

## IFRS Adoption and Analysts' Earnings Adjustments

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**Abstract:** We examine the extent of analysts' adjustments to reported earnings before and after the transition to International Financial Reporting Standards (IFRS) in the European Union and the European Economic Area. We use the magnitude of these earnings adjustments as an inverse indicator of analysts' perceived quality of domestic GAAP earnings in the pre-adoption period and IFRS earnings in the post-adoption period, respectively. We find that earnings adjustments decrease markedly after the IFRS transition. This is true both for mandatory adopters (in 2005) and voluntary early adopters (in varying prior years). We further find that the IFRS transition effect is larger for firms with prior poor domestic earnings quality and smaller for firms whose analysts had international coverage before the IFRS transition. On the country-level, we find evidence that the IFRS transition effect is larger for firms from countries with stronger legal enforcement and countries whose domestic accounting standards were more dissimilar to IFRS. Finally, results indicate that the IFRS transition effect on analysts' earnings adjustments is due to better perceived overall quality of IFRS earnings rather than increased comparability following from a common set of accounting standards after 2005.

# **IFRS Adoption and Analysts' Earnings Adjustments**

## **1. Introduction**

In this study, we examine the extent to which analysts make adjustments to reported earnings before and after firms' transitions to International Financial Reporting Standards (IFRS) in the European Union and the European Economic Area. We use the total magnitude of these earnings adjustments as a summary (inverse) indicator of analysts' perceived quality of domestic GAAP earnings and IFRS earnings, respectively. We investigate effects of the mandatory IFRS transition in 2005 and, separately, the effects for a set of voluntary adopters, i.e., firms that switched to IFRS before the mandatory adoption year of 2005.

The IFRS regulation passed the European Parliament and the European Council in 2002 (European Commission regulation No. 1606/2002). Article 3 of the regulation states that a common set of accountings standards can only be adopted if “[...] they meet the criteria of understandability, relevance, reliability and comparability required of the financial information needed for making economic decisions and assessing the stewardship of management.” As such, the criteria in the regulation correspond to accounting properties commonly associated with higher earnings quality also in the academic literature.

Prior literature on IFRS adoption and its consequences for earnings quality and associated capital market effects has not converged towards a consistent set of findings. For example, Barth et al. (2008) conclude that earnings quality under IFRS is better than quality under domestic GAAP for a sample of 21 countries. Ahmed et al. (2013) conclude the opposite – that IFRS adoption leads to poorer earnings quality. Atwood et al. (2011) conclude that there are no quality effects. Yip and Young (2012) find that mandatory IFRS adoption improves comparability, whereas Cascino and Gassen (2014) conclude that any improvement in

comparability is marginal at best. Christensen et al. (2013) conclude that IFRS adoption *per se* does not lead to improved market liquidity, whereas Barth and Israeli (2013) argue that one cannot draw such a conclusion based on the Christensen et al. analysis. In sum, results about the effects of IFRS adoption differ. The different findings have been ascribed to the use of different measures of earnings quality, to differences between firms that mandatorily adopt IFRS versus voluntary adopters (where the latter are often assumed to have stronger incentives to improve earnings quality), to varying degrees of enforcement in different countries, to the extent to which previous domestic accounting standards vary from IFRS, as well as to sample differences.

Dechow et al. (2010) define high quality earnings as earnings that “provide more information about the features of a firm’s financial performance that are relevant to a specific decision made by a specific decision-maker.” We investigate the quality effects of IFRS adoption from the perspective of financial analysts. Specifically, we look at analysts’ adjustments to reported earnings before and after IFRS adoption. Our argument is that analysts make larger adjustments if they perceive the quality of reported earnings to be poor, i.e., the total magnitude of adjustments is a result of multiple decisions made by analysts about how to treat various components of reported earnings. While researchers have not, to our knowledge, explicitly linked analyst adjustments to earnings quality, we believe the arguments and evidence in the analyst literature support linking analyst adjustments to several dimensions of earnings quality; that is we view analyst adjustments as a summary measure of earnings quality, as perceived by financial analysts.

There is a large literature about adjusted earnings, primarily based on U.S. samples, often referred to as pro-forma earnings, street earnings, core earnings, or simply non-GAAP earnings (e.g., Bradshaw and Sloan 2002, Bhattacharya et al. 2003, Doyle et al. 2003, Lougee and

Marquardt 2004, Gu and Chen 2004, Choi et al. 2007, Heflin and Hsu 2008, Kolev et al. 2008, Doyle et al. 2013). The difference between firms' reported (bottom-line) earnings and pro-forma earnings are the adjustments; in most cases the adjustments take on the form of exclusions (as opposed to additions) of income or expense items. Many prior studies use the analyst-adjusted earnings number as a proxy for the firm's pro-forma earnings disclosures (see Bradshaw 2011 for a discussion). In contrast, we are interested in the analyst adjustments *per se* as an indicator of how sophisticated users of financial reports perceive earnings numbers reported under different sets of accounting standards.

Analyst-adjusted earnings generally exclude items that are transitory or are believed to have low valuation relevance for other reasons (First Call 1999, Gu and Chen 2004), and permanent earnings and high value relevance are considered key dimensions of earnings quality in the literature (Dechow et al. 2010). In addition, interview and survey studies show that analysts explicitly state that they make adjustments to improve the quality of earnings along a number of quality dimensions (e.g., Graham et al. 2002, Barker and Imam 2008, Hjelström et al. 2014). Archival studies on US data also show that investors react more to analyst-adjusted earnings than to firms' reported earnings (e.g., Bradshaw and Sloan 2002).

In the first set of tests, we study analyst-followed firms in 19 countries that mandated the transition to IFRS in 2005, as required by the European Union and the European Economic Area.<sup>1</sup> Our main sample includes 17,589 firm-years 1999 to 2012 (1,840 unique firms), with accounting data from before as well as after the IFRS transition. Similar to much of the literature on pro-forma earnings in North-American samples, we define analyst adjustments as the

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<sup>1</sup> The European Economic Area includes Norway, Iceland and Lichtenstein as well as the EU countries. The additional non-EU countries have agreed to follow much (but not all) of EU regulation, including the IFRS regulation. The reason we do not have all EEA and EU countries in the sample is the limited number of analyst-followed firms in some of the smaller and less developed equity markets, especially in the pre-IFRS period.

difference between reported earnings and the so-called IBES “actual earnings.”<sup>2</sup> Using the absolute value of adjustments as an inverse earnings quality indicator, we find a marked decrease in the absolute adjustments following the IFRS transition across multiple test specifications. For example, in a test with firm-fixed-effects and controls for general economic conditions and the general time trend in adjustments (as documented by prior research), absolute adjustments decrease by about 45 % from the pre-IFRS level (significant at the .001 level).

We next investigate cross-sectional determinants of the IFRS adoption effect. We first analyze country-wide determinants of IFRS effects; specifically, a country’s degree of legal enforcement and the degree of difference between IFRS and a country’s domestic standards (“accounting distance”). For both measures we develop latent variable models, which we argue capture meaningful variation in enforcement and accounting distance better than traditional models. We find that the effect of IFRS adoption is more pronounced in countries with stricter enforcement, and that the effect is stronger in countries with larger accounting distance.<sup>3</sup>

Next, we analyze cross-sectional determinants at the firm level. We hypothesize and find that firms with poorer earnings quality prior to IFRS adoption (as proxied by absolute discretionary accruals according to the modified Jones 1991 model) experience a significantly larger IFRS effect, i.e., a larger decrease in earnings adjustments. The same is true for firms that showed higher analyst forecast dispersion prior to IFRS adoption, i.e., firms whose information environment was characterized by larger information uncertainty in the pre-IFRS period.

We also investigate whether the IFRS adoption effect is more pronounced for firms followed by analysts who also follow firms from other countries prior to IFRS adoption. The

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<sup>2</sup> As explained in more detail in Section 2, IBES actual earnings are the resulting earnings after analysts’ adjustments have been made to the firm’s reported earnings.

<sup>3</sup> Using traditional measures of enforcement and accounting distance (based on counts of enforcement dimensions and counts of accounting differences) lead to weaker results, albeit still marginally significant.

aim of this test is to distinguish between the main drivers of decreasing analyst adjustments, higher mechanical earnings comparability or better earnings quality more generally. If the smaller adjustments after IFRS adoption are entirely or largely due to earnings being more mechanically comparable (i.e., less need to adjust for mechanical accounting differences when all firms report under the same set of standards), we would expect a larger IFRS effect for firms with analysts who cover firms from different countries (that used different sets of domestic standards in the pre-IFRS period). We find the opposite, however, a smaller effect. We view this finding as inconsistent with cross-country mechanical comparability being the main driver for our results.

Our next set of tests uses a sample of firms that voluntarily adopted IFRS in years prior to 2005, where the (firm-specific) year of adoption now serves as the event year. Results are similar to the main results for mandatory adopters, both in terms of the magnitude of the effects and in terms of the statistical significance. In addition, we test for an incremental effect in the year 2005, when most other firms switched. We find no evidence of such an additional 2005 effect for the firms that had adopted IFRS in previous years. We believe this result indicates that there is no additional comparability effect when other firms adopt IFRS. We also include the 2005 variable on its own (without the voluntary-IFRS effect included). Also in this case, there is no 2005 effect for the voluntary adopters. We interpret these results as indicating that the IFRS effect we document for mandatory adopters (all of whom switch in 2005) is unlikely to be due to confounding events taking place around 2005, since similar effects are evident in previous adoption years for voluntary adopters and no effect is evident for them in 2005.

In summary, we interpret these results as consistent with the following conclusions. First, IFRS adoption leads to better perceived earnings quality among analysts. Second, the

magnitude of the effect is more pronounced in countries with stronger legal enforcement and larger accounting distance between domestic GAAP and IFRS; the effect is also larger for firms with initially poorer earnings quality according to other metrics. Third, the effect is robust to the type of adoption, mandatory versus voluntary. Fourth, the effects for mandatory adopters do not appear to be driven by mere mechanical comparability after mandatory IFRS adoption; nor do they appear to be driven confounding events around 2005.

The paper proceeds as follows. In Section 2, we describe related literature and the hypothesis development. Section 3 describes the research design and provides descriptive statistics of our sample and the main variables. Section 4 presents the main results and various additional tests. Section 5 contains the concluding discussion.

## **2. Background and research questions**

On a fundamental level this study is about whether earnings quality has increased with the adoption of IFRS. Answering such a question empirically requires a definition of earnings quality. Dechow et al. (2010) point out the many different definitions of earnings quality that exist in the literature and conclude that any statement about the quality of earnings will be contingent on the decision context and will contain information about a firm's performance relevant to a particular user or decision maker.

We concentrate on decisions made by a particular user group, financial analysts, when processing earnings information from companies. Analysts are arguably sophisticated users of accounting information and their role as information intermediaries in capital markets is well established (Schipper 1991). Specifically, we investigate earnings adjustments made by analysts to companies' reported earnings before and after the transition to IFRS from various sets of

domestic accounting standards; both mandatory transitions and, separately, voluntary transitions. We view the magnitude of such adjustments as an inverse indicator of the quality of earnings, as perceived by analysts. The definition of earnings quality is thus potentially quite broad, in the sense that it will include adjustments intended to increase earnings persistence (by, for example, excluding one-time charges), increase valuation-relevance (by excluding items believed to be irrelevant to investors), and increase earnings comparability across firms (by ensuring, as best as possible, that similar events are recognized similarly in firms' adjusted earnings). The quality definition is limited, however, in the sense that it focuses on a particular user group, financial analysts.<sup>4</sup> As such, it trades off user sophistication with user specificity.

## 2.1 Analysts' adjustments to earnings

One of the most important tasks for a financial analyst is to forecast future realizations of earnings or earnings per share (EPS), which can be used for valuation purposes (for example, price-earnings ratios, which is a very common valuation method; e.g., Demirakos et al. 2004). Briefly, many analysts model and forecast an EPS number that is “higher up” in their pro-forma income statements. When arriving at this number analysts make adjustments to a firm's reported EPS, and they typically exclude items believed to be transitory, non-value-relevant, or hindering comparability across firms. Analysts following a specific firm make their own adjustments (potentially partially guided by management). When the consensus forecast for the firm is produced by a forecast aggregator, such as IBES, it bases the adjustments on a majority rule (Christensen et al. 2011). After the firm has reported its earnings, IBES provides their version of adjusted realized EPS (“EPS actual”), which imputes into the firms' actual reported earnings the corresponding adjustments made to the consensus earnings forecast (Christensen et al. 2011,

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<sup>4</sup> As explained in Section 3, we use the aggregated adjusted earnings number provided by IBES following much of the research based on U.S. data. This design choice, necessitated by data restrictions, means that one cannot capture earnings adjustments made by individual analysts.



Bradshaw 2011). As a consequence, IBES earnings forecasts and IBES adjusted earnings follow the same basis of calculation. We follow earlier work by defining earnings adjustments as the difference between IBES adjusted earnings and earnings as reported by the firm.

There is fairly extensive evidence in the literature suggesting that analyst adjustments are associated with properties traditionally viewed as dimensions of earnings quality. In an interview study, Barker and Imam (2008) investigate financial analysts' perceptions of earnings quality. Based on interviews with analysts following FTSE 100 firms, i.e., large firms listed at the London Stock Exchange, they conclude that "a majority of the analysts describe high-quality earnings in terms of some aspect of the 'core' earnings of the firm" (p. 319). As a consequence, items perceived as low-quality items are excluded from earnings. In general, the excluded items are either transitory in nature or the result of re-measurement of assets or liabilities, and thus of little perceived value for the prediction of future earnings.<sup>5</sup> As mentioned above, Barker and Imam's findings are based on analysts following large firms listed in London, but their results are also generally in line with survey data from financial analysts in the U.S. (e.g., Graham et al. 2002).

Several quantitative studies also focus on analysts' adjustments to earnings, both their characteristics and their capital market consequences.<sup>6</sup> For example, Bradshaw and Sloan (2002) document an increasing time trend in the both the magnitude and the frequency of analyst

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<sup>5</sup> The most common adjustments include impairment losses on fixed assets, impairment or amortization of goodwill, gains and losses on financial assets as well as fixed assets, restructuring costs, etc. In many of the categories, however, analysts indicate that the adjustment decision depends on the firm and there is also variation across analysts in some of the categories, indicating that there appears to be no mechanical "one fits all" template for adjustments. The latter aspect is further supported by interview evidence in Hjelström et al. (2014). It is also not the case that firms universally exclude certain items when they calculate and disclose pro-forma earnings (see, e.g., Bhattacharya et al. 2003 and Entwistle et al. 2006).

<sup>6</sup> As discussed in Bradshaw (2011) several studies with a manager focus use analyst-adjusted earnings as a proxy for management's pro-forma earnings. There is a substantial overlap, but the two are not the same. For example, Bhattacharya et al. (2003) use a hand-collected dataset and show that the two coincide in about 60-70% of cases. Christensen et al. (2011) investigate the links between management guidance (pro-forma earnings) and analyst adjustments (street earnings).

adjustments. They further find that investors react more strongly to analyst-adjusted earnings than to firms' reported earnings, suggesting that investors perceive the former as more valuation-relevant. Gu and Chen (2004) find that non-recurring items that analysts do not exclude are less transitory and more value-relevant than non-recurring items that they do exclude, consistent with analysts having the experience and expertise to make such judgments. There is also evidence that when analyst adjustments deviate from those made by firms in calculations of pro-forma earnings, analyst-adjusted earnings are perceived by investors as more valuation relevant (Marques 2006).<sup>7</sup>

Combined, we believe that prior research supports the use of the magnitude of analysts' earnings adjustments as an inverse indicator of their perception of earnings quality. These studies notwithstanding, there is also research suggesting that financial analysts can be biased in their decisions on what to include and exclude from their earnings forecasts. For example, Doyle et al. (2003) find that items that analysts exclude from reported earnings still have predictive value for future firm performance. Focusing on analysts' incentives, Baik et al. (2009) find that analysts tend to bias adjusted earnings upwards for so-called glamour stocks (firms with high market-to-book ratios; they find no bias for value stocks). Analysts can also be (mis)guided by managements' (potentially opportunistic) guidance of pro-forma earnings (e.g., Andersson and Hellman 2007, Christensen et al. 2011). To the extent that such analyst bias decreases around 2005, this would constitute a confounding event that can also contribute to a decrease in earnings adjustments. We address alternative explanations for adjustment decreases in Section 4.3.

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<sup>7</sup> There is to our knowledge no prior research on the variation in analyst adjustments across countries. In related research, however, the practice among European firms (as opposed to analysts) of disclosing pro-forma earnings is documented by Isidro and Marques (2014). They use hand-collected data for 321 large European firms over the years 2003-2005 and investigate firms' propensity to disclose pro-forma earnings.

In summary, our reading of prior literature in this area indicates that (1) the market reacts more to analyst-adjusted earnings than to reported earnings, (2) the market puts a higher weight on analysts' adjustments compared to management's adjustments, (3) analysts' inclusions (i.e., items not excluded from adjusted earnings) are more persistent than exclusions, and (4) analysts themselves consider street earnings to be high quality earnings. We believe these facts lend support to using analyst earnings adjustments in a study of how analysts perceive the quality of earnings produced under different sets of accounting standards.

## 2.2 IFRS adoption effects on earnings quality and other outcome variables

There is a relatively large research literature on IFRS adoption and its effects on earnings quality, defined in various ways, and capital market outcomes (for a literature overview of voluntary adoption effects, see Soderstrom and Sun 2007, for overviews and discussions of the effects of mandatory IFRS adoption, see Pope and MacLeay 2011 and Brüggemann et al. 2013). The literature has not converged towards a consistent set of findings. For example, Brüggemann et al. (2013, Table 1) review nine studies that investigate various common earnings quality measures, such as value relevance, abnormal accruals, earnings persistence, the association between current period earnings and future cash flows, etc., before and after firms' mandatory IFRS adoptions.<sup>8</sup> They list three studies concluding that there is no IFRS effect, three studies finding an improvement after IFRS adoption, two studies finding a deterioration, and one study finding that the IFRS effect is either positive or negative, depending on which accounting property is investigated.<sup>9</sup>

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<sup>8</sup> The studies they list are Aharony et al. (2010), Callao and Jarne (2010), Lang et al. (2010), Wu and Zhang (2010), Atwood et al. (2011), Yip and Young (2012), Ahmed et al. (2013), Barth et al. (2014), Bhat et al. (2014).

<sup>9</sup> Studies that explicitly focus on accounting comparability effects also come to different conclusions. For example, Yip and Young (2012) conclude that mandatory IFRS adoption improves cross-country information comparability, whereas Cascino and Gassen (2014) conclude that any comparability effects are marginal at best, because of firm-level heterogeneity in IFRS compliance.

There are also differences in findings from samples of voluntary adopters versus samples of mandatory adopters, even when holding the measure of earnings quality constant. For example, Barth et al. (2008) find that voluntary adopters exhibit decreased income smoothing and increased timeliness of loss recognition following IFRS adoption, whereas Ahmed et al. (2013) conclude the opposite for mandatory adopters. Following arguments in Daske et al. (2008), Ahmed et al. attribute the opposite results to voluntary adopters having stronger incentives to increase reporting quality than mandatory adopters.

Schipper (2005) points out that firms' implementation choices and varying degrees of regulatory enforcement are also likely to affect the extent to which IFRS adoption influences financial reporting outcomes. Empirical evidence has subsequently confirmed this conjecture. For example, Kvaal and Nobes (2010), Glaum et al. (2013) and Verriest et al. (2013) document substantial variation in IFRS policy choices as well as in (non-)compliance with IFRS. Such implementation effects are partially determined by firm-level variables and incentives, partially determined by country-level variables. For example, IFRS effects are stronger in high legal enforcement countries (i.e., countries in which IFRS is more rigorously and uniformly implemented; see Pope and MacLeay 2011 for a detailed discussion).

Capital market outcomes of IFRS adoption also tend to be conditional on country- and firm-specific characteristics. For example, Li (2010) finds that IFRS adoption leads to a lower cost of equity capital for mandatory adopters, but only in countries with a strong legal enforcement. Daske et al. (2008) document a similarly crucial role for enforcement when investigating market liquidity, cost of capital, and Tobin's q around IFRS adoptions. They further document that capital market effects are stronger for voluntary adopters than for mandatory adopters. Christensen et al. (2013) conclude that the mandatory change to IFRS had

little effect on market liquidity, and that concurrent changes in enforcement is at least as important. Barth and Israeli (2013) discuss this evidence further and argue that both IFRS adoption and enforcement are important.<sup>10</sup>

Some studies have also investigated the impact of IFRS on financial analysts' information environment (e.g., Ernstberger et al. 2008, Beuselinck et al. 2010, Choi et al. 2010, Byard et al. 2011, Tan et al. 2011, Glaum et al. 2013, Horton et al. 2013, Choi et al. 2013). These studies typically focus on forecast errors and/or forecast dispersion, i.e., on metrics that are measured at the "street earnings" level. That is, they use analyst-adjusted numbers. Consequently, these studies do not focus on IFRS earnings *per se*, but rather use earnings-forecast based metrics as proxies for the overall information environment following the IFRS transition. Note that this design choice precludes our type of investigation, where firms' reported earnings are compared to analyst-adjusted earnings.

The empirical results of how the IFRS adoption has affected analysts' information environment are mixed (for an overview, see Horton et al. 2013). Generally, forecast accuracy appears to improve for firms that voluntarily adopt IFRS (e.g., Ashbaugh and Pincus 2001, Ernstberger et al. 2008), but for mandatory adopters, the effect is conditional on analyst-specific, firm-specific and/or country-specific determinants. Looking at within-country effects Choi et al. (2013) document improved forecast accuracy among analysts subsequent to the mandatory IFRS adoption in the UK. Horton et al. (2013), investigating the effect across many countries document a general decrease in forecast errors after the mandatory adoption of IFRS. Tan et al.

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<sup>10</sup> There is also a growing literature investigating the existence and magnitude of various other IFRS adoption effects, more or less directly linked to capital markets. Examples include foreign mutual fund ownership (DeFond et al. 2011), stock exchange listings (Han and He 2011), the U.S. investor home bias (Khurana and Michas 2011), institutional investment decisions (Florou and Pope 2012), dual-class share voting premia (Hong 2013), stock crash risk (DeFond et al. 2014), initial public offerings (Hong et al. 2014), and international portfolio holdings (Yu and Wahid 2014).

(2011), however, find no IFRS effect on forecast accuracy for local analysts (domiciled in the same country as the firm they follow), and they conclude that any improvement in forecast accuracy are attributable to foreign analysts (domiciled in another country). Similarly, Byard et al. (2011) find no general IFRS effect; they only find the IFRS effect in countries with high distance between IFRS and local GAAP and high legal enforcement (this result is in line with Bae et al. 2008 who find that forecast accuracy is negatively associated to the distance between different local sets of accounting standards).

In summary, the literature on the effects of IFRS adoption on various accounting attributes or “earnings quality” on capital market outcomes, and on firms’ and analysts’ information environment is mixed, with some studies documenting a positive IFRS effect, some studies finding a negative effect, and yet other studies concluding that there is no effect. The literature also shows that voluntary adoption effects can be different from those of a mandatory adoption.

Given the varying results in prior literature investigating other earnings quality measures, it is an empirically open question how analysts react in terms of their adjustments to reported earnings. Our main hypothesis is that analysts’ earnings adjustments decrease in magnitude after IFRS adoption if IFRS are perceived to be of higher quality than domestic standards. We test this hypothesis both for firms that mandatorily switched to IFRS in 2005 and, separately, for firms that voluntarily adopted IFRS in various earlier years. Our second set of tests, on cross-sectional determinants of changes in earnings adjustments, is partially aimed at separating the effects of an increase in perceived quality of IFRS earnings as opposed to an increase in the comparability of IFRS earnings across firms through standardization. The tests on voluntary adopters also inform on the question of comparability vs. general earnings quality. We believe

this is an important distinction to make, as comparability could have been achieved by any standardization, i.e. by widely adopting *any* accounting standard. Increases in earnings quality, however, are more specifically attributable to adopting IFRS in lieu of the local standards. We further document sensitivity of results to firm- and country specific factors that prior IFRS literature indicates can be important, as discussed above.

### **3. Research design and data**

Our sample of mandatory adopters consists of 17,589 firm-year observations of analyst earnings adjustments from 1999 to 2012.<sup>11</sup> The sample covers 1,840 analyst-followed firms from 19 countries that in 2005 were members of the EU or the European Economic Area. These firms were bound by the EU directive requiring listed firms to report according to IFRS by January 1, 2005 (European Commission Regulation No. 1606/2002). The sample firms and firm-year (observations) distributions by country is in Table 1. As expected, countries with large equity markets have the largest number of observations (Great Britain, followed by France, Italy and Germany). Some countries have very few observations (the Czech Republic, Hungary and Slovenia; results are not sensitive to whether we exclude such countries). We exclude firms that followed other non-domestic standards, such as US GAAP, prior to 2005. We require firms to have observations in both the pre- and post-IFRS period, and we require firms to be from countries where there is sufficient data to estimate measures of legal enforcement and accounting distance (i.e., a measure of the difference between local GAAP and IFRS, described further below). In this sample, we also exclude firms that voluntarily adopted IFRS prior to 2005 (but analyze these firms separately, see Section 4.2).

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<sup>11</sup> In robustness tests, we exclude the year 2005. Results are not sensitive to this exclusion.

Our main variable is analysts' earnings adjustments, calculated as the absolute difference between reported (local GAAP/IFRS) EPS and the analyst-adjusted EPS from IBES. We use the absolute value of the difference, as we are interested in the magnitude of analysts' adjustments, not their sign. Following Doyle et al. (2003), Heflin and Hsu (2008) and Kolev et al. (2008), we scale the earnings adjustments by total assets:<sup>12</sup>

$$AnalystAdjustments_{it} = \frac{abs[Reported\ EPS_{it} - Analyst-adjusted\ EPS_{it}]}{total\ assets\ per\ share_{i,t}} \quad (1).$$

We collect reported EPS data from Compustat Global, substituting net income divided by the number of shares outstanding when the EPS data item is missing.<sup>13</sup> The analyst-adjusted EPS number is measured as 'EPS Actual' collected from IBES International Summary File (Bradshaw and Sloan 2002, Brown and Sivakumar 2003, Doyle et al. 2003, among others). When subtracting analyst-adjusted EPS from reported EPS, we verify that they are both measured on a primary basis. We also find individual cases, mostly in the beginning of the sample period, where the IBES-based analyst-adjusted EPS is in Euros and the Compustat-based reported EPS is reported in local currency in countries that had decided to switch to the Euro. In those cases, we transform the analyst-adjusted EPS to local currencies (note that currency exchange rates were fixed already in 1999, our first sample year, for the countries that switched to the Euro).

Our basic research design is straightforward. The main tests investigate whether analyst adjustments have changed in magnitude following the adoption of IFRS in the European Union

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<sup>12</sup> Some earlier U.S. studies scale the adjustments by stock price; however, this can confound the results both over time and across countries if there are large systematic increases or declines in prices in some countries. For example, in some of the sample countries stock prices declined by more than 50% in the 2008-2009 crisis, whereas other sample countries had substantially smaller losses.

<sup>13</sup> There is a non-trivial loss of EPS observations in Compustat Global (approximately 20%). In robustness checks, we investigate the validity of our own calculated EPS for firms where we have the firms' reported EPS in Compustat Global. We find a Pearson (Spearman) correlation of 1.000 (1.000) between the calculated EPS and the Compustat EPS. We also verify that our main results are very similar in the sample of firms for which we have EPS from Compustat Global.



and the European Economic Area. We document results for mandatory adopters as well as for voluntary adopters, with and without controls for various determinants of analyst adjustments, such as macro-economic indicators and general over-time trends. To explore determinants of cross-sectional variation in the main effect, we further investigate country-level variables, such as legal enforcement and the accounting distance between domestic standards and IFRS, as well as the effects of various firm-level variables.

Panel A of Table 2 contains distributional statistics for our main variable of interest, *AnalystAdjustments*. In the total sample, the mean (median) scaled absolute adjustments are about 1.75 % (0.14%) of total assets. While not immediately comparable to studies that use signed adjustments and North American samples, we note that the order of magnitude for the median is not dissimilar. For example, Heflin and Hsu (2008) report a median of 0.22 %. We further report the absolute adjustments separately for the pre-IFRS period (up until 2004) and the post-IFRS period (starting in 2005). The mean is 1.93% in the pre-period and 1.62% in the post-period. The median is 0.22% in the pre-period and 0.11% in the post-period. Both the decrease in the mean and the decrease in the median are significant at the 0.001 level (using a two-sample t-test and a Wilcoxon test, respectively).

Panel B of Table 2 reports descriptive statistics for firm-specific variables that measure dimensions of earnings quality and analyst characteristics in the pre-IFRS period. This sample is further restricted by data availability ( $n = 12,835$ ) with analyst forecast dispersion being the most limiting variable (forecast dispersion requires at least three analysts following a firm).<sup>14</sup> We use two variables that prior literature has interpreted as accounting quality-related. The first one is absolute discretionary accruals, *AbsDiscrAcc*, based on the modified Jones (1991) model. We

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<sup>14</sup> To avoid varying the number of observation in the cross-sectional firm-specific tests, we maintain one subsample of 12,835 observations. Reported results are not affected qualitatively if we instead maximize the sample for each individual test.

use the cross-sectional approach from Ecker et al. (2013), where peer firms are identified by size (lagged total assets). The second variable is analyst forecast dispersion (*Dispersion*), taken from IBES International Summary File. Both variables have been used in the literature to proxy for accounting quality and/or the firm's information environment. In both cases, Panel B of Table 2 shows that the cross-sectional variation (standard deviation and interquartile range) is substantial compared to the measures of central tendency (mean and median).

The variable *ForeignDummy* captures whether or not a firm is followed by analysts with international coverage in the pre-IFRS period. Such analysts have to follow firms that report under different sets of local accounting standards, with potential quality and comparability issues. *ForeignDummy* takes the value 0 when all the firm's analysts follow only other firms that report under the same set of domestic standards. The variable takes the value 1 if at least one analyst also follows firm(s) reporting under a different set of domestic standards. The mean of 0.65 indicates that 65% of firm-years are characterized by analyst(s) with foreign coverage.<sup>15</sup>

In certain tests, we also use country-wide variables; specifically, *Crisis*, *Enforcement* and *AccountingDistance*, which are assigned to all firm-years in that particular country. *Crisis* is an indicator variable for country-specific economic crisis years. The variable takes the value 1 when the GDP growth is negative for the specific country and zero otherwise (GDP data come from the World Bank). We include *Crisis* as a control variable to capture the fact that certain common adjustments such as impairment charges and restructuring charges are more likely in crisis years, and the distribution of crisis years may not be equal before and after the IFRS adoption-year or across countries. On average 14.38% of sample observations are drawn from crisis years.

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<sup>15</sup> Our foreign coverage variable does not speak to the analysts' domicile relative to the firms in the covered sector, but rather the domicile of the covered firms only (see Tan et al. 2011 for details about analyst domiciles and IFRS adoption).

*Enforcement* measures the strength of the country-level regulatory enforcement. Specifically, we estimate a continuous enforcement factor from a confirmatory factor analysis on three categorical input variables. We start by collecting all enforcement indicators from Brown et al. (2014) for the year of IFRS transition in our sample, 2005, as our initial set of input variables. Brown et al. surveyed data from the International Federation of Accountants (IFAC), complemented by data from the World Bank and Commission of European Securities Regulators (CESR). The items include data on the existence, work, activity and resources of the enforcement bodies.<sup>16</sup>

In a preliminary step to create our enforcement factor, we exclude variables that are either constant or perfectly correlated in our sample, as they contain no incremental information about the construct of interest, i.e. the latent variable for enforcement rigor. *Enforcement* is a latent variable for enforcement rigor based on the three categorical input variables: specifically, whether (i) a regulatory body reviews financial statements, (ii) a regulatory body takes or has taken enforcement actions, and (iii) the level of resourcing, based on the number of staff of the regulatory body. Our confirmatory factor analysis treats variables (i) and (ii) as binary, while variable (iii) can take on three (ordered) values. The *Enforcement* factor is the weighted combination of the three input variables that maximizes the log likelihood from three logit regressions on the factor. Larger values for the enforcement factor correspond to higher enforcement rigor.

Our accounting distance factor is also constructed with confirmatory factor analysis across all sample observations. Our input variables are defined in the following way. For each country, we begin by coding a severity score for accounting differences between local (pre-

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<sup>16</sup> The detailed variable descriptions are in Brown et al. (2014). Their Table 2 contains the variable definitions and the data sources; while Appendix 2 lists the data on the input variables itself, by country.

IFRS) GAAP and IFRS, corresponding to the severity categorization in the 2001 GAAP survey (C. Nobes, Editor). To reduce complexity and ensure convergence of the factor estimation, we focus on the standards-level (not the paragraph level), whereby we assign the highest severity score across all paragraphs to the standard. In addition, we also reduce the input to the five standards with the highest severity score in our sample (i.e., those that differ most often and most severely from IFRS). Those five standards are, in decreasing order: IAS19, IAS22, IAS39, IAS32 and IAS35. The *AccountingDistance* factor is the weighted combination of the five input variables that maximizes the log likelihood from five logit regressions on the factor. Larger values for *AccountingDistance* correspond to larger difference between local GAAP and IFRS. The countries with the highest (lowest) accounting distance are Hungary and Slovakia (the Netherlands, Norway and Great Britain).

For completeness, descriptive statistics on the two country-level factors are at the bottom of Table 2, Panel B.

#### **4. Main tests**

This section describes our main tests for mandatory IFRS adopters (Section 4.1) and voluntary adopters (Section 4.2) followed by additional robustness tests (Section 4.3).

##### 4.1 Mandatory adopters

Our main test design includes an event indicator (IFRS adoption) and various determinants of the IFRS effect on analyst adjustments. The basic design structure follows Equation (2), below, and it is similar to Cohen et al. (2008) who study earnings management before and after the introduction of the Sarbanes-Oxley regulation in the United States.

$$|AnalystAdjustments_{it}| = \alpha_0 + \alpha_1 PostIFRS_t + \alpha_2 Time_t + \alpha_3 Crisis_{jt} + \varepsilon_{it} \quad (2)$$

$|AnalystAdjustments_{it}|$  is the absolute value of the analyst adjustments by firm and year.  $PostIFRS$  is a dummy variable that takes the value of 0 for pre-IFRS years and the value 1 for post-IFRS years.  $Time$  is equal to the difference between the current year and 1999 (the first year in our sample period), and we include it to capture the general time trend in earnings adjustments documented in prior research (e.g., Bradshaw and Sloan 2002, Brown and Sivakumar 2003).  $Crisis$  is a variable specific to country  $j$  that takes the value of 1 if GDP growth is negative and zero otherwise. We include it to control for macro-economic determinants of adjustments as described in Section 3.

To test for various cross-sectional determinants of the IFRS effect, we add them as described in Equation (3):

$$|AnalystAdjustments_{it}| = \alpha_0 + \alpha_1 PostIFRS_t + \alpha_2 Time_t + \alpha_3 Crisis_{it} + \alpha_4 PostIFRS_t \times Determinant_{i(j)} + \varepsilon_{it} \quad (3),$$

where  $Determinant_{i(j)}$  represents various other firm- or country-level variables hypothesized to influence the IFRS effect on analyst adjustments (also described in Section 3).

Table 3 reports results using either country-fixed effects (columns 1–3) or firm-fixed effects (columns 4–6). Results are qualitatively quite similar, and we concentrate on the firm-fixed effects results in the discussion below. Essentially, the firm-fixed effects results control for any firm-specific determinants of analyst adjustments not captured by other variables.<sup>17</sup>

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<sup>17</sup> These include, but are not limited to, innate variables proxying for the complexity of the business model, such as intangibles intensity (Heflin and Hsu 2008). They also include the company's propensity to disclose its own adjustments to earnings in the calculation of so-called pro-forma earnings numbers (Christensen et al. 2011).

We first estimate a reduced form of Equation (2), where we only include the indicator variable  $PostIFRS_t$ . Similar to the descriptive statistics in Table 2, the test shows a significant decrease in the magnitude of analyst adjustments following IFRS adoption, with a point estimate of -0.26% of total assets ( $t=-3.73$ ; firm-fixed effects included). When we add  $Crisis$  to control for adjustments specific to poor macro-economic conditions, the point estimate on  $PostIFRS_t$  increases somewhat in magnitude to -0.35% ( $t=-4.82$ ). The last model adds a time trend variable to control for a potential general time trend in the magnitude of adjustments, as indicated by research on US data. Our results indicate that this general time trend is also present in international data, with a point estimate of 0.08% of total assets per year ( $t=5.08$ ). The time trend is not significantly different in the pre- versus post-IFRS periods ( $p\text{-value}=0.30$ , not tabulated). Taking the time trend into consideration leads to a greater general decrease in the magnitude of adjustments after the IFRS transition, 0.87% of total assets ( $t=-6.93$ ).

Overall, Table 3 shows that the decrease in the magnitude of analysts' earnings adjustments is statistically significant regardless of the choice of control variables and regardless of whether we use country-fixed effects or firm-fixed effects ( $t$ -statistics range from -3.55 to -6.93). The decrease is also economically non-trivial. For example, the decrease with (without) control variables and with firm-fixed effects is 45.1% (13.5%) of the average pre-IFRS adjustment level.<sup>18</sup>

In Table 4, we add several potential determinants to investigate the cross-sectional variation in the adjustment decrease. Specially, we construct interaction variables between determinants and the  $PostIFRS_t$  variable (note that in a firm-fixed effects specification, a variable that is not shifting over time cannot be added alone as independent variable).

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<sup>18</sup> 13.5% decrease is the coefficient estimate of the IFRS decrease from Table 3 (0.26%) compared to the average pre-IFRS level in Table 2 (1.93%). 45.1% decrease is the coefficient estimate of the IFRS decrease from Table 3 with control variables (0.87%) compared to the average pre-IFRS level in Table 2 (1.93%).

Our first interaction variable is *Enforcement*, the construction of which is described in Section 3. Several studies that investigate the consequence of IFRS adoption note the importance of a country's enforcement regime, either as a main effect or interacted with an IFRS transition variable (e.g. Barth and Israeli 2013, Christensen et al. 2013). Following this literature, we expect the IFRS-related decrease in absolute earnings adjustments to be more pronounced when the level of enforcement is higher. The results in Column (1) of Table 4 indicates that this is the case ( $t = -2.77$ ). The second interaction variable is *Accounting-Distance*, also described in Section 3. Its inclusion is motivated by studies that hypothesize and (sometimes) find more pronounced IFRS effects in countries whose domestic GAAP is more dissimilar to IFRS (e.g., Bae et al. 2008, Byard et al. 2011, Christensen et al. 2013, Cascino and Gassen 2014). We find a statistically significant IFRS interaction effect associated with accounting distance in our sample ( $t = -3.15$ ).

Our next three variables are firm-specific determinants rather than country-wide determinants of IFRS adoption effects. All three variables are measured during the pre-IFRS period, in order to capture firms' different "starting points" in terms of each determinant. The first variable is *AbsDiscrAcc*, the absolute discretionary accruals (Jones 1991, Ecker et al. 2013) as described in Section 3, which is an alternative proxy for earnings quality under the domestic GAAP regime. We expect that firms with poor domestic earnings quality experience a larger decrease in earnings adjustments following IFRS adoption.<sup>19</sup> Results are consistent with this

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<sup>19</sup> We are agnostic about whether the variable captures mainly discretionary accounting quality (Jones 1991) or whether it mainly captures accounting quality effects of innate factors, such as business model volatility (Owens, Wu and Zimmerman 2014).

hypothesis as indicated by a significantly negative interaction effect between *AbsDiscrAcc* and *PostIFRS* ( $t=-4.43$ ).<sup>20</sup>

The next variable is analyst forecast dispersion, which is a measure of analyst disagreement about a firm's earnings prospects. It is often used in the literature to proxy for the quality of a firm's information environment. We expect high analyst forecast dispersion in the pre-IFRS period to be associated with a more pronounced decrease in earnings adjustments following the transition to IFRS. Results are consistent with this conjecture, albeit with marginal significance ( $t=-1.72$ ).

The final variable is the *ForeignDummy*, which proxies for whether the firm is followed by analysts with cross-border coverage prior to IFRS adoption. Analysts who prior to 2005 followed firms reporting under different sets of accounting standards potentially made adjustments to mechanically increase comparability across firms in their coverage portfolios. To the extent that the decrease in analyst adjustments is largely driven by such mechanical comparability effects of the IFRS transition, we expect *ForeignDummy* to have a negative interaction effect with *PostIFRS*. The interaction, however, is positive. We interpret the non-negative coefficient as evidence against comparability being the main driver of the IFRS effect. We discuss this and other tests with implications for comparability in Section 5.

## 4.2 Voluntary adopters

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<sup>20</sup> In non-tabulated tests we also test the *PostIFRS* interaction with an alternative proxy for earnings quality, *AbsDD\_Resid*, which is the absolute value of the residuals from a cross-sectional regression of working capital accruals on prior period, current period and next period cash flows from operations, following Dechow and Dichev (2002), augmented by net property plant and equipment and change in sales, following McNichols (2002). Results are very similar to those obtained using *AbsDiscrAcc*, both quantitatively and qualitatively.



In several European countries, firms had the choice to adopt IFRS prior to the mandatory adoption in 2005. As described in Section 2, several prior studies have investigated voluntary IFRS adopters and their incentives to improve earnings quality. While conclusions about earnings quality effects of mandatory IFRS adoption varies in prior literature, the quality effects of voluntary adoption are generally found to be positive (e.g., Barth et al. 2008, Daske et al. 2008).

In this section, we investigate analysts' earnings adjustments for voluntary adopters. To the extent that our analyst-based measure reliably captures earnings quality we expect absolute analyst adjustments to systematically decrease following voluntary IFRS adoption. An advantage of this setting is that the event year is not constant across firms; in fact, it is not clustered in any particular calendar year either. Thus, any transition effects we document for voluntary adopters are not likely to be due to potential confounding events in or around 2005. Furthermore, the results are informative about whether earnings quality or more mechanical comparability effects drive the main findings in the following sense: Voluntary adopters are free to choose the adoption date, independent of other firms in a given analyst's sector coverage. As such, any adjustment-decreasing effect is less likely to be driven by comparability only (since voluntary IFRS firms would need still to be benchmarked against non-adopting firms continuing to report under various domestic sets of standards); instead, an effect is more likely to be due to perceived earnings quality effects.

The dataset of voluntarily adopters comprises 1,412 firm-year observations for 153 firms that voluntarily shifted from domestic GAAP to IFRS between 1999 and 2004. We exclude firms that reported according to other non-domestic accounting principles (e.g., U.S. GAAP).

The final sample of voluntary adopters contains firms from 16 countries; however, there is a non-trivial concentration of German firms (67 firms).

We construct a new firm-specific  $PostIFRS_{i,t}$  variable that takes the value of 0 for years when the firm reports according to local GAAP and the value of 1 when the firm starts reporting under IFRS. The average absolute analyst adjustment (not tabulated) is 1.74% of total assets in the years prior to IFRS adoption and 0.93% in the years after IFRS adoption, i.e., a decrease of 47% (significant at the .0001 level, the median decrease is also significant at the .0001 level).

Table 5 reports the results from regression (2) for the voluntary adopter sample, using firm-fixed effects (country-fixed effects results are very similar; not tabulated). Results in Columns 1, 2 and 3 are qualitatively similar to the mandatory adopter results in Table 3 in that the  $PostIFRS_{i,t}$  effect is significantly negative, regardless of specification, with t-statistics ranging from -3.09 to -3.56. The point estimates range from 0.72% to 0.81% (of total assets). The magnitude of this effect corresponds to a 42-47% decrease from the average pre-IFRS-adoption level. Compared to the mandatory adopter sample, the IFRS effect for voluntary adopters is larger in magnitude without control variables (comparing Column 1 of Table 5 to Column 1 of Table 3) and comparable in magnitude when control variables are included (comparing Column 3 of Table 5 to Column 3 of Table 3). Unlike the results for mandatory adopters, the time trend variable ( $Time_t$ ) and control for crisis years ( $Crisis_{c,t}$ ) are not significant at conventional levels.

In Columns 4 and 5, we introduce a variable called  $Post2005_t$ , which is defined as follows: 0 for the years 1999 to 2004, and 1 thereafter. We introduce this “pseudo-event” variable for two reasons. First, we want to ensure that our  $PostIFRS_{i,t}$  variable for voluntary adopters is simply not a noisy proxy for 2005 (recall that one of the reasons for the voluntary

adopter analysis is to rule out potential confounding events in 2005, the mandatory adoption year). Second, the variable allows us to speak to whether there are any incremental effects in 2005; for example because of firms' accounting being more comparable after all firms have shifted to IFRS.

In Column 4, we note that the  $Post2005_t$  variable is not significant ( $t=0.72$ ), nor does it meaningfully alter coefficients on other variables. Specifically, the firm-specific  $PostIFRS_{i,t}$  effect remains significant ( $t=-3.17$ ) and with a very similar point estimate (-0.84%). In Column 5, we include  $Post2005_t$ , but exclude the  $PostIFRS_{i,t}$  variable. Also here  $Post2005_t$  is insignificant ( $t=0.18$ ).<sup>21</sup>

We draw two conclusions from these results. First, the firm-specific  $PostIFRS_{i,t}$  variable does not simply proxy for general 2005 effects, which we could not rule out in the analysis of mandatory IFRS adopters. We believe this supports our interpretation that the improvements in earnings quality, as proxied by the decrease in earnings adjustments, are attributable to the shift in accounting standards. Second, the fact that there is no  $Post2005_t$  main effect for the voluntary adopters indicates that there is no discernable additional comparability effect at the time of the general IFRS adoption in 2005.

### 4.3 Additional tests

#### *4.3.1 Prior IFRS experience*

One of the findings in Table 4, where we investigate cross-sectional determinants of the IFRS effect for the mandatory adopters, is that firms followed by analysts with cross-border coverage prior to IFRS adoption had a somewhat muted decrease in adjustments after the

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<sup>21</sup> In untabulated tests we also include cross-sectional determinants, similar to the tests reported in Table 4 for mandatory adopters. The  $PostIFRS$  coefficient remains significant regardless of other variables added to the regression. The statistical significance for the added variables is generally very weak (mostly insignificant at conventional levels), with the exception of *ForeignDummy* which is significant at the 5% level.

mandatory IFRS adoption. One possible reason is that at least some of these analysts had prior experience with IFRS, i.e., following other firms that had voluntarily adopted IFRS prior to 2005. To test this possibility, we measure the proportion of a firm's analysts in a given year (in the pre-2005 period) that also follow at least one IFRS firm. We next construct a variable,  $ExperienceIFRS_i$ , by averaging the proportions over the pre-2005 years. In other words,  $ExperienceIFRS_i$  is meant to capture the "IFRS intensity" among the firm's analysts. We then add an interaction variable of  $ExperienceIFRS_i$  and the  $PostIFRS_t$  dummy as a determinant of  $AnalystAdjustment$  similar to Equation (3). The results (not tabulated) show that the effect is insignificant at conventional levels.

#### 4.3.2 Learning

A potential alternative explanation for the result that analysts adjustments decrease significantly in magnitude after a firm's IFRS adoption is that analysts initially refrain from making adjustments due to lack of experience of the new accounting regime (see, for example, Ernstberger et al. 2008, who document higher forecast errors on the year of the transition). We believe this explanation is less likely, given the finding referenced in the prior section, which indicates that there is no difference due to IFRS experience. To further probe this issue, however, we check the frequency of zero adjustments over the sample years. If analysts refrain from making any adjustment because of lack of experience with IFRS, we would expect an increase in zero-adjustments in 2005 compared to the pre-IFRS period. We find the opposite, however. Zero-adjustments are less frequent in 2005 (and the following years) compared to the pre-IFRS period. That is, although we find a decrease in the absolute level of adjustments they seem to become more frequent over time.

### 4.3.3 2004 Analysis

As part of the IFRS transition firms were required to provide “as if” IFRS numbers for the financial year 2004 as part of their 2005 annual report. Consequently, there exists both local GAAP numbers (from the 2004 annual report) and IFRS numbers (from the 2005 annual report) for the fiscal year 2004. It follows that it is possible for us to calculate analyst adjustments relative to both local GAAP earnings and IFRS earnings holding the firm and year constant.

We collect IFRS earnings of 2004 from Worldscope Restated Time-Series Data provided by Datastream. We also collect local GAAP information for 2004 from Worldscope. In some cases the local GAAP Net Income from Worldscope differs from Net Income in Compustat Global. In such cases we delete the observation.<sup>22</sup> Following the same procedure as in the main tests, we calculate two analyst adjustment variables for 2004,  $AnalystAdjustment^{GAAP2004}$  and  $AnalystAdjustment^{IFRS2004}$ , as the absolute difference between bottom-line EPS (GAAP 2004 or IFRS 2004) and the street EPS from IBES. As reported in Column 1 of Table 6, for the full sample both the mean and median are significantly larger (at the 0.001 level) for  $AnalystAdjustment^{IFRS2004}$  than for  $AnalystAdjustment^{GAAP2004}$ . This 2004 finding is thus not consistent with other results.

A disadvantage of this test is that the analyst in 2004 (when adjustments are made) typically did not have access to information required under IFRS but typically not available under most local GAAPs. This would make it difficult or even impossible to make certain adjustments in 2004 (for example fair value adjustments for financial instruments, real estate and biological assets; information about income statement effects of intangible assets, e.g.,

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<sup>22</sup> We manually verify the database numbers in such cases with hand-collected annual reports. Compustat numbers are consistent with the actual annual report in the vast majority of the cases when Worldscope and Compustat disagree.

capitalization of development costs).<sup>23</sup> To probe the sensitivity to this issue, we investigate the magnitude of positive and negative adjustments separately. Our rationale is that 2004 was a year characterized by growth in the economy and increasing values in both capital and real estate markets. Consequently, income statement effects of these items under IFRS would largely have been positive, making  $AnalystAdjustment^{IFRS2004}$  appear larger. We acknowledge, of course, that a separation by the overall sign of the adjustment is a very crude proxy for items that affect IFRS income but were largely unknowable for analysts in 2004.

Column 2 of Table 6 reveal that positive adjustments are indeed greater in magnitude under IFRS compared to adjustments calculated relative to local GAAP (both the mean and median difference is significant at the 0.001 level). Column 3 shows that the opposite is true for negative adjustment – they are smaller in magnitude under IFRS compared to adjustments calculated relative to local GAAP (mean and median differences are significant at the 0.001 level).

While we hesitate to draw strong conclusions given the obvious limitations of this test, we believe that the result for negative adjustments, the items for which were largely known in 2004, is consistent with IFRS earnings being closer to what analysts perceive as higher quality earnings. The result for positive adjustments, however, is consistent two possible interpretations – either that analysts prefer local GAAP earnings or that adjustments that would have been done under IFRS could not be done because of lack of information in 2004.

#### 4.3.4 *Distribution of firm-years*

In the main tests, we require at least one observation each in the pre- and post-IFRS periods. This leads to some firms being imbalanced in terms of number of firm-years in the post-

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<sup>23</sup> While it in principle was possible for firms to voluntarily disclose such information in the notes to the (local GAAP) financial statements of 2004, such disclosures were not generally the norm

IFRS period versus the pre-IFRS period. To check the sensitivity to this issue, we increase the required number of observations to at least three each in the pre- and post-period in order for a firm to be included in the analysis. Results and inferences (not tabulated) are unaffected by this stricter sample inclusion criterion.

Finally, to the extent that analysts and preparers (firms) are uncertain about implementation issues, fiscal year 2005 may not be fully representative for the post-IFRS period. We verify that our qualitative results are not sensitive to including the year 2005 in the sample tests.

## **5. Summary and discussion**

In this study, we investigate whether earnings quality, as perceived by financial analysts, has improved after firms' adoption of IFRS in the European Union and the European Economic Area. We use the absolute magnitude of analysts' adjustments to reported earnings as an inverse indicator of perceived quality. This interpretation is supported by several studies on North American data, which document that analyst-adjusted earnings have properties consistent with high earnings quality, both in terms of stock market reactions to adjusted versus reported earnings and in terms of valuation-relevant properties, such as persistence and other earnings attributes. In addition, interview studies show that analysts themselves state that they make the adjustments in order to improve on various dimensions of earnings quality.

We find that analyst adjustments decrease significantly in magnitude after firms have adopted IFRS. This is true for firms that mandatorily adopted IFRS in 2005 as well as for firms that voluntarily switched to IFRS in earlier years. The effect is significant both economically and statistically, and it is robust to research design choices, such as estimation method and

inclusion of various control variables. The degree of the IFRS effect is influenced by several factors, such as the strength of a country's regulatory enforcement; however, the main IFRS effect remains significant regardless of what single mediating variable is chosen. We interpret the fact that analysts' adjustments decrease also after firms' voluntary adoption of IFRS (when the majority of firms still reports under various domestic set of standards) as inconsistent with the decrease in adjustments being due to a mere comparability argument. Nor do we find any incremental effect for early voluntary adopters when peer firms adopt IFRS during the mandatory adoption period in 2005, as would be expected if accounting standard comparability is a main driving force for analyst adjustments.<sup>24</sup> In summary, we believe that our evidence is consistent with financial analysts perceiving IFRS earnings to be of higher quality than prior domestic GAAP earnings, i.e. that the IFRS mandate (as well as some firms' earlier IFRS adoptions) was positive for earnings quality within the EU and the EEA.

Our main results and conclusions are largely robust to research design choices. In contrast, much prior research has concluded that earnings quality, accounting comparability, and capital market effects of IFRS adoption are conditional on factors such as a country's legal enforcement, whether the adoption is voluntary or mandatory, etc. For example, Ahmed et al. (2013) conclude that earnings quality (measured as various earnings attributes such as abnormal accruals, smoothing, etc.) has deteriorated following IFRS adoption, whereas Barth et al. (2008) conclude the opposite. Ahmed et al. attribute the differing results to the fact that Barth et al. concentrate on voluntary adopters, who are likely to have incentives to improve earnings quality, whereas Ahmed et al. focus on mandatory adopters. Another example is accounting compara-

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<sup>24</sup> Further reinforcing that results appear not to be driven by mere comparability effects is the fact that firms that had analysts with international coverage before the IFRS transition (i.e., analysts that had to deal with multiple sets of standards) did not experience a larger decrease in adjustments after the mandatory 2005 adoption of IFRS compared to firms that were followed by analysts with single-country coverage (as would have been expected if accounting standard comparability across an analysts' covered firms is a major driver of adjustments).



bility, where Yip and Young (2012) conclude that it improves after IFRS adoption, whereas Cascino and Gassen (2014) find that any improvement in comparability is marginal at best, and that it is conditional on firm-, region-, and country-level compliance determinants. Finally, we do not claim that our results speak more or less to the overall question about earnings quality effects of IFRS adoption compared to studies that have looked at other earnings quality measures. Rather, we agree with Dechow et al. (2010) that earnings quality is most meaningfully defined through a specific user's perspective and decision-making context, and, in that sense, we believe that financial analysts and their earnings adjustments provide an interesting arena to study questions about earnings quality.

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## Appendix A: Variable list

<i>AbsDiscrAcc</i>	Absolute discretionary accruals from estimates using the modified Jones model estimated using the cross-sectional approach from Ecker, Francis, Olsson and Schipper (2013)
<i>AccountingDistance</i>	A factor based on differences between country-specific accounting regimes and IFRS prior to IFRS adoption
<i>AnalystAdjustments</i>	The absolute value of the difference between bottom-line EPS and street earnings in period t deflated by total assets per share
<i>Crisis</i>	Dummy variable taking the value of 1 if the GDP growth is negative for a specific country
<i>Dispersion</i>	Analyst forecast dispersion according to IBES International Summary File
<i>Enforcement</i>	A factor based on Brown et al. (2014) indicator variables of the strength of country-level regulatory enforcement
<i>ForeignDummy</i>	Dummy variable taking the value of one the specific firm is followed by at least one analyst with coverage of at least one firm that does not follow the same accounting standards in the pre-IFRS period
<i>IFRSexperience</i>	Average over the pre-2005 periods of the firm-specific proportion of analysts following voluntary adopters of IFRS in the pre-2005 period.
<i>Post2005</i>	Dummy variable taking the value of 1 if the year is 2005 or later
<i>PostIFRS</i>	Firm-specific dummy variable taking the value of 1 if the relevant year is the firm-specific IFRS transition year or later
<i>Time</i>	A trend variable measured as the difference between the relevant year and 1999 (the first year of the sample period)

Table 1  
Sample Counts per Country

Country	Firm years	# Firms
Austria	94	12
Belgium	618	62
Czech Republic	24	2
Germany	1,086	126
Denmark	471	51
Spain	841	74
Finland	908	83
France	2,605	259
Great Britain	5,577	608
Greece	590	69
Hungary	24	3
Ireland	225	21
Italy	1,465	154
Netherlands	717	67
Norway	733	82
Poland	237	24
Portugal	242	23
Sweden	1,095	116
Slovenia	37	4
<b>Total</b>	<b>17,589</b>	<b>1,840</b>

Table 2  
Descriptive Statistics

*Panel A: Absolute Adjustments*

	<b># Obs.</b>	<b>Mean</b>	<b>Std. dev.</b>	<b>Q1</b>	<b>Median</b>	<b>Q3</b>
Total sample	17,589	0.0175	0.0467	0.0001	0.0014	0.0131
Pre-IFRS period	7,412	0.0193	0.0542	0.0001	0.0022	0.0136
Post-IFRS period	10,177	0.0162	0.0403	0.0001	0.0011	0.0125
Difference		0.0031	0.0139	0.0000	0.0011	0.0011
Significance of difference		< 0.0001			< 0.0001	

*Panel B: Descriptive Statistics of Other Variables*

<b>Variable</b>	<b># Obs.</b>	<b>Mean</b>	<b>Std. dev.</b>	<b>Q1</b>	<b>Median</b>	<b>Q3</b>
<i>Firm-year-specific Variables</i>						
Abs. Discr. Accruals	12,835	0.0806	0.0731	0.0404	0.0606	0.0956
Forecast Dispersion	12,835	0.9994	2.5939	0.0900	0.2940	1.0160
ForeignDummy	12,835	0.6500	0.3812	0.3333	0.8000	1.0000
<i>Country-specific Variables</i>						
Enforcement (Factor)	17,589	0.0817	1.5699	-0.8250	0.7200	1.1270
Accounting Distance (Factor)	17,589	0.0003	0.3433	-0.2830	-0.0680	0.2460

Table 3  
Main Results

Variable	Exp. Sign	(1)	(2)	(3)	(4)	(5)	(6)
<i>PostIFRS</i>	(-)	-0,0025 -3,55	-0,0035 -4,70	-0,0087 -6,56	-0,0026 -3,73	-0,0035 -4,82	-0,0087 -6,93
<i>Crisis</i>	(+)		0,0046 4,40	0,0040 3,81		0,0045 4,53	0,0040 3,98
<i>Time</i>	(+)			0,0008 4,74			0,0008 5,08
<i>Country-Fixed Effects</i>		<i>Yes</i>	<i>Yes</i>	<i>Yes</i>			
<i>Firm-Fixed Effects</i>					<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
<i>Adj. R<sup>2</sup></i>		0,0297	0,0307	0,0319	0,1646	0,1657	0,1670
<i># Obs.</i>		17 589	17 589	17 589	17 589	17 589	17 589

The dependent variable is equal to absolute earnings adjustments scaled by total assets, for firm  $i$  in year  $t$ . The independent variables are described in the list of variables in Appendix A.



Table 4  
Highlighting Cross-Sectional Variation in the Main Effect

Variable	Exp. Sign	(1)	(2)	(3)	(4)	(5)
<i>PostIFRS</i>	(-)	-0.0086 -6.82	-0.0088 -7.00	-0.0063 -3.76	-0.0097 -6.62	-0.0148 -7.58
<i>Crisis</i>	(+)	0.0040 3.99	0.0038 3.82	0.0030 2.63	0.0031 2.69	0.0031 2.70
<i>Time</i>	(+)	0.0008 5.05	0.0009 5.17	0.0009 4.83	0.0009 4.89	0.0009 4.81
<u>Interaction of <i>PostIFRS</i> with:</u> <i>Enforcement</i>	(-)	-0.0012 -2.77				
<i>Accounting Distance</i>	(-)		-0.0064 -3.15			
<i>Pre-IFRS Absolute Discretionary Accruals</i>	(-)			-0.0472 -4.43		
<i>Pre-IFRS Forecast Dispersion</i>	(-)				-0.0005 -1.72	
<i>Pre-IFRS Analyst Foreign Coverage</i>	(-)					0.0072 3.46
<i>Firm-Fixed Effects</i>		Yes	Yes	Yes	Yes	Yes
<i>Adj. R<sup>2</sup></i>		0.1675	0.1676	0.1485	0.1472	0.1479
<i># Obs.</i>		17,589	17,589	12,835	12,835	12,835

The dependent variable is equal to absolute earnings adjustments scaled by total assets, for firm  $i$  in year  $t$ . The independent variables are described in the list of variables in Appendix A.

Table 5  
Main Effects For Voluntary Adopters

Variable	(1)	(2)	(3)	(4)	(5)
<i>PostIFRS (firm-specific)</i>	-0,0072 -3,56	-0,0072 -3,53	-0,0081 -3,09	-0,0084 -3,17	
<i>CrisisYear</i>		0,0000 -0,01	-0,0002 -0,07	0,0000 0,01	-0,0004 -0,13
<i>Time</i>			0,0002 0,54	-0,0001 -0,18	-0,0005 -1,12
<i>Post2005</i>				0,0025 0,72	0,0006 0,18
<i>Firm-Fixed Effects</i>	Yes	Yes	Yes	Yes	Yes
<i>Adj. R<sup>2</sup></i>	0,1701	0,1694	0,1690	0,1686	0,1627

The dependent variable is equal to absolute earnings adjustments scaled by total assets, for firm  $i$  in year  $t$ . The independent variables are described in the list of variables in Appendix A.

Table 6  
2004 Analysis

	Full sample	Neg. Adjustments	Pos. Adjustments
Mean Absolute Adjustment under Local GAAP	0.0248	0.0261	0.0235
Mean Absolute Adjustment under IFRS	0.0280	0.0230	0.0324
Difference in means	-0.0032	0.0031	-0.0089
<i>t-stat</i>	-3.29	2.40	-6.45
Median Absolute Adjustment under Local GAAP	0.0065	0.0112	0.0046
Median Absolute Adjustment under IFRS	0.0098	0.0087	0.0105
Difference in median	-0.0033	0.0025	-0.0059
<i>p-value (Wilcoxon rank sum test)</i>	0.0001	0.0002	0.0001
# Obs.	749	353	396