.004***	0.005*	0.005*	0.005**
		(7.86)	(7.87)	(7.56)	(8.67)	(5.86)	(1.83)	(2.38)	(2.34)
Lambda	?	0.025	0.021	0.032	0.098***	−0.012	0.020	0.029	0.029
		(0.94)	(0.96)	(1.11)	(5.69)	(−0.30)	(0.65)	(0.94)	(0.80)
Intercept	?	1.930***	1.926***	1.905***	2.193***	62.103***	2.059**	1.922***	1.922***
		(13.50)	(13.54)	(13.45)	(22.77)	(3.05)	(3.98)	(3.89)	(3.82)
Year fixed effect		Yes	Yes	Yes	Yes	Yes		Yes	Yes
Industry fixed effect		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effect						Yes			
N		14,922	15,382	15,037	15,382	15,382	15,382	15,382	15,382

Table 6 continued

	Pred. sign	(1a) Full-adopter sample	(1b) Partial-adopter sample	(2) Excl. first adoption year	(3) WLS regression	(4) Firm fixed effects	(5) Fama–MacBeth	(6a) Year clustering	(6b) Year and firm double clustering
R^2 (%)		34.14	34.12	34.22	32.68	67.15		34.41	34.41

The variables and data sources are described in "Appendix 2". Here *, **, and *** represent the 10, 5, and 1 % levels of significance, respectively, for a two-tailed test. In column 1a, *DIFRS* is defined as one if a firm adopts a full set of IFRS and zero otherwise, and a firm is excluded from the sample if it adopts a partial set of IFRS. In column 1b, *DIFRS* is defined as one if a firm adopts either a partial or full set of IFRS and zero otherwise. While not tabulated, the *t* value of the difference in coefficients between columns 1a and 1b is 2.03, significant at less than the 5 % level. In column 2, the first year of adoption is excluded from the sample. In column 3, a WLS regression is estimated, with an equal weight assigned to each country. In column 4, firm fixed effects are included in the model. Three variables (*Nind*, *Cross*, and *GDiff*) are excluded since they do not vary over year, industry, or firm. The Fama–MacBeth regression is estimated in column 5. The reported coefficient estimate for each variable is the average value of seven annual coefficients obtained from annual regressions for each year over the 1998–2004 sample period, and the reported *t* statistics are based on the standard errors of the empirical distributions of each coefficient over 1998–2004. In column 6a, the *t* statistics are based on standard errors adjusted for clustering over years. In column 6b, the *t* statistics are based on standard errors adjusted for clustering over years and firms. In all columns, except for 3, 5, 6a, and 6b, the reported *t* statistics are based on standard errors adjusted for clustering at the firm level

full-adopter sample (-0.097 with $t = -2.44$) than for the partial-adopter sample (-0.075 with $t = -2.30$). This difference between the two coefficients is significant at less than the 5 % level. The above results suggest that the market perceives the adoption of a full set of IFRS to be a more credible commitment to enhanced disclosure than the adoption of a partial set of IFRS.

7.3 Additional robustness checks

Our main regression does not consider the timing of IFRS adoption. To check whether our results are driven by any unusual effect during the year of adoption, we exclude observations in the first year of adoption from our full sample and thus obtain a reduced sample of 15,037 firm-year observations. Column 2 of Table 6 reports the results using this reduced sample. As shown, the coefficient of *DIFRS* (*DIFRS***Foll*) remains significantly negative (positive), which is consistent with the results reported in Panel B of Table 4. This finding suggests that our main results are unlikely to be driven by an unusual year-of-adoption effect.

As shown in Panel A of Table 1, the number of firm-years for each country varies from 3,778 for Japan to 14 for Spain and the Czech Republic. To check whether our full-sample results reported in Panel B of Table 4 are unduly influenced by the unequal sizes of sample firms across different countries, we re-estimate Eqs. (3) and (4) using the weighted least squares (WLS) procedure with an equal weight assigned to each sample country. As shown in column 3 of Table 6, the WLS results are qualitatively similar to those reported in Panel B of Table 4, suggesting that the main regression results reported in Table 4 are robust to the unequal sizes of sample firms across different countries.

To check whether the observed relation between synchronicity and IFRS adoption is driven by time-invariant firm-specific effects that are not explicitly controlled for, we re-estimate Eq. (4) after adding firm fixed effects. Column 4 of Table 6 presents the results of regression with year, industry, and firm fixed effects.[24] As shown, the coefficients on our test variables remain qualitatively unaltered.

To address potential problems arising from residual cross-correlations, we also estimate Eqs. (3) and (4) for each of the seven sample years using annual observations. We then compute the average of seven annual regression coefficients and the associated standard errors using the Fama and MacBeth (1973) procedure. As reported in column 5 of Table 6, the results for our main variables of interest are qualitatively similar to those reported in Table 4.

As a further sensitivity check, we also estimate standards errors after applying clustering over year as well as (firm and year) double clustering, following the procedures spelled out by Gow et al. (2010) and Petersen (2009). The signs and

[24] In so doing, we exclude three variables, namely, *Nind*, *Cross*, *GDiff*, because we find that their inclusion produces undesirable coefficient estimates for these variables in our firm fixed effect regression. The STATA and SAS results of our firm fixed effect regression including these three variables show that the coefficients of these variables are biased for technical reasons. These biases may stem from the fact that these three variables do not vary much by year, industry, or firm. Specifically, *Nind* does not vary much across industry years, *Cross* is constant across firm years, and *GDiff* does not vary over years.

significance of *DIFRS* and *DIFRS*Foll*, as shown in columns 6a and 6b of Table 6, remain similar to those reported in Panel B of Table 4.

As shown in Panel A of Table 1, 10 countries (for example, Japan) have zero IFRS adopters, while two countries (Germany and Switzerland) have a large number of IFRS adopters. As further sensitivity checks, we re-estimate Eqs. (3) and (4) using two reduced samples: (1) one that excludes observations from the 10 countries with no IFRS adopters and (2) the other which excludes observations from Germany and Switzerland. Though not tabulated here for brevity,[25] we find that the results using the two reduced samples remain qualitatively similar to the full-sample results reported in Panel B of Table 4. The results again remain unaltered. In short, the results in Table 6 suggest that our main regression results presented in Panel B of Table 4 are, overall, robust to a variety of sensitivity checks.

8 IFRS adoption and institutional infrastructure

To see if the synchronicity-reducing effect of IFRS adoption is conditioned upon the strength of a country's institutions, we estimate the regression

$$\begin{aligned} Synchronicity = {} & \gamma_0 + \gamma_1 DIFRS + \gamma_2 INST + \gamma_3 DIFRS * INST \\ & + \Sigma_k \gamma_k Firm\text{-}specific\ Controls_k + \Sigma_j \gamma_j Country\text{-}level\ Controls_j \\ & + (Year\ fixed\ effect) + (Industry\ fixed\ effect) + error\ term \end{aligned} \quad (5)$$

where *INST* represents the strength of a country's institutions and all variables are as defined in "Appendix 2".[26] Here *INST* is proxied by three different measures of a country's governance and enforcement mechanisms: (1) the index representing the quality of local accounting standards (*CIFAR*), (2) the good government index (*GoodGov*), and (3) the investor protection index (*InvPro*). The *CIFAR* index measures the quality of a country's public disclosures in terms of the number of items disclosed in published accounting reports. The other two institutional variables are composite scores constructed by combining country-level scores on institutional efficacy from various data sources, as explained in detail in Panel D of "Appendix 2".

In sections A, B, and C of Table 7, the first column reports the regression results for Eq. (5) when *INST* is proxied by *CIFAR, GoodGov*, and *InvPro*, respectively.[27]

[25] The full results are available from the authors upon request.

[26] The firm-specific controls included in Eq. (5) are the same as those included in our main regression, with two exceptions: (1) *Accr* is included only when its interaction with *DIFRS* is used as an additional test variable and (2) *Cross*INST* is included in the regression to make our results comparable with those of Fernandes and Ferreira (2008). For country-level controls, we include only *GDiff* and *GDP* in Eq. (5) because the other country-level control variables (*Nlist*, *CtySize*, and *VarGDP*) are highly correlated with *INST*.

[27] In estimating Eq. (5), we delete a firm from the sample if the country-level scores required to measure *INST* are missing for the country to which the firm belongs. For this reason, the number of firm-years in Table 5 ($N = 15{,}230$) is smaller than the number of firm-years in panel B of Table 4 ($N = 15{,}382$).

Table 7 Results with institutional variables

	Pred. sign	Section A *INST = CIFAR*			Section B *INST = GoodGov*			Section C *INST = InvPro*		
		(1a)	(2a)	(3a)	(1b)	(2b)	(3b)	(1c)	(2c)	(3c)
DIFRS	−	−1.032***	−1.205***	−1.003***	−0.573*	−0.668**	−0.569*	−0.262***	−0.428***	−0.245***
		(−4.62)	(−5.70)	(−5.40)	(−1.92)	(−2.15)	(−1.92)	(−3.78)	(−5.51)	(−3.55)
INST	−	−0.016***	−0.016***	−0.016***	−0.065***	−0.065***	−0.064***	−0.003***	−0.003***	−0.003***
		(−17.32)	(−17.69)	(−17.17)	(−12.56)	(−12.51)	(−12.43)	(−10.43)	(−10.72)	(−10.36)
*DIFRS*INST*	?	0.013***	0.013***	0.013***	0.020*	0.017*	0.020*	0.004***	0.004***	0.004***
		(4.24)	(4.43)	(4.20)	(1.87)	(1.95)	(1.91)	(2.86)	(3.00)	(2.85)
Foll	+		0.052***			0.048***			0.054***	
			(5.80)			(5.44)			(5.96)	
*DIFRS*Foll*	?		0.085***			0.077***			0.075***	
			(3.31)			(2.99)			(2.89)	
Accr	+			0.005***			0.005***			0.005***
				(5.40)			(5.61)			(6.11)
*DIFRS*Accr*	?			−0.008**			−0.008**			−0.008**
				(−2.33)			(−2.22)			(−2.40)
Big4	−	0.015	0.012	0.015	−0.009	−0.012	−0.008	−0.027**	−0.030***	−0.026**
		(1.38)	(1.11)	(1.39)	(−0.77)	(−1.05)	(−0.75)	(−2.35)	(−2.63)	(−2.31)
Size	+	0.095***	0.076***	0.096***	0.100***	0.082***	0.101***	0.100***	0.081***	0.102***
		(27.35)	(16.75)	(27.69)	(28.78)	(18.53)	(29.11)	(28.96)	(18.07)	(29.32)
Nind	+	0.001	0.006	0.001	−0.003	0.002	−0.002	0.020***	0.026***	0.021***
		(0.02)	(0.99)	(0.21)	(−0.57)	(0.31)	(−0.37)	(3.22)	(4.04)	(3.37)
Leverage	?	0.024	0.033	0.017	0.018	0.026	0.011	0.030	0.038	0.023
		(0.89)	(1.25)	(0.65)	(0.67)	(1.00)	(0.43)	(1.10)	(1.41)	(0.84)

Table 7 continued

	Pred. sign	Section A*INST = CIFAR*			Section B*INST = GoodGov*			Section C*INST = InvPro*		
		(1a)	(2a)	(3a)	(1b)	(2b)	(3b)	(1c)	(2c)	(3c)
Freq	−	−0.036***	−0.031**	−0.036***	−0.020***	−0.015***	−0.020***	−0.020***	−0.015***	−0.021***
		(−7.40)	(−6.34)	(−7.40)	(−4.04)	(−3.03)	(−4.05)	(−4.21)	(−3.15)	(−4.23)
StdROA	?	0.129***	0.135***	0.119***	0.131***	0.137***	0.122**	0.111**	0.117**	0.100**
		(2.84)	(2.85)	(2.62)	(2.75)	(2.74)	(2.53)	(2.39)	(2.42)	(2.14)
Cross	−	−0.813***	−0.861***	−0.806***	−0.029	−0.012	−0.024	−0.128***	−0.144***	−0.127***
		(−5.90)	(−6.29)	(−5.83)	(−0.19)	(−0.08)	(−0.16)	(−3.72)	(−4.15)	(−3.70)
*Cross*INST*	?	0.011***	0.011***	0.011***	0.001	−0.001	0.001	0.002***	0.002***	0.002***
		(5.56)	(5.94)	(5.49)	(0.08)	(−0.04)	(0.05)	(3.34)	(3.80)	(3.32)
GDiff	+	0.001	−0.001	−0.001	0.010***	0.010***	0.010***	0.006***	0.006***	0.006***
		(0.01)	(−0.16)	(−0.11)	(7.21)	(7.16)	(7.02)	(4.00)	(3.82)	(3.84)
GDP	−	−0.338***	−0.308***	−0.345***	0.591***	0.614***	0.572***	−0.383***	−0.349***	−0.390***
		(−5.94)	(−5.45)	(−6.09)	(5.38)	(5.59)	(5.21)	(−7.19)	(−6.58)	(−7.36)
Intercept		1.069***	0.963***	1.053***	−0.560***	−0.670***	−0.560***	−0.064	−0.192*	−0.071
		(9.89)	(8.83)	(9.78)	(−4.32)	(−5.11)	(−4.33)	(−0.56)	(−1.66)	(−0.63)
Year fixed effect		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effect		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N		15,230	15,230	15,230	15,230	15,230	15,230	15,230	15,230	15,230
R^2 (%)		27.63	28.14	27.80	26.78	27.22	26.96	25.85	26.36	26.07

The variables and data sources are described in "Appendix 2". Here *, **, and *** indicate the 10, 5, and 1 % levels of significance, respectively, for a two-tailed test. All reported *t* statistics are based on standard errors adjusted for clustering at the firm level. Section A uses *CIFAR* as the institutional variable. Section B uses *GoodGov* as the institutional variable. Section C uses *InvPro*. Firms are deleted from the sample if a country's score for a specific institutional variable was not available. In column 1 of each section, *DIFRS*, *INST*, and *DIFRS*INST* are included in the model. In columns 2 and 3 of each section, *Foll* and *DIFRS*Foll* and *Accr* and *DIFRS*Accr* are added to the model, respectively

As shown in the first column (1a, 1b, and 1c) of each section, the coefficients of *DIFRS* are significantly negative across all three columns, even after controlling for the strength of institutions. Moreover, the coefficients of *DIFRS***INST* are significantly positive across all three columns. In other words, the synchronicity-reducing effect of IFRS adoption is less pronounced in countries with relatively stronger institutions, irrespective of which proxy is used for *INST*. This finding is consistent with the substitution view that enhanced disclosures via IFRS adoption facilitate the incorporation of firm-specific information into stock prices to a greater (lesser) extent when IFRS is adopted in countries with weak (strong) institutions. We also find that the coefficients of *INST* are highly significant, with a negative sign, across all columns, indicating that stock prices are less synchronous in countries with strong institutions than in countries with poor institutions. This result is consistent with the findings of previous research (Morck et al. 2000; Jin and Myers 2006).

To better understand the mechanism through which voluntary IFRS adoption influences firm-specific information flows in the market, we now add to Eq. (5) two firm-specific variables, analyst following (*Foll*) and accounting opacity (*Accr*), together with their interactions with *DIFRS*. Our main objective here is to isolate the synchronicity-reducing effect of IFRS adoption from the same effect of analyst following or accounting opacity associated with IFRS adoption. As shown in columns 2a, 2b, and 2c of Table 7, the coefficients of *DIFRS* and *DIFRS***INST* remain significantly negative and positive, respectively, irrespective of whether *Foll* and *DIFRS***Foll* are added to Eq. (5). Consistent with the results reported in Panel B of Table 4, the coefficients of *Foll* and *DIFRS***Foll* are both significantly positive.

As reported in columns 3a, 3b, and 3c of Table 7, we find that the coefficients of *Accr* and *DIFRS***Accr* are significantly positive and negative, respectively, across all three columns. This suggests that (1) accounting opacity, proxied by *Accr*, increases synchronicity by deterring firm-specific information from being capitalized into stock prices, a finding consistent with that of Jin and Myers (2006) and Hutton et al. (2009), and (2) the synchronicity-reducing role of IFRS adoption, captured by *DIFRS***Accr*, is more pronounced for firms with greater accounting opacity.

With respect to the other control variables, we note the following. The coefficient of *Cross* is highly significant, with an expected negative sign, as reported in sections A and C of Table 7 (where *INST* is proxied by *CIFAR* and *InvPro*, respectively). The coefficient of *Cross***INST* is significantly positive in both sections A and C of Table 7, suggesting that the synchronicity-reducing effect of cross-listing is attenuated when cross-listed firms are from countries with strong institutions. We find, however, that the coefficients of both *Cross* and *Cross***INST* are insignificant when *INST* is proxied by *GoodGov* (section B of Table 7). With respect to other firm-specific and country-level control variables, we find similar results as those reported in Table 4, though the coefficient of *GDiff* is insignificant when *INST* is proxied by *CIFAR* (section A of Table 7).[28]

[28] We conjecture that these insignificant coefficients of *GDiff* may be driven by the high correlation between *GDiff* and *CIFAR*.

In summary, the results in Table 7 show that IFRS adoption facilitates firm-specific information flows into the market, which in turn reduces synchronicity. Moreover, we find that the synchronicity-reducing effect of IFRS adoption is magnified in countries with weak institutions, which is in line with the substitution scenario on the interplay of firm-level and country-level governance mechanisms.

9 Summary and conclusions

With a large sample of 15,382 firm-years, with both IFRS adopters and non-adopters from 34 countries over the period 1998–2004, we find that voluntary IFRS adopters incorporate more firm-specific information into stock prices than do non-adopters, or, equivalently, stock price is less synchronous for the former than for the latter, even after controlling for all other factors, including analyst following, accounting opacity, reporting frequency, cross-listing, and differences between local GAAP and IFRS. Our results indicate that the market perceives the adoption of a full set of IFRS to be a more credible commitment to enhanced disclosure than the adoption of only a partial set of IFRS. For IFRS adopters, we find that the synchronicity decreases significantly from the pre-adoption period to the post-adoption period, confirming our cross-sectional results. We also find that the synchronicity-reducing effect of IFRS adoption is magnified for firms with low analyst followings, strong accrual quality, and poor institutional infrastructure. The above results are robust to a variety of sensitivity checks. Evidence reported in this paper supports the view that voluntary IFRS adoption encourages informed traders to collect, process, and trade on firm-specific information, which in turn leads to a decrease in synchronicity or an increase in firm-specific information capitalized into stock prices.

While previous research provides voluminous evidence that IFRS adoption improves the quantity and quality of firm-specific public information in the market, it has paid little attention to the role of voluntary IFRS adoption in facilitating firm-specific information flows into the market. Our study helps to better understand the role of enhanced disclosure in a broader context: enhanced disclosure via voluntary IFRS adoption not only improves the quality of accounting information but also facilitates the price formation process of incorporating firm-specific information into stock prices in a timely and accurate manner, particularly in an information environment with low analyst following, high accounting opacity, and poor institutional infrastructure.

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Appendix 1

See Table 8.

Table 8 Worldscope description of accounting standards followed (field 07536)

Worldscope field 07536	Worldscope description
01	Local standards
02	International accounting standards
03	U.S. standards (GAAP)
04	Commonwealth countries standards
05	EU standards
06	International standards and some EU guidelines
07	Specific standards set by the group
08	Local standards with EU and IASC guidelines
09	Not disclosed
10	Local standards with some EU guidelines
11	Local standards—inconsistency problems
12	International standards—inconsistency problems
13	U.S. standards—inconsistency problems
14	Commonwealth standards—inconsistency problems
15	EEC standards—inconsistency problems
16	International standards and some EU guidelines—inconsistency problems
17	Local standards with some OECD guidelines
18	Local standards with some IASC guidelines
19	Local standards with OECD and IASC guidelines
20	U.S. GAAP reclassified from local standards
21	Local standards with a certain reclassification for foreign companies
22	Other
23	IFRS

Appendix 2

See Table 9.

Table 9 Variable definitions and data sources

Variable	Definition	Data source
Panel A: Dependent and test variables		
Synchronicity	Stock price synchronicity, computed using Eq. (2)	Datastream
Foll	Natural log of one plus number of analysts providing an EPS forecast for a firm	IBES International
*DIFRS**	Ex ante probability that a firm will adopt IFRS	Worldscope
DIFRS	One for IFRS adopters and zero otherwise	Worldscope
Panel B: Firm-specific control variables		
Size	Firm size measured by the natural log of the total market value at the end of the year (in euros)	Worldscope
Leverage	Ratio of short-term and long-term debts to total assets	Worldscope
Growth	Natural log of long-term earnings growth	IBES International
ForSale	Percentage of foreign sales to total sales	Worldscope
Cross	One if a firm's shares are traded on foreign exchanges and zero otherwise	Worldscope
Accr	Absolute value of accounting accruals scaled by the absolute value of operating cash flows	Worldscope
Big4	One if a firm is audited by one of the Big 4 auditors and zero otherwise	Compustat Global and Worldscope
Nind	Natural log of the number of firms in the industry to which a firm belongs	Worldscope
Freq	Reporting frequency, measured by the number of interim financial reports disclosed by a firm	Worldscope
StdROA	The historical standard deviation of ROA computed over the preceding 5 years	Worldscope
Lambda	Inverse Mills ratio obtained from the probit IFRS adoption model in Eq. (3)	
Panel C: Country-level control variables		
GDiff	Difference between domestic accounting standards and IFRS	Bae et al. (2008)
GDP	Natural log of the gross domestic product per capita (in euros) in year *t*	IMF, World Economic Outlook Database
Nlist	Natural log of the number of listed firms for a given country in year *t*	World Bank WDI
CtySize	Natural log of the geographical size in square kilometers	World Bank WDI
VarGDP	Sample variance of the annual GDP per capita growth	IMF, World Economic Outlook Database
Panel D: Institutional variables		
CIFAR	Index created by examining and rating companies' 1995 annual reports on their inclusion or omission of 90 items. These items fall into seven categories (general information, income statements, balance sheets, funds flow statements, accounting standards, stock data, and special items). A minimum of three companies in each country were studied. The companies represent a cross section of various industry groups: industrial companies represent 70 %, and financial companies represent the remaining 30 %	International accounting and auditing trends 1995, Center for International Financial Analysis and Research

Table 9 continued

Variable	Definition	Data source
Corruption	International Country Risk's (ICR) assessment of corruption in government. Lower scores indicate that "high government officials are likely to demand special payments" and "illegal payments are generally expected throughout lowers levels of government" in the form of "bribes connected with import and export licenses, exchange controls, tax assessment, policy protection, or loans." Average of the months of April and October of the monthly index between 1982 and 1995. The scale is from zero to 10, with lower scores for higher levels of corruption	La Porta et al. (1998)
Expropriation	The ICR's assessment of "outright confiscation" or "forced nationalization." Average of the months of April and October of the monthly index between 1982 and 1995. The scale is from zero to 10, with lower scores for higher risks	La Porta et al. (1998)
Repudiation	ICR's assessment of the "risk of a modification in a contract taking the form of a repudiation, postponement, or scaling down" due to "budget cutbacks, indigenization pressure, a change in government, or a change in government economic and social priorities." Average of the months of April and October of the monthly index between 1982 and 1995. The scale is from zero to 10, with lower scores for higher risks	La Porta et al. (1998)
GoodGov	Sum of *Corruption*, *Expropriation*, and *Repudiation*	
AntiDir	An index of anti-director rights, which is formed by adding one when (1) the country allows shareholders to mail their proxy vote, (2) shareholders are not required to deposit their shares prior to the general shareholders' meeting, (3) cumulative voting or the proportional representation of minorities on the board of directors is allowed, (4) an oppressed minorities mechanism is in place, (5) the minimum percentage of share capital that entitles a shareholder to call for an extraordinary shareholders' meeting is less than or equal to 10 % (the sample median), and (6) shareholders have preemptive rights that can only be waived by a shareholder meeting. The range for the index is from zero to six	La Porta et al. (1998, 2002)
EffJud	Assessment of the efficiency and integrity of the legal environment as it affects business, particularly foreign firms, produced by the country risk rating agency Business International Corp. It "may be taken to represent investors' assessment of conditions in the country in question." Average between 1980 and 1983. The scale is from zero to 10, with lower scores representing lower efficiency levels	La Porta et al. (1998)
LawRule	Assessment of the law and other traditions in the country produced by the ICR. Average of the months of April and October of the monthly index between 1982 and 1995. The scale is from zero to 10, with lower scores for less tradition for law and order	La Porta et al. (1998)
InvPro	Arithmetic mean of the percentage ranks of *AntiDir*, *EffJud*, and *LawRule*	

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