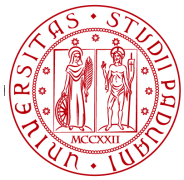

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SCUOLA DI DOTTORATO DI RICERCA IN

ECONOMIA E MANAGEMENT

CICLO XXV

Firm and Country Determinants of the Quality of Financial Information

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Contents

Introduction

A large number of countries around the world have done considerable efforts to improve the regulation of security markets, corporate governance and financial reporting in recent years. Many of these reforms were introduced in response to the accounting scandals and corporate frauds at the beginning of the 2000s, with the general objective to enhance the quality of financial reporting and investors' confidence over the functioning of financial markets (Leuz 2011). Moreover, many countries have converged to a unique set of reporting rules by moving to the International Financial Reporting Standards, under the assumption that the adoption of unique set of capital market oriented accounting standards should uniformly lead to an increase in comparability, corporate transparency, and financial reporting quality.

Despite this considerable effort, a growing stream of study in the international accounting literature claims that differences in the observed reporting behavior, and hence in financial reporting quality, across firms are likely to persist even if enforcement and accounting standards were held constant. Overall, this literature suggests that the outcomes of the financial reporting process strongly depends on firm incentives to provide financial information of high or low quality (Ball et al. 2000; Ball et al. 2003; Leuz et al. 2003; Burgsthaler et al. 2006). Therefore, a regulatory change is unlikely to have a direct effect on financial reporting quality, rather reporting incentives would shape the effects of regulatory changes on financial reporting quality. Firm reporting incentives are affected by many forces such as country's legislation, the strength of the legal enforcement, capital market pressure, security regulation, accounting standards and other firm-level factors (e.g. auditor quality, ownership structure and governance structure).

In this vein, international accounting literature (see Wysocki 2011 for an overview) has

started to examine how across country institutional and legal differences and within country variation in firm characteristics affect the properties of accounting numbers, firms' disclosure strategy, corporate transparency, and economic or capital market outcomes. This literature points out the pivotal role of these institutional reporting incentives for the observed heterogeneity in financial reporting practices and attempts to identify: (i) which are the country-level institutions and firm-level factors that cause firms' reporting choices; (ii) how country-level institutions interact, by complementing or substituting one with the others, in shaping financial reporting outcomes; (ii) how and whether firm-level mechanisms interact with country-level institutions; (iv) which are the mechanisms or channels by which institutions influence firms' reporting practices, corporate transparency and economic outcomes. Despite several studies have already contributed to this area, international accounting literature still provides scant evidence on the specific factors that shape the outcomes of the financial reporting process (Holthausen et al. 2009). In particular, it is not obvious which is the marginal effect of the legal enforcement, capital markets' regulation, disclosure requirements, accounting standards and firm-level monitoring mechanisms on the quality of financial reporting. Most importantly, international accounting research still treats firms homogenously within a given country while many authors suggest that this approach is unlikely to be very productive (Holthausen 2003; Wysocki 2011). Indeed, this design does not take into account that some firms in a given country may bear incentives to opt out of their institutional regime by, for example adopting good governance mechanisms or cross-listing in more demanding legal environment. Without considering this variation, cross-country studies are unlikely to properly identify the marginal effect of the forces that shape financial reporting quality as the results of these studies "…will be a weighted average of all firms in the economy…" (Holthausen 2003, p. 282).

Given the importance of financial reporting quality and corporate transparency for economic growth, the efficient allocation of financial resources and the development of capital markets (Levine 1998, Rajan and Zingales 2002, Francis et al. 2009) it appears relevant and timely to identify, and disentangle one from the others, the effects of country-level institutions, legal enforcement, disclosure regulation, accounting standards, and firm-level factors on firms' reporting choices to explain differences in the observed financial reporting quality.

This thesis is in three research papers. The first chapter, a joint work with Saverio Bozzolan (University of Padova) investigates the effect of the legal enforcement on the use of income increasing earnings management and downward expectation management to meet or beat analyst forecasts. The second chapter, a join work with Antonio Parbonetti (University of Padova) and Michel L. Magnan (Concordia University) examine how firm-level governance, as proxied by board attributes, and country-level enforcement interplay in affecting financial reporting quality once IFRS became mandate. The third chapter examines whether information environment benefits following cross-listing in the U.S. vanish when the financial reporting process suffers by internal control deficiencies according to the Section 302 of the Sarbanes-Oxley Act.

More in detail, the first chapter investigates the effect of the legal enforcement on the use of income increasing earnings management and downward expectation management to meet or beat analyst earnings forecasts. Literature suggests that managers put considerable emphasis on reporting earnings that meet or beat analyst earnings forecasts (Graham et al. 2005), as long as capital markets reward firms that meet analyst forecasts and penalizes firms that do not (Bartov et al. 2002; Brown and Caylor 2005). To the extent that managers recognize the signalling implications of meeting/beating analyst forecasts, research suggests that they bear strong incentives to take actions to avoid a negative earnings news. When a firm's realized earnings fall short of analyst forecasts, managers seek to avoid negative earnings surprises by manipulating

accruals upward (EAR) or guide analyst earnings forecasts downward (EXP) as a competing or substitute action. Research has mainly examined EAR and EXP in isolation by assuming that they are simply used in conjunction (Matsumoto 2002, Athanasakou et al. 2010). Only recently, literature has considered that EAR and EXP might be substitutes or complements with respect to their constraints: firms rely more on expectations management when firms' capacity for earnings management is bounded. So far, literature has exploited only cross-sectional variation in firm-specific constraints as determinant of the choice between EAR and EXP. On the other side, international accounting literature underlies that country-level institutional characteristics such as the quality of a country's enforcement environment shape firms' reporting choices and, thus, across-country differences in observed reporting behavior.

This chapter examines how EAR and EXP contribute to meeting or beating analyst forecasts in relation to legal enforcement, while holding constant accounting standards. Using a sample of 4,934 firms from fourteen European countries, we document that the strength of the legal enforcement is negatively associated with EAR and positively associated with EXP. We provide evidence of a substitution effect between EAR and EXP only in the presence of strong legal enforcement, while they are complements when the legal enforcement is weak. We show that the capital market rewards meeting/beating firms, but it penalizes the concurrent use of EAR and EXP and this penalty is positively associated with the strength of the legal enforcement only for EAR. Our results show that legal enforcement has a significant role in the choice between EAR and EXP and that a change in the strength of legal enforcement drives how firms operate to meet or beat analyst forecasts.

The contribution of this chapter is two-fold. This is the first study that examine how cross-country variation in the level of enforcement affects the interaction between EAR and EXP to

meet/beat analyst forecasts. Second, this chapter is the first study to explore whether the capital market's reaction to an earnings surprise is related to the strength of the legal enforcement.

The second chapter exploits cross-sectional variation in board-based monitoring intensity and country-level enforcement to examine heterogeneity in IFRS mandatory adoption consequences on financial reporting quality. Extant research shows substantial economic benefits stemming from IFRS mandatory adoption, suggesting that a simple switch from local GAAP to IFRS leads to an increase in comparability, transparency, and financial reporting quality. However, it is still challenging to attribute such benefits to the IFRS adoption *per se*. To the extent that the application of accounting standards provides insiders with substantial discretion, research stresses that firms' reporting behavior, and hence the observed financial reporting quality, is likely to be shaped by institutional factors and firm-level reporting incentives, rather than a simple change in accounting standards (Ball et al. 2003; Leuz et al. 2003; Burgsthaler et al. 2006). Consistently, much of the previous studies (Daske et al. 2008; Christensen et al. 2012) document substantial heterogeneity in the effects of IFRS adoption due to differences in legal institutions, pointing out the role of country-level infrastuctures for across-country differences in observed reporting practices. Those studies explain the heterogeneous effects of IFRS adoption across countries but miss to examine heterogeneity across firms within similar legal environment.

This paper tries to fill this gap in the literature analyzing how firm-level governance, as proxied by board attributes, and country-level enforcement interplay in affecting financial reporting quality. We operationalize financial reporting quality using earnings informativeness, accruals management, and real earnings management. We use a treatment sample of 3,476 firm-year observations from 14 European countries that mandatorily adopt IFRS in 2005 and 29,596 firm-year observations from 11 non-IFRS adoption countries. To account for the confounding effects of general trends in financial reporting quality or concurrent events unrelated to IFRS

adoption, we estimate annual panel regressions for IFRS adopter firms and non-IFRS adopter firms using industry-country and separate year fixed effects for the treatment and control sample.

Three key findings emerge from our analyses. First, IFRS adoption is, on average, associated with an increase in financial reporting quality. However, there is considerable heterogeneity in financial reporting quality changes, suggesting that IFRS mandatory adoption is not sufficient, *per se*, to change firms' reporting practices. Second, in countries characterized by weak enforcement, strong board-level monitoring appears to enhance financial reporting quality, thus suggesting a substitutive effect between firm- and country-level governance. Third, in countries characterized by strong enforcement, firms with strong board-level monitoring exhibit a higher level of financial reporting quality than firms with weak board-level monitoring, thus suggesting that country- and firm-level governance are complementary.

The chapter contributes to the literature in two ways. First, this is the first study that examines whether board-based monitoring mechanisms shape IFRS mandatory adoption consequences on financial reporting quality. Second, the chapter contributes to the growing literature on the interplay between firm-level governance and country institutional characteristics. The findings point toward a substitution effects between firm-level monitoring mechanisms and country-level enforcement mechanisms when the legal system is lax, while board monitoring and legal enforcement complement each other when the legal system gets stricter.

The third chapter examines whether informational environment benefits following cross-listing in the U.S. vanish when the financial reporting process suffers by internal control deficiencies according to the Section 302 of the Sarbanes-Oxley Act (SOX, hereafter). Previous literature documents an increase in the quality of the firm information environment following cross-listing in the U.S. and motivates this result with the bonding effect. Indeed, cross-listing in the U.S. provides an effective way for firms incorporated in countries where investors rights are

weak and bad enforced to credibly commit to increase corporate transparency as they voluntary subject themselves to U.S. security law and SEC enforcement. The stronger capital market and enforcement scrutiny triggers an increase in the availability of information of higher quality and enhances firm information environment.

This study disputes the idea that the cross-listing *per se* enhances the quality of firms' information environment. We challenge this idea considering whether the quality of the information environment for cross-listed firms depends on an effective commitment to achieve higher levels of corporate transparency. As research setting, we use Section 302 of the SOX that requires to disclose any discovered internal control deficiency on internal controls over financial reporting.

To account for the impact of general trends or concurrent events unrelated to SOX302 disclosures on information environment of cross-listed firms, we employ as benchmark group all firms listed in their home market but not in the U.S. In addition, we employ propensity-score matching models to take into account differences in firm-characteristics between cross-listed and non-cross-listed firms while estimating SOX302 disclosure treatment effect. Our analyses encompasses both changes and cross-sectional association tests. We show that cross-listed firms disclosing internal control deficiencies do not have a better information environment than their home-country peers, but only after the first disclosure on internal control deficiencies according to SOX302. Second, we show that cross-listed firms experience an improvement in the information environment if they remediate to previously disclosed internal control deficiencies. Finally, we show that these results hold only for firms domiciled in countries with weak legal institutions, while cross-listed firms from countries with strong legal institutions do not experience a significant change in the quality of the information environment once they became cross-listed, irrespective from the disclosure of an internal control deficiency.

The study contributes to the literature on cross-listing in two ways. First, we show the existence of substantial heterogeneity in cross-listing effects on firm information environment, driven by the adoption of adequate internal controls over financial reporting. Second, we add to the literature on the effects of the SOX. Literature shows that cross-listed firms experience a decrease in the level of opaqueness after the adoption of the SOX. We add to this literature the evidence that the decline in the level of opaqueness depends on cross-sectional differences in corporate transparency and hence it is not homogenous across all firms.

Introduzione

Nell'ultimo decennio, molti paesi in tutto il mondo hanno compiuto notevoli sforzi per migliorare la regolamentazione dei mercati finanziari, della corporate governance e della reportistica di bilancio. Molte di queste riforme sono state introdotte in risposta agli scandali contabili e alle frodi finanziarie avvenute nei primi anni 2000, con l'obiettivo di migliorare la qualità dell'informativa finanziaria e la fiducia degli investitori sul corretto funzionamento dei mercati finanziari (Leuz 2011). Inoltre, numerosi paesi hanno adottato i principi contabili internazionali (IFRS), in base al presupposto che l'adozione di un unico set di principi contabili, orientati ai bisogni conosciti degli investitori finanziari, dovrebbe condurre ad un uniforme aumento della comparabilità, della trasparenza societaria, e della qualità della comunicazione economico-finanziaria.

Nonostante questo notevole sforzo, la letteratura di *international accounting* sostiene che differenze nel comportamento di comunicazione delle imprese, e quindi nella qualità dell'informativa finanziaria, tra le imprese continueranno a persistere anche se le imprese stesse fossero soggette agli stessi principi contabili o agli stessi meccanismi di *enforcement*. Infatti, la letteratura suggerisce come la qualità dell'informativa di bilancio dipenda dal sistema di incentivi di ogni impresa a fornire informazioni finanziarie di alta o bassa qualità (Ball et al 2000; Ball et al 2003; Leuz et al 2003; Burgsthaler et al. 2006). Pertanto, è improbabile che un cambiamento legislativo abbia un effetto diretto sulla qualità della comunicazione economico-finanziaria, piuttosto sarà il sistema di incentivi di ogni impresa a determinare gli effetti delle modifiche legislative sulla qualità dell'informativa finanziaria (Mahoney 2004; Holthausen 2009). Il sistema di incentivi di ogni impresa a fornire informazioni economiche-finanziarie di una data qualità è

influenzato da molte forze, quali la legislazione del paese, il sistema di *enforcement*, l'importanza del mercato dei capitali, i principi contabili e altri fattori a livello di impresa (ad esempio la qualità del revisore contabile, la struttura proprietaria o il sistema di governance). In tale prospettiva, la letteratura di *international accounting* (Wysocki 2011) ha iniziato a esaminare in che modo le differenze tra i paesi, in termini di istituzioni o sistemi giuridici, e la variabilità all'interno di ogni paese in termini delle caratteristiche di impresa, influiscano sulla qualità dell'informativa contabile, sulle politiche di *disclosure*, e sulla trasparenza aziendale. Questa letteratura sottolinea il ruolo fondamentale di questi incentivi nello spiegare l'eterogeneità nella qualità dell'informativa economico-finanziaria e tenta di identificare: (i) quali siano le istituzioni ed i fattori, a livello paese ed a livello impresa, che causano le scelte di comunicazione delle imprese, (ii) come le istituzioni a livello paese interagiscono tra loro nella determinazione della qualità dell'informativa economico-finanziaria, (ii) in che modo i meccanismi a livello impresa interagiscono con le istituzioni a livello paese, (iv) quali sono i meccanismi o canali attraverso i quali le istituzioni influenzano le pratiche di rendicontazione delle imprese e la trasparenza aziendale. Nonostante diversi studi abbiano già contribuito a questo filone di ricerca, la letteratura di *international accounting* non ha ancora fornito evidenze dirette sui fattori specifici che determinano la qualità dell'informativa economico-finanziaria (Holthausen et al. 2009). In particolare, non è tuttora evidente quali siano gli effetti marginali del sistema di *enforcement*, della regolamentazione dei mercati dei capitali, dei principi contabili, e dei meccanismi di monitoraggio a livello di impresa sulla qualità dell'informativa economico-finanziaria. Inoltre, la letteratura di *international accounting* considera ancora le imprese all'interno di un dato paese in modo omogeneo, mentre molti autori suggeriscono come questo approccio non sia in realtà molto produttivo (Holthausen 2003; Wysocki 2011), nella misura in cui non tiene conto del fatto che alcune imprese in un determinato paese possano avere incentivi a segnalare il loro impegno ad

raggiungere un livello di trasparenza nella comunicazione verso i mercati superiore a quello richiesto dal sistema giuridico e dalle caratteristiche dei mercati dei capitali nel quale operano. Senza considerare questa variabilità nel sistema di incentivi di ogni impresa, è improbabile che questi studi identifichino correttamente l'effetto marginale di ognuna delle forze che modellano la qualità dell'informazione economico-finanziaria, nella misura in cui i risultati di questi studi "... *will be a weighted average of all firms in the economy*..." (Holthausen 2003, p. 282). Data l'importanza della qualità dell'informativa economico-finanziaria e della trasparenza aziendale per la crescita economica, l'efficiente allocazione delle risorse finanziarie e lo sviluppo dei mercati dei capitali (Levine 1998; Rajan e Zingales 2002; Francis et al. 2009) appare pertinente e rilevante identificare quale sia l'effetto marginale delle istituzioni di un paese, dei principi contabili, e dei fattori a livello di impresa sulle scelte di comunicazione delle imprese per spiegare le differenze nella qualità dell'informativa economico-finanziaria osservata.

La tesi si compone di tre *research paper*. Il primo capitolo, frutto di un lavoro congiunto con Saverio Bozzolan (Università di Padova) indaga l'effetto del *legal enforcement* sull'uso dell'*income incresing earnings management* e del *downward expectation management* al fine di battere le stime degli analisti finanziari. Il secondo capitolo, frutto di un lavoro congiunto con Antonio Parbonetti (Università di Padova) e Michel Magnan (Concordia University) esamina l'effetto congiunto del sistema di governance a livello impresa e delle caratteristiche istituzionali a livello paese sulla qualità dell'informativa economico-finanziaria attorno all'adozione dei principi contabili internazionali. Il terzo capitolo esamina se i benefici in termini di *firm information environment* seguenti al cross-listing negli Stati Uniti svaniscano se il sistema di controllo interno soffra di *material weaknesses* secondo la sezione 302 del Sarbanes- Oxley Act.

Nello specifico, Il primo capitolo si propone di esaminare l'effetto del *legal enforcement* a livello paese sull'uso dell'*income incresing earnings management* e del *downward expectation*

management al fine di battere le previsioni degli analisti finanziari.

La letteratura suggerisce che i manager abbiano forti incentivi a raggiungere o battere le stime degli analisti finanziari (Graham et al 2005), in quanto il mercato premia le imprese che sono in grado di battere le stime e penalizza le imprese che invece riportano utili inferiori alle attese (Bartov et al 2002;. Brown e Caylor 2005). Nella misura in cui i manager riconoscono le implicazioni che derivano dal battere le stime degli analisti, la letteratura sostiene che essi abbiano forti incentivi a intraprendere azioni volte ad evitare di riportare utili inferiori alle attese del mercato. Se gli utili reali, privatamente osservati dal management, sono inferiori alle attese degli analisti, i manager possono manipolare gli utili reali verso l'alto (*income increasing earnings management*: EAR) o guidare verso il basso le previsioni sugli utili degli analisti (*downward expectation management*: EXP). In letteratura queste due azioni sono state principalmente esaminate separatamente, assumendo che le imprese le utilizzino semplicemente in combinazione (Matsumoto 2002; Athanasakou et al. 2010). Solo di recente, la letteratura ha considerato che EAR e EXP possano essere azioni sostitute o complementari in funzione dei rispettivi vincoli: le imprese ricorreranno maggiormente al *downward expectation management*, se la capacità di ricorrere all'*income increasing earnings management* è limitata. Fino ad ora, la letteratura ha esaminato se differenze nelle caratteristiche specifiche delle imprese determinino la scelta di ricorrere a EAR e EXP. Tuttavia, la letteratura di *international accounting* rileva come le caratteristiche istituzionali di un paese, come la qualità del sistema di *enforcement*, abbiano un effetto sulle decisioni di comunicazione economico finanziaria delle imprese.

Questo capitolo esamina come EAR e EXP contribuiscano a battere le previsioni degli analisti in funzione del livello di *enforcement.* Utilizzando un campione di 4,934 osservazioni anno-impresa da quattordici paesi europei, il presente lavoro documenta che la qualità del sistema di *enforcement* sia associato negativamente a EAR e positivamente a EXP. Inoltre, il presente

lavoro fornisce evidenza di un effetto di sostituzione tra EAR e EXP in presenza di una forte sistema di *enforcement*, mentre EAR e EXP risultano essere complementari quando il sistema di *enforcement* è debole. Il mercato dei capitali premia le imprese che battono le stime degli analisti, ma penalizza l'uso di EAR e EXP, tuttavia solo la penalità per l'uso di EAR dipende positivamente dal sistema di *enforcement*.

Il contributo di questo capitolo alla letteratura è duplice. Questo è il primo studio che esamina come il livello di *enforcement* determini la scelta tra EAR e EXP per battere le previsioni degli analisti. In secondo luogo, questo studio è il primo ad analizzare se la reazione del mercato dei capitali all'uso di EAR e EXP al fine di battere le stime degli analisti dipenda dal sistema di *enforcement*.

Il secondo capitolo sfrutta la variabilità nelle caratteristiche del sistema di monitoraggio a livello impresa per esaminare l'esistenza di eterogeneità nelle conseguenze dell'adozione obbligatoria degli IFRS sulla qualità dell'informazione economico-finanziaria. La letteratura ha mostrato l'esistenza di notevoli benefici economici derivanti dalla adozione degli IFRS, suggerendo come un semplice passaggio dai principi contabili nazionali agli IFRS possa portare ad un aumento della comparabilità, della trasparenza, e della qualità della comunicazione economico-finanziaria. Tuttavia, risulta essere problematico, sia da un punto di vista teorico che empirico, attribuire tali benefici all'adozione degli IFRS. Nella misura in cui l'applicazione dei principi contabili preveda una notevole discrezionalità, la ricerca sottolinea come il comportamento di reportistica delle imprese, e quindi la qualità osservata nella comunicazione economico-finanziaria, sia determinata da fattori istituzionali a livello paese e dagli incentivi a livello di impresa, piuttosto che un semplice cambiamento dei principi contabili (Ball et al 2003; Leuz et al 2003; Burgsthaler et al 2006). Coerentemente, la maggior parte degli studi precedenti (Daske et al 2008; Christensen et al 2012) documenta una sostanziale eterogeneità negli effetti

dell'adozione degli IFRS a causa di differenze nelle caratteristiche istituzionali tra i paesi. Tali studi spiegano l'eterogeneità negli effetti dell'adozione degli IFRS tra i paesi, ma non analizzano l'eterogeneità che può sussistere tra le imprese anche all'interno di un dato paese.

Questo articolo cerca di colmare questa lacuna nella letteratura analizzando come il sistema di governance, a livello impresa, e le caratteristiche istituzionali, a livello paese, determinino congiuntamente la qualità dell'informativa economico-finanziaria a seguito dell'adozione degli IFRS. Il lavoro utilizza un campione di 3,476 osservazioni provenienti da 14 paesi europei che hanno adottato obbligatoriamente gli IFRS nel 2005 (*treatment sample*) e 29,596 osservazioni provenienti da 11 paesi che non hanno adottato gli IFRS (*control sample*). La qualità dell'informativa economico-finanziaria viene misurata considerando l'*earnings informativeness, accruals management, and real earnings management*.

Al fine di controllare per i *confounding effects* derivanti da trend generali nella qualità dell'informativa economico-finanziaria e per eventi concomitanti ma estranei all'adozione degli IFRS, il presente lavoro impiega regressioni panel annuali per le imprese che adottano gli IFRS e per il campione di controllo utilizzando effetti fissi per paese-settore ed anno, separati per il *treatment sample* e per il *control sample*. Dall'analisi emergono tre principali risultati. In primo luogo, l'adozione degli IFRS è, in media, associata ad un aumento della qualità dell'informativa economico-finanziaria. Tuttavia, vi è una notevole eterogeneità negli effetti, il che suggerisce che l'adozione obbligatoria degli IFRS non sia sufficiente, di per sé, a modificare le politiche di reporting delle imprese. In secondo luogo, in paesi caratterizzati da un sistema di *enforcement* debole, le imprese che adottano buoni sistemi di governance, migliorano la qualità dell'informazione economico-finanziaria, suggerendo l'esistenza di un effetto sostitutivo tra sistema di governance a livello impresa e di *enforcement* a livello paese, quando l'*enforcement* è debole. In terzo luogo, in paesi caratterizzati da un sistema di *enforcement* forte, le imprese che

adottano buoni sistemi di governance migliorano la qualità dell'informativa economico-finanziaria in misura superiore rispetto alle imprese che adottano deboli sistemi di governance, suggerendo l'esistenza di una relazione di complementarietà tra governance a livello impresa ed *enforcement* a livello paese quando quest'ultimo è più forte.

Il capitolo contribuisce alla letteratura in due modi. In primo luogo, questo è il primo studio che analizza se i meccanismi di monitoraggio a livello di impresa influenzino le conseguenze derivanti dall'applicazione obbligatoria degli IFRS sulla qualità dell'informazione economico-finanziaria. In secondo luogo, il capitolo contribuisce alla crescente letteratura sull'interazione tra sistemi di governance a livello impresa e caratteristiche istituzionali a livello paese. Il lavoro suggerisce l'esistenza di un effetto di sostituzione tra i due, quando il sistema di *enforcement* è debole, e l'esistenza di una relazione di complementarietà quando il sistema di *enforcement* è più efficace.

Il terzo capitolo esamina se i benefici in termini di *firm information environment* conseguenti al *cross-listing* negli Stati Uniti svaniscano se il sistema di controllo interno soffra di *material weaknesses* secondo la sezione 302 del Sarbanes- Oxley Act. La letteratura documenta un aumento della qualità del *firm information environment* a seguito del *cross-listing* negli Stati Uniti e spiega questo risultato con la *bonding theory*. Infatti, il *cross-listing* negli Stati Uniti risulta essere uno strumento efficace per le imprese avente sede in paesi con un sistema di *enforcement* debole, per segnalare il proprio impegno ad aumentare la trasparenza dell'informativa economico-finanziaria, nella misura in cui queste imprese si sottopongono volontariamente alla giurisdizione della SEC. Il maggior monitoraggio esercitato dal mercato dei capitali statunitense determina un aumento della disponibilità di informazioni di qualità superiore e dunque migliora il *firm information environment.* Questo studio disputa l'idea che il *cross-listing* di per sé migliori la qualità del *firm information environment.* In particolare, questo studio

esamina se il cambiamento nella qualità del *firm information environment* dipenda dall'effettivo impegno delle imprese cross-listate ad incrementare il proprio livello di trasparenza contabile. Come setting di ricerca, il presente lavoro utilizza la sezione 302 della SOX che richiede di comunicare ogni *material weaknesses* che possa inficiare l'efficacia del sistema di controllo interno.

Al fine di controllare per l'effetto di trend generali o per eventi concomitanti ed estranei all'adozione della sezione 302 della SOX sul *firm information environment* delle imprese cross-listate, il presente lavoro utilizza come campione di controllo le imprese quotate nei rispettivi mercati nazionali ma non cross-listate negli Stati Uniti. Inoltre, utilizziamo un *propensity score matching* al fine di controllare per eventuali differenze sistematiche nelle caratteristiche di impresa tra imprese cross-listate e non, nello stimare l'effetto specifico della sezione 302 della SOX sul *firm information environment.*

I risultati mostrano che le imprese cross-listate che comunicano l'esistenza di *material weaknesses* nel sistema di controllo interno non hanno un *firm information environment* migliore rispetto alle imprese non cross-listate, ma questo solo a seguito dell'adozione della sezione 302 della SOX. In secondo luogo, il lavoro mostra che le imprese cross-listate migliorano il *firm information environment* se comunicano di aver rimediato a *material weaknesses* dichiarate in precedenza. Infine, si dimostra che questi risultati valgono solo per le imprese con sede in paesi con deboli sistemi di *enforcement*, mentre le imprese cross-listate provenienti da paesi con sistemi di *enforcement* efficaci, non subiscono un cambiamento significativo nella qualità del *firm information environment* a seguito del cross-listing, indipendentemente dalla comunicazione di *material weaknesses* nel sistema di controllo interno.

Il contributo di questo studio alla letteratura è duplice. In primo luogo, il lavoro dimostra l'esistenza di una sostanziale eterogeneità negli effetti del cross-listing, determinati dall'adozione

di adeguati sistemi di controllo interno. In secondo luogo, il presente lavoro contribuisce alla letteratura sugli effetti della SOX. La letteratura in materia mostra come le imprese cross-listate negli Stati Uniti aumentino la qualità del *firm information environment* dopo l'adozione della SOX. Il lavoro aggiunge alla letteratura l'evidenza che il miglioramento della qualità del *firm information environment* non è omogeneo per tutte le imprese, ma dipende da differenze nel livello di trasparenza tra le imprese cross-listate.

Chapter 1

Earnings and Expectation Management to Avoid Negative Earnings Surprises under Different Levels of Legal Enforcement

1.1. Introduction

The financial press and the business community put considerable emphasis on firms' ability to report earnings that meet or beat analyst earnings forecasts. Empirical evidence from a broad sample of corporate executives suggests that managers are aware of the importance of reporting a positive earnings surprise (Graham et al. 2005), while archival studies document that the capital market rewards firms that meet analyst forecasts and penalizes firms that do not (Bartov et al. 2002; Lopez and Rees 2002; Skinner and Sloan 2002; Brown and Caylor 2005). When manager observe that pre-managed earnings are below analyst forecasts, managers can exert their power over the financial reporting process to increase earnings (income-increasing earnings management, or EAR hereafter) or can guide analyst earnings forecasts downward (downward expectation management, or EXP hereafter) as a competing or substitute action.

The literature has generally studied EAR and EXP in isolation (Burgstahler and Dichev 1997; Degeorge et al. 1999; Bartov et al. 2002; Kaszink and McNichols 2002; Skinner and Sloan 2002) under the assumption that these actions are not related but are simply used in

conjunction (Matsumoto 2002; Burgstahler and Eames 2006; Athanasakou et al. 2010). Only recently the literature has considered that EAR and EXP can be substitutes or complements depending on their respective constraints. Brown and Pinello (2007) document that firms substitute EAR with EXP in the fourth quarter because of the stricter board and auditor scrutiny over the firm's annual reporting process. Das et al. (2011) find that EAR and EXP are complements when the constraints on earnings management are low and that they are substitutes when earnings management is bounded as firms rely more on expectations management when firms' capacity for earnings management is constrained. These studies focus on single countries and analyze cross-sectional/longitudinal firm-specific constraints as drivers of the choice between EAR and EXP. At the same time, existing accounting research recognizes that the level of enforcement shapes reporting incentives, generates tradeoffs in firms' reporting decisions, and relates strongly to earnings quality (Leuz et al. 2003; Burgstahler et al. 2006; Leuz 2010), to transparency (Ball et al. 2000), and to reporting conservatism (Bushman and Piotroski 2006). Therefore, results regarding the choice between EAR and EXP, their relationship, and their role in meeting/beating analyst forecasts may not be equal under different levels of legal enforcement.

Using a sample of 4,934 firm-years across fourteen European countries that require IFRS reporting, this paper examines whether (i) European firms use EAR and/or EXP to meet/beat analyst forecasts, (ii) whether the use of EAR and/or EXP is related to the level of legal enforcement, and (iii) whether the capital market rewards firms that meet/beat forecasts differently according to the use of EXP or EAR and conditionally on the strength of legal enforcement. This international sample is an ideal setting because it permits us to test whether firms that operate under the same accounting regime use EAR or EXP as complements or substitutes, conditional on the strength of the legal enforcement.

We start our analyses by exploring the actions that firms use to meet/beat analyst forecasts. We use positive discretionary accruals from the modified Jones model (Dechow et

al. 1995) as a proxy for EAR and the unexpected analyst earnings forecasts (Matsumoto 2002) to identify EXP. We find that European firms use both EAR and EXP to increase the likelihood of meeting/beating analyst forecasts.

Next, we focus on the use of EAR and EXP under different levels of legal enforcement. We analyze EAR and EXP separately in relation to the strength of the legal enforcement and find that EAR (EXP) is negatively (positively) associated with the strength of the legal enforcement, that is, EAR (EXP) is used less (more) under a strong legal enforcement regime. Then we analyze the relationship between EAR and EXP directly, conditional on the strength of the legal enforcement. We show that this relationship depends significantly on the strength of the legal enforcement: under weak enforcement regimes, firms tend to use EAR and EXP as complements, while they substitute EAR with EXP under strong legal enforcement regimes.

Finally, we first investigate the market reaction to meeting/beating analyst forecasts, without considering the levels of legal enforcement. We show that the capital market rewards firms that meet/beat forecasts, after controlling for current-year reported earnings and firm performance. Our evidence also shows that such firms experience a significant market penalty if they meet/beat analyst forecasts using EAR or EXP. When we condition this market penalty on the strength of the legal enforcement, we find that the penalty for the use of EAR increases with the strength of the legal enforcement, while the market penalty for the use of EXP is unchanged.

Our study offers several contributions to the literature. To the best of our knowledge, this is the first study that analyzes the use of EAR and EXP to meet/beat analyst forecasts in an international setting. Previous literature has studied firms listed in a single market, such as those in the NYSE (Matsumoto 2002; Bartov et al. 2002; Burgstahler and Eames 2006; Brown and Pinello 2007; Das et al. 2011) or the London Stock Exchange (Athanasakou et al. 2010). Only one paper looks at the use of EXP (only) to meet/beat analyst forecasts in relation

to the level of enforcement (Brown and Higgins 2005). Even if we know from international accounting literature that the enforcement environment affects firm's reporting decisions, no study investigates directly whether country level institutional characteristics (i.e. the quality of a country's enforcement environment) shape firm's choice between EAR and EXP. This study adds to the literature direct evidence that the relationship between EAR and EXP varies in an international setting. As expected from results on other financial reporting behaviors in an international setting, we find direct evidence that the relationship between EAR and EXP depends on the strength of the legal enforcement and that the choice between EAR and EXP is not driven only by firm-level characteristics. Our evidence directly shows that when the legal enforcement is weak, the two actions are used as complements, while firms substitute EAR with EXP when the legal enforcement is strong. In this vein, our evidence extends the results of Koh et al. (2011), which show that a change in regulation in a single country (i.e. increased enforcement scrutiny following SOX) affects the choice between EAR and EXP.

In relation to the earnings – expectation management literature, we also add evidence on the capital market's reaction to meeting/beating analyst forecasts and on the market penalty for using EAR and EXP under different levels of legal enforcement. We are the first to study whether the capital market's reaction to an earnings surprise is related to the strength of the legal enforcement. Conditioning the penalty imposed for using earnings and expectation management on the level of legal enforcement, we find that the level of enforcement influences how the market penalizes the use of EAR and EXP. The market penalty for meeting/beating analyst forecasts through EAR is higher in strong enforcement regimes than in weak enforcement regimes, while the use of EXP is not significantly associated with the level of enforcement.

1.2. Related literature and predictions

Empirical findings (Brown and Caylor 2005; Dechow et al. 2003) document that analyst forecasts have become the most attractive earnings benchmark, substituting for the traditional targets of positive earnings and previous year earnings. Beyer (2008) analytically demonstrates that the capital market response to an earnings surprise is asymmetric since the penalty for a negative earnings surprise is stronger than the reward following a positive earnings surprise. Several empirical papers (Kasznik and McNichols 2002; Skinner and Sloan 2002; Keung et al. 2010; Koh et al. 2011) show that the capital market rewards firms that achieve analyst forecasts and penalizes firms that miss them. Kasznik and McNichols (2002) provide evidence that firms that consistently meet analyst forecasts experience higher market valuations than firms that miss them do. Skinner and Sloan (2002) show that firms with high growth prospectus experience a more profound market reaction to a negative earnings surprise than they do to a positive or no earnings surprise. Finally, Koh et al. (2011) document that the market premium for meeting/beating analyst forecast is lower in the post-accounting-scandal world. In their survey on CFOs, Graham et al. (2005) document that managers fully recognize the signalling implications of meeting/beating analyst forecasts and take actions to avoid a negative earnings news to "build credibility with capital market" by maintaining or increasing stock price. When a firm's realized earnings fall short of analyst forecasts, managers seek to avoid a negative earnings surprise by manipulating accruals upward (EAR) or analyst expectation downward (EXP) to shelter them from a negative price reaction that could hurt their reputation in the capital market or their job security.

Empirical studies focus on firms listed in the NYSE (Matsumoto 2002; Bartov et al. 2002; Burgstahler and Eames 2006; Brown and Pinello 2007; Das et al. 2011) or the London Stock Exchange (Athanasakou et al. 2010). These studies refer to countries characterized by legal enforcement regimes (that foster the development of an efficient capital market) in

which the incentives to meet/beat analyst forecasts are similar. As the first step of our analysis, we investigate whether the incentive to meet/beat analyst forecasts and the use of EAR and/or EXP to meet/beat analyst forecasts depend on the strength of the enforcement, which we interpret as a constraint for EAR. Previous research explores whether the relationship between EAR and EXP varies according to their constraints (Brown and Pinello 2007; Das et al. 2011). Brown and Pinello (2007) contend that, because of the independent audit and stricter expense recognition rules in the fourth quarter, accrual manipulation is likely to be detected more easily in the annual reporting process than in the interim ones. They find that EAR is more likely to be used in the interim quarters, whereas EXP is more prevalent in the fourth quarter. Das et al. (2011) model the probability of engaging in EAR and EXP as a function of the firm's constraints and suggest that EAR and EXP are used as complementary actions until EAR constraints get stronger, when a firm substitutes EAR with EXP. These studies provide evidence that EAR and EXP are part of a unique strategy that firms adopt to meet/beat analyst forecasts.

On the other side, international accounting literature suggests that the level of legal enforcement shapes financial reporting incentives (Ball et al. 2000; Leuz et al. 2003; Brown and Higgins 2005; Burgstahler et al. 2006; Bushman and Piotroski 2006). Ball et al. (2000) argue that accounting income incorporates losses, making earnings more transparent, in a more timely way under strong enforcement regimes than under weak ones. Bushman and Piotroski (2006) provide evidence that the level of legal enforcement determines the equilibrium level of accounting conservatism. Leuz et al. (2003) document that earnings management is less prevalent in countries with developed capital markets, strong investors' rights, and strict legal enforcement. Burgstahler et al. (2006) consider both private and public firms to show that capital market pressures and institutional factors shape firms' incentives to report earnings that reflect their true economic performance. Their findings suggest that, as the legal enforcement strengthens, earnings management is more constrained.

We contend that the level of legal enforcement as a constraint for earnings management plays a pivotal role in the strategy used to meet/beat analyst forecasts. We expect that the level of legal enforcement interacts with the choice of actions used to meet/beat analyst forecasts. As the legal enforcement strengthens, we expect that the probability to engage in EAR activity decreases and the probability to engage in EXP activity increases. We also expect that the substitution effect between EAR and EXP is stronger when the capital market is more demanding in terms of rules and enforcement, resulting in EAR being more bounded.

EAR and EXP are effective when the market is not perfectly aware of how the analyst forecast is met and when outside investors are not able to recognize fully selfish practices by insiders. When the market is able to detect the extent and effect of EAR on reported earnings and the impact of EXP on analyst forecast revisions, two firms with the same forecast error and the same positive (negative) earnings surprise should be rewarded (punished) differently if one has engaged in EAR or EXP and the other has not.

Literature shows that EAR and EXP are rationale when the market premium for meeting/beating analyst forecasts is higher than the penalty for undertaking EAR and EXP (De Fond and Park 2001; Balsam et al. 2002; Rogers and Stocken 2005; Bartov et al. 2002; Das et al. 2011; Keung et al. 2010). Beyer (2008) demonstrates that a manager should trade off the disutility for missing analyst forecasts with the costs of EAR or EXP and play the earnings surprise game when extent that the latter is lower than the former. De Fond and Park (2001) and Balsam et al. (2002) provide evidence that the market discounts reported earnings if EAR is suspected. Rogers and Stocken (2005) find that EXP is costly, as a downward revision of current earnings expectation entails a negative stock reaction at the forecast revision date. Bartov et al. (2002) show that, when analyst forecasts are met through EAR or EXP, the market premium is lower, but still exists. Das et al. (2011) confirm the evidence, documenting a negative market response to the use of EAR and EXP but a positive net

premium to meeting/beating earnings forecasts. Keung et al. (2010) find that firms that only slightly meet/beat analyst forecasts incur a market penalty because investors believe that the target has been achieved through EAR/EXP.

Following this evidence, we expect that firms that meet/beat analyst forecasts enjoy a market reward but also that the capital market punishes the use of EAR and EXP. We take into consideration the level of legal enforcement and contend that capital markets characterized by different levels of legal enforcement react differently to the use of EAR and EXP. Therefore, we expect that the penalty for the use of EAR and EXP in meeting/beating analyst forecasts is higher in a more demanding capital market in term of rules and enforcement than it is in a less demanding financial market.

1.3. Research design

EAR and EXP metrics

Consistent with prior research, we use the modified cross-sectional Jones model of discretionary accruals described in Dechow et al. (1995) as a proxy for EAR. Specifically, we estimate the model for each year and country and every industry, classified by its two-digit SIC code. Thus, we partially control for industry changes in economic conditions that affect total accrual while allowing the coefficients to vary across groups (DeFond and Jiambalvo 1994). Next, we create a binary variable (POSDA) to classify firm-years into firms suspected of engaging in EAR. Specifically, POSDA takes the value of one if discretionary accruals (i.e. DA) are positive, and zero otherwise.

We use the Matsumoto model (Matsumoto 2002), adapted for annual data (Brown and Higgins 2005; Burgstahler and Eames 2006), to measure EXP. The Matsumoto model compares the expected forecast with the current forecast at the time of the earnings announcement. The expected forecast is calculated by exploiting the earnings serial

correlation and the additional information included into stock prices: if the expected forecast is above the effective forecast, then a firm is suspected of having walked down analyst earnings forecasts. We compute the expected portion of analyst forecasts by modeling the change in earnings as a function of the prior period's change in earnings and excess returns cumulated over the current period:

$$\frac{\Delta EPS_{ijt}}{P_{ijt-1}} = \alpha_{jt} + \beta_{1jt}\frac{\Delta EPS_{ijt-1}}{P_{ijt-2}} + \beta_{2jt}CRET_{ijt} + \varepsilon_{ijt} \qquad (1)$$

where

ΔEPS_{ijt} = firm *i*'s earnings per share in year *t* in two-digit sic code *j*, less earnings per share for the same firm one year prior,

P_{ijt} = price per share for firm *i* in two-digit sic code *j* at the end of the year *t* as reported by Compustat Global–Security daily,

$CRET_{ijt}$ = cumulative daily excess returns for firm *i* in two-digit sic code *j* in year *t* obtained from Compustat Global–Security Daily. Returns are cumulated from three days after the prior period's earnings announcement to twenty days before the current period's period earnings announcement.

Given that the analysts' expected forecasts should be based only on data available to analysts when they make their forecasts, we use parameter estimates for the prior firm-year to determine the expected changes in EPS (E[ΔEPS]). Then we add this value to the earnings per share from the prior year to get the expected forecast (E[F]) for the current year:

$$E[\Delta EPS_{ijt}] = [\hat{\alpha}_{jt-1} + \hat{\beta}_{1jt-1}\frac{\Delta EPS_{ijt-1}}{P_{ijt-2}} + \hat{\beta}_{2jt-1}CRET_{ijt}] \times P_{ijt-1} \qquad (2)$$

$$E[F_{ijt}] = EPS_{ijt} + \Delta E[EPS_{ijt}] \quad (3)$$

Finally, we subtract the expected forecast from the last consensus analyst forecast before the current annual earnings announcement to obtain the unexpected portion of the forecast (UEF). If a manager tries to walk-down analyst forecasts to beat or meet analyst forecasts, then the actual consensus forecast for the current period will be less than the expected forecast for the current period, and UEF is negative. Then we create a binary variable (DOWN) that takes the value of one if UEF is negative, and zero otherwise. Firm-years with DOWN equal to one are classified as having EXP.

To directly capture the existence of a trade-off between EAR and EXP, we combine DA and UEF into one measure. Specifically, we first percentile both rank DA and the negative of UEF. Then, we take the difference between the two percentile rank, scaled by 100 (TRADE_OFF).

How firms meet/beat analyst earnings forecasts

We start our analysis by investigating the actions firms use to meet/beat analyst forecasts. We use a logistic regression (equation 4) to model the probability of meeting/beating analyst forecasts at the earnings announcement date as a function of EAR and EXP.

$$PROB(MBE=1) = G(\beta_0 + \beta_1 DOWN + \beta_2 POSDA + \beta_3 POSDA \times DOWN + \beta_4 LAW + \sum_k \beta_k CONTROLS + \varepsilon_{itk}) \quad (4)$$

MBE is a binary variable that equals one if firm-year actual earnings are equal or above the last analyst consensus before the current annual earnings announcement. We measure the last analyst consensus as the median of the last analyst forecast made by each

analyst between the current annual earnings announcement and the prior period earnings announcement. LAW is the rule of law variable from Kaufmann et al. (2011) for the years 2006-2009. This variable, which considers differences in the legal enforcement across countries, is measured at country level since it represents the "perception of the extent to which agents have confidence in … the quality of contract enforcement, property rights, the police, and the courts" (Kaufmann et al. 2011). In order to capture any changes in the regulation, we use the variable LAW calculated for each year.

Consistent with prior studies, we control for the firm-specific constraints on EAR or EXP. Following Barton and Simko (2002), we include the net operating assets at the beginning of the fiscal year (NOA) to control for a cross-sectional constraint on EAR. We measure NOA as the difference between operating assets and operating liabilities, scaled by lagged total assets. The higher the NOA is, the lower the manager's ability to inflate earnings upward is. Following Das et al. 2011), we consider the price sensitivity to earnings news (SENS) as a constraint for EXP. We measure SENS as the abnormal return per unit of earnings surprise, defined as the three-day cumulative abnormal return around the earnings announcement, scaled by the corresponding surprise in the reported earnings. Higher sensitivity implies a larger negative price reaction to a downward revision in expected earnings, making EXP more costly.

We include controls that are expected to affect jointly the probability to meet/beat analyst forecasts and both EAR and EXP. Given that larger firms are more likely to avoid negative earnings news, we control for firm size (SIZE) using the log of the firm market value at the end of the year (closing price times the number of shares outstanding). We control for the growth prospectus using the percentage change in sales (GROWTH). A manager of a high-growth firm has a greater incentive to meet/beat analyst earnings forecasts than a manger of a low-growth firm does because the asymmetry in the price reaction to positive vs. negative earnings news is higher for low-growth firms (Skinner and Sloan 2002). In addition, growth

prospectus is found to influence earnings management (Barth et al. 2008). LEV, which is the end-of-year total liabilities divided by the end-of-year book value of equity, takes into account debt-contracting motivations for earnings management (DeFond and Jiambalvo 1994). A higher total debt-to-asset ratio implies a higher likelihood that there are violations of debt covenants, which boosts the incentive for EAR. We also control for firm performance using the return on assets (ROA), computed as net income over total assets. Beating or meeting analyst forecasts is more difficult for firms that are facing more forecasting uncertainty (Brown and Pinello 2007). We proxy for forecasting uncertainty using the absolute value of the current year's actual EPS, less the earliest analyst consensus, measured as the median of the first forecast each analyst made within ninety days after the earnings announcement of the prior period, scaled by the closing price at the end of the year (ABS_FE). The literature argues that the managerial incentive to report positive financial news instead of negative news is higher for firms that rely on implicit claims with stakeholders. Following Matsumoto (2002), we add a binary variable (DUR) that equals one if a firm is in a durable goods industry, and zero otherwise. Moreover, we include research and development expenditures scaled by total assets (R&D), which serves as a proxy for the extent to which a manager relies on implicit claims. Firms in high-risk industries are more likely to meet/beat analyst earnings forecasts than are firms in low-risk industries because negative earnings news affects such firms more heavily than it does others. We define LIT as a dummy variable that indicates membership in a high-risk industry (SICs 2833-2836, 3570-3577, 7370-7374, 3600-3674, 5200-5961). ESTIMATE, measured as the log of the number of analyst forecasts made during the year, controls for the strength of the monitoring carried out by analysts, as well as the pressure on managers to make earnings targets (Das et al. 2011). DUAL[1] is a dummy variable that equals one if a firm is listed on any U.S. exchange, and zero otherwise. We also control

[1] Cross-listing in the US and cross-listing in the UK is considered highly demanding in terms of transparency. In our sample, only a few firm-year observations are cross-listed in the UK. In our robustness tests, we split the DUAL variable to take into account cross-listing in the UK, considering firms that are cross-listed in both the countries to be cross-listed only in the US. Results (not tabulated) remain unchanged.

for macroeconomic factors using the log transformed average gross domestic product per capita (GDP), taken from the World Bank, over the period 1990-2000. This variable, which captures the extent to which a country is developed or developing, is widely used in international accounting research (Haw et al. 2004) to deal with the effect of unobserved country-specific factors that may be associated with both EAR and EXP.

Equation (4) is estimated using years and industry fixed effects based on the industry classification in Campbell (1996), with robust standard errors clustered by firm.[2] We expect that mangers of European firms use EAR and EXP to increase the likelihood of meeting/beating analyst earnings forecasts. Hence, we predict that β_1 and β_2 are positive and significant in equation (4).

Meeting/beating analyst forecasts using EAR and EXP under different levels of legal enforcement

To test the impact of the level of legal enforcement on the likelihood to use EAR and/or EXP to meet/beat analyst forecasts, we estimate three set of equations. Equations (5A-5B) and (6A-6B) separately model the probability to engage in EAR and EXP as a function of the strength of legal enforcement. Specifically, for EAR we estimate the following logistic and OLS models:

$$PROB(POSDA=1)=G(\beta_0+\beta_1 LAW+\sum_k \beta_k CONTROLS+\varepsilon_{itk}) \quad (5A)$$

$$DA=\beta_0+\beta_1 LAW+\sum_k \beta_k CONTROLS+\varepsilon_{itk} \quad (5B)$$

[2] Petersen (2008), Gow et al. (2010).

Similarly for EXP, we estimate the following logistic and OLS models[3]:

$$PROB(DOWN = 1) = G(\beta_0 + \beta_1 LAW + \sum_k \beta_k CONTROLS + \varepsilon_{itk}) \quad (6A)$$

$$UEF = \beta_0 + \beta_1 LAW + \sum_k \beta_k CONTROLS + \varepsilon_{itk} \quad (6B)$$

All the variables are as previously defined. Equations (5) and (6) are estimated using years and industry fixed effects based on the industry classification in Campbell (1996), and with robust standard errors clustered by firm. In addition, to the extent that firms that meet or slightly beat analyst forecasts are more likely to be engaged in both EAR and EXP than are firms that beat or fall short of analyst forecasts by large amounts, we also estimate models 5A-6A conditioning the estimation sample on firm-years with earnings surprises that do not exceed the absolute value of five cents. We expect that the strength of the legal enforcement affects the probability to engage in EAR and EXP: specifically, a negative relationship between EAR and LAW and a positive relationship between EXP and LAW. Hence, we predict β_1 will be negative and significant in equations 5A-5B, while β_1 will be positive and significant in equations 6A-6B.

This preliminary analysis allows us to validate LAW as a determinant of EAR, and to verify whether EXP is positively related to the determinant of EAR. However, it does not shed light on how firms trade-off between these actions in respect to the strength of the legal enforcement. To explore this issue further, we model the probability that the firm will engage in EAR, considering both POSDA and DA as a function of DOWN, LAW, and the interaction between LAW and DOWN. Specifically, we estimate the following logistic and OLS models:

[3] For ease of exposition, we multiple UEF for minus one in equation 6B, so that a positive coefficient on LAW implies a positive association between EXP and LAW.

$$PROB(POSDA=1)=G(\beta_0+\beta_1 LAW+\beta_2 DOWN+\beta_3 DOWN*LAW+\sum_k \beta_k CONTROLS+\varepsilon \quad (7A)$$

$$DA=\beta_0+\beta_1 LAW+\beta_2 DOWN+\beta_3 DOWN*LAW+\sum_k \beta_k CONTROLS+\varepsilon \quad (7B)$$

All the variables are as previously defined. Equations (7A-7B is estimated using years and industry fixed effects based on the industry classification in Campbell (1996), and with robust standard errors clustered by firm. We also estimate equation 7A conditioning the estimation sample only on firms that report small earnings surprises. By estimating equations (7A-7B), we intend to verify whether EAR and EXP are complements or substitutes in respect to the strength of the legal enforcement. The interaction between LAW and DOWN measures the extent to which the relationship between EAR and EXP varies with the strength of the legal enforcement. We contend that EXP is positively related to EAR when LAW is low (i.e., complementary actions) and that this relationship becomes negative for high values of LAW (i.e., substitute actions). Hence, β_2 is expected to be positive and significant, whereas β_3 is expected to be negative and significant.

Next, we model directly the trade-off between EAR and EXP, using as dependent variable TRADE_OFF which capture the extent to which a firm relies on EAR or EXP to meet or beat analyst forecasts. Specifically, we estimate the following OLS model:

$$TRADE\_OFF=\beta_0+\beta_1 LAW+\sum_k \beta_k CONTROLS+\varepsilon \quad (7C)$$

All the variables are as previously defined. Equation 7C is estimated using year-industry fixed effects based on the industry classification in Campbell (1996), and with robust standard errors clustered by firm. We also estimate equation 7C conditioning the estimation sample only on firms that report small earnings surprise. Whether firms substitute EAR with EXP as the legal enforcement gets stronger, β_1 is expected to be negative and significant.

In equations 7A-7B, we model EAR as a function of EXP. However, to the extent that these two actions are employed to achieve the same goal of meeting/beating analyst forecasts, it is likely that they are jointly determined. To adress this issue, we re-estimate the relation between EAR and EXP as a system of equations using two-stage least square (2SLS). Following Das et al. (2011), we use the stock price sensitivity to an earnings news (SENS) as an instrument, for EXP while we use litigation risk (LIT) as an instrument for EAR. To assess the moderating effect of LAW, we estimate the 2SLS separately by partitioning the sample according to the mean of LAW.

Market reaction to EAR and EXP under different levels of legal enforcement

In the final set of our empirical tests, we examine how the capital market reacts to meeting/beating analyst forecasts and to the use of EAR and EXP. Specifically, we estimate the following models:

$$CAR\_1\_t = \beta_0 + \beta_1 MBE + \beta_2 POSDA + \beta_3 DOWN + \beta_4 SIZE + \beta_5 GROWTH + \beta_6 ROA + \beta_7 LOSS + \beta_8 ABS\_FE + \varepsilon \quad (8)$$

$$CAR\_1\_t = \beta_0 + \beta_1 MBE + \beta_2 POSDA + \beta_3 DOWN + \beta_4 POSDA \times LAW + \beta_5 DOWN \times LAW + \beta_6 SIZE + \beta_7 GROWTH + \beta_8 ROA + \beta_9 LOSS + \beta_{10} ABS\_FE + \varepsilon \quad (9)$$

CAR_1_*t* is the daily cumulative market-adjusted returns from one day before to *t* (3, 5, 10, 20, 30) days after firm *i*'s earnings announcement. All the other variables are as previously defined. Models (8) and (9) are estimated using years and industry fixed effects based on the industry classification in Campbell (1996) and with robust standard errors clustered by firm and year.

Through equation (8), we intend to verify whether capital markets reward firms that meet/beat analyst forecasts with an incremental premium. We expect the estimate coefficient of MBE (β_2) to be positive and significant. Then we explore to what extent the capital market is sensible to how analyst forecasts are achieved. When the capital market is able to detect EAR and EXP actions fully, investors should be able to separate "good" firms, which beat earnings forecasts with their unbiased earnings and expectation, from "bad" firms, which achieve analyst forecasts through EAR and/or EXP. In the latter case, the coefficients of POSDA and DOWN (β_2 and β_3) are expected to be negative and significant. In particular, if the market is able to detect fully the amount of EAR and EXP, we expect both $\beta_1 + \beta_2$ (for EAR) and $\beta_1 + \beta_3$ (for EXP) to equal zero. Through equation (9), we explore whether the capital market reacts differently to the use of EAR or EXP under different levels of legal enforcement. We include the interaction between POSDA and LAW and that between DOWN and LAW. If the penalty for the use of EAR or EXP is incremental based on the strength of the legal enforcement, the estimated coefficients of both interactions, respectively β_4 and β_5 are negative and significant.

1.4. Sample and data

Our study analyses non-financial firms listed in fourteen European capital markets. Unlike prior works on EXP or on the relationship between EAR and EXP that have used quarterly data,[4] we employ annual data because firms are not mandated to provide financial statements on a quarterly basis in all the countries of our sample.[5] The majority of UK firms provide interim reports only on a semi-annual basis. In addition, interim earnings announcements and analyst forecasts are sparsely covered in the I/B/E/S International files,

[4] Brown and Higgins (2005), Bollinger and Kast (2004) and Athanasakou et al. (2010) are exceptions.
[5] IAS 34 requires firms to provide interim financial reports, but the interim period is defined as a financial reporting period shorter than a full financial year. Therefore, a firm can choose to provide financial information on a quarterly or a semi-annual basis.

adding a possible self-selection bias[6] (Landsman et al. 2012).

We obtain accounting and market data from the Compustat Global and analyst forecasts and actual earnings from I/B/E/S international (split unadjusted) database.[7] First, we sample from Compustat Global all firm-years from European countries that require IFRS reporting[8] from 2005 to 2009. We merge the initial sample with I/B/E/S international, a restriction that yields 19,334 firm-year observations. To be included in the sample, firms must satisfy three criteria:[9] (i) there are at least three individual earnings forecasts made at least twenty trading days apart, (ii) the release data of the earliest forecast is at least one trading day after the previous period's earnings release, and (iii) the release date of the latest forecast precedes the current period's earnings release date by at least three days.[10] We eliminate firms in the financial industry (SIC codes between 6000 and 6999) and require at least ten observations in each two-digit SIC grouping per year and country as a condition for the modified Jones model (for EAR) and the Matsumoto model (for EXP)[11].

Table 1.1
Sample selection

European listed non-financial firms-years in COMPUSTAT Global	19,433
Less firm-years non covered by I/B/E/S international	*-100*
Intermediate sample	19,334
Less firms-years with not enough data to calculate the modified Jones model (Dechow et al. 1995)	*-9,101*
Intermediate sample	10,233
Less firm- years with not enough data to calculate the modified Matsumoto model (Matsumoto 2002)	*-5,299*
Final sample	**4,934**

[6] Brown and Pinello (2007) show that the probability of engaging in EAR is lower in the fourth quarter than in the interim quarters because of auditor and board scrutiny on the annual reporting process. Since this issue should be less severe in the interim quarters, EXP is more prevalent in the fourth quarter and EAR is more prevalent in the interim ones. That firms make an inter-temporal substitution between these two actions could introduce a bias in our results by causing an upward bias in EXP and a downward bias in the EAR proxy. To deal with this bias, we perform additional analyses on a quarterly basis and find that our results are robust to the use of quarterly instead of annual data.

[7] Consistent with previous literature (Philbrick and Ricks 1991, Abarbanell and Lehavy 2000), we use the same database (I/B/E/S) for both forecast and actual earnings

[8] The sample encompasses two non-EU-member countries (Norway and Switzerland) that also mandated IFRS adoption beginning in 2005.

[9] Such a sample criterion is consistent with previous studies on expectations management (Bartov et al. 2002, Brown and Higgins 2005, Brown and Pinello 2007).

[10] We follow prior works for the last two criteria such that, if more than one earnings forecast is released, we take the mean.

[11] To mitigate the undue influence of outliers, we winsorize all variables entered in the modified Jones model and in the Matsumoto model at the 1st and 99th percentiles.

The final sample consists of 4,934 firm-year observations, representing 1,844 unique firms, from fourteen European countries between 2006 and 2009. Table 1.1 summarizes the sample selection.

Table 1.2 illustrates the sample distribution by country and descriptive statistics on the legal enforcement variable (LAW). The legal enforcement variable (LAW) shows both cross-sectional and time variation.

Table 1.2

Distribution of Observations by Country

Country	Unique firms	Firm-years	Firm-years as percent of total sample	LAW (2006)	LAW (2007)	LAW (2008)	LAW (2009)	LAW (Mean)
Belgium	15	27	0.55	1.20	1.30	1.31	1.37	1.28(0)
Denmark	20	54	1.09	1.85	1.96	1.91	1.87	1.90(1)
Finland	40	128	2.59	1.93	1.86	1.86	1.94	1.90(1)
France	242	684	13.86	1.41	1.38	1.43	1.43	1.41(0)
Germany	269	721	14.61	1.69	1.70	1.67	1.63	1.67(1)
Greece	13	28	0.57	0.83	0.80	0.79	0.64	0.76(0)
Ireland	5	5	0.10	1.70	1.73	1.71	1.71	1.71(1)
Italy	89	217	4.40	0.31	0.40	0.38	0.39	0.37(0)
Netherlands	34	86	1.74	1.73	1.74	1.72	1.78	1.74(1)
Norway	105	249	5.05	1.95	1.90	1.94	1.88	1.92(1)
Spain	23	67	1.37	1.06	1.08	1.12	1.13	1.10(0)
Sweden	79	233	4.72	1.82	1.86	1.88	1.93	1.87(1)
Switzerland	98	305	6.18	1.78	1.82	1.79	1.75	1.78(1)
United Kingdom	813	2.13	43.18	1.70	1.66	1.63	1.71	1.67(1)
Total	1,884	4,934	100					
Mean								1.51
Std. Dev.								0.46

Table 1.2 reports the sample distribution. The full sample comprises 4.934 firm-year observations representing 1.884 distinct firms from 12 EU countries plus Norway and Switzerland during the period from 2006 to 2009. See APPENDIX I.A for variable definitions.

Table 1.3 presents descriptive statistics. The mean of MBE is 0.503, which indicates that firms meet/beat analyst forecasts in half of the firm-year observations. The mean of POSDA is 0.440, suggesting that approximately 44 percent of the firm-year observations engage in EAR. The mean of DOWN is 0.692, which indicates that 69 percent of the observations engage in EXP.

Table 1.3

Descriptive statistics for variables used in regression analyses

Variable	*N*	Mean	Std. Dev	P5	P25	Median	P75	P95
MBE	4,934	0.503	0.500	0.000	0.000	1.000	1.000	1.000
DA	4,934	-0.013	0.095	-0.151	-0.047	-0.007	0.027	0.111
UEF	4,934	2.462	25.958	-1.454	0.023	0.343	1.406	7.980
POSDA	4,934	0.441	0.496	0.000	0.000	1.000	1.000	1.000
DOWN	4,934	0.692	0.461	0.000	0.000	1.000	1.000	1.000
SIZE	4,934	5.739	2.058	2.762	4.237	5.477	7.032	9.696
NOA	4,934	0.577	0.352	0.082	0.382	0.510	0.706	1.120
SENS	4,934	0.861	79.552	-71.146	-5.451	0.014	6.328	69.016
GROWTH	4,934	12.507	58.287	-34.727	-7.039	4.964	18.142	65.138
LEV	4,934	2.405	19.095	0.222	0.642	1.253	2.126	4.691
ROA	4,934	0.026	0.150	-0.241	0.013	0.049	0.089	0.184
R&D	4,934	0.041	0.109	0.000	0.000	0.000	0.037	0.198
ABS_FE	4,934	0.532	27.258	0.000	0.006	0.019	0.061	0.395
ESTIMATE	4,934	2.865	1.224	1.098	1.791	2.833	3.871	4.852
DUR	4,934	0.313	0.463	0.000	0.000	0.000	1.000	1.000
LIT	4,934	0.352	0.477	0.000	0.000	0.000	1.000	1.000
DUAL	4,934	0.065	0.246	0.000	0.000	0.000	0.000	1.000

Table 1.3 reports descriptive statistics for the dependent variables and the continuous and binary independent variables. The full sample comprises 4,934 firm-year observations representing 1,884 distinct firms from 12 EU countries, plus Norway and Switzerland, during the period from 2006 to 2009. See APPENDIX I.A for variable definitions.

Table 1.4 reports the Pearson (Spearman) correlations below (above) the diagonal. LAW and GDP are excluded as long as they are measured at the country level and do not vary at the firm level.

Table 1.4

Correlation matrix

	MBE	POSDA	DOWN	SIZE	SENS	NOA	GROWTH	ABS_FE	LEV	ROA	R&D	ESTIMATE	DUR	LIT	DUAL
MBE		0.07**	0.07**	0.07**	0.03**	-0.04*	0.05**	-0.16**	0.01	0.04***	-0.01	0.06**	0.00	-0.03**	0.02
POSDA	0.07**		0.01	-0.04	0.00	-0.06**	0.03*	-0.06**	-0.06**	0.08***	-0.02	-0.06**	0.03*	-0.03	-0.04**
DOWN	0.06**	0.00		0.23**	-0.01	0.13**	-0.05	-0.18**	0.08**	0.36***	-0.10**	0.18**	0.00	-0.01**	0.06**
SIZE	0.05**	-0.05**	0.24**		0.00	0.19**	-0.02	-0.2**	0.46**	0.12***	-0.12**	0.79**	0.06**	-0.29**	0.34**
SENS	0.02	-0.01	0.00	0.01		-0.01	0.01	0.00	0.02*	0.00	-0.03**	0.00	-0.02*	0.00	-0.02*
NOA	-0.01	0.01	0.02	-0.02	0.00		0.10**	0.05**	-0.06**	0.08***	0.01	0.11**	-0.07**	-0.06**	0.09**
GROWTH	0.00	0.03*	-0.13	-0.06**	0.00	0.02**		-0.25**	-0.02*	0.07***	-0.04**	-0.02	-0.07**	0.05**	-0.04**
ABS_FE	-0.01	-0.01	0.03*	-0.01	0.00	0.00	0.03		-0.02	-0.29	0.03**	-0.23**	0.01	0.07**	-0.11**
LEV	-0.02	-0.03*	0.00	0.03*	0.00	0.00	0.01	0.00		-0.13	-0.23**	0.28**	-0.03	-0.23**	0.1**
ROA	0.05***	0.04***	0.38***	0.26***	-0.01	0.13***	-0.14***	-0.01	-0.02		-0.06**	-0.29	0.05***	-0.06**	0.05**
R&D	-0.03	-0.02	0.18**	-0.24**	0.01	0.03	0.05**	0.00	-0.01	-0.40**		0.04**	0.28**	0.3**	0.14**
ESTIMATE	0.04**	-0.07**	0.19**	0.79**	0.01	-0.04**	-0.06	-0.02	0.00	0.23***	-0.08**		0.05**	-0.12**	0.33**
DUR	0.01	0.02	0.01	0.06**	-0.01	-0.03*	-0.06	-0.01	0.00	0.05***	0.03	0.05**		-0.13**	0.01
LIT	-0.03**	-0.02*	0.09**	-0.26	-0.01	0.00	0.02	-0.01	-0.03*	-0.13**	0.30**	-0.10**	-0.13**		-0.03
DUAL	0.01	-0.04**	0.06**	0.42**	0.00	-0.01	-0.04	0.00	0.01	0.05**	0.01	0.36**	0.01	-0.03	

Table 1.4 reports Pearson (Spearman) correlations below (above) the diagonal.

See APPENDIX I.A for variable definitions.

** and * denote significance at 1% and 5% levels (two-sided), respectively.

1.5. Results

We first analyze the actions to meet or beat analyst forecasts. Table 1.5 presents the coefficients and firm clustered adjusted z-statistics (in parentheses) of the logit analysis of the probability of meeting/beating analyst forecasts (equation (4)). The dependent variable is the probability of meeting/beating analyst forecasts. Columns (1)-(2) include only one action. Column (1) shows that the estimate coefficient of DOWN is positive and highly significant (0.229, $p < 0.001$) so when a firm engages in EAR, the probability of meeting/beating earnings forecasts increases, while column (2) shows that the estimate coefficient on POSDA is positive and highly significant (0.285, $p < 0.001$). In column (3), which allows the probability to meet or beat analyst forecasts to depend on POSDA and DOWN, the estimate coefficients of POSDA and DOWN are still positive and significant (0.228, $p < 0.001$, 0.285, $p < 0.001$, respectively). In column (4), which includes the interaction between POSDA and DOWN in order to isolate firms that contemporaneously manage earnings upward and analyst forecasts downward, the estimate coefficients of POSDA and DOWN are still positive and highly significant, while their interaction is not significant. This result reveals that the joint use of EAR and EXP does not affect the probability of a firm's meeting/beating analyst forecasts, possibly because only firms whose pre-managed earnings are far from analyst consensus use both actions, but given the magnitude of this difference, not even the joint use of EAR and EXP makes it possible to obtain a positive earnings surprise. In all models, the level of enforcement is positively associated with MBE: when the legal enforcement is strong, firms have greater incentive to meet/beat analyst forecasts. Among the control variables, the estimate coefficient of NOA is negative and statistically significant. Consistent with prior research, the higher NOA is, the more difficult accrual manipulation is.

Table 1.5

Logit analysis of the probability of meeting/beating analyst forecasts

	(2) MBE	(1) MBE	(3) MBE	(4) MBE
DOWN	0.229***	-	0.228***	0.289***
	(2.984)		(2.962)	(2.945)
POSDA	-	0.285***	0.285***	0.394***
		(4.756)	(4.743)	(3.151)
DOWN*POSDA	-	-	-	-0.139
				(-0.990)
LAW	0.349**	0.387**	0.373**	0.372**
	(2.282)	(2.514)	(2.438)	(2.425)
GDP	-1.047***	-1.123***	-1.088***	-1.085***
	(-4.261)	(-4.542)	(-4.411)	(-4.398)
SIZE	0.018	0.0270	0.019	0.019
	(0.642)	(0.969)	(0.696)	(0.687)
ROA	0.003	0.004**	0.003	0.003
	(1.373)	(1.999)	(1.120)	(1.121)
NOA	-0.347***	-0.314***	-0.319***	-0.319***
	(-3.833)	(-3.428)	(-3.492)	(-3.487)
SENS	0.000	-0.000	0	0
	(-1.031)	(-1.171)	(-1.120)	(-1.113)
GROWTH	0.001	0.001	0.001	0.001
	(1.185)	(0.802)	(1.015)	(0.996)
LEV	-0.004	-0.004	-0.004	-0.004
	(-1.439)	(-1.361)	(-1.472)	(-1.478)
ABS_FE	-0.012	-0.011	-0.009	-0.009
	(-0.453)	(-0.383)	(-0.376)	(-0.373)
DUR	-0.039	-0.023	-0.036	-0.032
	(-0.160)	(-0.094)	(-0.146)	(-0.130)
R&D	-0.264	-0.251	-0.244	-0.247
	(-0.826)	(-0.784)	(-0.765)	(-0.772)
LIT	-0.096	-0.101	-0.091	-0.091
	(-1.203)	(-1.261)	(-1.131)	(-1.135)
DUAL	-0.162	-0.154	-0.147	-0.148
	(-1.063)	(-1.017)	(-0.964)	(-0.973)
ESTIMATE	0.082*	0.087**	0.089**	0.089**
	(1.924)	(2.056)	(2.104)	(2.096)
CONSTANT	10.116***	10.759***	10.315***	10.234***
	(4.247)	(4.496)	(4.318)	(4.285)
Industry controls	Yes	Yes	Yes	Yes
Year controls	Yes	Yes	Yes	Yes
Log likelihood	-3346.94	-3339.54	-3335.31	-3334.83
Chi- Square	130.12	142.76	151.45	151.65
p-value	0.000	0.000	0.000	0.000
R-squared	0.021	0.023	0.024	0.025
Observations	4,934	4,934	4,934	4,934

Table 1.5 reports the results from the estimation of model (4). The table reports logistic coefficient estimates and z-statistic (in parentheses) See APPENDIX I.A for variable definitions.

***, ** and * denote significance at 1% , 5% and 10% levels (two-sided), respectively.

Previous analysis does not allow us to disentangle the roles of EAR and EXP one from the other. Thus, we explore to what extent the relationship between EAR and EXP varies according to the strength of the legal enforcement. Table 1.6 reports the results from a logistic and OLS analysis on the effects of the legal enforcement on EAR and EXP (equations (5A-5B) and equations (6A-6B)). We use firm clustered adjusted z-statistics (t- statistics depending on the model specification) and year-industry fixed effects. Consistent with our predictions, LAW is negatively and significantly associated with EAR. In column (1) the coefficient on LAW is negative and significant (-0.301, $p<0.050$), and this negative association still holds when we measure income increasing earnings management as a continuous variable (column 2: -0.011, $p < 0.050$) or when we restrict the estimation sample only to firms with small earnings surprises (column 3: -0.216, $p < 0.050$). On the other side, LAW is positively associated with EXP irrespective from the model specification (column 4: 0.532, $p < 0.050$, column 5: 8.553, $p < 0.050$, column 6: 0.740, $p < 0.050$). The strength of the legal enforcement affects the choice between EAR and EXP such that, as the legal enforcement gets stronger, a firm is less likely to engage in EAR and more likely to engage in EXP. The estimate coefficients of firm-specific constraints on EAR and EXP are both significant and in the expected direction. For example, looking at column (1) NOA is strongly negatively associated with POSDA (-0.402, $p < 0.001$), while SENS is negatively associated with DOWN in model (4) (-0.003, $p < 0.001$) but not with POSDA. These results are consistent with the evidence Das et al. (2011) obtained. Table 1.6 provides evidence that, after controlling for firm-specific constraints on EAR and EXP (i.e., NOA and SENS), the legal enforcement has an additional explanatory power in the choice between EAR and EXP. This result supports the prediction that the level of legal enforcement determines the choice between EAR and EXP, and it provides preliminary evidence of a substitution effect between EAR and EXP conditional on the level of legal enforcement.

Table 1.6

Logit analysis of the effect of the legal enforcement on EAR and EXP

	(1) Full sample	(2) Full sample	(3) Small Earnings Surprise	(4) Full sample	(5) Full sample	(6) Small Earnings Surprise
	POSDA	DA	POSDA	DOWN	UEF	DOWN
LAW	-0.301**	-0.011**	-0.216**	0.532**	8.553**	0.740**
	(-2.253)	(-2.135)	(-2.049)	(2.237)	(2.330)	(2.358)
GDP	0.535**	0.008	0.148	-1.156***	-0.004	-1.830***
	(2.127)	(0.638)	(0.509)	(-3.525)	(-0.898)	(-3.244)
SIZE	-0.023	0.000	-0.004	0.217***	2.223***	0.299***
	(-0.771)	(0.018)	(-0.146)	(4.775)	(2.579)	(4.566)
ROA	0.007***	0.001***	0.005*	0.062***	0.054***	0.049***
	(2.692)	(3.216)	(1.947)	(9.213)	(2.884)	(7.073)
NOA	-0.402***	-0.019***	-0.386***	0.072	-1.570*	0.149
	(-4.192)	(-3.549)	(-3.989)	(0.585)	(-1.859)	(0.875)
SENS	-0.000	0.000	0.000**	-0.000**	0.000	-0.000*
	(-0.095)	(0.622)	(2.147)	(-2.373)	(0.168)	(-1.812)
GROWTH	0.001**	-0.000	0.001***	-0.003***	-0.000	-0.003**
	(2.246)	(-0.423)	(4.264)	(-3.388)	(-1.356)	(-2.408)
LEV	-0.012	-0.000**	-0.014***	-0.000	-0.622**	0.001
	(-1.533)	(-2.097)	(-3.117)	(-0.095)	(-2.147)	(0.484)
ABS_FE	-0.682***	-0.000***	-0.449	-0.017	0.001	-0.100
	(-2.943)	(-6.423)	(-0.773)	(-1.012)	(0.486)	(-0.600)
DUR	-0.055	0.008	-0.145***	0.192	-0.117	-0.270
	(-0.288)	(0.906)	(-2.728)	(0.710)	(-0.109)	(-0.847)
R_D	-0.386	-0.025	-0.576	0.181	-0.122	0.418
	(-1.171)	(-1.051)	(-1.557)	(0.297)	(-0.059)	(0.845)
LIT	-0.077	-0.007*	0.150**	-0.292***	-1.015	0.166
	(-0.951)	(-1.793)	(2.271)	(-2.622)	(-0.833)	(1.074)
DUAL	-0.240	-0.009	0.109	-0.175	-5.061**	-0.443
	(-1.469)	(-1.604)	(0.567)	(-0.823)	(-1.978)	(-1.489)
ESTIMATE	-0.119***	-0.002	-0.144***	-0.049	-2.481**	-0.148
	(-2.783)	(-1.133)	(-2.706)	(-0.738)	(-2.500)	(-1.474)
CONSTANT	-4.252*	-0.067	-0.381	10.705***	-17.249**	17.188***
	(-1.736)	(-0.537)	(-0.131)	(3.383)	(-2.325)	(3.072)
Industry controls	Yes	Yes	Yes	Yes	Yes	Yes
Year controls	Yes	Yes	Yes	Yes	Yes	Yes
Log likelihood	-3295.07	-	-1716.52	-2104.79	-	-1061.71
Chi- Square	117.73	-	103.98	359.23	-	209.76
p-value	0.000	0.000	0.000	0.000	0.000	0.000
R-squared	0.026	0.026	0.023	0.198	0.021	0.181
Observations	4,934	4,934	2,564	4,934	4,934	2,564

Table 1.6, column 1 reports the results of a logit analysis of the probability to engage in income increasing accrual management as a function of rule of law and control variables. Column 2 reports the results of an OLS regression of income increasing accrual management as a function of rule of law and control variables. Column 3 reports the results of a logit analysis of the probability to engage in income increasing accrual management as a function of rule of law and control variables for a sub-sample of firms with a small earnings surprise. Column 4 reports the results of a logit analysis of the probability to engage in downward expectation management as a function of rule of law and control variables. Column 5 reports the results of an OLS regression of downward expectation management as a function of rule of law and control variables. Column 6 reports the results of a logit analysis of the probability to engage in downward expectation management as a function of rule of law and control variables for a sub-sample of firms with a small earnings surprise Each model includes year and industry fixed effects based on the classification in Campbell (1996). The table reports coefficient estimates and z-statistic (t-statistics depending on the specifications) based on robust standard errors that are clustered by firm.

See APPENDIX I.A for variable definitions.

***, ** and * denote significance at 1% , 5% and 10% levels (two-sided), respectively.

To clarify how the strength of the legal enforcement affects the relationship between EAR and EXP, we perform a logistic and OLS analysis of the trade-off between EAR and EXP, conditional on the level of legal enforcement. Specifically, we regress our proxies for the likelihood of engaging in EAR on EXP, LAW, and the interaction between EXP and LAW (equations (7A-7B)). We use firm clustered adjusted z-statistics (t- statistics depending on the model specification) and year-industry fixed effects. Our variables of interest are DOWN, which captures EXP without being conditional on the level of enforcement, and its interaction with LAW. Including the interaction between DOWN and LAW allows the relationship between EAR and EXP to vary with the strength of the legal enforcement. Results are presented in Table 1.7.

Column (1) shows that the estimate coefficient on DOWN is positive albeit only marginal significant (0.051, $p < 0.100$), suggesting that EAR and EXP are complements, while the coefficient on the interaction between DOWN and LAW is negative and significant (-0.028, $p < 0.05$), suggesting that firms substitute EAR with EXP when the level of legal enforcement gets stricter. Columns (2-3) provide essentially the same results using DA as dependent variable or by restricting the estimation sample only on firms with small earnings surprises.

In columns (4-5) we estimate directly the trade-off between EAR and EXP, as the dependent variable captures the extent to which a firm relies on the former or on the latter to meet/beat an analyst forecasts. In column (4) LAW is negatively associated with TRADE_OFF (-0.077, $p < 0.001$), meaning that when the legal enforcement is stronger, firms relies more on EXP than on EAR. This evidence still holds when we restrict the estimation sample only on firms with small earnings surprises.

Table 1.7

Analysis of the trade-off between EAR and EXP conditioned on the legal enforcement

	(1) Full sample	(2) Full sample	(3) Small Earnings Surprise	(4) Full sample	(5) Small Earnings Surprise	(6) 2SLS	
						LOW_LAW	HIGH_LAW
	POSDA	DA	POSDA	TRADE_OFF	TRADE_OFF	DA	
LAW	-0.313**	-0.011**	-0.042	-0.077***	-0.107***	-	-
	(-2.113)	(-2.136)	(-0.168)	(-2.688)	(-3.048)		
DOWN	0.051*	0.003*	1.304**	-	-	0.038*	-0.021***
	(1.694)	(1.816)	(2.067)			(1.891)	(-3.143)
LAW*DOW	-0.028**	-0.001**	-0.819**	-	-	-	-
	(-1.991)	(-2.216)	(-1.988)				
GDP	0.514**	0.006	0.377	-0.035	0.180**	-0.038	-0.001
	(2.022)	(0.456)	(0.857)	(-0.660)	(2.331)	(-1.134)	(-0.283)
SIZE	-0.014	0.001	-0.026	-0.049***	-0.038***	-0.001	-0.047***
	(-0.450)	(1.095)	(-0.542)	(-8.421)	(-5.091)	(-0.051)	(-4.951)
ROA	0.006**	0.001***	0.006*	-0.039**	-0.039*	-0.002	0.002**
	(2.282)	(2.763)	(1.800)	(-2.195)	(-1.815)	(-0.261)	(1.987)
NOA	-0.389***	-0.022***	-0.460***	-0.000	0.000***	-0.013	-0.059**
	(-4.074)	(-3.890)	(-3.336)	(-0.656)	(6.814)	(-0.311)	(-1.982)
SENS	0.000	0.000	0.000	-0.000	0.000***	-	-
	(-0.093)	(0.631)	(0.711)	(-0.656)	(6.814)		
GROWTH	0.000	-0.000	0.001	-0.000	-0.000	0.000	0.000
	(0.832)	(-0.428)	(0.970)	(-0.590)	(-1.185)	'(0.151)	'(0.392)
LEV	-0.045**	-0.004***	-0.022	-0.001***	-0.006***	0.003	-0.021
	(-2.385)	(-3.705)	(-0.841)	(-5.184)	(-4.714)	(0.275)	(-0.381)
ABS_FE	-0.676***	-0.000***	-0.432	-0.000	-0.000	-0.027	-0.000
	(-2.911)	(-6.820)	(-0.905)	(-0.960)	(-0.546)	(0.271)	(-0.682)
DUR	-0.544	-0.010	-0.179**	-0.107***	-0.068*	0.009	0.016
	(-0.291)	(-1.130)	(-2.451)	(-2.643)	(-1.666)	(0.967)	(1.061)
R&D	-0.384	-0.025	-0.704	-0.115*	-0.134**	0.047	0.008
	(-1.103)	(-1.009)	(-1.487)	(-1.896)	(-2.123)	(1.571)	(0.080)
LIT	-0.086	-0.008**	0.078	0.036**	0.007	0.023	-0.205**
	(-1.054)	(-2.079)	(0.495)	(2.176)	(0.361)	(1.181)	(-1.996)
DUAL	-0.263	-0.011**	0.086	-0.015	0.019	-0.002	-0.005**
	(-1.609)	(-2.025)	(0.360)	(-0.506)	(0.553)	(-0.891)	(-1.985)
ESTIMATE	-0.119***	-0.003	-0.124*	-0.009	-0.022*	-0.010	-0.056***
	(-2.760)	(-1.431)	(-1.779)	(-0.951)	(-1.873)	(-0.924)	(-3.771)
CONSTANT	-3.984	-0.042	-4.026	0.958*	-1.225	0.364	-0.021
	(-1.611)	(-0.328)	(-0.931)	(1.793)	(-1.594)	(0.104)	(-0.681)
Industry controls	Yes	Yes	Yes	Yes	Yes	No	No
Year controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Log likelihood	-3295.87	-	-1684.24	-	-	-	-
Chi- Square	120.79	-	263.85	-	-	-	-
p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000
R-squared	0.026	0.030	0.039	0.132	0.139	-	-
Observations	4,934	4,934	2,564	4,934	2,564	1,023	3,911

Table 1.7, column 1 reports the results of a logit analysis of the relationship between income increasing earnings management (POSDA) and downward expectation management for the full sample of firms. Column 2 reports the results of an OLS regression of the relationship between income increasing earnings management (DA) and downward expectation management for the full sample of firms. Column 3 reports the results of a logit analysis of the relationship between income increasing earnings management (POSDA) and downward expectation management for a sub-sample of firms with small earnings surprises. Columns 4-5 report the results of an OLS t analysis of the trade-off between income increasing earnings management and downward expectation management for the full sample and for a sub-sample of firms with small earnings surprises.Column 6 reports 2SLS estimation of the relationship between income increasing earnings management (DA) and downward expectation management for the full sample of firms. The 2SLS estimation is carried out separately for two sub-group (HIGH_LAW – LOW_LAW), defined according to the mean value of LAW across 2006-2009. Year and industry fixed effects based on the classification in Campbell (1996) are included. The table reports coefficient estimates, z-statistic (t-statistic) and two-tailed p-value based on robust standard errors that are clustered by firm.

See APPENDIX I.A for variable definitions.

***, ** and * denote significance at 1% , 5% and 10% levels (two-sided), respectively.

Finally, in column (6) we estimate DA and DOWN jointly trough a 2SLS[12]. To the extent that we are interested in the moderating effect of LAW on the relationship between EAR and EXP, we estimate the system of equations separately for sub-groups of LOW_LAW and HIGH_LAW. The results confirm prior evidence. DOWN is positively associated with DA when the legal enforcement is weak (0.038, $p < 0.100$), while they are negatively associated when the legal enforcement is strong (-0.021, $p < 0.001$). These findings suggest that firms use EAR and EXP as complements when the legal enforcement is weak, but when it is difficult to manage accruals because of a strong legal enforcement, firms substitute EAR with EXP.

Market reaction to EAR and EXP under different levels of legal enforcement

In the final step of our analysis, we investigate how the capital market reacts to the use of EAR and EXP. Table 1.8 reports the results of univariate tests. For each specification of the cumulative market adjusted returns (CAR_1_*t*), we report a 2×2 table to separate the mean values of CAR_1_*t* between firms that meet/beat forecasts and those that do not, and within each group in relation to the use of EAR and EXP. For all specifications of CAR_1_*t*, firms that meet/beat forecasts experience higher CAR than those that do not. Most importantly, within the subsample of firms that meet/beat forecasts, firms experience higher abnormal returns when they do not engage in EAR and this difference is statistically significant.

[12] We employ a Durbin-Wu-Hausman χ^2 to test the exogeneity of DOWN in equation 7B. The test reject the null that DOWN is exogenous in the regression reported in table 7, model 2 ($p<0.010$).

Table 1.8

Market reactions to EAR and EXP: univariate analysis

	ACTION	MBE = 1	MBE =0
CAR 1-3	POSDA = 1	-0.023	-0.065
	POSDA = 0	0.008	-0.072
H_0: diff = 0		p=0.004	p=0.600
CAR 1-5	POSDA = 1	-0.022	-0.066
	POSDA = 0	0.008	-0.072
H_0: diff = 0		p=0.007	p=0.604
CAR 1-10	POSDA = 1	-0.022	-0.067
	POSDA = 0	0.007	-0.072
H_0: diff = 0		p=0.010	p=0.721
CAR 1-20	POSDA = 1	-0.027	-0.072
	POSDA = 0	0.006	-0.077
H_0: diff = 0		p=0.003	p=0.713
CAR 1-30	POSDA = 1	-0.029	-0.077
	POSDA = 0	0.008	-0.083
H_0: diff = 0		p=0.002	p=0.526
CAR 1-3	DOWN = 1	-0.008	-0.074
	DOWN = 0	0.001	-0.058
H_0: diff = 0		p=0.526	p=0.301
CAR 1-5	DOWN = 1	0.002	-0.074
	DOWN = 0	-0.008	-0.059
H_0: diff = 0		p=0.477	p=0.327
CAR 1-10	DOWN = 1	-0.009	-0.073
	DOWN = 0	0.002	-0.059
H_0: diff = 0		p=0.451	p=0.362
CAR 1-20	DOWN = 1	-0.011	-0.078
	DOWN = 0	-0.006	-0.065
H_0: diff = 0		p=0.901	p=0.427
CAR 1-30	DOWN = 1	-0.011	-0.083
	DOWN = 0	-0.006	-0.074
H_0: diff = 0		p=0.7706	p=0.587

Table 1.8 reports mean values of the market adjusted returns (CAR_1_t) cumulated from 1 day before to t (3, 5, 10, 20, 30) days after a firm's *i* earnings announcement. Returns are reported separately for firms that meet/beat analyst earnings forecasts (MBE=1) and those which fail to meet analyst earnings forecasts (MBE=0).

See APPENDIX A for variable definitions.

We report p-values for differences in mean using a t-test with different variance.

Then we condition the sample on meeting/beating firms and for each different specification of CAR by splitting the sample into four sub-groups according to POSDA and DOWN. Through this analysis, we measure the difference in the market premium for meeting/beating analyst forecasts in relation to the actions used to achieve the target. Table1.9

reports the results. Meeting/beating firms that do not engage both in EAR and EXP experience a market reward, on average, of 4.21 percent (for CAR 1-3), whereas capital markets do not reward meeting/beating firms that use both the actions as the CAR 1-3 when POSDA and DOWN are equal to one is -2.55 percent.

Table 1.9

Market reactions to EAR and EXP: univariate analysis conditioned on meeting/beating firms

CAR 1 -3			
	POSDA = 0	POSDA = 1	diff.
DOWN = 0	0.042	-0.043	0.086
DOWN = 1	0.001	-0.018	0.018
diff.	0.042	-0.026	0.067
p-value	0.019	0.093	
CAR 1 -5			
	POSDA = 0	POSDA = 1	
DOWN = 0	0.043	-0.0422	0.085
DOWN = 1	0.000	-0.0172	0.016
diff.	0.043	-0.025	0.068
p-value	0.017	0.101	
CAR 1 -10			
	POSDA = 0	POSDA = 1	
DOWN = 0	0.041	-0.039	0.080
DOWN = 1	-0.000	-0.018	0.017
diff.	0.042	-0.021	0.068
p-value	0.022	0.141	
CAR 1 -20			
	POSDA = 0	POSDA = 1	
DOWN = 0	0.035	-0.054	0.089
DOWN = 1	0.000	-0.021	0.021
diff.	0.035	-0.033	0.068
p-value	0.049	0.059	
CAR 1 -30			
	POSDA = 0	POSDA = 1	
DOWN = 0	0.035	-0.050	0.085
DOWN = 1	0.001	-0.024	0.025
diff.	0.034	-0.026	0.060
p-value	0.057	0.114	

Table 1.9 reports mean values of the market adjusted returns (CAR_1_t) cumulated from 1 day before to t (3, 5, 10, 20, 30) days after a firm's *i* earnings announcement for firms that meet/beat analyst earnings forecasts (MBE =1). POSDA is a binary variable coded as 1(0) if DA is greater than or equal to (less than) 0.
See APPENDIX I.A for variable definitions.
We report p-values for differences in mean using a t-test with different variance.

Overall, genuine achievers (i.e., firms that use neither EAR or EXP) experience a market reward higher by 6.73 percent ($p < 0.010$) of that gained by cheating achievers (i.e., firms that use both EAR and EXP).

Table 1.10 presents estimation results of the market reactions to meet/beat analyst forecasts, and to the use of EAR and EXP (see equation (8)). In all of the specifications of CAR_1_*t*, firms that meet/beat forecasts enjoy higher cumulative abnormal returns than firms that do not. As we move from the short window (one to three days) to the long window (one to thirty days), the magnitude of the estimate coefficient of MBE increases slightly and remains strongly significant. On the other side, the estimate coefficients of POSDA and DOWN are negative and statistically significant in all models, as the capital market imposes a significant penalty on the use of EAR and EXP. The presence of such a penalty suggests that, to a certain extent, investors react to earnings surprises rationally to the extent that they are able to separate "managing" from "non-managing" firms. However, the market premium for meeting/beating analyst forecasts is still significantly positive, after that, the penalties for EAR and EXP are taken into account. Indeed, firms that meet/beat analyst forecasts using EAR experience a positive market reward that averages 0.027 ($p < 0.050$, column 3), and firms that meet/beat forecasts using EXP enjoy a market reward of 0.021 percent ($p < 0.100$, column 3). On the other side, achievers that engage both in EAR and EXP receive no market reward, as the coefficient of MBE, when both POSDA and DOWN are equal to one, is negative but not significant.

Table 1.10

Market reactions to EAR and EXP

	(1) CAR_1-3	(2) CAR_1-5	(3) CAR_1-10	(4) CAR_1-20	(5) CAR_1-30
MBE	0.051***	0.051***	0.051***	0.053***	0.059***
	(6.082)	(6.003)	(6.081)	(6.020)	(6.576)
POSDA	-0.024***	-0.023***	-0.024***	-0.028***	-0.028***
	(-2.910)	(2.793)	(-2.872)	(-3.210)	(-3.192)
DOWN	-0.030**	-0.031**	-0.032**	-0.031**	-0.032**
	(-2.410)	(2.452)	(2.516)	(-2.321)	(-2.335)
SIZE	-0.007***	-0.006***	-0.007***	-0.007***	-0.331***
	(-3.042)	(-2.845)	(-2.891)	(-2.692)	(-2.356)
GROWTH	0.000	0.000	0.000	0.000	0.000
	(-0.210)	(-0.054)	(-0.120)	(-0.332)	(-0.026)
ROA	0.003***	0.003***	0.003***	0.003***	0.003***
	(4.785)	(4.745)	(4.868)	(5.131)	(5.120)
LOSS	-0.036*	-0.038*	-0.038*	-0.030	-0.033
	(-1.792)	(-1.887)	(-1.843)	(-1.426)	(-1.531)
ABS_FE	0.002***	0.002***	0.002***	0.002***	0.0022***
	(12.624)	(12.273)	(11.751)	(19.247)	(12.701)
CONSTANT	0.069	0.058	0.055	0.021	0.0099
	(1.291)	(1.109)	(1.070)	(0.373)	(0.180)
Industry controls	Yes	Yes	Yes	Yes	Yes
Year controls	Yes	Yes	Yes	Yes	Yes
Country controls	Yes	Yes	Yes	Yes	Yes
R-squared	0.101	0.099	0.099	0.099	0.101
Observations	4,897	4,897	4,897	4,897	4,897
Test of coefficients					
$\beta_1 + \beta_2=0$	0.026**	0.027**	0.027**	0.025**	0.030**
	(2.261)	(2.301)	(2.276)	(2.010)	(2.395)
$\beta_1 + \beta_3=0$	0.020	0.019	0.021*	0.022*	0.027*
	(1.531)	(1.502)	(1.780)	(1.851)	(1.910)
$\beta_1 + \beta_2 + \beta_3=0$	-0.003	-0.004	-0.005	-0.006	-0.002
	(-0.230)	(-0.229)	(-0.292)	(-0.350)	(-0.101)

Table 1.10 reports the results of regressions of market adjusted returns (CAR_1_t) cumulated from 1 day before to t (3, 5, 10, 20, 30) days after a firm's i earnings announcement. Year, country and industry fixed effects based on the classification in Campbell '(1996) are included.

See APPENDIX I.A for variable definitions.

***, ** and * denote significance at 1% , 5% and 10% levels (two-sided), respectively.

Table 1.11 reports the results of the regression models on the market reactions to EAR and EXP conditional on the strength of the legal enforcement (equation (9)). In these regressions, we condition POSDA and DOWN on LAW by including the interactions between POSDA and LAW and between DOWN and LAW.

Table 1.11

Market reactions to EAR and EXP conditioned on the legal enforcement

	(1) CAR_1-3	(2) CAR_1-5	(3) CAR_1-10	(4) CAR_1-20	(5) CAR_1-30
MBE	0.052***	0.052***	0.053***	0.055***	0.061***
	(6.351)	(6.282)	(6.349)	(6.321)	(6.841)
POSDA	-0.007	-0.007	-0.006	-0.011	-0.008
	(0.610)	(-0.656)	(-0.536)	(-0.912)	(-0.687)
DOWN	-0.037***	-0.038***	-0.040***	-0.038***	-0.041***
	(-2.719)	(-2.701)	(-2.850)	(-2.591)	(-2.733)
POSDA*LAW	-0.044*	-0.042*	-0.048**	-0.046*	-0.053**
	(-1.893)	(-1.750)	(-1.998)	(-1.896)	(-2.179)
DOWN*LAW	0.009	0.008	0.012	0.007	0.011
	(0.532)	(0.481)	(0.681)	(0.412)	(0.583)
SIZE	-0.006***	-0.006***	-0.006***	-0.005**	-0.004
	(-2.671)	(-2.548)	(-2.556)	(-2.131)	(-1.513)
GROWTH	0.000	0.000	0.000	0.000	0.000
	(-0.495)	(-0.313)	(-0.414)	(-0.667)	(-0.353)
ROA	0.003***	0.003***	0.003***	0.003***	0.003***
	(4.901)	(4.871)	(4.993)	(5.301)	(5.330)
LOSS	-0.033*	-0.036*	-0.036*	-0.027	-0.029
	(-1.671)	(-1.772)	(-1.745)	(-1.281)	(-1.351)
ABS_FE	0.002***	0.002***	0.002***	0.002***	0.002***
	(13.961)	(13.071)	(12.651)	(16.350)	(19.447)
CONSTANT	0.042	0.036	0.028	0.010	-0.007
	(1.470)	(1.291)	(0.990)	(0.321)	(-0.240)
Industry controls	Yes	Yes	Yes	Yes	Yes
Year controls	Yes	Yes	Yes	Yes	Yes
Country controls	Yes	Yes	Yes	Yes	Yes
R-squared	0.094	0.094	0.094	0.092	0.092
Observations	4,897	4,897	4,897	4,897	4,897

Table 1.11 reports the results of regressions of market adjusted returns (CAR_1_t) cumulated from 1 day before to t (3, 5, 10, 20, 30) days after a firm's i earnings announcement. Year, country and industry fixed effects based on the classification in Campbell (1996) are included.

See APPENDIX A for variable definitions.

***, ** and * denote significance at 1% , 5% and 10% levels (two-sided), respectively.

The estimate coefficient of MBE is positive and statistically significant across all of the CAR specifications. The coefficient of DOWN is negative and significant, while the interaction between DOWN and LAW is not statistically different from zero. This finding suggests that the penalty for EXP is not affected by the level of legal enforcement. On the other hand, the estimate coefficient of POSDA is negative, even if not significant, while the coefficient of the interaction between POSDA and LAW is negative and significant. This finding indicates that only the penalty for EAR increases with the strength of legal enforcement.

1.6. Robustness checks[13]

Alternative proxies for EAR and EXP

The literature has developed several proxies with which to capture EXP, as it is not directly observable. To validate our measures and verify that our findings are robust to alternative measures, we replicate our analyses using two proxies (Brown and Pinello 2007; Das et al. 2011). According to Bartov et al. (2002), EXP is suspected when a firm that meets or beats analyst forecasts has a negative forecast error. Hence, we define EXP as a binary variable that takes the value of one if a firm-year has a negative forecast error and MBE is equal to one, and zero otherwise. The second proxy (WLKDN) measures the size of the "walk-down" of analyst earnings forecasts as the difference between the first and last consensus analyst forecasts. A firm is suspected of engaging in EXP if WLKDN is positive.[14]

Next, we test whether our results are robust to the use of two alternative measures of discretionary accruals. First, we use working capital accruals instead of total accruals (De Fond

[13] All the results of robustness checks are available from the authors upon request.
[14] Given that EXP is conditioned on the firms' ability to meet/beat analyst earnings forecasts, we cannot include such a proxy in equation (1). When EXP is equals to one, it predicts perfectly MBE equals to one, given that it is conditioned on that value. Therefore, we use only WLKDN for equation (1), while we use EXP and WLKDN for the others.

and Park 2001). Second, we estimate normal accruals controlling for the lagged return on assets (Kothari et al. 2005). The results of these sensitivity checks (not tabulated) shows that our main findings are robust to alternative measures EXP and EAR.

Alternative Proxies for the Level of Legal Enforcement

We use the variable "rule of law" proposed by Kaufmann et al.'s (2011) as a proxy for the strength of the legal enforcement. While this variable is widely used in international accounting studies (Daske et al. 2008; Li 2010; Byard et al. 2011), we test the robustness of our results by employing other proxies to capture the difference in the level of legal enforcement: (i) following Byard et al. (2011), we use the "governance score" and the "regulatory quality" for 2005 from Kaufman et al. (2007), (ii) following Leuz et al. (2003), we take the average score of the efficiency of the juridical system, rule of law, and corruption from La Porta et al. (1998), and (iii) following Preiato et al. (2010), we use the "audit and enforcement score." Results (not tabulated) are qualitatively unchanged from those reported in the paper.

Quarterly instead of Annual Data

We also test whether our results are robust to the use of quarterly data instead of annual data. Given data limitations, we are not able to compute the research and development intensity (RandD) in this robustness check. Following Brown and Spina Pinello (2007), we add a fourth-quarter dummy variable (FOURTH QUARTER). Because Compustat Global does not provide the gross value of the property, plan, and equipment on a quarterly basis, we use only the working capital accrual model to measure EAR. We exclude the UK and Ireland, as firms in these countries provide quarterly financial statements only on a voluntary basis. The results confirm the evidence we acquired using annual data.

Additional Robustness Tests

In the main analyses, we control for macroeconomic factors using the log-transformed average gross domestic product per capita (GDP) over the period 1990-2000. This variable, which captures the extent to which a country is developed or developing, is widely used in international accounting research (Haw et al. 2004) to deal with the effect of unobserved country-specific factors that could be associated with both EAR and EXP. However, we also control for the level of a country's financial development using the stock market capitalization divided by GDP (World Bank). Finally, we test to determine whether the main analyses (based on annual data) are robust to the exclusion of the UK, which represents 44 percent of the sample. Results (not tabulated) are consistent with those obtained in the main analyses.

1.7. Conclusisions

Research on EAR and EXP has focused on the use of one of these actions alone without considering that these two actions might be jointly determined and that a firm can choose to use one and not the other. Only a few papers explore the relationship between EXP and EAR directly as a function of their respective firm-specific constraints (Brown and Pinello 2007; Das et al. 2011). Even if we know from international accounting literature that the enforcement environment affects a firm's reporting decisions, no study investigates directly and in detail whether country level institutional characteristics (i.e. the quality of a country's enforcement environment) shape firms' choice between EAR and EXP. We extend this literature by obtaining direct empirical evidence that the relationship between EAR and EXP varies in an international setting. We find direct evidence that the relationship between EAR and EXP depends on the strength of the legal enforcement and that the choice between EAR and EXP is not driven only by

firm-level characteristics. We directly document that, after controlling for the traditional firm-specific constraints on EAR and EXP, the strength of the legal enforcement has an additional explanatory power in a firm's choice between EAR and EXP. Specifically, firms exploit EAR and EXP as complementary or substitute actions based on whether the legal enforcement is weak or strong, respectively.

Using a sample of European firms in the post-mandatory-IFRS-adoption period, we show that firms use both EAR and EXP to increase the likelihood of meeting/beating analyst forecasts. We provide evidence of a substitution effect between EAR and EXP as a function of the legal enforcement, which extends beyond the traditional firm-specific constraints. We find that, when the legal enforcement is weak, firms use EAR and EXP as complements, and when the legal enforcement is strong, firms substitute EAR with EXP. In this vein, our evidence extends the results of Koh et al (2011), which shows that a change in regulation in a single country (i.e. the enforcement of the SOX) affects the choice between EAR and EXP.

We also document that the capital market rewards firms that meet/beat analyst forecasts and penalizes the use of EAR and EXP. Despite such penalties, the market premium for meeting/beating analyst forecasts is still positive if firms use only one of the actions. However, the premium disappears if firms use EAR and EXP contemporaneously. Finally, we document that the market penalty for the use of EAR increases as the strength of the legal enforcement increases, while the penalty for the use of EXP is not strongly affected by the level of the legal enforcement.

Appendix I.A: Variable definitions

Variable	Definition
MBE	Binary variable equals to one if a firm-year's actual earnings meet/beat the last consensus analyst forecast made before the current annual's earnings announcement, zero otherwise.
DA	Discretionary accruals derived from the modified Jones model (Dechow 1995).
UEF	Modeled measure of unexpected earnings forecast according to the Matsumoto model (2002) time minus one.
POSDA	Binary variable equals to one if DA is greater than or equal to zero, zero otherwise.
DOWN	Binary variable equal to one if the modeled measure of unexpected earnings forecast from the Matsumoto model (2002) is negative, zero otherwise.
TRADE_OFF	Difference between the percentile rank of DA and UEF, scaled by 100.
SIZE	Natural logarithm of total assets at the beginning of the year.
LAW	Rule of law variable from Kaufmann et al. 2011) for the years 2006-2009.
NOA	Net operating assets at the beginning of the year, computed as the difference between operating assets and operating liabilities scaled by lagged total assets.
SENS	Price sensitivity to earnings news as the abnormal return per unit of earnings surprise, defined as the three-day cumulative abnormal return around the earnings announcement in period t, scaled by the corresponding surprise in the reported earnings.
GROWTH	Annual percentage change in sales.
LEV	End of the year total liabilities divided by end of the year equity book value.
ROA	Net income over the mean value of total assets D is research and development expenditures scaled by total assets.
R&D	Research and development expenditures scaled by total assets.
ABS_FE	Earnings surprise, defined as the difference between the actual earnings per share and the analyst consensus earnings forecast before the earnings announcement, scaled the closing price at the fiscal year end.
ESTIMATE	Logarithm of the number of analyst earnings forecasts made during the year.
DUR	Binary variable equal to one if a firm is member of durable goods industry (SICs 1500-1790, 1450, 2500-2590, 2830, 3010, 3240-3990), zero otherwise.

LIT	Binary variable indicating membership in high-risk industry (SICs 2833-2836, 3570-3577, 7370-7374, 3600-3674, 5200-5961).
DUAL	Binary variable equals to one if the firm is listed on any US stock exchange, zero otherwise.
CAR_1_t	Daily cumulative market-adjusted returns from one day before to *t* (3, 5, 10, 20, 30) days after firm *i*'s earnings announcement.

Chapter 2

The Influence of Country- and Firm-Level Governance on Financial Reporting Quality: Evidence from IFRS Mandatory Adoption

2.1 Introduction

This paper explores how country and firm-level governance mechanisms interplay and dovetail one another in influencing the quality of reported earnings. While there is extensive research on the mapping between a firm's governance mechanisms and financial reporting quality as well as on the impact of country-level institutions and financial reporting quality, there is scant evidence as to how these two levels of governance jointly affect the quality of financial reporting.

In that regard, the adoption of International Financial Reporting Standards (IFRS) provides an interesting setting to assess the relative influence of each governance level. It is one of the most fundamental change in accounting regulation and, not surprisingly, has been studied broadly (see Soderstrom and Sun 2007; Barth 2008; Hail et al. 2010 for an overview). However, evidence regarding the economic benefits stemming from IFRS mandatory adoption is rather mixed. On one hand, it appears that the switch from local generally accepted accounting principles (GAAP)

to IFRS leads to an increase in comparability, transparency, and financial reporting quality. On the other hand, it is challenging, conceptually and empirically, to attribute such benefits to IFRS reporting *per se*, with some evidence suggesting that most of the perceived benefits from IFRS adoption actually result from concurrent changes in the regulatory and enforcement environments (Christensen et al. 2012). To the extent that the application of accounting standards provides insiders with substantial discretion, research highlights that firms' reporting behavior, and hence the observed financial reporting quality, is likely to be shaped by institutional factors and firm-level characteristics, rather than by a simple change in accounting standards (Ball et al. 2003; Leuz et al. 2003; Burgsthaler et al. 2006). Consistently, many previous studies (Daske et al. 2008) suggest that there is a substantial heterogeneity in the effects of IFRS adoption due to differences in the legal enforcement. This literature emphasizes the importance of the enforcement regime as the key driver of observed heterogeneity in financial reporting quality. In this vein, previous studies point toward an increase in financial reporting quality around IFRS adoption only in countries with strong legal enforcement (Daske et al. 2008; Byard et al. 2011; Landsman et al. 2012), thus amplifying the divergence among countries: firms incorporated in countries with stricter enforcement rules benefit from IFRS adoption while all others do not.

Those studies explain the heterogeneous effects of IFRS adoption across countries but miss to examine heterogeneity within similar legal environment leaving several relevant questions unanswered. For example, does financial reporting quality remain uniformly unchanged around IFRS adoption for all firms located in weak legal enforcement countries, and do firms in strong legal enforcement uniformly increase financial reporting quality? Moreover do firms located in weak enforcement countries sustain the costs of IFRS adoption without the benefits? In other words, is there any possibility for a firm located in a weak enforcement country to overcome the effect of legal enforcement and to benefit from IFRS adoption?

This paper tries to fill this gap in the literature by analyzing the joint effects of board-based monitoring mechanisms with country legal enforcement. To do so, using a panel dataset with an extensive fixed effects structure, we test whether board-based monitoring mechanisms substitute or complement legal enforcement in shaping the effects of IFRS on financial reporting quality. We operationalize financial reporting quality considering earnings informativeness (Landsman et al. 2012), accrual-based and real earnings management. Our sample consists of 3,476 IFRS firm-year observations from 14 European countries (treatment sample) and 29,596 firm-year observations from 11 non-IFRS adoption countries (control sample). Following Landsman et al. (2012), we measure earnings informativeness using abnormal return variability and abnormal trading volume. We measure accrual-based earnings management using the modified Jones model (Dechow et al. 1995). We estimate real earnings management considering the abnormal level of cash flow from operations, production costs and discretionary expenses (advertising, R&D and SG&A). In line with previous research (Cohen et al. 2008), we combine the three measures of real earnings management into two aggregate metrics of real earnings management.

The most compelling challenge to our analysis is that the mandatory IFRS adoption occurs at the same time for all publicly listed companies in European countries. To ascertain that general trends in financial reporting quality or concurrent events unrelated to IFRS adoption do not drive the results, we estimate annual panel regressions for IFRS adopter firms and non-IFRS adopter firms using industry-country and separate year fixed effects for the treatment and the control sample. In this way, we allow time-trend effects to vary within the treatment and control group as well as account for yearly shocks in financial reporting quality unreleated to IFRS mandatory adoption (Christensen et al. 2012).

We perform four steps of analysis: 1) an examination of the average effect of the IFRS adoption *per se*; 2) an examination of the effect of the mandatory IFRS adoption considering

board-based monitoring mechanisms; 3) an examination of the effect of the mandatory IFRS adoption considering the country-level of legal enforcement and board-based monitoring mechanisms; 4) an examination of the effect of the mandatory IFRS adoption considering board-based monitoring mechanisms, cross-sectional differences in the the country-level of legal enforcement and changes in the country-level of legal enforcement over financial reporting.

In the first analysis, using firm-year data from 2002 to 2008, we examine the change in earnings informativeness metrics, accrual and real-based earnings management in the treatment sample (mandatory IFRS adopters) to the change for the control sample (non-IFRS adopters) around the time of mandatory IFRS adoption. Specifically, we regress our proxies for financial reporting quality on an indicator variable marking IFRS adopters in the post-IFRS mandatory adoption period, a set of control variables, country-industry and separate year fixed effects.

Next, still using firm-year data from 2002 to 2008, we test whether the effects of mandatory IFRS adoption on earnings informativeness, accrual and real-based earnings management are different with respect to cross-sectional variation in the board monitoring level, as represented by various board attributes. To summarize the underlying latent construction of board monitoring, we create a standardized level of strict board monitoring intensity based on the principal component factor analysis of the board and directors characteristics. Specifically, in the second test we replace the single IFRS indicator with two non-overlapping indicator variables marking IFRS adopters with strong board-based monitoring mechanisms and IFRS adopters with weak board-based monitoring mechanisms. We further relax the assumption that board monitoring does not vary over time, by exploiting time-changes in the firm-level corporate governance attributes to examine whether firms that experience an increase in the monitoring effectiveness carried out by board of directors are associated with an higher increase in financial reporting quality once IFRS become mandate.

In the third test, we investigate the joint effect of board-based monitoring level and the strength of legal enforcement on earnings informativeness, accrual, and real-based earnings management around the mandatory IFRS adoption. The idea is to analyze whether different board-monitoring levels interplay with the legal enforcement regimes in determining the effects of mandatory IFRS adoption. Finally, we try to account for concurrent changes in the level of enforcement by partitioning strong legal enforcement countries with respect to the introduction or not of stricter enforcement procedure aroud IFRS mandatory adoption (Christensen et al. 2012). This test allows us to estimate differential IFRS effects on financial reporting quality for strong versus weak governance in countries with weak legal institutions, strong legal institutions and countries that experience an increase in the level of enforcement over financial reporting.

We find evidence that the mandatory switch to IFRS is, on average, associated with an increase in financial reporting quality. However, there is considerable heterogeneity in financial reporting quality changes, suggesting that IFRS mandatory adoption is not sufficient, *per se*, to change firms' reporting practices. Indeed, we find that firm-level monitoring mechanisms have an effective role in shaping firms' reporting quality after a change in accounting standards. Indeed, we document an increase in financial reporting quality only for firms which have strong board-based monitoring mechanisms irrespective of the country of incorporation. Despite country-specific institutional characteristics, firm-level monitoring mechanisms, i.e. board composition, are a substantial determinant of financial reporting quality around IFRS mandatory adoption. When we employ both the country-level and firm-level partitioning variables, we find an increase in financial reporting quality for strong monitoring firms in weak legal enforcement countries and a negligible increase for weak monitoring firms in a strong legal environment. However, the latter effect is much larger for strong monitoring firms in the strong legal environment. Thereby, if firm-level monitoring mechanisms are a substitute for the legal system

when it is weak, firm- and country-level monitoring mechanisms turn to be complements as the latter gets stronger. Finally, when we take into accout also time-changes in the level of enforcement over financial reporting, we find only a marginal and not significant difference in financial reporting changes for strong versus weak monitoring firms in countries that experience an increase in the level of enforcement.

We also test whether firm-level corporate governance is just the observable outcome of firm-level reporting incentives to provide financial information of higher quality, with no additional explanatory power, by allowing separate IFRS effects for strong and weak monitoring firms and within these two groups between firms with strong and weak reporting incentives. We find that board monitoring intensity has an additional and different role in explaining financial reporting changes around IFRS mandatory adoption. Our results are robust to the use of firm fixed effects, alternative ways to measure firm-level corporate governance mechanisms and financial reporting quality metrics, and to the use of voluntary adopters as control sample.

Our paper contributes to the literature in two ways. First, to the best of our knowledge, this is the first paper that provides evidence of the role of board-based monitoring mechanisms into the consequences of mandatory IFRS adoption. Using the board monitoring level as partitioning variable, we capture firm-level heterogeneity in financial reporting quality around IFRS mandatory adoption. So far, research has explored the average impact of IFRS adoption or has focused on cross-country differences. Firm level heterogeneity is not so well explored. Only a few papers try to explore this point (Byard et al. 2011; Daske et al. forthcoming), but they focus only on firm level reporting incentives. Our results show that, despite the country of incorporation and after controlling for firm-specific reporting incentives, firms can take advantage from IFRS adoption to the extent they adopt strong board-based monitoring mechanisms. In doing so, we add to the literature on IFRS adoption that considers reporting

quality stemming from the country-level legal institutional framework (Daske et al. 2008; Byard et al. 2011; Landsman et al. 2012).

Second, the paper contributes to the growing literature on the interplay between firm-level governance and country institutional characteristics. The findings point toward a substitution effects between firm-level monitoring mechanisms and country-level enforcement mechanisms when the legal system is lax, while board monitoring and legal enforcement complement each other when the legal system gets stricter. In a sense, our findings may help bridge the contrasting evidence provided by Durnev and Kim (2005) and Doidge et al. (2007) and suggest that complementarity or substitution in firm- and country-level governance is contextual.

2.2 Related literature and predictions

Related literature

Extant research documents substantial economic benefits around mandatory IFRS adoption. Among other things, there are positive market reactions to events associated with mandatory IFRS adoption (Armstrong et al. 2010), an increase in market liquidity and a decline in the cost of capital (Daske et al. 2008; Li 2010), higher information content of earnings (Landsman et al. 2012), an increase in stock price informativeness (Beuselinck et al. 2009; DeFond et al. 2011), an improvement in analyst information environment (Byard et al. 2011; Tan et al. 2011), and higher foreign investments (Bruggeman et al. 2009; Beneish et al 2010). While the evidence consistently points towards positive capital market effects around IFRS adoption, results on the impact of IFRS on financial reporting quality are mixed and rather controversial. Barth et al. (2008) find evidence of an increase in earnings quality while Ahmed et al. (forthcoming) suggest that because of the principle-based nature of IFRS and the lack of

implementation guidance, earnings quality decreased after the mandatory adoption of IFRS. However, theoretically, accounting flexibility could be used to increase accounting numbers quality as well as to decrease financial reporting quality.

Moreover, it is challenging to attribute capital market or financial reporting quality effects to the IFRS adoption *per se*. To the extent that the application of any set of accounting standards provides insiders with substantial discretion, research stresses that firms' reporting behaviors, and hence the observed financial reporting quality, is likely to depend on countries' institutional frameworks, market pressures and firm-level characteristics rather than to a change in accounting standards (Ball et al. 2000; Leuz et al. 2003; Burgstahler et al. 2006; Wysocki 2011). In this vein, Christensen et al. (2012) argue that the aforementioned benefits are not fully ascribable to IFRS mandatory reporting. Rather, to the extent that some European Union (EU) countries have started to make financial reporting enforcement mechanisms tighter around 2005, the documented capital-market benefits may be caused by both an IFRS effect or by a change in enforcement effect. Although it is a very difficult task to disentangle them, they find an increase in market liquidity around IFRS mandatory adoption only in five European countries that adopt stricter accounting enforcement mechanisms concurrent with IFRS mandatory adoption. This evidence suggests that care is needed in interpreting capital-market or financial reporting effects around IFRS mandatory adoption.

To sum up, extant literature agrees that, by itself, a change in accounting standards, even toward a supposedly higher quality set, does not matter much for a change in firms' reporting practices. Wysocki (2011) underscores the importance of country-level factors and firms specific characteristics in shaping the effects of a change of accounting standards. Therefore, the application of a common set of accounting standards is unlikely to generate similar outcomes in term of financial reporting quality across different countries and firms. So far, researches have

focused only on the role of country-level institutions, while how and whether firm-level characteristics and the interplay among country and firm characteristics shape financial reporting outcomes have been rarely analyzed. Daske et al. (forthcoming) find that only firms that experience a substantial change in their reporting incentives are perceived to derive significant capital market benefits while other firms that switch to IFRS under a "tick-box" mentality do not experience capital market benefits. These results are interpreted as evidence that: (i) IFRS mandatory adoption *per se* has little effect on firms reporting practices; (ii) country-level infrastructures do not to account for all firm-level heterogeneity in firm reporting quality.

However, there is considerable evidence supporting the hypothesis that monitoring-oriented boards increase financial reporting quality by, for example, constraining earnings management (Dechow et al. 1996; Klein et al. 2002; Peasnell et al. 2005; Faleye et al. 2011). Therefore the board of directors and its monitoring intensity could drive the change in financial reporting quality around IFRS mandatory adoption. Although there is widespread consensus about the role of governance monitoring mechanisms on financial reporting quality, firm-level corporate governance has received little attention[15] in previous research on mandatory IFRS adoption. This paper tries to fill this gap in the literature analyzing the role of corporate governance on financial reporting quality after the mandatory IFRS adoption.

Predictions: Monitoring role of board of directors and IFRS adoption

The idea underlying this paper is to exploit cross-sectional variation in board-based monitoring intensity to examine heterogeneity in financial reporting quality changes around IFRS mandatory adoption. So far, research shows substantial cross-sectional heterogeneity in the

[15] Verriest et al. (2012) is an exception. Verriest et al. (2012), focusing on a small sample of European firms, document a positive association between the strength of firm-level corporate governance and firms compliance with the first-time IFRS adoption requirements, providing early evidence on the crucial role played by firm-level monitoring mechanisms, at least in the degree of compliance at the first-time IFRS adoption.

consequences of IFRS mandatory adoption. Evidence of changes of financial reporting quality is mixed and controversial with several studies pointing toward an increase in accounting quality (Barth et al. 2008, 2012; Gordon et al. 2009) while other papers suggest a decrease in accounting quality (Ahmed et al. forthcoming; Atwood et al. 2011). Indeed, managers can use accounting flexibility either to convey critical information or to lower accounting quality. On the one hand, Barth et al. (2008) purports that IFRS can improve accounting quality because principles-based accounting standards are more difficult to be circumvented. On the other hand, their principles-based nature and the lack of implementation guidance provide significant flexibility that can be used to reduce accounting information quality.

In this context, governance monitoring mechanisms can play a pivotal role in shaping reporting quality. Firms under the scrutiny of sound boards and managers may use the inherent flexibility of accounting regulation to convey information of higher quality more than to increase information asymmetries (Beyer et al. 2010). According to agency theory (Jensen and Meckling 1976), board independence, the independence of the audit committee and the financial expertise of independent audit committee members reduce managerial leeway thus increasing transparency and financial reporting quality. There is considerable evidence supporting the hypothesis that monitoring-oriented boards constrain earnings management, thus increasing financial reporting quality (Dechow et al. 1996; Klein et al. 2002; Peasnell et al. 2005; Song et al. 2010; Faleye et al. 2011). For example, Peasnell et al. (2005) show a negative effect of board independence on earnings management. In a consistent manner, Song et al. (2010) show that board independence reduces the concern over the reliability of fair value information.

More recently, there is evidence that points out how the degree of financial expertise of board members plays the major role in determining financial reporting quality, most likely by making the board of directors more effective in carrying out its monitoring duties (DeFond et al.

2005, Krishnan et al. 2011). De Fond et al. (2005) show that markets react positively to the appointment of a financial expert on the audit committee. Moreover, Carcello et al. (2006) show the importance of accounting expertise on financial reporting quality, thus corroborating the idea that independence is not the only variable which affects accounting quality. Overall, prior research links firm-level corporate governance with financial reporting quality. Hence, we posit our first hypothesis:

H1: On average, firms with strong board-based monitoring enjoy a larger increase in financial reporting quality around IFRS mandatory adoption than firms with weak board-based monitoring.

According to the new institutional accounting theory (Wysocki 2011), the outcomes of a change in accounting standards are shaped both by country-level institutions and firm-level characteristics, like the structure of board of directors, ownership structure or auditor quality. According to Wysocki (2011), financial reporting outcomes are likely to depend both on macro-institutions (e.g., capital markets' regulation, corporate law prescriptions and the legal enforcement) and micro-institutions (e.g., corporate governance). So far, literature highlights the importance of countries' legal frameworks for reporting incentives by comparing across-countries differences in the consequences of IFRS adoption with respect to a given outcome variable while firm-level governance has not been examined.

However, how firm-level corporate governance and country-level legal institutions interact is still controversial. On the one hand, in countries where investors rights are stronger and better enforced, capital markets are more developed, firms practice better governance and are valued higher than in less investor friendly countries (La Porta et al. 1997). A country institutional

system frames firm-level corporate governance attributes that a firm decides to adopt (Doidge et al. 2007), through its effect on the cost of implementing governance practices. In contrast, in weak investor protection countries, it could be overly expensive for a firm to adopt strong corporate governance mechanisms, as the pay-off could be negligible. In stronger investor protection countries, firms may expect more benefits from adopting strong governance mechanisms as effective legal infrastructures make it economically feasible to bond to good governance. As a consequence, we may expect that governance and the strength of legal enforcement complement each other in countries with strong regulatory oversight while there are negligible effects in weak enforcement countries.

On the other hand, stronger and well-disciplined corporate governance mechanisms should be more valuable and important in mitigating the negative effects of an ineffective legal system where the regulation is lax and investor rights are weak and badly enforced (Durnev and Kim 2005, Chen et al. 2009). In such countries, indeed, investors cannot rely on the legal system to monitor insiders' behaviors. In this vein, we may expect that governance complements the legal system where the legal system is strong and substitute country-level enforcement where it is lax.

However, the literature on the relation between country-level legal/institutional factors and firm-level corporate governance mechanisms is still tentative and provides mixed evidence. Hence, it remains an empirical question as to how firm-level monitoring mechanism and country level institutional factors jointly shape the consequences of IFRS mandatory adoption on financial reporting quality. Our second hypothesis, stated in the null form, is:

H2: The strength of country-level legal enforcement does not moderate the effect of board-based monitoring mechanisms on financial reporting quality after IFRS mandatory adoption.

2.3. Data and research design

Sample selection

We start the sample selection procedure by identifing from Compustat Global all public companies domiciled in Europe from 2002 to 2008. We obtain accounting and market data from Compustat Global, while analyst forecast and earnings annoucement data from the I/B/E/S international (split unadjusted) database. We eliminate firms in banking and financial industry (SIC codes between 6000-6500), firms cross-listed in the U.S or following U.S. GAAP, and require at least eight observations in each two-digit SIC grouping per year and country to estimate accrual-based and real earnings management metrics. We combine accounting and market data with analyst forecast and earnings annoucement information from I/B/E/S used to measure return and volume metrics. To be included in the sample, we require each firm to have data available for at least one period before and one period after the mandatory adoption deadline (i.e. fiscal years beginning on or after the January 1, 2005). Finally, we require that each firm-year observation have data necessary to calculate the variables used in the analysis.Next, we identify mandatory IFRS adopters by retrieving information on a firm's accounting standards followed from Compustat Global. We define mandatory adopters those firms that do not adopt IFRS until it becomes mandatory[16] (i.e. fiscal-years beginning on or after 01/01/2005). These restrictions yield a final treatment sample of 3,476 mandatory IFRS adopters firm-year observations from 14 European countries from 2002 to 2008.

Then, we augmented the treatment sample with a control sample of local GAAP firms from countries that do not require IFRS reporting during the test period for which we have the necessesary data to calculate the variables used in our analysis. This yields a final control sample

[16] A firm is classified as mandatory IFRS adopters if the data item "astd" in Compustat global does not equal "DI" prior to fiscal year beginning on or after January 1, 2005. If a firm adopts IFRS after 2005, we drop it from the sample.

of 29,596 firm-year observations from 11 non IFRS adopters countries from 2002 to 2008.

Table 2.1
Distribution of Observations by Country

Country	Firms-years	Institutional Variables		
		Adoption of Mandatory IFRS reporting	Rule of Law	Proactive Review
IFRS adoption countries				
Austria	3	12/31/2005	1.8 (1)	(0)
Belgium	50	12/31/2005	1.4 (0)	(0)
Switzerland	70	12/31/2005	2.0 (1)	(0)
Germany	278	12/31/2005	1.7 (1)	(1)
Denmark	56	12/31/2005	1.9 (1)	(0)
Spain	95	12/31/2005	1.1 (0)	(0)
Finland	152	12/31/2005	1.9 (1)	(1)
France	662	12/31/2005	1.3 (0)	(0)
United Kingdom	1,435	12/31/2005	1.6 (1)	(1)
Greece	22	12/31/2005	0.7 (0)	(0)
Italy	222	12/31/2005	0.5 (0)	(0)
Netherlands	87	12/31/2005	1.7 (1)	(1)
Norway	123	12/31/2005	1.9 (1)	(1)
Sweden	221	12/31/2005	1.8 (1)	(0)
Total	3,476			
Non-IFRS adoption countries				
Canada	2,592			
Chile	77			
China	2,289			
India	194			
Indoneia	1,469			
Japan	17,273			
Korea	2,386			
Mexico	149			
Malaysia	1195			
Thailand	659			
Taiwan	1,313			
Total	29,596			

Table 1 reports the sample distribution. The treatment sample comprises 3,476 firm-year observations from 12 EU countries plus Norway and Switzerland during the period from 2002 to 2008. The control sample comprises 29,596 firm-year observation from 11 countries that do not require IFRS reporting over the entire test period.

Finally, for each firm-year in the treatment sample, we manually identify the composition of the board from the annual reports, and extract information about each director role,

independence status, service on board audit committee, and work experiences. Information about directors' primary occupation in these filings is often missing or incomplete. Hence, we collect additional information from other sources (i.e. BoardEx, Thomson One, LexisNexis). Data on the ownership structure comes from Amadeus - Bureau van Dyck database.

Table 2.1 illustrates the sample distribution by country. The number of observations varies widely across countries: Austria has the lowest number of observations (3), and the UK has the highest (1,435). In the fourth column we report the values of the legal enforcement variable (Kaufman et al. 2007), which documents a substantial variation in the legal enforcement across the sample countries: Italy has the lowest value (0.50), and Switzerland has the largest (2.0). In the last column we report whether an IFRS adoption country has changed the level of enforcement over financial reporting around IFRS mandatory adoption (Christensen et al. 2012).

Financial reporting quality metrics

Following Landsman et al. (2012), we capture market-based financial reporting quality using the information content of earnings announcements operationalized with abnormal stock return volatility and abnormal trading volume. We compute abnormal stock return volatility at the earnings announcement dates as the ratio between the event window return volatility and the non-event window return volatility. To estimate the market model, we employ a non-event window of $t - 60$ to $t - 10$ and $t + 10$ to $t + 60$, while the event window runs from t–1 to t+1, where t is the earnings announcement date.

$$R_{it} = \alpha_i + \beta_i R_{mt} + \varepsilon_{it} \quad (1)$$

Where R_{it} is the stock return of firm i for day t, and R_{mt} is the equal-weighted return for all

within country firms in the sample for day *t* (DeFond et al., 2007), and α_i and β_t are firm *i*'s market model estimates, each of which is calculated during the non-event period. The estimated coefficients of the market model are used to estimate daily abnormal returns using the equation:

$$AR_{it} = R_{it} - (\hat{\alpha}_i + \hat{\beta}_i R_{mt}) \quad (2)$$

Abnormal stock return volatility (AVAR) is the ratio between the mean of the squared market model abnormal returns ($E[AR^2_{it}]$), and the variance of a firm's *i* market model residuals during the non-event window (σ^2_i). To reduce the skewness, we take the natural logarithm (i.e. $AVAR = \log(E[u^2_{it}]/ \sigma^2_i)$).

We measure the abnormal trading volume (AVOL) as the ratio between the mean of the event window volume (V_{it}) and the mean of the non-event window trading volume ($E[V_i]$). Daily volume around earnings announcement date, V_{it}, is number of shares of firm *i* traded during day *t* divided by share outstanding of firm *i* at day *t*-1, *t* =0 and *t*+1, where *t* is the earnings announcement day; V_i is the average daily trading volume for firm *i* for days *t*-60 to *t*-10 and *t*+10 to *t*+60 relative to the I/B/E/S earnings announcement date. As for AVAR, this ratio is highly skewed, hence our measure of abnormal trading volume is the natural logarithm of this ratio (i.e., $AVAL = \log(E[V_{it}]/ E[V_i])$). For AVAR and AVOL, higher values represent a higher information content of earnings announcements and hence a higher financial reporting quality.

We use a cross-sectional model of discretionary accruals, where for each year and country we estimate the model for every industry classified by its two-digit SIC code. In this way, we partially control for industry changes in economic conditions that affect total accrual while allowing the coefficient to vary across groups (DeFond and Jiambalvo 1994). We estimate the

modified cross-sectional Jones model (Jones 1991) as described in Dechow et al. (1995):

$$\frac{TA_{ijt}}{Assets_{ijt-1}} = \beta_1 \frac{1}{Assets_{ijt-1}} + \beta_2 \frac{\Delta REV_{ijt}}{Assets_{ijt-1}} + \beta_3 \frac{PPE_{ijt}}{Assets_{ijt-1}} + \varepsilon_{ijt} \quad (3)$$

Where TA_{ijt} is a firm's *i* total accruals in year *t* and two-digit sic code *j*, measured as net income before extraordinary items and discontinued operations minus operating cash flow; $Assets_{ijt}$ is a firm's *i* total assets in year *t-1* and two-digit sic code *j*; ΔREV_{ijt} is the change in revenues from the preceding year for firm *i* in two-digit sic code *j* ; PPE_{ijt} is the gross property plan and equipment for firm *i* in two-digit sic code *j* in year *t* .[17] The coefficient estimates from equation (3) are used to estimate firm-specific normal accruals (NA) for the sample firms:

$$NA_{ijt} = \hat{\beta}_1 \frac{1}{Assets_{ijt-1}} + \hat{\beta}_2 \frac{\Delta REV_{ijt} - \Delta AR_{ijt}}{Assets_{ijt-1}} + \hat{\beta}_3 \frac{PPE_{ijt}}{Assets_{ijt-1}} \quad (4)$$

where ΔAR_{ijt} is the change in account receivable from the preceding year for firm *i* in two-digit sic code *j*. Our measure of discretionary accruals is the difference between total accruals and the predicted normal accruals from equation (4), defined as $DA_{ijt} = (TA/Asset_{ijt-1}) - NA_{ijt}$. To the extent that we do not predict any given direction for accrual-based earnings management, we compute the absolute value of discretionary accruals and refer to it as ABS_DA.

Following Roychowdhury (2006), we use the abnormal level of cash flow from operation (R_FCFO), the abnormal level of production costs (R_PROD) and the abnormal level of discretionary expenses to capture the manipulation of real activities (R_DISC). We model the

[17] To mitigate the undue influence of outliers, we winsorize all variables entering in the modified Jones model at the 1st and 99th percentiles.

normal level of cash flow from operations as a linear function of sales and change in sales. Consistent with prior works, we estimate the following model for each country-year and industry defined by its two-digit SIC code.

$$\frac{CFO_{ijt}}{Assets_{ijt-1}} = \beta_1 \frac{1}{Assets_{ijt-1}} + \beta_2 \frac{SALES_{ijt}}{Assets_{ijt-1}} + \beta_3 \frac{\Delta SALES_{ijt-1}}{Assets_{ijt-1}} + \varepsilon_{ijt} \quad (5)$$

Where CFO_{ijt} is cash flow from operations for firm's *i* in year *t* and two-digit sic code *j*; $SALES_{ijt}$ is the net sales for firm's *i* in year *t* and two-digit sic code *j*; $\Delta SALES_{ijt}$ is the change in net sales from the preceding year for firm *i* in two-digit sic code *j*. Firms that engage in real earnings management have a lower level of abnormal cash flow than do other firms. Hence, we multiple the abnormal level of cash flow by minus one, so that higher values represent higher value of manipulation.

To estimate the normal level of production cost, first we model normal cost of goods sold (COGS) and inventory growth (ΔINV) using the following linear functions:

$$\frac{COGS_{ijt}}{Assets_{ijt-1}} = \beta_1 \frac{1}{Assets_{ijt-1}} + \beta_2 \frac{SALES_{ijt}}{Assets_{ijt-1}} + \varepsilon_{ijt} \quad (6)$$

$$\frac{\Delta INV_{ijt}}{Assets_{ijt-1}} = \beta_1 \frac{1}{Assets_{ijt-1}} + \beta_2 \frac{\Delta SALES_{ijt}}{Assets_{ijt-1}} + \beta_3 \frac{\Delta SALES_{ijt-1}}{Assets_{ijt-1}} + \varepsilon_{ijt} \quad (7)$$

Using equations (6) and (7), we estimate the normal level of production cost for each country, year, and industry as follows:

$$\frac{PROD_{ijt}}{Assets_{ijt-1}} = \beta_1 \frac{1}{Assets_{ijt-1}} + \beta_2 \frac{SALES_{ijt}}{Assets_{ijt-1}} + \beta_3 \frac{\Delta SALES_{ijt}}{Assets_{ijt-1}} + \beta_4 \frac{\Delta SALES_{ijt-1}}{Assets_{ijt-1}} + \varepsilon_{ijt} \quad (8)$$

Where $PROD_{ijt}$ is defined as the sum of the cost of goods sold ($COGS_{ijt}$) and the change in inventory (ΔINV_{ijt}) for firm's *i* in year *t* and two-digit sic code *j*; $SALES_{ijt}$ is the net sales for firm's *i* in year *t* and two-digit sic code *j*; $\Delta SALES_{ijt}$ is the change in net sales from the preceding year for firm *i* in two-digit sic code *j;* The abnormal level of production costs (R_PROD) is defined as the residuals from equation (8). The higher the residuals, the higher the inventory overproduction, and the larger is the increase in the earnings by reducing the cost of goods sold.

The normal level of discretionary expenses is modeled as

$$\frac{DISCX_{ijt}}{Assets_{ijt-1}} = \beta_1 \frac{1}{Assets_{ijt-1}} + \beta_2 \frac{SALES_{ijt-1}}{Assets_{ijt-1}} + \varepsilon_{ijt} \qquad (9)$$

Where $DISCX_{ijt}$ is a firm's *i* discretionary in year *t* and two-digit sic code *j* , and it is computed as the sum of advertising expenses, R&D expenses, and Selling, General and Administrative expenses. Abnormal discretionary expenditures (R_DISC) is defined as the residuals from equation (9). We multiply the residuals by minus one, such that higher values indicate greater amounts of discretionary expenditures cutting to inflate earnings upward. Finally, we aggregate the three real earning management proxies into two aggregate measures by taking their sum. We compute REAL_1 as the sum between abnormal production cost and abnormal discretionary expenses, and REAL_2 as the sum between abnormal discretionary expenses and abnormal cash flow from operations. Higher values of both these proxies suggest a higher level of real earnings management.

Corporate governance score

We capture the strength of board-based monitoring mechanisms for each IFRS reporting sample firm by combing six governance attributes into a binary variable through a factor analysis (Larcker et al. 2007; Song et al. 2010). To the extent that strong governance on manifold facets reveals a stronger governance environment, our proxy should better summarize the overall strength of a firm governance mechanisms than a single measure (Bushman et al. 2004; DeFond et al. 2005). The six governance attributes include (1) board independence (INDEPENDENT), as the number of independent directors divided by board size; (2) a dummy variable (AUDIT) for the presence of an audit committee; (3) audit committee size (AUDIT_SIZE); (4) audit committee independence (INDEPENDENT_AUDIT), as the number of independent board members serving on the audit committee over audit committee size; (5) audit committee financial expertise (FINANCIAL_EXPERT_AUDIT), as the number of audit committee members with financial expertise divided by the size of the audit committee; (6) total percent shares held by institutional investors (INST_OWN).[18] Such measures depict several attributes of a firm corporate governance strength that we employ to measure board-based monitoring intensity.

Independent directors are believed to be willing to stand up to the insiders and more effective than non--independent directors in mitigating agency problems between insiders and outside investors (Fama 1980). The audit committee's main duty is to oversee the financial reporting process to guarantee the integrity and the credibility of financial reports. We first consider: (1) the presence of an audit committee (Peasnell et al. 2005), (2) the audit committee size (DeFond et al. 2005), (3) its degree of independence (Klein 2002). Next, we consider the financial expertise of its members (DeFond et al. 2005). As recognized by the US Congress and

[18] We do not include board size as a determinant of the governance factor score for several reasons. Literature provides mixed evidence on the effect of board size on monitoring effectiveness. In addition, board size is highly correlated with firm size and, in a cross-country sample it is subjected to the different national legislations. However, we do compute the factor score also including board size. The results remain unchanged.

the SEC, financial expertise is a necessary condition to ensure that the audit committee fulfils its monitoring duties (SEC 2003b). To construct this variable, we follow the DeFond et al. (2005) and the SOX Section 407 definition of financial expertise in both the first version proposed and in the last implemented by the SEC (SEC 2002, SEC 2003a, 2003b,). We read each board member biographical sketch to classify each independent director into one of the following categories: (1) SOX financial experts as all directors who have financial expertise as defined in the last version of SOX; (2) Nonfinancial expert as all directors who do not meet the definition of a SOX financial expert. Even if this coding requires some judgment, we strictly follow the guidelines provided in the proposed and final SEC rules. We label a director as a financial expert if she has experiences as public accountant, auditor, CFO, controller, chief accounting officer (these are inferred from the proposed rules by the SEC) or has experience as CEO of executives of a for-profit organization (these are drawn from the final version of SOX implemented by the SEC). Audit committee members with financial expertise should be more familiar with the ways that earnings can be managed. On the contrary, an audit committee without financial expert members may be largely ceremonial. Finally, we consider the percentage of shares held by institutional investors as they improve a firm corporate governance environment by constraining insiders' behaviour (Nesbitt 1994).

Table 2.2, panel A provides descriptive statistics for governance attributes. Next, we apply a principal component factor analysis to the six governance attributes (Larcker et al. 2007; Song et al. 2010). The first and primary factor exhibits the expected loadings (Table 2, panel B). The factor analysis generates an eigenvalue of 3.463[19], which accounts for about 57.97% of the total variance in the original variables. Table 2, panel B, second column reports the Kaiser-Meyer-Olkin measure of sampling adequacy. Each variable is associated with a value greater than 0.6,

[19] The second factor is associated with an eigenvalue of 0.9372. For this reason it is not retained.

and the mean KMO value is about 0.759, indicating that the GOVSCORE is able to capture well the underlying common factor of the six individual variables. Panel C reports the descriptive statistics for GOVSCORE. Due to the standardization, such a variable has mean 0 and standard deviation of 1. Next, we take the firm-specific mean of GOVSCORE across the sample years, and we create a binary variable (GOOD_GOV) based on the sample median of the firm-specific mean of GOVSCORE[20]. Specifically, we classify firms with above sample median value of the firm-specific mean of GOVSCORE as strong board-based monitoring firms (GOOD_GOV equals to one).

Table 2.2

Descriptive Statistics for Governance Attributes

Panel A: Descriptive statistics

	N	Mean	Std. Dev	P5	P25	Median	P75	P95
INDEPENDENT	3,724	0.347	0.216	0.000	0.200	0.333	0.500	0.714
AUDIT	3,724	0.635	0.482	0.000	0.000	1.000	1.000	1.000
AUDIT_SIZE	3,724	2.101	1.877	0.000	0.000	3.000	3.000	5.000
INDEPENDENT_AUDIT	3,724	0.412	0.382	0.000	0.000	0.500	0.750	1.000
FINANCIAL EXPERT_AUDIT	3,724	0.306	0.328	0.000	0.000	0.310	0.600	1.000
INST_OWN	3,724	0.556	0.497	0.000	0.000	1.000	1.000	1.000

Panel B: Governance factor score and sample adequacy

	Factor Loading Coefficients	Kaiser-Meyer-Olkin Measure of Sampling Adequacy
INDEPENDENT	0.444	0.687
AUDIT	0.911	0.725
AUDIT_SIZE	0.861	0.795
INDEPENDENT_AUDIT	0.944	0.738
FINANCIAL EXPERT_AUDIT	0.877	0.806
INST_OWN	0.251	0.859
Variation Explained	57.97%	Mean KMO = 0.759
Eigenvalue	3.478	

Panel C: Descriptive statistics of governance factor score

	N	Mean	Std. Dev	P5	P25	Median	P75	P95
GOVSCORE	3,724	0.000	1.000	-1.221	-1.149	0.215	0.877	1.466

Table 2, panel A reports descriptive statistics for the corporate governance variables for firms from IFRS adoption countries. Panel B presents the results of the principal component factor analysis. Panel C of the corporate governance factor score.
See APPENDIX II.B for variable definitions

[20] In this way we do not assume that board monitoring intensity does not vary over time. Instead ,we assume that the cross-sectional difference in board monitoring intensity across firms does not. We relax this assumption later and obtain similar results. Note, however, that less than 6% of the firms in our sample went from being classified as weak (strong) board-based monitoring firms in the pre mandatory adoption period to being classified as strong (weak) board-based monitoring firms in the post mandatory adoption period. Overall, board composition and thus monitoring intensity seems to be quite stable over time.

Identification strategy

We investigate the impact of firm-level board monitoring intensity and the interactive effect of firm-level board monitoring intensity and country-level legal enforcement on financial reporting quality around IFRS mandatory adoption by employing a panel dataset with an extensive fixed effects structure. Our identification strategy encompasses four steps. First, we use a treatment sample of mandatory IFRS firm-year observations and a control sample of non-IFRS firm-year observations. Specifically, our final sample consists of observations from European Union countries that require IFRS reporting from 2005, and countries that do not mandate IFRS reporting over the entire test period. The use of this benchmark group allows us to take into account global time-trends in financial reporting quality metrics. Moreover, this approach allows us to exploit both cross-sectional and time variation in IFRS reporting to identify the change in financial reporting quality metric in the pre- versus post-mandatory adoption period for mandatory adopters relative to the change for the benchmark firms, thus taking into account unobserved heterogeneity across firms or time-invariant selection bias. This issue may be particularly severe in corporate governance research, to the extent that board composition is endogenous. In our setting, there may be many reasons for board composition and financial reporting quality to be jointly determined by some unobserved firm characteristics. If these unobserved firm characteristics are time invariant, then a design which exploits IFRS mandatory adoption as an exogenous shock addresses simultaneous determination problems.

Second, we classify IFRS adopters firms with respect to the strength of board-based monitoring mechanisms, by using the factor score from a principal component factor analysis of a comprehensive set of board and directors characteristics[21]. This should alleviate concerns about

[21] Note that we do not collect data and thus do not partitioning the control sample with respect to strengh of firm-level corporate governance, to the extent that our identification strategy does not require to split the control according to the partitioning variables used for the treatment sample (Christensen et al. 2012).

random measurement error of single corporate governance metric as well as better identify the overall strength of a firm governance mechanisms (Baik et al. 2009). In the main analysis, we focus on board monitoring *levels*, by taking the firm-specific average of the factor across the years. We further relax the assumption that cross-sectional differences in board monitoring remain constant over time by allowing the board-based monitoring mechanism metric to vary.

Third, we distinguish IFRS adopter countries according to both the historical strength of the legal enforcement and changes in financial reporting enforcement concurrent to IFRS mandatory adoption (Christensen et al. 2012). Following previous studies (Daske et al. 2008; Byard et al. 2011) we first consider a cross-sectional measure to capture differences in the quality of legal enforcement. We use the Rule of Law (RULE_LAW) variable for 2005 developed by Kaufmann et al. (2007). Higher values represent countries with stricter enforcement regimes. We next transform this measure (RULE_LAW) into a binary variable (HIGH_LAW) based on whether a country specific value is above or below the treatment sample country median. Then, to explicity account for changes in accounting enforcement mechanisms aroud IFRS adoption, we follow Christensen et al. (2012) and code up a binary variable (PROACTIVE) marking countries that have increased the strength of financial reporting mechanisms around IFRS mandatory, through the introduction of the proactive review over financial statements (i.e. United Kingdom, Germany, Finland, Netherlands and Norway).

The last element of the identification strategy consists of an extensive fixed-effects structure. We include country, industry and separate year fixed effects for the treatment sample and the control sample. This fixed effect structure allows to control for general trends or shocks common to firms in the control and treatment sample in a given year while focusing only on time variation in term of IFRS adoption within each group. We further check the stability of our results replacing the countries and industry fixed effects with firm-fixed effects. We thus propose

the following estimation model (without firm and time subscripts):

$$FRQ = \beta_0 + \beta_1 IFRS + \sum_j \beta_j CONTROLS_j + \sum_i \beta_i FIXED\ EFFECTS_i + \gamma \quad (10)$$

where FRQ (i.e. financial reporting quality) stands for the abnormal return variability, abnormal trading volume, and the earnings management metrics (accrual-based and real earnings management). IFRS is a binary variable that takes the value of one for firms that apply IFRS only when it becomes mandatory in 2005 for fiscal-years beginning on or after 01/01/2005, and zero otherwise. CONTROLS denotes the set of control variables that differ according to the particular dependent variable used, while FIXED EFFECTS represents country-industry and separate year fixed effects. In equation (10) we do not include indicator variables for the strength of board-based monitoring mechanisms, legal enforcement or the increase in the level of accounting enforcement. Rather, in the empirical analysis we add to equation (10) a set of non-overlapping binary variables to estimate separate IFRS effects on financial reporting quality conditional on board-based monitoring mechanisms, the legal enforcement (and the changes) and the interaction between the two.

When we exploit cross-sectional variation in the level of board-based monitoring to examine the marginal effect of firm-specific monitoring intensity on financial reporting quality around IFRS mandatory adoption, we propose the following model (without firm and time subscripts):

$$FRQ = \beta_0 + \beta_1 IFRS_{\text{HIGH\_GOV}} + \beta_2 IFRS_{\text{LOW\_GOV}} + \sum_j \beta_j CONTROLS_j + \sum_i \beta_i FIXED\ EFFECTS_i + \gamma \quad (11)$$

In this model, we replace the single IFRS indicator from equation (10) with two non-overlapping indicator variables for (i) firms in the IFRS treatment sample with strong board-based monitoring mechanisms (i.e. $IFRS_{HIGH\_GOV}$), (ii) firms in the IFRS treatment sample with weak board-based monitoring mechanisms (i.e. $IFRS_{LOW\_GOV}$).

When we explore the interplay between country-level institutional characteristics and firm-level board-based monitoring intensity, we instead replace the the single IFRS indicator variable from equation (10) with four binary variables marking: (i) firms from IFRS countries with strong enforcement institutions and strong board-based monitoring mechanisms (i.e. $IFRS_{HIGH\_LAW\_HIGH\_GOV}$), (ii) firms from IFRS countries with strong enforcement institutions and weak board-based monitoring mechanisms (i.e. $IFRS_{HIGH\_LAW\_LOW\_GOV}$), (iii) firms from IFRS countries with weak enforcement institutions and strong board-based monitoring mechanisms (i.e. $IFRS_{LOW\_LAW\_HIGH\_GOV}$), (iv) firms from IFRS countries with strong enforcement institutions and weak board-based monitoring mechanisms (i.e. $IFRS_{LOW\_LAW\_LOW\_GOV}$).

$$FRQ = \beta_0 + \beta_1 IFRS_{\text{HIGH\_LAW\_HIGH\_GOV}} + \beta_2 IFRS_{\text{HIGH\_LAW\_LOW\_GOV}} + \beta_3 IFRS_{\text{LOW\_LAW\_HIGH\_GOV}} + \beta_4 IFRS_{\text{LOW\_LAW\_LOW\_GOV}} + \sum_j \beta_j CONTROLS_j + \sum_i \beta_i FIXED\ EFFECTS_i + \gamma \quad (12)$$

Control variables

All the models include country-industry (using the Campbell (1996) industry classification) and separate fixed effects and heteroskedasticity-corrected standard errors, adjusted at firm-level clustering (Gow et al. 2010). According to the financial reporting quality metrics used, we include a set of controls variables. In the AVAR and AVOL regressions, we control for firm size (SIZE) using the log of a firm total asset to control for the effect of firms' size on financial

reporting quality. We also consider firm leverage (LEV) using the ratio between the end-of-year total liabilities and the end-of-year book value of equity. As long as negative earnings are less informative than positive earnings (Hayn, 1995), we control for loss reporting firms by including a binary variable equals to one if the reported earnings per share per I/B/E/S is less than zero (LOSS). We include also: the difference between the actual earnings per share and the analyst consensus earnings forecast before the earnings announcement, scaled by the closing price at the fiscal year end to capture the uncertainty in the analyst information environment (AFE); the standard deviation of analyst forecasts prior to the earnings announcement, scaled by the closing price as the end of the year (DISPERSION); and the logarithm of the number of days between the firm's fiscal year end to the earnings announcement (REP_LAG). We include the log of the number of analyst forecasts made during the year to account for the strength of the monitoring carried out by analysts (FOLLOWING). Finally, we account for macroeconomic factors using the log of the annual change in the ratio of stock market capitalization and gross domestic product per capita (ΔCAP/GDP), taken from the World Bank. This variable is used in international research (Haw et al. 2004) to deal with unobserved country-specific factors that may be associated with financial reporting quality.

In the accrual-based and real earnings management regressions, we control for several factors that are associated with financial reporting quality. We control for firm size (SIZE). We control for performance using return on assets (ROA), as net income over the end of the year total assets and LOSS. We include growth prospects, as the percentage change in sales (GROWTH) as there is evidence that it influences earnings management (Barth et al. 2008). We take into account debt-contracting motivations for earnings management (DeFond and Jiambalvo 1994) using the end-of-year total liabilities divided by the end-of-year book value of equity (LEV), the percentage change in total liabilities (DISSUE), and the percentage change in totoal

equity (EISSUE). Turnover is computed as sales divided by end of the year total assets (TURN). We control for innate factors relating to the firm's operating environment that are likely to be associated with financial reporting quality (Hribar and Nichols 2007). We include the variability in operating cash flows [σ(FCFO)], variability in sales [σ(SALES)], both measured as a rolling standards deviation over the past five years, and the length of the operating cycle (OPER_CYCLE). Finally, we account for macroeconomic factors using ΔCAP/GDP.

2.4. Results

Table 2.3 presents descriptive statistics for the variables used in the regression analyses. The mean (median) of AVAR is 0.040 (0.065), while the mean (median) of AVOL is 0.412 (0.356). The mean (median) of the absolute discretionary accruals is 0.059 (0.035), while the average (median) for the aggregate real earnings management variables are -0.045 (-0.029) for REAL_1 and -0.067 (-0.038) for REAL_2.

We start our empirical analyses by exploring the average change in financial reporting quality, operationalized as the information content of annual earnings announcements, around IFRS mandatory adoption. Table 2.4, columns (1)-(2) reports the estimate coefficients and (in parentheses) t-statistics from the estimation of equation (10) using AVAR and AVOL as dependent variables. Consistenly with Landsman et al. (2012), we find a significant increase in both AVAR and AVOL. The control variables behave as expected. Collectively, thes findings corroborate extant evidence on the change in earnings informativeness around IFRS adoption.

Table 2.3

Descriptive statistics for Variables Used in Regression Analyses

Variable	*N*	Mean	Std. Dev	P5	P25	Median	P75	P95
AVAR	33,320	0.040	1.014	-1.588	-0.529	0.065	0.641	1.570
AVOL	33,320	0.412	0.823	-0.785	-0.083	0.356	0.851	1.815
ABS_DA	33,320	0.059	0.086	0.003	0.015	0.035	0.072	0.197
REAL 1	33,320	-0.045	0.187	-0.359	-0.121	-0.029	0.048	0.213
REAL 2	33,320	-0.067	0.299	-0.566	-0.172	-0.038	0.073	0.319
SIZE	33,320	5.904	1.670	3.446	4.760	5.730	6.886	9.023
LEV	33,320	1.678	22.615	0.182	0.555	1.096	1.967	5.136
LOSS	33,320	0.168	0.374	0.000	0.000	0.000	0.000	1.000
AFE	33,320	0.387	8.054	0.002	0.026	0.090	0.156	0.823
DISPERSION	33,320	3.419	321.508	0.000	0.001	0.007	0.011	0.364
REP_LAG	33,320	3.975	0.379	3.332	3.784	3.912	4.127	4.710
FOLLOWING	33,320	0.000	26.447	-26.764	-14.237	-4.976	6.375	44.801
ROA	33,320	3.593	11.047	-8.585	0.986	3.446	7.243	16.801
GROWTH	33,320	25.280	316.464	-18.615	0.137	11.469	25.327	68.248
DISSUE	33,320	20.247	84.230	-26.484	-4.925	6.928	23.980	92.975
EISSUE	33,320	2,515.759	44,9194.500	-11.430	1.036	6.100	13.555	61.982
OPER_CYCLE	33,320	172.360	3500.141	30.754	73.571	114.972	159.039	254.427
TURN	33,320	1.071	0.624	0.296	0.664	0.946	1.331	2.297
σ(CFO)	33,320	66.212	249.591	1.550	4.885	12.424	37.383	252.647
σ (SALES)	33,320	329.902	1,340.159	5.123	18.895	53.203	171.914	1,298.545

Table 3 reports descriptive statistics for the dependent variables and the continuous and binary independent variables. The treatment sample comprises 3,476 firm-year observations from 12 EU countries plus Norway and Switzerland during the period from 2002 to 2008. The control sample comprises 29,596 firm-year observation from 11 countries that do not require IFRS reporting over the entire test period. See APPENDIX A for variable definitions.

See APPENDIX II.A for variable definitions

Next, we examine the effectiveness of the board of directors monitoring intensity in determining financial reporting quality around IFRS mandatory adoption. We claim that stronger board monitoring is likely to enhance the credibility and integrity of firms' financial reports. As a result, we expect that, irrespective from the country of incorporation, firms which bond themselves under the scrutiny of a more monitoring-oriented board should be associated with an increase in financial reporting quality relative to firms for which the boards are less monitoring oriented**.** To do this, we replace the IFRS indicator with two non-overlapping indicator variables. One for firms in the IFRS treatment sample with strong board-based monitoring mechanisms (i.e. $IFRS_{HIGH\_GOV}$), and one for firms in the IFRS treatment sample with weak board-based

monitoring mechanisms (i.e. $IFRS_{LOW\_GOV}$). Table 2.4, columns (3)-(4) report the estimated coefficients and firm-level clustered adjusted t-statistics from the estimation of equation (11), using AVAR and AVOL as dependent variables. In both the models, we find that only mandatory IFRS adopters with strong board-based monitoring mechanisms experience an increase in financial reporting quality once IFRS became mandated (0.201, $p < 0.001$; 0.282, $p < 0.001$, respectively), while firms for which board of directors are poor monitors do not experience a significant change in financial reporting practices around IFRS mandatory adoption. Overall, our results show that, by itself, IFRS adoption has little effect on firms' reporting behavior. By contrast, we find that board-based monitoring mechanisms have an effective role in shaping firms' reporting quality after a change in accounting standards.

In the last two columns of table 2.4, we relax the assumption that firm-level corporate governance monitoring intensity is costant over time, by allowing our firm-level corporate governance indicator to vary over time. Specifically, we add to equation (10) a variable that takes the value of zero in the pre-IFRS mandatory adoption period, and the values of GOVSCORE in the post-IFRS mandatory adoption period. We also replace the country-industry fixed effects with firm fixed effects to better account for time invariant unobservable heterogeneity across firms. By this analysis, we intend to test whether IFRS adopters firms that make board-based monitoring mechanisms stricter yield an higher increases in financial reporting quality than firms that do not experience such change. Irrespective from the dependent variable we employ, we document an higher increase in financial reporting quality for firms that experience an increase in board-based monitoring intensity.

Table 2.4

OLS regressions on AVAR and AVOL conditional on firm-level board monitoring intensity

	(1) AVAR	(2) AVOL	(3) AVAR	(4) AVOL	(5) AVAR	(6) AVOL
IFRS	0.154***	0.198**	-	-	0.129	0.367***
	(5.257)	(2.248)			(0.798)	(3.770)
$IFRS_{HIGH\_GOV}$	-	-	0.210***	0.282***	-	-
			(4.091)	(3.160)		
$IFRS_{LOW\_GOV}$	-	-	0.078	0.117	-	-
			(1.536)	(1.297)		
$IFRS_{GOODGOV}$	-	-	-	-	0.081**	0.071***
					(2.455)	(2.865)
F-Test (p-values)						
$IFRS_{HIGH\_GOV} = IFRS_{LOW\_GOV}$	-	-	0.019	0.000	-	-
$IFRS = IFRS_{GOODGOV}$	-	-	-	-	0.076	0.000
SIZE	0.059***	-0.041***	0.055***	-0.042***	0.109***	-0.053**
	(4.468)	(-12.814)	(4.336)	(-13.029)	(4.148)	(-2.487)
LEV	0.000	-0.000	-0.000	-0.000	-0.001	-0.001
	(-0.341)	(-1.040)	(-0.268)	(-0.938)	(-0.281)	(-1.416)
LOSS	-0.086***	-0.017	-0.093***	-0.019	-0.110***	-0.082***
	(-3.855)	(-1.182)	(-3.357)	(-1.334)	(-4.420)	(-4.075)
AFE	0.000	0.001*	-0.000	0.001	-0.000	0.001*
	(0.072)	(1.757)	(-0.692)	(1.569)	(-0.723)	(1.907)
DISPERSION	-0.000***	-0.000	0.055	0.025	0.000	-0.000
	(-3.503)	(-1.161)	(1.297)	(1.117)	(1.115)	(-1.635)
REP_LAG	-0.023	-0.199***	-0.023	-0.198***	0.029	-0.152***
	(-0.374)	(-15.859)	(-0.376)	(-15.743)	(0.813)	(-5.113)
FOLLOWING	0.000	-0.000**	-0.000	-0.001**	-0.001	-0.001
	(0.099)	(-2.113)	(-0.223)	(-2.441)	(-1.300)	(-1.496)
ΔGDP/CAP	0.370***	0.149***	0.376***	0.143***	0.442***	0.082**
	(3.745)	(8.452)	(3.803)	(8.014)	(8.854)	(2.236)
COSTANT	0.162	1.611***	0.244	1.589***	-0.603***	1.418***
	(0.607)	(10.216)	(0.885)	(10.298)	(-2.837)	(8.074)
Country fe	Yes	Yes	Yes	Yes	No	No
Industry fe	Yes	Yes	Yes	Yes	No	No
Firm fe	No	No	No	No	Yes	Yes
Year fe	Global & EU	Global & EU	Global & EU	Global & EU	Global & EU	Global & EU
Observations	33,072	33,072	33,072	33,072	33,072	33,072
R-squared	0.032	0.036	0.033	0.037	0.292	0.344

See APPENDIX II.A for variable definitions. In parentheses are reported t-statics based on robust standard errors that are clustered at firm level.

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

However, partitioning the sample only with respect to firm-level corporate governance mechanisms does not account for cross-sectional differences in the legal frameworks within EU

member states. This variation is likely to shape the impact of a change in accounting standards on financial reporting quality, beyond firm-level characteristics. To the extent that existing research stresses the importance of a country legal enforcement for reporting incentives, we now replace the IFRS indicator with two non-overlapping binary variables marking: firms in the treatment IFRS sample from countries with a strong legal enforcement (i.e. $IFRS_{HIGH\_LAW}$), and firms in the treatment IFRS sample from countries with a weak legal enforcement (i.e. $IFRS_{LOW\_LAW}$). This approach allows us a benchmark with prior studies. Indeed, if IFRS mandatory adoption has different effects across countries in function of the legal enforcement, we should observe an increase in the information content of annual earnings announcements only in strong legal enforcement countries. Table 2.5, columns (1)-(2) reports the coefficients and firm-level clustered adjusted t-statistics. For both AVAR and AVOL the coefficient on $IFRS_{LOW\_LAW}$ is not significant, while the estimated coefficient on $IFRS_{HIGH\_LAW}$ is in the positive and significant (0.167, $p < 0.001$; 0.220, $p < 0.050$, respectevely).

We now examine the interplay between country-level and firm-level board monitoring intensity. So far, we show that strong board-based monitoring mechanisms can substitute for lax legal enforcement. Nevertheless, we cannot derive fully correct inferences about the effectiveness of board monitoring as a substitute of the legal system from prior analyses. First, almost 67 per cent of the strong-monitoring firms come from strong enforcement countries, thus the results from the estimation of prior models could be driven only by strong-monitoring firms which also come from these countries. Most importantly, we have to test whether, holding constant the legal environment, strong monitoring firms behave in a different fashion from weak monitoring firms to assess that firm-level monitoring mechanisms substitute for country-level monitoring mechanisms. Still using our panel fixed effects stucture to disentangle the effect of concurrent events around IFRS adoption, we now present the results from the estimation of model (12) to

compare the changes in financial reporting quality among four groups of firms: (i) strong monitoring firms in a strong legal enforcement; (ii) weak monitoring firms in a strong legal enforcement; (iii) strong monitoring firms in a weak legal enforcement; (iv) weak monitoring firms in a weak legal enforcement.

Table 2.5, columns (3)-(4) reports the estimation results. Consistent with the substitution effect between firm-level and country-level monitoring mechanisms, we find an increase in the AVAR and AVOL for strong monitoring firms in weak legal enforcement countries (0.245, $p < 0.001$; 0.209, $p < 0.050$, respectively), while no significant change for weak monitoring firms in the weak legal enforcement countries. Most importantly, keeping fixed the the legal enforcement, i.e. comparing the changes in financial reporting quality for firms that are forced to switch to IFRS in the same weak legal environment between strong versus weak monitoring firms, we find that the difference between the changes of the two groups is significant for both AVAR and AVOL ($p < 0.001$; $p < 0.100$, respectively). In other words, strong monitoring firms are able to separate themselves from weak monitoring firms in a weak legal environment, and are thus able to enhance reporting quality further once IFRS are mandated. Turning to strong legal environment countries, we document an improvement in financial reporting quality for strong monitoring firms for both AVAR and AVOL (0.201, $p < 0.001$; 0.301, $p < 0.001$, respectively), while for weak monitoring firms the increase in AVAR and AVOL is just slightly above the conventional level of significance. However, the difference between the changes of these two groups is significance only for AVOL ($p < 0.100$).

Finally, we split strong legal enforcement countries in two groups to account for time-variant differences in the level of enforcement on financial reporting (Christensen et al. 2012).

Table 2.5

OLS regressions on AVAR and AVOL conditional on firm-level board monitoring intensity and country-level legal enforcement

	(1) AVAR	(2) AVOL	(3) AVAR	(4) AVOL	(5) AVAR	(6) AVOL
$IFRS_{HIGH\_LAW}$	0.1667***	0.220**	-	-	-	-
	(4.965)	(2.473)				
$IFRS_{LOW\_LAW}$	0.109	0.149	-	-	-	-
	(1.584)	(1.517)				
$IFRS_{HIGH\_GOV\_HIGH\_LAW}$	-	-	0.201***	0.301***	-	-
			(5.035)	(3.305)		
$IFRS_{LOW\_GOV\_HIGH\_LAW}$	-	-	0.109	0.116	-	-
			(1.585)	(1.253)		
$IFRS_{HIGH\_GOV\_LOW\_LAW}$	-	-	0.245***	0.209**	0.236***	0.210**
			(3.313)	(2.135)	(3.217)	(2.151)
$IFRS_{LOW\_GOV\_LOW\_LAW}$	-	-	0.035	0.120	0.026	0.121
			(0.531)	(1.246)	(0.402)	(1.266)
$IFRS_{HIGH\_GOV\_HIGH\_LAW\_proactive}$	-	-	-	-	0.203***	0.305***
					(5.287)	(3.332)
$IFRS_{LOW\_GOV\_HIGH\_LAW\_proactive}$	-	-	-	-	0.198***	0.148
					(2.802)	(1.573)
$IFRS_{HIGH\_GOV\_LOW\_LAW\_non\_proactive}$	-	-	-	-	0.157**	0.278**
					(2.104)	(2.266)
$IFRS_{LOW\_GOV\_LOW\_LAW\_non\_proactive}$	-	-	-	-	-0.264**	0.012
					(-2.599)	(0.117)
F-Test (p-values)						
$IFRS_{HIGH\_LAW} = IFRS_{LOW\_LAW}$	0.342	0.073	-	-	-	-
$IFRS_{HIGH\_GOV\_HIGH\_LAW} = IFRS_{LOW\_GOV\_HIGH\_LAW}$	-	-	0.401	0.079	-	-
$IFRS_{HIGH\_GOV\_LOW\_LAW} = IFRS_{LOW\_GOV\_LOW\_LAW}$	-	-	0.000	0.084	0.000	0.084
$IFRS_{HIGH\_GOV\_HIGH\_LAW\_proactive} = IFRS_{LOW\_GOV\_HIGH\_LAW\_proactive}$	-	-	-	-	0.931	0.200
$IFRS_{HIGH\_GOV\_HIGH\_LAW\_non\_proactive} = IFRS_{LOW\_GOV\_HIGH\_LAW\_non\_proactive}$	-	-	-	-	0.000	0.009
SIZE	0.055***	-0.040***	0.055***	-0.041***	0.055***	-0.041***
	(4.402)	(-12.761)	(4.378)	(-12.929)	(4.342)	(-12.938)
LEV	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(-0.295)	(-1.028)	(-0.295)	(-0.985)	(-0.289)	(-0.984)
LOSS	-0.092***	-0.019	-0.087***	-0.016	-0.086***	-0.016
	(-3.291)	(-1.343)	(-3.939)	(-1.137)	(-3.882)	(-1.131)
AFE	-0.000	0.001	0.000	0.001*	0.000	0.001*
	(-0.687)	(1.570)	(0.062)	(1.760)	(0.061)	(1.758)
DISPERSION	0.055	0.025	-0.000***	-0.000	-0.000***	-0.000
	(1.276)	(1.126)	(-3.490)	(-1.169)	(-3.489)	(-1.169)
REP_LAG	-0.024	-0.198***	-0.022	-0.198***	-0.023	-0.199***
	(-0.391)	(-15.700)	(-0.355)	(-15.801)	(-0.374)	(-15.832)
FOLLOWING	-0.000	-0.000**	-0.000	-0.000**	-0.000	-0.000**
	(-0.068)	(-2.247)	(-0.068)	(-2.301)	(-0.098)	(-2.248)
ΔGDP/CAP	0.373***	0.147***	0.373***	0.139***	0.377***	0.142***
	(3.747)	(8.207)	(3.799)	(7.802)	(3.856)	(7.882)
COSTANT	0.158	1.591***	0.199	1.584***	0.563*	1.575***
	(0.577)	(10.015)	(0.716)	(10.201)	(1.924)	(10.107)
Country fe	Yes	Yes	Yes	Yes	Yes	Yes
Industry fe	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	Global & EU	Global & EU	Global & EU	Global & EU	Global & EU	Global & EU
Observations	33,072	33,072	33,072	33,072	33,072	33,072
R-squared	0.032	0.036	0.033	0.037	0.033	0.037

See APPENDIX II.A for variable definitions. In parentheses are reported t-statics based on robust standard errors that are clustered at firm level.

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

We thus replace the IFRS indicator variable in model (10) with six binary variables marking: (i) weak monitoring firms in a weak legal enforcement; (ii) strong monitoring firms in a weak legal enforcement; (iii) weak monitoring firms in a strong legal enforcement countries without a change in the level of enforcement; (iv) strong monitoring firms in a strong legal enforcement countries without a change in the level of enforcement; (v) weak monitoring firms in a strong legal enforcement countries with a change in the level of enforcement; (iv) strong monitoring firms in a strong legal enforcement countries with a change in the level of enforcement. Table 2.5, columns (5)-(6) reports the estimation results. Of course, the estimated coefficients for firms in weak legal enforcement countries remain stable as long as the composition of the groups identified by these indicator variables does not change. Looking at firms in strong legal enforcement countries without a change in the level of enforcement over financial reporting, we find an increase in both AVAR and AVOL only for strong monitoring firms. On the contrary, for firms incorporated in strong legal enforcement countries with an increase in the level of enforcement aroud IFRS mandatory reporting, the difference in change between strong and weak board-based monitoring firms is not significant for both the dependent variables ($p = 931$; $p = 0.200$, respectively). These finding suggests that the effectiveness of firm-level governance in shaping the effect of a change in accounting standards decrease in the level of enforcement.

So far, we have explored the effect of firm-level monitoring mechanisms around IFRS mandatory adoption on the information content of annual earnings announcements to test whether board monitoring affects the investors' assessment of the reliability of the outcomes of firms' financial reporting process. As noted by DeFond et al. (2007), the information content of annual earnings announcements is an earnings attribute strongly affected by the reliability of accounting information. Since earnings for firms that engage less in earnings management are more reliable,

in the sense that such earnings are more likely to depict firms' underling performances (DeFond et al. 2007; Bamber et al. 2011), we should observe a decrease in abnormal accruals after IFRS mandatory adoption only for strong board-based monitoring firms, irrespective from the country of incorporation. In addition, to the extent that a firm may substitute accrual based earnings management with real earnings management, we also examine the impact of IFRS mandatory adoption on real based earnings management.

Table 2.6, presents the results from the estimation of equation (10) using ABS_DA, REAL_1 and REAL_2 as dependent variables Columns (1)-(3) report results without distinguishing firms on the basis of legal enforcement or board monitoring intensity. Overall, it appears that the advent of mandatory IFRS (coefficient on IFRS) has a negligible effect on ABS_DA (-0.014, $p < 0.100$) and do not translate into more real earnings management, as proxied by higher REAL_1 and REAL_2. To investigate the issue further, we now partition IFRS mandatory adopters on the basis of board monitoring intensity (Columns (4)-(6)). The coefficient on $IFRS_{LOW\_GOV}$ is not statistically significant for ABS_DA, REAL_1 and REAL_2. In contrast, the coefficient for $IFRS_{HIGH\_GOV}$ is negative for ABS_DA (-0.015, $p < 0.050$) and positive for both REAL_1 and REAL_2 (0.043, $p < 0.001$; 0.036, $p < 0.001$, respectively). Hence, there seems to be a trade-off between accrual management (less) and real earnings management (more) in firms with strong board monitoring following the advent of IFRS.

Overall, results from table 2.6 show that, by itself, IFRS mandatory adoption has little if no effect on firms' reporting behavior. By contrast, we find that board monitoring mechanisms have an effective role in shaping firms' reporting quality after a change in accounting standards. The next analysis investigates this issue further.

Table 2.6

OLS regressions on accrual and real earnings management conditional on firm-level board monitoring intensity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	ABS_DA	REAL_1	REAL_2	ABS_DA	REAL_1	REAL_2	ABS_DA	REAL_1	REAL_2
IFRS	-0.014*	0.035	0.025	-	-	-	-0.013	0.059**	0.038
	(-1.912)	(1.044)	(0.814)				(-1.558)	(2.025)	(1.427)
$IFRS_{HIGH\_GOV}$	-	-	-	-0.015**	0.043***	0.036**	-	-	-
				(-2.117)	(5.372)	(2.616)			
$IFRS_{LOW\_GOV}$	-	-	-	-0.012	0.020	0.005	-	-	-
				(-1.492)	(1.559)	(0.380)			
$IFRS_{GOODGOV}$	-	-	-	-	-	-	-0.001	0.022***	0.031***
							(-0.423)	(2.937)	(3.955)
F-Test (p-values)									
$IFRS_{HIGH\_GOV}$ = $IFRS_{LOW\_GOV}$	-	-	-	0.208	0.067	0.030	-	-	-
IFRS = $IFRS_{GOODGOV}$	-	-	-	-	-	-	0.092	0.006	0.015
SIZE	-0.008***	0.007***	0.013***	-0.009***	0.0071	0.013*	-0.025***	0.034***	0.041***
	(-16.114)	(4.939)	(5.350)	(-16.089)	(1.639)	(1.859)	(-6.073)	(5.084)	(4.299)
ROA	-0.001***	-0.001**	-0.000	-0.001***	-0.0009	-0.000	-0.001***	-0.002***	-0.002***
	(-4.379)	(-2.438)	(-0.244)	(-4.380)	(-0.870)	(-0.068)	(-2.922)	(-7.190)	(-4.781)
GROWTH	0.000*	-0.000*	-0.000	0.000*	-0.0000*	-0.000	0.000	-0.000	-0.000
	(1.934)	(-1.822)	(-1.120)	(1.934)	(-1.955)	(-1.639)	(1.427)	(-1.266)	(-1.069)
LEV	-0.000	0.000	0.000	-0.000	0.0001	0.000	-0.000	0.000	0.000
	(-0.539)	(1.275)	(1.289)	(-0.544)	(1.311)	(1.481)	(-0.735)	(1.610)	(1.212)
DISSUE	0.000***	-0.001**	-0.000	0.000***	-0.0001***	-0.000	0.000***	-0.000	0.000
	(6.033)	(-2.466)	(-0.681)	(6.033)	(-4.894)	(-0.560)	(3.588)	(-0.366)	(0.070)
EISSUE	-0.000***	-0.000***	-0.000	-0.000***	-0.0000***	-0.000	-0.000***	-0.000***	-0.000***
	(-5.148)	(-15.653)	(-0.261)	(-5.117)	(-9.626)	(-0.172)	(-4.681)	(-2.875)	(-3.699)
OPER_CYCLE	0.000	0.000***	0.000***	0.000	0.0000***	0.000***	0.000	-0.000	-0.000
	(1.333)	(4.625)	(4.765)	(1.333)	(6.223)	(3.952)	(1.277)	(-1.453)	(-0.532)
TURN	0.006***	0.084***	0.119***	0.006***	0.0839***	0.119***	0.014***	0.002	-0.008
	(4.770)	(18.019)	(15.017)	(4.765)	(13.805)	(5.892)	(2.959)	(0.342)	(-0.713)
LOSS	0.019***	0.033***	0.025***	0.019***	0.0326***	0.025	0.016***	0.005*	0.000
	(8.859)	(8.152)	(3.781)	(8.863)	(3.101)	(1.265)	(8.507)	(1.709)	(0.069)
σ (SALES)	-0.000	0.000**	0.000*	-0.000	0.0000*	0.000	0.000	0.000*	0.000**
	(-1.619)	(2.573)	(1.707)	(-1.605)	(1.808)	(1.703)	(0.687)	(1.699)	(2.480)
σ(CFO)	0.000***	-0.001***	-0.000***	0.000***	-0.0001***	-0.000***	0.000***	-0.000***	-0.000***
	(7.295)	(-5.933)	(-5.590)	(7.289)	(-3.717)	(-4.942)	(4.850)	(-3.606)	(-2.877)
ΔGDP/CAP	-0.002	0.017***	0.075***	-0.002	0.0172	0.076	0.001	-0.013**	0.031***
	(-0.869)	(3.249)	(7.626)	(-0.907)	(1.242)	(1.564)	(0.373)	(-2.108)	(3.007)
COSTANT	0.106***	-0.442***	-0.368***	0.105***	-0.4313***	-0.356***	0.184***	-0.251***	-0.286***
	(7.235)	(-8.172)	(-6.873)	(7.056)	(-8.852)	(-4.331)	(6.813)	(-6.089)	(-4.928)
Country fe	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Industry fe	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
firm fe	No	No	No	No	No	No	Yes	Yes	Yes
Year fe	Global & EU	Global & EU	Global & EU	Global & EU	Global & EU	Global & EU	Global & EU	Global & EU	Global & EU
Observations	33,072	33,072	33,072	33,072	33,072	33,072	33,072	33,072	33,072
R-squared	0.229	0.111	0.090	0.229	0.111	0.090	0.559	0.780	0.847

See APPENDIX II.A for variable definitions. In parentheses are reported t-statics based on robust standard errors that are clustered at firm

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

Table 2.7, columns (1)-(3) present the estimation results using the strength of the legal enforcement as partitioning variable (HIGH_LAW). It appears that most of the IFRS effect

derives from firms subject to stricter legal enforcement, as none of the coefficients for $IFRS_{LOW\_LAW}$ are statistically significant at conventional levels (such coefficient would capture the effect for firms under weak legal enforcement). In contrast, the coefficient for $IFRS_{HIGH\_LAW}$ is negative for ABS_DA (-0.015, $p < 0.050$) and positive for both REAL_1 and REAL_2 (0.058, $p < 0.001$; 0.052, $p < 0.01$, respectively). Hence, there seems to be a trade-off between accrual management (less) and real earnings management (more) in firms under strong legal enforcement following the advent of IFRS. Finally, we examine the interplay between country-level enforcement and firm-level monitoring mechanisms around IFRS mandatory adoption also for accrual-based and real earnings management metrics. Table 2.7, columns (4)-(6) presents the results from the estimation of equation (12) using ABS_DA, REAL_1 and REAL_2 as dependent variables, We find a decrease in ABS_DA for strong monitoring firms in weak legal enforcement countries (-0.026, p-value<0.001). Keeping fixed the strength of the legal enforcement, i.e. comparing the changes in ABS_DA for firms forced to switch to IFRS in the same legal environment between strong versus weak monitoring firms, we still find a significant difference ($p < 0.100$). We also document a decrease in ABS_DA for mandated adopters with strong monitoring in strong legal enforcement countries, while the decrease is lower for weak monitoring firms in the same legal environment and the difference between the changes is not significant. For the real earnings management metrics, we find an increase in both REAL_1 and REAL_2 only for strong board-based monitoring firms in strong legal enforcement countries. To sum up, if firm-level corporate governance is a substitute for the legal system when it is weak, firm and country-level monitoring mechanisms turn to be complements as the latter gets stronger. Firm-level monitoring mechanisms seems to matter most when they are scarce, that is in weak legal enforcement countries as long as the improvement in financial reporting quality for strong monitoring firms relative to weak monitoring firms is larger in lax legal environment.

Table 2.7

OLS regressions on accrual and real earnings management conditional on firm-level board monitoring intensity and country-level legal enforcement

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	ABS_DA	REAL_1	REAL_2	ABS_DA	REAL_1	REAL_2	ABS_DA	REAL_1	REAL_2
$IFRS_{HIGH\_LAW}$	-0.014**	0.044***	0.039**	-	-	-	-	-	-
	(-2.019)	(5.587)	(2.382)						
$IFRS_{LOW\_LAW}$	-0.012	-0.000	-0.024	-	-	-	-	-	-
	(-1.545)	(-0.014)	(-1.259)						
$IFRS_{HIGH\_GOV\_HIGH\_LAW}$	-	-	-	-0.015**	0.058***	0.052***	-	-	-
				(-2.042)	(6.501)	(3.782)			
$IFRS_{LOW\_GOV\_HIGH\_LAW}$	-	-	-	-0.015*	0.013	0.009	-	-	-
				(-1.917)	(1.003)	(0.638)			
$IFRS_{HIGH\_GOV\_LOW\_LAW}$	-	-	-	-0.026***	-0.022	-0.032	-0.026***	-0.021	-0.033
				(-3.345)	(-1.248)	(-1.349)	(-3.323)	(-1.245)	(-1.421)
$IFRS_{LOW\_GOV\_LOW\_LAW}$	-	-	-	-0.019	0.010	-0.023	-0.019	0.011	-0.024
				(-1.320)	(0.660)	(-1.222)	(-1.292)	(0.710)	(-1.315)
$IFRS_{HIGH\_GOV\_HIGH\_LAW\_proactive}$	-	-	-	-	-	-	-0.015**	0.057***	0.054***
							(-2.105)	(6.451)	(3.825)
$IFRS_{LOW\_GOV\_HIGH\_LAW\_proactive}$	-	-	-	-	-	-	-0.011	0.014	0.009
							(-1.278)	(0.981)	(0.518)
$IFRS_{HIGH\_GOV\_LOW\_LAW\_non\_proactive}$	-	-	-	-	-	-	-0.029***	0.071***	0.015
							(-3.298)	(2.800)	(0.566)
$IFRS_{LOW\_GOV\_LOW\_LAW\_non\_proactive}$	-	-	-	-	-	-	-0.008	0.014	0.000
							(-0.965)	(0.311)	(0.008)
F-Test (p-values)									
$IFRS_{HIGH\_LAW} = IFRS_{LOW\_LAW}$	0.561	0.002	0.007	-	-	-	-	-	-
$IFRS_{HIGH\_GOV\_HIGH\_LAW} = IFRS_{LOW\_GOV\_HIGH\_LAW}$	-	-	-	0.906	0.000	0.003	-	-	-
$IFRS_{HIGH\_GOV\_LOW\_LAW} = IFRS_{LOW\_GOV\_LOW\_LAW}$	-	-	-	0.087	0.011	0.535	0.087	0.011	0.535
$IFRS_{HIGH\_GOV\_HIGH\_LAW\_proactive} = IFRS_{LOW\_GOV\_HIGH\_LAW\_proactive}$	-	-	-	-	-	-	0.315	0.000	0.001
$IFRS_{HIGH\_GOV\_HIGH\_LAW\_non\_proactive} = IFRS_{LOW\_GOV\_HIGH\_LAW\_non\_proactive}$	-	-	-	-	-	-	0.002	0.031	0.055
SIZE	-0.009***	0.007	0.013*	-0.010***	0.007	0.013*	-0.010***	0.007	0.0131*
	(-16.115)	(1.643)	(1.866)	(-17.756)	(1.638)	(1.859)	(-17.742)	(1.635)	(1.858)
ROA	-0.001***	-0.001	-0.002	-0.000***	-0.001	-0.000	-0.001***	-0.001	-0.0001
	(-4.376)	(-0.879)	(-0.075)	(-3.869)	(-0.874)	(-0.071)	(-3.873)	(-0.875)	(-0.071)
GROWTH	0.000*	-0.000*	-0.000	0.000**	-0.000*	-0.000	0.000**	-0.000*	-0.0000
	(1.934)	(-1.959)	(-1.647)	(1.986)	(-1.957)	(-1.642)	(1.986)	(-1.957)	(-1.642)
LEV	-0.000	0.000	0.000	-0.000	0.000	0.000	-0.000	0.000	0.0001
	(-0.540)	(1.265)	(1.432)	(-0.697)	(1.174)	(1.403)	(-0.702)	(1.173)	(1.405)
DISSUE	0.000***	-0.000***	-0.000	0.000***	-0.000***	-0.000	0.000***	-0.000***	-0.0000
	(6.033)	(-4.910)	(-0.563)	(6.555)	(-4.900)	(-0.563)	(6.553)	(-4.905)	(-0.562)
EISSUE	-0.000***	-0.000***	-0.000	-0.000***	-0.000***	-0.000	-0.000***	-0.000***	-0.0000
	(-5.145)	(-9.592)	(-0.162)	(-4.790)	(-9.596)	(-0.183)	(-4.800)	(-9.591)	(-0.189)
OPER_CYCLE	0.000	0.000***	0.000***	0.000*	0.000***	0.000***	0.000*	0.000***	0.0000***
	(1.333)	(6.216)	(3.954)	(1.881)	(6.215)	(3.955)	(1.880)	(6.216)	(3.958)
TURN	0.006***	0.084***	0.119***	0.003**	0.084***	0.119***	0.003**	0.084***	0.1187***
	(4.769)	(13.810)	(5.886)	(2.346)	(13.767)	(5.880)	(2.322)	(13.754)	(5.885)
LOSS	0.019***	0.033***	0.024	0.019***	0.033***	0.025	0.019***	0.033***	0.0248
	(8.858)	(3.100)	(1.266)	(8.645)	(3.101)	(1.265)	(8.646)	(3.100)	(1.266)
σ (SALES)	-0.000	0.000*	0.000*	0.000	0.000*	0.000*	0.000	0.000*	0.0000*
	(-1.623)	(1.816)	(1.737)	(0.172)	(1.817)	(1.725)	(0.181)	(1.821)	(1.715)
σ(CFO)	0.000***	-0.000***	-0.001***	0.000***	-0.000***	-0.000***	0.000***	-0.000***	-0.0001***
	(7.296)	(-3.703)	(-4.918)	(6.006)	(-3.705)	(-4.927)	(6.006)	(-3.700)	(-4.910)
ΔGDP/CAP	-0.002	0.018	0.076	0.004**	0.019	0.078	0.004**	0.019	0.0781
	(-0.890)	(1.271)	(1.572)	(2.249)	(1.388)	(1.589)	(2.382)	(1.372)	(1.592)
COSTANT	0.106***	-0.449***	-0.379***	0.132***	-0.428***	-0.361***	0.131***	-0.428***	-0.3535***
	(7.269)	(-9.537)	(-4.718)	(9.438)	(-8.951)	(-4.413)	(9.407)	(-7.008)	(-3.767)
Country fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	Global & EU	Global & EU	Global & EU	Global & EU	Global & EU	Global & EU	Global & EU	Global & EU	Global & EU
Observations	33,072	33,072	33,072	33,072	33,072	33,072	33,072	33,072	33,072
R-squared	0.229	0.111	0.090	0.202	0.112	0.090	0.202	0.112	0.090

See APPENDIX II.A for variable definitions. In parentheses are reported t-statics based on robust standard errors that are clustered at firm level.

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

2.5. Additional analysis

Monitoring versus reporting incentives hypothesis

This paper exploits cross-sectional variation in the board-based monitoring intensity to examine heterogeneity in IFRS mandatory adoption consequences on financial reporting quality. We rely on agency theory to partition the sample according to board monitoring intensity, under the assumption that board structured to be effective watchdogs of insiders' behaviors are more likely to provide financial information of higher quality to reduce agency costs. We examine neither why firms should adopt strong monitoring mechanisms, nor whether the decision to adopt such mechanisms is the observable outcome of firm reporting incentives. Indeed, a firm decision to bond itself to the scrutiny of strong board-based monitoring mechanisms may follow from a particular set of reporting incentives. More profitable firms with greater needs for external financing and higher growth opportunities should have strong incentives to provide more reliable financial information to providers of finance. A firm may try to adopt bonding mechanisms, for example corporate governance ones, to credibly commit not to expropriate investors, especially if the comes from a lax legal system. If this argument holds, we should observe an improvement in financial reporting quality after IFRS mandatory adoption for firms with strong reporting incentives, despite the presence of strong board-based monitoring mechanisms. In this case, corporate governance is just an observable outcome, with no additional explanatory power. To explore this issue, we replicate our main analyses by augmenting the models with a reporting incentives partitioning variable. Specifically, considering only the IFRS treatment sample, we apply a factor analysis to the following variables: SIZE, LEV, ROA, GROWTH. The first factor (out of two which are retained) exhibits the expected loadings and we use it as our "reporting incentives" variable. Following Daske et al. (forthcoming), we calculate the reporting incentives

variable as a rolling average over the previous three years (i.e., years *t*, *t*-1, *t*-2). Next, we subtract for each firm the rolling average in year *t*-1 from the rolling average in year *t*+3 relative to the year *t* of IFRS adoption. Finally, we create a binary variable (REP_INCENTIVES) based on the sample distributions of the changes around IFRS adoption. Firms with above sample median value of "reporting incentives" are classified as strong reporting incentives firms (REP_INCENTIVES equals to one). Table 2.8, panel A presents the estimation results. For sake of brevity we report only the coefficients on the variable of interests even if the estimation results stem from the full model with the set of control variables. We find that only strong board-based monitoring firms exhibit an increase in AVAR and AVOL, and a decline in ABS_DA, irrespective of the reporting incentives although the improvement in all three measures is stronger for those firms that hold also strong reporting incentives.

Most importantly, we do not find a decline in ABS_DA and an increase in AVAR and AVOL for firms that have strong reporting incentives but do not adopt strong board-based monitoring mechanisms. Together, these results suggest that our proxy for board monitoring intensity it is not just the observable outcome of a given set of reporting incentives. Rather, it has an additional and different role in explaining financial reporting changes around IFRS mandatory adoption. Interestingly, when we examine real earnings management, we document an increase in REAL_1 and REAL_2 only for strong board-based monitoring firms with low reporting incentives.

Table 2.8

Monitoring versus reporting incentives

	(1) AVAR	(2) AVOL	(3) ABS_DA	(4) REAL_1	(5) REAL_2
$IFRS_{HIGH\_GOV\_HIGH\_HIGH\_REP\_INC}$	0.185***	0.295***	-0.016**	0.030	0.026
	(4.400)	(3.052)	(-2.291)	(0.893)	(0.836)
$IFRS_{LOW\_GOV\_HIGH\_HIGH\_REP\_INC}$	0.023	0.087	-0.009	0.023	0.023
	(0.614)	(0.933)	(-1.255)	(0.582)	(0.632)
$IFRS_{HIGH\_GOV\_LOW\_LOW\_REP\_INC}$	0.179**	0.269***	-0.015**	0.063*	0.052*
	(2.732)	(2.981)	(-2.094)	(1.750)	(1.665)
$IFRS_{LOW\_GOV\_LOW\_LOW\_REP\_INC}$	0.098*	0.152	-0.013*	0.073**	0.074**
	(1.934)	(1.607)	(-1.853)	(1.976)	(2.157)
F-Test (p-values)					
$IFRS_{HIGH\_GOV\_HIGH\_REP\_INC} = IFRS_{LOW\_GOV\_HIGH\_REP\_INC}$	0.061	0.001	0.811	0.727	0.885
$IFRS_{HIGH\_GOV\_LOW\_REP\_INC} = IFRS_{LOW\_GOV\_LOW\_REP\_INC}$	0.526	0.031	0.371	0.613	0.296
Constant	0.328	1.599***	0.124***	-0.129***	-0.202***
	(0.868)	(10.366)	(9.992)	(-2.660)	(-4.183)
Country fe	Yes	Yes	Yes	Yes	Yes
Industry fe	Yes	Yes	Yes	Yes	Yes
Year fe	Global & EU	Global & EU	Global & EU	Global & EU	Global & EU
Observations	33,072	33,072	33,072	33,072	33,072
R-squared	0.021	0.037	0.206	0.092	0.069

See APPENDIX II.A for variable definitions. In parentheses are reported t-statics based on robust standard errors that are clustered at firm level.

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

2.6. Robustness checks

In this section, we present the results of a battery of robustness tests. All the results of the robustness checks are untabulated but are available from the authors upon request.

1. To the extent that 2005 is the year of the mandatory switch to IFRS, we replicate all the analyses after excluding the transition year. Results are unchanged.
2. United Kingdom firms represent almost 34 percent of the sample. We replicate all the analyses after excluding United Kingdom. Results are consistent with those reported.
3. We test whether the results are robust to the use of alternative measures of accrual-based and real earnings management. We measure the abnormal working capital accruals as in DeFond and Park (2001) and inferences are unchanged. In addition, we use the abnormal

accruals adjusted for firm performances (Kothary et al. 2005), results are qualitatively unchanged. We also use alternative proxies for real earnings management following Cohen et al. (2012) who adjust real earnings management models by controlling for firms' performances and the results are consistent with those reported in the paper.

4. We use the earnings response coefficient as an alternative proxy for the earnings informativeness. Inferences are unchanged.
5. We replace the industry-country fixed effects structure with firm-fixed effects to better absorb time-invariant unobservable heterogeneity across firms. Results are unchanged.
6. The empirical results of this paper rely on the ability of the board monitoring proxy (i.e. GOOD_GOV) to partition the sample into strong board-based monitoring firms and weak board-based monitoring firms. If the approach used to classify observations fails to properly capture the strength of firm-level monitoring mechanisms, reported findings may be misleading. As a result, we replicate all the analyses employing alternative identification schemes:
    i. the extent firm level governance may be affected by a given country corporate law and securities legislation. Hence, we first split the sample into four groups according to a country legal origin (i.e. French, Anglo-Saxon, German, Scandinavian). Then, for each of these groups, we take firms in the upper 20 percent of the firm-specific mean of GOVSCORE and set them as strong board-based monitoring firms. Results are unchanged. We replicate all the analyses using GOVSCORE instead of GOOD_GOV and the results are consistent with those reported;
    ii. firm-level corporate governance may change over time, but it evolves slowly. Therefore, we do not focus on board monitoring *changes* but, rather, on board

monitoring *levels*, under the assumption that cross-sectional differences in terms of board monitoring remain constant over time while firm-specific board monitoring intensity may vary. Indeed, less than 6 percent of firms in the sample went from being classified as weak (strong) board-based monitoring firm in the pre mandatory adoption period (i.e. 2004) to being classified as strong (weak) board-based monitoring firms in the post mandatory adoption period (i.e. 2006). However, we replicate all the analyses after taking into account the fact that some firms may be misclassified between the pre and post IFRS adoption. Specifically, we do two separate factor analysis for the year 2004 and 2006 with the same variables used to define GOVSCORE. Then, we re-define GOOD_GOV equals to one if a firm is above the sample median in both 2004 and 2006, zero if it is below the median in both the years. In the other cases, firms are excluded from the sample. Results are consistent with those reported in the paper.

2.7. Conclusions

This paper revisits the joint effect of country-level legal enforcement and firm-level governance on the quality of financial reporting. Using a large sample of firms adopting IFRS, we find that firms evolving in weak legal enforcement countries enhance their earnings quality by building up their board-level governance monitoring. This suggests that in weak enforcement countries, firm-level board monitoring and country-level legal enforcement are substitute governance mechanisms. In contrast, in countries with strong legal enforcement, firms with weak board monitoring may still see an improvement in earnings quality but it is smaller than for firms with strong board monitoring. This finding suggests that in strong enforcement countries, firm-

level board monitoring is actually a complementary governance mechanism to country-level institutions. Overall, our findings suggest that IFRS adoption by itself does not much affect earnings quality and that any such effect is conditional upon firm- and country-level governance.

Appendix II.A: Variable definitions

Variable	Definition
GOOD_GOV	Binary variable equals to one for firm in the IFRS-treatment sample if the firm-specific mean of the factor score of the governance attributes is above the sample median, zero otherwise.
HIGH_LAW	Binary variable equals to one if the "Rule of Law" variable for the year 2005 (Kaufmann et al., 2007) is above the sample median of the treatment sample, zero otherwise.
PROACTIVE	Binary variable equals to one if a treatment sample country that introduce the proactive review process of financial statements around 2005, zero otherwise.
IFRS	Binary variable equals to one for firms that apply IFRS only when it becomes mandatory in 2005 for fiscal-years beginning on or after 01/01/2005, and zero otherwise.
$IFRS_{HIGH\_GOV}$	Binary variable equals to one for firms with IFRS reporting with strong corporate governance (i.e. GOOD_GOV equals to one) only when it becomes mandatory in 2005 for fiscal-years beginning on or after 01/01/2005, and 0 otherwise.
$IFRS_{LOW\_GOV}$	Binary variable equals to one for firms with IFRS reporting with weak corporate governance (i.e. GOOD_GOV equals to zero) only when it becomes mandatory in 2005 for fiscal-years beginning on or after 01/01/2005, and 0 otherwise.
$IFRS_{HIGH\_LAW\_HIGH\_GOV}$	Binary variable equals to one for firms with IFRS reporting with strong corporate governance (i.e. GOOD_GOV equals to one) in countries with strong legal enforcement (i.e. HIGH_LAW equals to one) only when it becomes mandatory in 2005 for fiscal-years beginning on or after 01/01/2005, and 0 otherwise.
$IFRS_{HIGH\_LAW\_LOW\_GOV}$	Binary variable equals to one for firms with IFRS reporting with weak corporate governance (i.e. GOOD_GOV equals to zero) in countries with strong legal enforcement (i.e. HIGH_LAW equals to one) only when it becomes mandatory in 2005 for fiscal-years beginning on or after 01/01/2005, and 0 otherwise.
$IFRS_{LOW\_LAW\_HIGH\_GOV}$	Binary variable equals to one for firms with IFRS reporting with strong corporate governance (i.e. GOOD_GOV equals to one) in countries with weak legal enforcement (i.e. HIGH_LAW equals to zero) only when it becomes mandatory in 2005 for fiscal-years beginning on or after 01/01/2005, and 0 otherwise.
$IFRS_{LOW\_LAW\_LOW\_GOV}$	Binary variable equals to one for firms with IFRS reporting with weak corporate governance (i.e. GOOD_GOV equals to zero) in countries with weak legal enforcement (i.e. HIGH_LAW equals to zero), only when it becomes mandatory in 2005 for fiscal-years beginning on or after 01/01/2005, and 0 otherwise.

$IFRS_{HIGH\_LAW}$	Binary variable equals to one for firms with IFRS in countries with strong legal enforcement (i.e. HIGH_LAW equals to one) only when it becomes mandatory in 2005 for fiscal-years beginning on or after 01/01/2005, and 0 otherwise.
$IFRS_{LOW\_LAW}$	Binary variable equals to one for firms with IFRS reporting in countries with weak legal enforcement (i.e. HIGH_LAW equals to zero) only when it becomes mandatory in 2005 for fiscal-years beginning on or after 01/01/2005, and 0 otherwise.
$IFRS_{HIGH\_LAW\_HIGH\_GOV\_proactive}$	Binary variable equals to one for firms with IFRS reporting with strong corporate governance (i.e. GOOD_GOV equals to one) in countries with strong legal enforcement (i.e. HIGH_LAW equals to one) and introduce the proactive review process of financial statements around 2005 (i.e. PROACTIVE equals to one) only when it becomes mandatory in 2005 for fiscal-years beginning on or after 01/01/2005, and 0 otherwise.
$IFRS_{HIGH\_LAW\_LOW\_GOV\_proactive}$	Binary variable equals to one for firms with IFRS reporting with weak corporate governance (i.e. GOOD_GOV equals to zero) in countries with strong legal enforcement (i.e. HIGH_LAW equals to one) and introduce the proactive review process of financial statements around 2005 (i.e. PROACTIVE equals to one) only when it becomes mandatory in 2005 for fiscal-years beginning on or after 01/01/2005, and 0 otherwise.
$IFRS_{LOW\_LAW\_HIGH\_GOV\_non\_proactive}$	Binary variable equals to one for firms with IFRS reporting with strong corporate governance (i.e. GOOD_GOV equals to one) in countries with strong legal enforcement (i.e. HIGH_LAW equals to one) and do not introduce the proactive review process of financial statements around 2005 (i.e. PROACTIVE equals to zero) only when it becomes mandatory in 2005 for fiscal-years beginning on or after 01/01/2005, and 0 otherwise.
$IFRS_{LOW\_LAW\_HIGH\_GO\_non\_proactive}$	Binary variable equals to one for firms with IFRS reporting with weak corporate governance (i.e. GOOD_GOV equals to zero) in countries with strong legal enforcement (i.e. HIGH_LAW equals to one) and does not introduce the proactive review process of financial statements around 2005 (i.e. PROACTIVE equals to zero) only when it becomes mandatory in 2005 for fiscal-years beginning on or after 01/01/2005, and 0 otherwise.
AVAR	Abnormal return variability computed as in Landsman et al. (2012).
AVOL	Abnormal trading volume computed as in Landsman et al. (2012).
ABS_DA	Absolute value of discretionary accruals computed as in Dechow et al. (1995).

REAL_1	Sum of the abnormal level of production and the abnormal level of discretionary expenses (time minus one), both computed as in Roychowdhury (2006).
REAL_2	Sum of Abnormal level of discretionary expenses (time minus one) and the abnormal level of cash flow (time minus one), both computed as in Roychowdhury (2006).
SIZE	Natural logarithm of total assets at the beginning of the year.
LEV	End of the year total liabilities divided by end of the year equity book value.
LOSS	Dummy variable equals to one if the actual earnings per share is less than zero, zero otherwise.
AFE	Earnings surprise, defined as the difference between the actual earnings per share and the analyst consensus earnings forecast before the earnings announcement, scaled the closing price at the fiscal year end.
DISPERSION	Standard deviation of analyst earnings forecasts prior to the earnings announcement, scaled by the closing price as the end of the year.
REP_LAG	Logarithm of the number of days between the firm's fiscal year end to the earnings announcement.
FOLLOWING	The logarithm of the number of analyst that follow a fir during the year of the earnings announcement.
ROA	Net income before extraordinary items divided by the end of the year total assets.
GROWTH	Percentage change in sales.
DISSUE	Percentage change in total liabilities divided by end of the year equity book value.
TURN	Sales divided by end of year total assets.
σ(CFO)	Standard deviation of the operating cash flow, measured over the previous 5 year.
σ (SALES)	Standard deviation of the sales, measured over the previous 5 year.
OPER_CYCLE	The operating cycle in days.
ΔCAP/GDP	Logarithm of the annual change in the ratio of stock market capitalization and gross domestic product per capita.

Appendix II.B: Governance attributes definitions

Variable	Definition
BOARD SIZE	Number of board members.
INDEPENDENT	Number of independent directors over number of board members.
OUTSIDERS	Number of outsiders directors over number of board members.
INSIDERS	Number of insiders directors over number of board members.
FINANCIAL EXPERT	Number of financial expert independent directors over number of board members.
ACCOUNTING	Number of accounting expert independent directors over number of board members.
AUDIT	Dummy equals to 1 if the board has set up an audit committee, 0 otherwise.
AUDIT_SIZE	Number of board members serving on the audit committee.
INDEPENDENT_AUDIT	Number of independent directors serving on the audit committee over number of board members serving on the audit committee.
OUTSIDERS_AUDIT	Number of outsiders directors serving on the audit committee over number of board members serving on the audit committee.
INSIDERS_AUDIT	Number of insiders directors serving on the audit committee over number of board members serving on the audit committee.
FINANCIAL EXPERT_AUDIT	Number of independent directors financial expert serving on the audit committee over number of board members serving on the audit committee.
ACCOUNTING_AUDIT	Number of independent directors accounting expert serving on the audit committee over number of board members serving on the audit committee.

Chapter 3

Cross-listing and Firm Information Environment: Does SOX Section 302 Have any Material Effect?

3.1 Introduction

Previous literature suggests that by cross-listing in the U.S. firms are bonding themselves to more extensive disclosure requirements, SEC scrutiny and a tighter threat of litigation that jointly foster corporate transparency and the quality of the firm information environment. This paper examines whether cross-listing benefits on the firm information environment vanish if the financial reporting process suffers by internal control deficiencies according to the Section 302 of the Sarbanes-Oxley Act (SOX302, hereafter). By using the properties of analyst forecasts as a proxy for the firm information environment (Lang et al. 2003a; Arping and Saunter forthcoming), we show that cross-listed firms disclosing internal control deficiencies do not have a better information environment and do not differentiate themselves from their home-country peers, but only after the first disclosure on internal control deficiencies according to SOX302. Second, we document that cross-listed firms experience an improvement in the information environment if they remediate to previously disclosed internal control deficiencies. Finally, we show that these results hold only for firms

domiciled in countries with weak legal institutions, while cross-listed firms from countries with strong legal institutions do not experience a significant change in the quality of the information environment once they became cross-listed, irrespective from the disclosure of an internal control deficiency. Our results are robust to adjustments to potential endogeneity of cross-listing decision and to unobservable factors related to the disclosure of internal control deficiencies. Overall, our findings support the hypothesis that the quality of the firm information environment increases following cross-listing only if cross-listed firms *effectively* commit themselves to higher levels of corporate transparency, and not merely in name just mimicking the adoption of stricter rules (Siegel 2005).

Previous literature shows an enhancement of information environment following cross-listing in the U.S. (Lang et al. 2003a), suggesting that by cross-listing a firm credibly commits to achieve a higher level of corporate transparency. Cross-listing is associated with an improvement in firm corporate governance because it bonds the firm to a greater transparency, which should reduce the potential diversion of firm cash flow to managers and controlling shareholders (Coffee, 1999). Lombardo and Pagano (2002) argue that cross-listing adds value because the greater transparency increases the willingness of both international and local investors to commit capital and Lang et al. (2003a) show that firms cross-listed in U.S. markets are bonding themselves to an increased level of disclosure and scrutiny. However, a growing literature suggests that cross-listed firms do not behave in the same fashion once they became cross-listed, and hence do not get the same pay-off from cross-listing to the extent that reporting incentives of cross-listed firms are still shaped by institutional characteristics of their home countries (Licht et al. 2003; Siegel 2005; Leuz 2006).

Since 2002, firms listed in U.S. markets are subjected to SOX, which strengthens the credibility of listings in the U.S. as a bonding mechanism (Piotroski and Srinisavan 2008).

According to SOX302 firms have to disclose any discovered deficiencies in internal control systems over financial reporting. Through this disclosure, firms reveal the quality of their financial information, allowing capital markets to directly infer the reliability of financial reporting. The disclosure of internal control deficiencies is a signal that the financial reporting process is scanty, making financial information of lower quality (Kim et al. 2009). Literature suggests that SOX302 is useful for investors to better evaluate cost of capital and earnings quality (Beneish et al. 2006; Ashbaugh-Skaife et al. 2008; Ashbaugh-Skaife et al. 2009; Doyle et al. 2007a).

Using a sample of 913 cross-listed firm-year observations, this paper exploits internal control deficiency disclosures under SOX302 to explore the existence of heterogeneity in the information environment benefits stemming from cross-listing. The most compelling challenge of our research design is that the adoption of SOX302 occurs at the same time for all firms listed in U.S. stock markets. To ascertain that general trends or concurrent factors unrelated to SOX302 disclosures affect the firm information environment of cross-listed firms, we employ as benchmark group all firms listed in their home market but not in the U.S. In addition, we employ propensity-score matching models (Francis et al. 2010; Lawrence et al. 2011) to take into account differences in firm-characteristics between cross-listed and non-cross-listed firms while estimating SOX302 disclosure treatment effect. Our analyses encompasses both changes and cross-sectional association tests.

In the first step of the empirical analysis we examine the effect of the first disclosure of internal control deficiency according to SOX302 using a difference-in-difference design. We classify cross-listed firms with respect to the content of the first disclosure on internal control deficiencies and then we compare the change in firm information environment metrics for cross-listed firms that will not disclose internal control deficiencies and for cross-listed firms

that will disclose internal control deficiencies with the change for the control sample of non cross-listed firms around the time of the first SOX302 disclosure.

Next, we exploit data from 2002 to 2006, to examine the average effect of the disclosure of internal control deficiency according to SOX302 on the information environment of cross-listed firms relative to the full control sample of non cross-listed firms and to the propensity score matched sample.

In the third step of the empirical analysis, we examine the change in the information environment of cross-listed firms after the disclosure of a remediation of an internal control deficiencies according to SOX302, using as benchmark the sample of non cross-listed firms and cross-listed firms that never disclose intern al control deficiencies. Finally, we examine the effect of the disclosure of internal control deficiency according to SOX302 on the information environment of cross-listed firms conditional on the legal and enforcement characteristics of their home countries.

Our study contributes to the literature on cross-listing. Extant research outlines that firms that cross-list in the U.S. experience several benefits in terms of cost of capital (Hail and Leuz 2008, 2009) share price informativeness (Fernandes and Ferreira 2008), higher valuation (Doidge et al. 2004), and information environment (Lang et al. 2003a). In this paper, we analyse the information environment effects stemming from cross-listing in relation with the ability to properly adopt more stringent laws. We directly test the bonding hypothesis used to explain cross-listing benefits and find evidence that the benefits in terms of firm information environment are not homogeneous across all cross-listed firms. The magnitude of these benefits depends on the adoption of adequate internal controls over financial reporting: firms that only mimic the adoption of stricter rules lose information benefits, being not different from their home-country peers.

Our paper contributes also to the literature on the effects of SOX. Cohen et al. (2008) show an increase of earnings quality after SOX and Iliev (2010) find evidence supporting less aggressive earnings practices. Begley et al. (2009) show a temporary increase of the accuracy of analyst forecast once SOX came into force. Kim et al. (2009) investigate the effect of SOX Section 404 disclosures for U.S. firms. They find that firms disclosing internal control deficiencies have a poor analysts information environment, consistently with the notion that effective control systems enhance the quality of analyst forecast. A concurrent paper by Arping and Saunter (forthcoming) studies the impact of SOX on cross-listed firm's reporting transparency. They adopt a research design similar to that used in this paper and find that, over time, cross-listed firms experience a decrease in the level of opaqueness larger than for not cross-listed firms. This implies that, relative to control firms, cross-listed firms became more transparent. However, they do not exploit the information on internal control deficiencies to examine heterogeneity in the information environment effects stemming from cross-listing. We add to this literature the evidence that the decline in the level of opaqueness depends on financial reporting quality and hence it is not homogenous across all firms.

3.2 Related literature and predictions

Cross-listing

Extant research shows that cross-listing in the U.S. fosters capital market scrutiny, increases the availability of information of higher quality and, consequently, enhances the firm information environment. Baker et al. (2002) find that around the time of cross-listing firms have more visibility, as measured by analyst and media coverage. Lang et al. (2003a) document an increase in analyst forecast accuracy and in analyst coverage following cross-

listing. Lang et al. (2003b) show that earnings quality is higher for cross-listed than for not cross-listed firms. Bayley et al. (2006) explain the greater volatility and trading activity around earnings announcements following cross-listing with a substantial change in the firm information environment. Fernandes and Ferreira (2008) show that share prices of cross-listed firms incorporate firm-specific information in a more accurately and timely manner than not cross-listed peers, while Goto et al. (2009) find that the time-series properties of share returns change when a firm cross-list and experiences a large change in disclosure. Hope et al. (2012) find that voluntary disclosures of cross-listed firms are positively associated with analyst forecast accuracy. This suggests that cross-listing makes voluntary disclosure a viable mechanism for improving the firm information environment. All these findings support the idea that, by cross-listing, foreign firms increase corporate transparency and experience an enhancement in the information environment.

SOX disclosure

Since 29 August 2002, all SEC filers, and foreign firms that trade by way of ADR levels II-III, have to comply with SOX302. SOX302 requires management (i) to evaluate the effectiveness of firm internal controls over financial reporting, (ii) to certify the accuracy of the outcomes of the financial reporting process, (iii) to disclose any discovered internal control deficiency[22] in the internal controls (SEC, 2002)[23]. Through SOX302 disclosures, financial market can directly infer the reliability of financial reporting on a regular basis (Beneish et al. 2006). Research shows that the presence of internal control deficiencies is associated with

[22] Internal control deficiencies are categorized into three groups according to the degree of severity: "material weakness", "significant deficiency", or "deficiency"

[23] SOX Section 404 (SOX404) is related to SOX302 is. It requires that the management should certify the effectiveness of the internal control systems in the annual SEC filings (302), and that the external auditor confirms the management assessment of internal control effectiveness (404). For foreign firms accelerated filers (cross-listed on Level-II and Level-III ADRs) SOX404 became effective for fiscal years ending on or after July 15, 2006.

lower earnings quality (Doyle et al. 2007b; Ashbaugh-Skaife et al. 2008) and a higher cost of capital (Ashbaugh-Skaife et al. 2009). Kim et al. (2009) find that the quality of internal controls is positively associated with forecast accuracy and analyst following, while Begley et al. (2007) show a temporary increase in the precision of the public information after the adoption of SOX. These findings suggest that SOX302 allows investors to discriminate across firms with respect to the reliability of financial information while, before its adoption, investors could rely only on private information or indirect measures as abnormal accruals (Doyle et al. 2007a). To sum up, extent research provides evidence that SOX302 disclosures help to directly assess the quality of financial reporting and affects a firm information environment.

SOX disclosure and cross-listing

Research on SOX disclosures for cross-listed firms is still germinal. These studies examine whether SOX adoption is beneficial for cross-listed firms with respect to their home country peers and U.S. listed firms. Gong et al. (2011) show that SOX302 disclosures provided by cross-listed firms have less power in predict earnings quality than disclosures provided by U.S. firms. This result implies that SOX302 is less useful for cross-listed than for U.S. firms to separate high quality earnings firms from low quality firms. The same authors (Gong et al. forthcoming) also argue that cross-listed firms are less likely to report an internal control deficiency than U.S. firms. Arping and Sautner (forthcoming) document that cross-listed firms became less opaque than their home country firms once SOX404 came into force. Berger et al. (2011) document an incremental legal bonding benefit following the adoption of SOX for cross-listed firms.

Cross-listing effects on firm information environment are explained with the bonding theory (Coffee 1999, 2002; Stultz 1999): cross-listing in the U.S. provides an effective means for firms domiciled in weak investor protection countries to credibly commit to increase corporate transparency as they voluntary subject themselves to U.S. security law and SEC enforcement. The stronger capital market and enforcement scrutiny triggers an increase in the availability of information of higher quality and enhances firm information environment. According to the bonding theory, these benefits follow as a mechanic legal consequence that a firm experiences just for renting the U.S. legislation. In this vein, studies about SOX effects on cross-listed firms are based on the underlying premise that the consequences of SOX disclosures are homogenously distributed across all cross-listed firms. These studies do not take into account that cross-listed firms might be characterized by effective or ineffective internal controls, and hence similar levels of corporate transparency. SOX302 disclosures make information on the adequacy of internal control system common knowledge, allowing investors to discriminate across firms with respect to the reliability of the financial reporting. As a result, extant research misses to examine whether cross-listed firms have achieved the same level of transparency and hence the same quality in firm information environment once the became cross-listed.

Indeed, it is unlikely that all cross-listed firms behave in the same fashion once became cross-listed and hence get the same pay-off from it as long as the legal framework is only one factor that shape firms' behaviors (Holthausen 2009). Leuz (2006) provides preliminary evidence that cross-listed firms with different ownership concentration differ in term of financial reporting quality. Siegel (2005) documents variation in the likelihood of extracting private benefits within the population of Mexican cross-listed firms. This literature suggest that the outcomes of firms' financial reporting process is shaped by several factors like

managers' incentives, auditor quality, regulation, market pressure and legal enforcement. As a result, there is predictable heterogeneity even in the behaviors of cross-listed firms and hence in the information environment benefits stemming for cross-listing. Research does not explore this issue even if the adoption of SOX302 (and SOX404) provides an ideal setting as it allows investors to discriminate cross-listed firms between those that effectively commit themselves to higher level of transparency and those that just mimic the adoption of stricter rules.

H1: Cross-listed firms lose the information environment benefits stemming from cross-listing when they disclose internal control deficiencies under SOX302.

Previous studies on SOX302 investigate whether the successful remediation of internal control deficiencies has positive effects in terms of earnings quality, cost of capital and firm information environment. Beneish et al. (2008) show that capital market does not react to the remediation of a previously disclosed internal control deficiency of U.S. firms, even if earnings quality increases (Ashbaugh-Skaife et al. 2008) and the cost of equity decrease after a successful remediation (Ashbaugh-Skaife et al. 2009). Kim et al. (2009) find that a successful remediation strategy bears to an increase in analyst following and to a decrease in both forecast error and dispersion. Considering these results, we examine whether the remediation of previously disclosed internal control deficiencies allows cross-listed firms to plug the transparency and credibility gap with other cross-listed and to separate themselves from their home country firms by gaining cross-listing benefits.

H2: Cross-listed firms that remediate to a previously disclosed internal control deficiency claw back information environment benefits stemming from cross-listing.

Previous literature suggests that cross-listing benefits follow from a change in the regulatory and enforcement environment that each firm is willing to experience to signal its commitment to transparency. Firms from countries with a weak disclosure regulation and a feeble capital market scrutiny have more to get from cross-listing as they experience a larger increase in market scrutiny and legal enforcement than firms from countries where the latter are already high. Several empirical findings corroborate this intuition: Hail and Leuz (2009) finds that cross-listed firms from weak legal enforcement countries experience a larger decline in the cost of equity capital than cross-listed firms from strong legal enforcement countries. In this vein, the analysis of the effects of SOX302 on information environment of cross-listed firms should take into account the characteristics of the country in which the cross-listed firms is domiciled. On the one side, firms from countries with a strong disclosure regulation and capital market scrutiny exhibit negligible cross-listing benefits, but have less to lose whether an internal control deficiency is disclosed. On the other side, cross-listed firms from countries with a weak disclosure regulation and enforcement get the higher pay-off from cross-listed, as they experience a larger regulatory change. At the same time, they are likely to lose more whether they disclose an internal control deficiency.

> *H3: The difference in the information environment benefits between cross-listed disclosing and not disclosing internal control deficiencies under SOX302 is greater when the firm is domiciled in a weak legal environment country.*

3.3. Data and research design

Sample selection

Our analysis focuses on firms cross-listed in the three major U.S. stock exchanges (NYSE, AMEX, or NASDAQ) at some point in time over the period August 2002 – July 2006. As long as cross-listed firms that trade by way of OTC listings (Level-I ADRs) and Rule 144a private placement offerings (Level-IV ADRs) have not to comply with SOX provisions, we focus on Level-II and Level III ADRs.

Our sample selection procedure is as follows. We first identify from the Compustat Global database all cross-listed firms on Level-II and Level-III ADRs, but Canadian-based firms[24], by relying on Compustat incorporation code, FIC, and cross-check with other data sources such as SEC filings and Audit Analytics. This procedure yields 2,292 cross-listed firm-year observations, from 702 unique firms. Next, we merge this sample of cross-listed firms from the Compustat Global database with the I/B/E/S International database (split unadjusted)[25] necessary to calculate the properties of the firm information environment. We these restrictions we have a sample of 913 cross-listed firms-year observations that represent 379 unique firms, from 48 countries, subjected to SOX302 between August 2002 and July 2006. We obtain data on SOX302 disclosures from the Audit Analytics' Disclosure Controls database. It encompasses all SEC registrants who have to disclose since August 2002 management certification of internal controls in periodic SEC filings.

Our control sample includes all the listed firms from the 48 countries with at least a

[24] Following prior research, Canadian firms are excluded because they can directly list their shares on U.S. exchanges without using depository receipts. Moreover, Canadian firms are exempted from certain U.S. reporting requirements under the Multi-Jurisdictional Disclosure System (Hail and Leuz 2009). However, inference is unchanged if we keep Canadian cross-listed firms in the sample.

[25] We use split unadjusted data from the I/B/E/S international database for both cross-listed and non-cross-listed firms to avoid rounding problem with the earnings per share data (Payne and Thomas 2003). All firm-level data are converted in U.S. dollars for ease of analysis.

cross-listed firms in our final cross-listed sample, covered by I/B/E/S from August 2002 to July 2006, which are not cross-listed in the U.S. under the four different cross-listing alternative (Level-II and Level-III ADRs, or by way of OTC listings and Rule 144a private placement offerings). After the merge with Compustat Global to compute the variables used in the regression analysis, we come up with a control sample of 9,909 firm-year observations of non-cross-listed firms. Overall, our main analyses are carried out using a sample of 10,822 firm-year observations. Table 3.1 presents the sample selection procedure.

Table 3.1
Sample selection criteria

Cross-listed firms	
Cross-listed firms in the U.S. between 2002 and 2006 covered by COMPUSTAT and audit analytics databases	2,292
Minus	
Observations not covered by I/B/E/S database	1,341
Missing observations for analyst information environment metrics	28
Final sample of cross-listed firm-years	**913**
Final sample of unique cross-listed firms	*379*
Not Cross-listed firms	
Non cross-listed firms domiciled in the country where there is at least a cross-listed firm in the final sample country firms between 2002 and 2006 covered by COMPUSTAT GLOBAL	72,786
Minus	
Observations not covered by I/B/E/S database	49,802
Missing observations for analyst information environment metrics	13,075
Not cross-listed firm-years	**9,909**
Unique not cross-listed firms	*4,034*
Total sample	**10,822**

Research design and empirical models

We first examine the change in cross-listed firms information environment after the first disclosure of internal control deficiencies according to SOX302 relative to the change for the control sample of non cross-listed firms. By employing this difference-in-difference design, we can take into account the effects of potentially confounding events around the first SOX302 disclosure as well as concerns about unobserved heterogeneity across firms or time-invariant selection bias. We regress firm information environment metrics on binary variables marking cross-listed firms on the basis of the first disclosure on internal control deficiencies, the time period and interaction terms:

$$FIE_{it} = \alpha_0 + \alpha_1 GOOD\_first_{it} + \alpha_2 BAD\_first_{it} + \alpha_2 POST_{302_{it}} + \alpha_1 GOOD\_first_{it} \times POST\_302_{it} + \alpha_2 BAD\_first_{it} \times POST\_302_{it} + \sum_k \alpha_k CTRL_{it}^k + \varepsilon_{it} \quad (1)$$

where FIE stands for firm information environment metrics. GOOD_first takes the value of one if a cross-listed firm does not disclose an internal control deficiency at the time of the first SOX302 disclosure, zero otherwise. BAD_first takes the value of one if a cross-listed firm disclose an internal control deficiency at the time of the first SOX302 disclosure, zero otherwise. POST_302 takes the value of zero in the year before the analyst knowledge of the first SOX302 disclosure (i.e. fiscal year ending from 08/31/2002 to 07/31/2003), zero in the year after the analyst knowledge of the first SOX302 disclosure (i.e. fiscal year ending from 08/31/2003 to 07/31/2004). The interaction between GOOD_first (BAD_first) and POST_302 captures the change in firm information environment metrics around the first SOX302 disclosure. We expect to find no difference between cross-listed firms before the first disclosure on SOX302 internal control deficiencies, and significant differences between cross-

listed firms and the control sample. On the other side, we expect to find a decrease in the quality of firm information environment only for cross-listed firm disclosing internal control deficiencies.

Next, we examine the association between the quality of internal controls over financial reporting and the firm information environment. Since the knowledge about the quality of internal controls over financial reporting of period *t* precedes the earnings forecast in year *t+1*, we regress the proxies for the quality of the firm information environment in year *t+1* on the information disclosed under SOX302 in year *t.* We code up two binary variables that identify cross-listed firms according to the information disclosed under SOX302. GOOD is the binary variable equal to one if a cross-listed firm does not disclose internal control deficiencies in year *t*, zero otherwise. BAD is the binary variable equal to one if a cross-listed firm discloses internal control deficiencies in year *t*, zero otherwise. Hence, we estimate the following regression model:

$$FIE_{it+1} = \alpha_0 + \alpha_1 GOOD_{it} + \alpha_2 BAD_{it} + \sum_k \alpha_k CTRL_{it}^k + \varepsilon_{it} \quad (2)$$

By estimating the intercept and the two coefficients on GOOD and BAD we compare three groups of firms: cross-listed firms not disclosing internal control deficiencies (α_1) to not cross-listed firms (α_0), cross-listed firms disclosing internal control deficiencies to not cross-listed firms (α_2), and "GOOD" cross-listed firms (α_1) to "BAD" cross-listed firms (α_2). We expect that (i) cross-listed firms not disclosing internal control deficiencies (GOOD) are associated with a higher quality in the firm information environment than not cross-listed firms ($\alpha_1 > 0$); (ii) cross-listed firms not disclosing internal control deficiencies are associated with a higher quality in the analyst information environment than cross-listed firms disclosing

internal control deficiencies ($\alpha_1 > \alpha_2$); and (iii) cross-listed firms disclosing internal control deficiencies (BAD) have a quality in the analyst information environment worse or not different from not cross-listed firms, i.e. they lose the benefits stemming from cross-listing ($\alpha_2=0$ or $\alpha_2<0$). We employ to alternative benchmark: the full sample of non cross-listed firms and a propensity score matched sample to control for differences in firm characteristics between cross-listed and non-cross-listed firms while estimating SOX302 disclosure treatment effect on firm information environment. Propensity-score matching models (Rosenbaum and Rubin 1983) match observations with respect to the probability of be treated, which in our setting is the likelihood of being a cross-listed firm[26]. Using data between 2000 and 2002, that is before the first SOX302 disclosure, we thus model the probability to be a cross-listed firm using a logit model (Lawrence et al. 2011). To the extent that matching models do not require exclusion restrictions, we include a comprehensive set of firm characteristics that prior research found to be associated with the probability of cross-listing in the U.S. We consider firm size (logarithm of total assets at the beginning of the year), financial leverage (total liabilities over total assets), return on assets (net income over total assets), growth opportunities (annual change in sales), and needs for financing (change in total liabilities and change in common stock), as well as country and industry fixed effects. Next we match, without replacement, each cross-listed firm with a non cross-listed firm using the closet predicted value from the propensity score matching regression.

In the third set of empirical analyses we examine whether cross-listed firms

[26] Propensity score matching models seem to be particular suitable in our setting. First, this approach creates samples in which cross-listed and not cross-listed firms are similar, providing a good framework to assess how SOX302 disclosures shape cross-listing effects on firm information environment. Second, selection or treatment effect models (Heckman 1979) used in cross-listing literature (Lang et al. 2003) rely on a specific functional form to provide an indirect estimate of cross-listing effect. Matching models do not rely on a specific functional form. In addition, selection models in cross-listing literature might estimate biased treatment effects to the extent that is difficult to identify instrument that affect the likelihood to cross-list and not the effect of cross-listing (Lennox et al. 2012). However, this approach does not take into account unobservable heterogeneity across firms in estimating treatment effects.

information environment change after a remediation of an internal control deficiency. By this change analysis, we can overcome issues stemming from correlated omitted variables, and better disentangle the marginal effect of a remediation of a previously disclosed internal control deficiency on financial information environment properties from firm-level time invariant factors (Wooldridge 2003). We code up a dummy variable marking cross-listed firms that remediate to a previously disclosed internal control deficiency. UP is a binary variable equals to one if a firm has disclosed an internal control deficiency in period *t-1* and no internal control deficiencies in period *t*[27]. We thus propose the following model:

$$\Delta FIE_{it+1;t} = \alpha_0 + \alpha_1 UP_{it} + \sum_k \alpha_k \Delta CTRL_{it}^k + \varepsilon_{it} \quad (3)$$

where ΔFIE stands for the change in the firm information environment proxy between the period *t* and *t+1*. In model (3), we also control for firm-specific time varying factors that might affect the change in firm information environment as well as the likelihood to remediate to a previously disclosed internal control deficiency. For instance, a large change in reported earnings from one period to the other might affect analyst uncertainty and the likelihood to disclose an internal control deficiency (Duru and Reeb 2002; Ashbaugh-Skaife et al., 2007). Following prior literature (Kim et al., 2009; Wooldridge, 2003) we thus include in model (3) each control variable used in model (1) in the first-order difference form, that is the we difference each control variable between period *t* and *t-1*. We test our hypothesis two with two control samples: cross-listed firms that never disclose internal control deficiencies and all non-cross-listed firms. The intercept (α_0) captures the change from year *t* and year *t+1* in the

[27] It could have been of interest to study the association between a decrease of the quality of internal controls over financial reporting and the firm information environment by defining a variable DOWN as a dummy equals to 1 if company *i* has disclosed no internal control deficiencies in period *t-1* ($GOOD_{it-1} = 1$) and internal control deficiencies in period *t* ($BAD_{it} = 1$). We were not able to perform this analysis because only 8 firm-year observations have DOWN = 1.

properties of the firm information environment for the control sample. The coefficient on UP (α_1) captures the difference in the change in the properties of the firm information environment between cross-listed firms that remediate to an internal control deficiency and control firms. If the remediation of the internal control deficiencies identified in the previous period allows cross-listed firms to plug the transparency and credibility gap then α_1 is expected to be positive and significant.

Finally, we investigates whether the legal and enforcement characteristics of the countries where cross-listed firms are domiciled are associated with cross-sectional differences in the effects of SOX302 disclosures across cross-listed firms. To measure the extent to which countries differ in terms of legal and enforcement characteristics, we use the following variables taken from Kaufman et al. (2007) for the year 2005: (1) Government Effectiveness; (2) Regulatory Quality; (3) Rule of Law; (4) Control of Corruption. Higher values of each of these variables implies higher levels of legal enforcement. To partition the sample, we first take the sum of these legal environment variables, then we split the sample according to the sample median. Next, we code up a binary variable (LAW) equals to one if an observation comes from a country that is above the sample median, zero otherwise. As a consequence, firms for which LAW is equal to zero are categorized as firms incorporated in lax legal environment countries. To test our last set of hypotheses, we estimate the following model:

$$FIE_{it+1} = \alpha_0 + \alpha_1 LAW_i + \alpha_2 GOOD_{it} + \alpha_3 BAD_{it} + \alpha_4 GOOD_{it} * LAW_i + \alpha_5 BAD_{it} * LAW_i + \sum_k \alpha_k CTRL_{it}^k + \varepsilon_{it} \quad (4)$$

According to the bonding hypothesis, cross-listing effects should be stronger for cross-listed firms domiciled in country with weak legal enforcement. As a consequence, we expect

that the difference in the firm information environment benefits between firms disclosing and not disclosing internal control deficiencies to be stronger for firms from weak legal environment countries. The coefficient of the interaction between GOOD and LAW (BAD and LAW) captures if the relationship between the successful (mimicking) adoption of stricter rules in terms of internal controls over financial reporting on the firm information environment is associated with the strength of the enforcement. In both the cases (successful adoption and mimicking adoption) we expect the coefficient to be negative (α_4 and α_5 respectively) whether the effects are weaker in countries with strong legal enforcement. In addition, α_2 is expected to be significantly larger than α_3, while $\alpha_1 + \alpha_4$ is expected to be not different from $\alpha_3 + \alpha_5$.

Firm information environment

Following previous literature (Lang and Lundholm 1996; Hutton and Palepu 1999; Gebhardt et al. 2001), we operationalize the firm information environment using the properties of analyst earnings forecasts. We first focus on forecast accuracy, dispersion and analyst following as previous studies suggest that be followed by more analysts with more accurate and less dispersed forecasts indicates a better information environment (Lang and Lundholm 1996; Hutton and Palepu 1999; Gebhardt et al. 2001).

We calculate forecast accuracy (*ACC*) as the negative of the absolute value of the analyst forecast accuracy, deflated by the stock price at the beginning of the fiscal year: $ACC_{it} = |Actual\ Earnings_{it} - Median\ Forecast_{it}| / Stock\ Price_{it,}$, where $Actual\ Earnings_{it}$ is the Actual I/B/E/S annual EPS for firm *i* in year *t*, $Median\ Forecast_{it}$ is the median of forecasts made by analysts in our sample from the 11th month of the fiscal year to 3 days before the annual earnings announcement for firm *i* and year *t*, and $Stock\ Price_{it}$ is the stock price of firm *i* at the

end of year *t*.[28] We remove the effect of stale forecasts by employing the last forecast made by each analyst if they issue more than one forecast. Using the same forecast window we calculate forecast dispersion (DISP) as the *Standard Deviation of Forecasts/Stock Price.* Analyst following (FOLL) is the number of analysts who issue at least one annual forecast for a given firm-year. Following prior research (Byard et al., 2011), we use a use a logarithm transformation to reduce the skewness.

These measures on the characteristics of the firm information environment might depend on changes in common or idiosyncratic information. For this reason, we employ the measures proposed by Barron et al. (1998) (BKLS, hereafter): the precision of analyst public information (*H*), private information (*S*), and analyst consensus (*CONS*)[29].

We consider BKLS because analysts have two sources of information: an information signal common to all analysts and a signal observed separately by each analyst. These measures allow us to disentangle to what extent differences in the quality of firms information environment are driven by differences in the commonality of information among analysts or in the private information acquisition by single analysts. Our setting is particularly adequate for the BKLS measures because the characteristics of the internal controls over financial reporting are inherently unobservable from outside bringing to idiosyncratic information. The adoption of the SOX302 makes available to all market participants the information upon the adequacy of internal controls over financial reporting, leveling the information field. As a result, a change in the firm information environment can be achieved through an increase of the precision of common information that might be accompanied by a decrease in the precision of private information.

[28] The results are similar when we use the mean forecast rather than the median forecast.
[29] See Appendix A for details about the calculation of the BKLS metrics.

Control variables

All models include year-country-industry fixed effects using the industry classification as in Campbell (1996) and heteroskedasticity-corrected standard errors, which are adjusted at firm-level clustering (Gow et al. 2010). In addition, the models include a set of control variables that prior research finds to be associated with the properties of analyst information environment. The size of a firm is related to the level of pre-disclosure information, thereby we control for firm size (SIZE), using the natural logarithm of the total assets at the beginning of the year. Hwang et al. (2002) finds that analyst forecast for firms reporting losses are less accurate than for firms reporting a profit. We control for loss reporting firms through a dummy variable that is equal to one if actual earnings per share are less than zero, and zero otherwise (LOSS). Earnings skewness and the magnitude of the annual change in earnings are likely to affect the properties of analyst earnings forecast (Lang and Lundholm 1996, Gu and Wu 2001, Duru and Reeb 2002). Skewed earnings are associated with more optimistic forecasts, while larger changes in earnings from one year to the other make more difficult for analyst to predict expected earnings. We control for earnings skewness (SKEW) using the statistical definition of skewness over the past five years, while we measure the change in earnings (ΔEAR) using as the absolute value of the difference between the current year earnings per share and the last year's earnings per share, scaled by the closing price as the end of the current year. We include the standard deviation of the return on assets over the past five years (σROA) to control for the possible effects of earnings volatility on firm information environment (O'Brien and Bhushan, 1990; Lang and Lundholm, 1996; Frankel et al., 2006). In all but the analyst forecast dispersion regression, we include forecast dispersion as a control, to the extent that previous empirical evidence (Lang and Lundholm 1996; Bamber et al. 1997; Gu and Wu

2001) documents that the amount of dispersion among analyst reflects uncertainty and lack of consensus about the impact of future events on firms expected performances. We hence control for forecast dispersion as the standard deviation of analyst earnings forecasts, scaled by stock price as the beginning of the year (DISP). Finally, we consider firm performance, using return on asset (ROA), measured as the ratio between net income and total assets as the beginning of the year, and financial leverage (LEV) as the ratio between total debts and total assets as the beginning of the year.

3.4. Results

Descriptive statistics and univariate analysis

Table 3.2 provides the sample distribution by country. The overall sample consists of 10,822 firm-year observations between August 2002 and July 2006. Column (2) shows that the number of observations varies widely across the sample countries: from a maximum of 2,614 non cross-listed firms domiciled in Japan (26% of the total sample) to a minimum of 2 domiciled in Ghana and from a maximum of 152 cross-listed firms domiciled in the UK to a minimum of domiciled in Hungary and Turkey.

Table 3.2
Sample distribution by country

Country	Firm-years	Non Cross-listed firms	Cross-listed firms
Australia	498	480	18
Austria	77	74	3
Bahamas	24	19	5
Belgium	141	138	3
Bermuda	200	167	33
Brazil	104	68	36
Cayman Island	103	66	37
Chile	32	19	13
China	183	162	21
Denmark	158	150	8
Finland	246	237	9
France	719	656	63
Germany	508	475	33
Ghana	3	2	1
Greece	148	141	7
Hong Kong	126	105	21
Hungary	17	16	1
India	299	287	12
Indonesia	90	86	4
Ireland	69	45	24
Israel	52	15	37
Italy	297	276	21
Japan	2,701	2,614	87
Korea	403	387	16
Liberia	8	5	3
Luxembourg	22	10	12
Mexico	51	37	14
New Zealand	80	76	4
Norway	224	208	16
Panama	13	8	5
Papua New Guinea	6	4	2
Peru	17	14	3
Philippines	25	22	3
Portugal	73	67	6
Russia	15	10	5
Singapore	233	223	10
South Africa	188	165	23
Spain	215	198	17
Sweden	318	298	20
Switzerland	285	254	31
Taiwan	230	213	17
The Netherlands	313	257	56
Turkey	21	20	1
United Kingdom	1,286	1,135	152
Total	**10,822**	**9,909**	**913**

Table 3.2 reports the sample distribution. The full sample comprises 10,822 firm-year observations from 44 countries around the world during the period from 2002 to 2006.

Table 3.3 panel A presents the descriptive statistics relating to the variables used in the full sample. The mean (median) of ACC is -0.017 (-0.005), which indicates the mean (median) difference between analyst consensus forecast and actual earnings is about -1.74 percent (-0.45 percent) of the lagged share price. The mean (median) of DISP is 0.016 (0.004) of lagged share price indicating that the mean (median) dispersion is about 1.52 percent (0.42 percent) of lagged share price. The mean (median) of the logarithm of analyst following is 3.206 (3.218). The mean (median) of public (H) and private information (S) is 0.859 (0.363) and 0.803 (0.243), respectively.

Table 3.3

Descriptive Statistics for Variables Used in Regression Analyses

Panel A: Descriptive Statistics for dependent and control variables

Variable	N	Mean	Std.Dev	P5	P25	Median	P75	P95
ACC	10,822	-0.017	0.052	-0.066	-0.012	-0.005	-0.002	0.000
DISP	10,822	0.016	0.077	0.000	0.002	0.004	0.011	0.054
FOLL	10,822	3.206	0.956	1.609	2.485	3.218	3.871	4.796
H	10,557	0.859	1.292	0.000	0.026	0.363	1.108	3.574
S	10,557	0.803	1.894	0.023	0.097	0.246	0.680	3.122
CONS	10,557	0.471	0.402	0.000	0.010	0.468	0.902	0.992
LOSS	10,822	0.096	0.295	0.000	0.000	0.000	0.000	1.000
σ(ROA)	10,822	0.045	0.065	0.005	0.013	0.025	0.049	0.151
ΔEAR	10,822	125.750	652.790	0.019	0.177	1.200	12.271	261.120
SKEW	10,822	-0.039	0.649	-1.189	-0.472	0.000	0.384	1.078
SIZE	10,822	6.684	1.714	3.996	5.471	6.621	7.809	9.612
LEV	10,822	1.678	1.843	0.210	0.626	1.173	2.005	4.906
ROA	10,822	0.037	0.127	-0.076	0.015	0.041	0.076	0.156

Panel B: Cross-listed firms distribution according to SOX302 disclosure

GOOD	861
BAD	52
UP	29
DOWN	14

Table 3.3 reports descriptive statistics for the dependent variables and the continuous and binary independent variables.
The full sample comprises 10,822 firm-year observations from 44 countries around the world during the period from 2002 to 2006.
See APPENDIX III.B for variable definitions.

The mean (median) of analyst consensus is 0.471 (0.468). The sample distribution of the control variables used in the analyses is comparable to that reported in prior research. Table 3.3. Panel B shows that out of 913 cross-listed firm-years, 52 firms disclose at least one internal control deficiency according to SOX302 in term of "material weakness", "significant deficiency", or "deficiency" in internal control systems during the period August 2002 – July 2006. On the other side, 861 cross-listed firm-year observations do not disclose any internal control deficiency during the same time period[30].

Table 3.4 reports the Pearson correlations among the variables used in the empirical analyses. Cross-listing (XLIST) is positively and significantly associated with ACC ($p < 0.050$), FOLL ($p < 0.001$) and CONS ($p < 0.050$). These associations are still significant only for cross-listing firms not reporting internal control deficiencies (GOOD) while are not significant for cross-listing firms that report internal control deficiencies (BAD). The associations among the dependent variables are in the expected direction. Forecast accuracy is negatively and significantly associated with forecast dispersion, and positively with H, and CONS. At the same time, correlations among control variables are in the expected direction.

[30] The disclosures of internal control deficiencies by cross-listed firms are about ineffective control environment, inadequate qualified staff, who are familiar with U.S. GAAP, complexity of transactions such as derivatives, taxes and stock option compensation, etc. Due to the small sample size, we do not separately analyze each category of internal control deficiencies in our empirical analyses.

Table 3.4

Correlation matrix

	XLIST	GOOD	BAD	ACC	DISP	FOLL	S	H	CONS	LOSS	σ(ROA)	ΔEAR	SKEW	SIZE	LEV	ROA
XLIST	1															
GOOD	0.96***	1														
BAD	0.29***	0.18***	1													
ACC	0.02**	0.02*	0.00	1												
DISP	0.00	0.00	0.00	-0.40***	1											
FOLL	0.29***	0.28***	0.06*	0.00	0.00	1										
S	0.03***	0.01	0.00	-0.66***	0.58***	0.04***	1									
H	-0.02***	-0.02***	-0.01	0.05***	-0.13***	-0.12***	-0.11***	1								
CONS	0.05***	0.06***	0.01*	0.05***	-0.24***	0.01	-0.23***	0.39***	1							
LOSS	0.01	0.00	0.01	-0.16***	0.29***	-0.07***	0.19***	-0.09***	-0.10***	1						
σ(ROA)	0.03***	0.02***	0.01**	-0.06***	0.14***	-0.07***	0.05***	-0.02***	-0.03***	0.20***	1					
ΔEAR	-0.02***	-0.02***	-0.01*	-0.05***	0.05***	0.08***	0.25***	-0.08***	-0.13***	0.04***	0.01	1				
SKEW	0.02***	0.02***	0.01	0.04***	-0.06***	0.05***	-0.04***	0.00	0.04***	-0.13***	-0.08***	0.00	1			
SIZE	0.30***	0.29***	0.07***	0.00	0.00	0.54***	0.05***	-0.12***	0.03	-0.11***	-0.24***	0.02***	-0.01	1		
LEV	0.01	0.01	0.01	-0.11***	0.13***	0.05***	0.13***	-0.08***	-0.06***	0.09***	-0.04***	0.03***	-0.05***	0.29***	1	
ROA	-0.02**	-0.01	-0.01**	0.09***	-0.21***	0.10***	-0.12***	0.11***	0.07***	-0.40***	-0.24***	-0.02*	0.12***	-0.03***	-0.16***	1

Table 3.4 reports Pearson correlations. See APPENDIX A for variable definitions.

See APPENDIX III.B for variable definitions.

***, ** and * denote significance at 1%, 5% and 10% levels (two-sided), respectively.

Table 3.5 presents descriptive statistics of the analyst information environment variables. We split the sample in four groups: (i) not cross-listed firms (column 1); (ii) cross-listed firms (column 2); and within the latter group between (iii) cross-listed firms not disclosing internal control deficiencies (column 3), and (iv) cross-listed firms disclosing internal control deficiencies (column 4).

Table 3.5

Univariate Tests of differences in analyst information environment metrics between the groups of firms

Variable	Non XLIST (1) Mean (median) [STD]	XLIST (2) Mean (median) [STD]	GOOD (3) Mean (median) [STD]	BAD (4) Mean (median) [STD]	*p*-value of testing (2)-(1) *t*-test Ranksum	*p*-value of testing (3)-(1) *t*-test Ranksum	*p*-value of testing (4)-(1) *t*-test Ranksum	*p*-value of testing (3)-(4) *t*-test Ranksum
ACC	-0.018	-0.016	-0.016	-0.018	0.020	0.019	0.540	0.019
	(-0.005)	(-0.003)	(-0.003)	(-0.004)	0.046	0.046	0.210	0.046
	[0.052]	[0.045]	[0.046]	[0.034]				
DISP	0.016	0.011	0.011	0.011	0.285	0.289	0.256	0.241
	(0.004)	(0.004)	(0.004)	-0.004)	0.184	0.151	0.158	0.194
	[0.080]	[0.018]	[0.019]	[0.015]				
FOLL	3.112	4.152	4.166	3.881	0.011	0.008	0.092	0.043
	(3.135)	(4.204)	(4.219)	(3.891)	0.009	0.001	0.089	0.051
	[0.914]	[0.889]	[0.885]	[0.931]				
H	0.8528	0.927	0.934	0.813	0.095	0.053	0.480	0.301
	(0.362)	(0.365)	(0.371)	(0.351)	0.000	0.003	0.889	0.410
	[1.251]	[1.667]	[1.669]	[1.386]				
S	0.796	0.867	0.893	0.536	0.278	0.095	0.235	0.099
	(0.241)	(0.329)	(0.338)	(0.322)	0.000	0.000	0.977	0.474
	[1.893]	[1.897]	[1.948]	[0.818]				
CONS	0.473	0.466	0.466	0.459	0.731	0.821	0.837	0.507
	(0.742)	(0.432)	(0.433)	-0.437	0.003	0.002	0.954	0.455
	[0.404]	[0.377]	[0.377]	[0.369]				

Table 3.5 reports univariate tests of differences in analyst information environment metrics among cross-listed firms disclosing internal control deficiencies, cross-listed firms non disclosing internal control deficiencies and not cross-listed firms.
See APPENDIX III.B for variable definitions.
***, ** and * denote significance at 1% , 5% and 10% levels (two-tailed), respectively

Through this preliminary (descriptive) analysis we find that analyst forecast accuracy is significantly higher for cross-listed firms than for not cross-listed firms [(2) – (1): p-value =

0.020], consistently with the literature on cross-listing. When we split the sub-sample of cross-listed firms according to the content of the SOX302 (disclosure or non-disclosure of internal control deficiencies), we find that cross-listing benefits are experienced only by those who do not disclose internal control deficiencies [(3) – (1): p-value = 0.019], while cross-listed disclosing internal control deficiencies are not different from the not cross-listed firms [(4) – (1): p-value = 0.540]. We do not find a similar pattern for forecast dispersion since there is not a statistically significant difference in each of the pairs considered for the comparison. For analyst following we find that the results are driven both by the cross-listing status since cross-listed firms experience as expected more analyst following than not cross-listed firms [(2) – (1): p-value = 0.011] and by the adequacy of internal controls since cross-listed firms without internal control deficiencies have more analyst following than cross-listed firms showing internal control deficiencies [(3) – (4): p-value = 0.043].

Multivariate analysis

We start our empirical analysis by examining the effect of the first disclosure of internal control deficiency according to SOX302 using a difference-in-difference design. Table 3.6, panel A reports the regression results from the estimation of model (1) using ACC, DISP and FOLL as dependent variables. The coefficient on the interaction between POST_302 and GOOD_first is insignificant for all the dependent variables, suggesting the cross-listed firms not disclosing internal control deficiencies do not experience a change in firm information environment after the first SOX302 disclosure. On the other side, we find that cross-listed firms disclosing internal control deficiencies experience a decrease in the quality of firm information environment after the first SOX302 disclosure.

Table 3.6: panel A

Diff-in-diff at the SOX320 first adption

	(1) ACC	(2) DISP	(3) FOLL
GOOD_FIRST	0.047**	-9.071*	0.453***
	(1.996)	(-1.901)	(8.839)
BAD_FIRST	0.033*	-2.014*	0.348
	(1.711)	(-1.717)	(1.220)
POST_302	0.000	0.635	0.135***
	(0.074)	(0.967)	(9.222)
POST_302*GOOD_FIRST	-0.001	-9.684	-0.214
	(-0.217)	(-1.143)	(-0.959)
POST_302*BAD_FIRST	-0.128**	5.402*	-0.224***
	(-2.035)	(1.783)	(-6.141)
SD(ROA)	-0.082**	51.363	0.476***
	(-2.344)	(0.947)	(4.071)
DISP	-0.970***	-	-0.680***
	(-24.681)		(-4.182)
ΔEAR	0.000	0.023	0.000***
	(0.837)	(0.783)	(2.715)
LOSS	-0.029***	0.685	-0.095***
	(-5.777)	(0.571)	(-3.162)
EAR_SKEW	0.005***	-0.028	0.021
	(3.072)	(-0.104)	(1.159)
SIZE	0.001	1.025	0.321***
	(0.832)	(1.139)	(36.699)
LEV	-0.000	-0.003	-0.001
	(-1.446)	(-1.251)	(-0.907)
ROA	0.002	20.517	0.164
	(0.131)	(0.908)	(1.437)
COSTANT	-0.010	-12.538	1.347***
	(-0.930)	(-0.887)	(5.892)
Year fe	No	No	No
Industry fe	Yes	Yes	Yes
Country fe	Yes	Yes	Yes
Observations	4,341	4,341	4,341
R-squared	0.651	0.095	0.490

Table 3.6 reports estimated coefficients and reported t-statics based on robust standard errors that are clustered at firm level (in parentheses) from the estimation of model (1). See APPENDIX III.B for variable definitions. ***, ** and * denote significance at 1% , 5% and 10% levels (two-tailed), respectively

In panel B we combine the coefficients of the variables of interests and test the significance of the aggregate coefficients. Results show that before the first SOX302 disclosure there is not difference across cross-listed firms while after the first SOX302 disclosure, only cross-listed firm non disclosing internal control deficiencies are still different from their home countries peers. On the contrary, cross-listed firms disclosing internal control deficiencies suffer by a worse firm information environment than home country firms.

Table 3.6: panel B - Two-by-two analysis on the effect of SOX302 disclosure on cross-listed firms information environment

ACC					DISP					FOLL				
		(a) PRE	(b) POST	(b) - (a)			(a) PRE	(b) POST	(b) - (a)			(a) PRE	(b) POST	(b) - (a)
GOOD_FIRST	(i)	0.037	0.036	-0.001	GOOD_FIRST	(i)	-21.609	-30.658	-9.049	GOOD_FIRST	(i)	1.800	1.721	-0.079
BAD_FIRST	(ii)	0.023	-0.105	-0.128**	BAD_FIRST	(ii)	-14.552	-8.515	6.037*	BAD_FIRST	(ii)	1.695	1.606	-0.089***
CONTROL	(iii)	-0.010	-0.010	0.000	CONTROL	(iii)	-12.538	-11.903	0.635	CONTROL	(iii)	1.347	1.482	0.135***
	(i)-(ii)	0.014	0.141**	0.127**		(i)-(ii)	-7.057	-22.143*	-15.086*		(i)-(ii)	0.105	0.115	0.011
	(i)-(iii)	0.047**	0.046**	-0.001		(i)-(iii)	-9.071*	-18.755*	-9.684		(i)-(iii)	0.453***	0.239	-0.214
	(ii)-(iii)	0.033**	-0.095	-0.128**		(ii)-(iii)	-2.014*	3.388	5.402*		(ii)-(iii)	0.348	0.124	-0.224***

We next exploit data from 2002 to 2006 to examine the average association between the quality of the internal controls over financial reporting and cross-listed firm information environment Table 3.7 presents the regression results from the estimation of model (2) using ACC, DISP and FOLL as dependent variable. Columns 1-3 confirm the beneficial effects of cross-listing (XLIST) on the firm information environment. Consistent with literature, we find that cross-listing firms experience, on average, a higher forecast accuracy (XLIST = 0.04, $p < 0.001$), less forecast dispersion (XLIST = -0.014, $p < 0.001$) and more analyst following (XLIST = 0.255, $p < 0.001$) than non cross-listed firms. These results still hold after considering differences in firm-characteristics between cross-listed and non-cross-listed firms by propensity score matching models (Columns 4-6).

Columns (7)-(9) report our main findings. We claim the within the population of cross-listed firms there is not a pooling equilibrium in which all cross-listed firms experience the same cross-listing benefits. We contend that there is a substantial heterogeneity in term of the firm information environment benefits and that this cross-sectional variation is associated with SOX302 disclosures on the adequacy of the internal controls over financial reporting. We find that the positive and significant association between cross-listing status and forecast accuracy is still significant only for cross-listing firms not disclosing internal control deficiencies (GOOD: 0.004, $p < 0.001$). But, when we consider cross-listed firms disclosing internal control deficiencies, we find that these firms suffers by a worse forecast accuracy than home country firms (BAD: -0.003, $p < 0.1$). These firms not only lose the positive effects of cross-listing on the firm information environment but show a worse information environment than their non cross-listed peers. We find similar results for dispersion and analyst following. The decrease of dispersion, that represents a better firm information environment, holds only for cross-listed firms that effectively adopt stricter internal controls. Firms that have ineffective internal controls show more dispersed earnings forecasts than non cross-listed firms (GOOD: -0.013, $p < 0.001$; BAD: 0.006, $p < 0.1$). Finally, we find that the positive and significant association between cross-listing status and analyst following is still significant only for cross-listing firms not disclosing internal control deficiencies (GOOD: 0.268, $p < 0.001$) while we do not find any statistically significant differences between cross-listed firms disclosing internal control deficiencies and non cross-listed firms (BAD: -0.003, $p = 0.509$). We obtain similar results in the propensity score matching sample. These results suggest that cross-listing is associated with a higher quality of information environment only for firms that have effectively adopted stricter internal controls.

Table 3.7
Base regression

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Full Sample				*PSM*			*Full Sample*			*PSM*	
	ACC	DISP	FOLL	ACC	DISP	FOLL	ACC	DISP	FOLL	ACC	DISP	FOLL
XLIST	0.004***	-0.014***	0.255***	0.005**	-0.045**	0.585***	-	-	-	-	-	-
	(2.971)	(-3.548)	(5.512)	(1.977)	(-2.321)	(8.689)						
GOOD	-	-	-	-	-	-	0.004***	-0.013***	0.268***	0.004*	-0.042**	0.566***
							(2.751)	(-3.295)	(6.153)	(1.778)	(-2.265)	(8.644)
BAD	-	-	-	-	-	-	-0.003*	0.006*	-0.077	-0.002	-0.021	-0.049
							(-1.787)	(1.922)	(-0.665)	(-0.726)	(-1.208)	(-0.335)
LOSS	-0.021***	0.045***	-0.053	-0.028***	0.372	-0.284***	-0.021***	0.041***	-0.053	-0.028***	0.373	-0.275***
	(-4.152)	(5.527)	(-1.413)	(-2.875)	(1.461)	(-3.101)	(-4.154)	(4.903)	(-1.417)	(-2.857)	(1.462)	(-2.998)
SD'(ROA)	-0.006	0.128***	0.497***	0.003	-0.243	-0.973**	-0.006	0.115***	0.500***	0.004	-0.252	-0.910**
	(-0.738)	(3.355)	(2.722)	(0.211)	(-0.827)	(-2.191)	(-0.729)	(3.253)	(2.746)	(0.267)	(-0.856)	(-2.216)
DISP	-0.719***	-	-0.936***	-0.701***	-	-0.2247	-0.719***	-	-0.942***	-0.702***	-	-0.2927
	(-17.037)		(-5.181)	(-7.796)		(-0.452)	(-17.042)		(-5.232)	(-7.799)		(-0.588)
ΔEAR	0.000	0.000***	0.000*	0.000	0.000	0.000***	0.000	0.000	0.000*	0.000	0.000	0.001***
	(-0.999)	(6.597)	(1.862)	(-0.019)	(0.821)	(5.948)	(-1.005)	(1.536)	(1.847)	(-0.065)	(0.826)	(5.878)
EAR_SKEW	0.000	-0.001	0.052***	0.000	-0.019	-0.0124	0.001	-0.001	0.052***	0.001	-0.019	-0.013
	(1.380)	(-0.779)	(2.810)	(0.362)	(-0.929)	(-0.303)	(1.398)	(-0.602)	(2.834)	(0.359)	(-0.924)	(-0.309)
SIZE	-0.002	0.004***	0.374***	-	-	-	0.000	0.003***	0.375***	-	-	-
	(-0.449)	(3.307)	(33.128)				(-0.355)	(2.954)	(33.513)			
LEV	-0.001**	0.000	-0.0006	-	-	-	-0.000**	0.000	-0.001	-	-	-
	(-2.360)	(-0.121)	(-1.155)				(-2.350)	(-0.102)	(-1.146)			
ROA	0.004	-0.021	0.671**	-	-	-	0.004	-0.072***	0.667**	-	-	-
	(0.768)	(-1.477)	(2.517)				(0.754)	(-3.058)	(2.506)			
COSTANT	-0.007	-0.011	0.172	-0.002	0.245	3.686***	-0.007	-0.009	0.141	-0.002	0.245	3.686***
	(-0.963)	(-1.556)	(0.655)	(-0.513)	(1.064)	(46.875)	(-1.029)	(-1.222)	(0.539)	(-0.520)	(1.064)	(46.875)
Test on coeff.												
GOOD=BAD	-	-	-	-	-	-	2.07	-2.93	4.63	1.98	-2.06	4.23
Year fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,822	10, 822	10,822	1,565	1,565	1,565	10,822	10,822	10,822	1,565	1,565	1,565
R-squared	0.517	0.077	0.564	0.535	0.022	0.137	0.517	0.077	0.564	0.535	0.022	0.133

Table 3.7 reports estimated coefficients and reported t-statics based on robust standard errors that are clustered at firm level (in parentheses) from the estimation of model (2). Columns 1-3 and columns7-9 employ as control sample the worldwide population of non cross-listed firms, while colums 4-6 and columns 10-12 use a control sample obtained with a propensity score matching
See APPENDIX III.B for variable definitions.
***, ** and * denote significance at 1% , 5% and 10% levels (two-tailed), respectively

Next, we examine to what extent differences in the quality of the firm information environment are driven by differences in the commonality of information among analysts or in the private information acquisition by single analysts. Table 3.8, columns (1)-(3), shows that cross-listing is associated with a higher precision of public information (0.159, $p < 0.050$) and analyst consensus (0.094, $p < 0.001$), while no association has been fond with the precision of private information. Also for the BKLS metrics, we fail to find any significance differences between cross-listed disclosing internal control deficiencies and not cross-listed firms. A possible interpretation of this result is that internal controls over financial reporting are inherently unobservable by outsiders. Under SOX302, this information becomes available to the market, leading to a higher the precision of public information and consensus (the level of communality among analysts).

When SOX302 disclosures inform that internal controls are ineffective, financial analysts make equal cross-listed firms with ineffective internal controls (firms over which we have a bad information) and non cross-listed firms (firms over which we do not have information on internal controls).

Table 3.8

Base regressions on BKLS metrics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Full Sample			*PSM*			*Full Sample*			*PSM*		
	S	H	CONS	S	H	CONS	S	H	CONS	S	H	CONS
XLIST	0.005	0.159**	0.094***	0.008	0.107	0.105***	-	-	-	-	-	-
	(0.949)	(2.089)	(5.163)	(1.539)	(1.356)	(5.305)						
GOOD	-	-	-	-	-	-	0.006	0.177**	0.097***	0.009*	0.131*	0.109***
							(1.127)	(2.236)	(5.636)	(1.825)	(1.669)	(5.601)
BAD	-	-	-	-	-	-	-0.009	-0.158	0.025	-0.011*	-0.121	0.011
							(-1.501)	(-1.131)	(0.683)	(-1.794)	(-0.665)	(0.336)
LOSS	0.015**	-0.225***	-0.033	-0.006	-0.293***	-0.031	0.015**	-0.226***	-0.033	-0.006	-0.292***	-0.0311
	(2.021)	(-6.623)	(-1.015)	(-0.343)	(-3.093)	(-0.989)	(2.016)	(-6.643)	(-1.026)	(-0.324)	(-3.082)	(-0.988)
SD'(ROA)	-0.034*	-0.044	0.004	-0.059***	-0.076	-0.169***	-0.034*	-0.043	0.004	-0.057***	-0.064	-0.161**
	(-1.815)	(-0.330)	(0.107)	(-3.830)	(-0.245)	(-2.597)	(-1.815)	(-0.321)	(0.126)	(-3.588)	(-0.207)	(-2.582)
DISP	1.973***	-3.101***	-1.973***	2.260***	-2.408***	-1.750***	1.973***	-3.108***	-1.975***	2.257***	-2.427***	-1.759***
	(11.633)	(-10.217)	(-15.679)	(5.263)	(-3.419)	(-9.187)	(11.633)	(-10.247)	(-15.681)	(5.257)	(-3.450)	(-9.206)
ΔEAR	0.000***	0.000	0.000***	0.000***	0.000***	0.000***	0.000***	0.000	-0.000***	0.000***	-0.000***	-0.000***
	(5.127)	(-1.341)	(-5.147)	(9.924)	(-11.606)	(-5.482)	(5.127)	(-1.340)	(-5.149)	(9.904)	(-11.358)	(-5.464)
SKEW	-0.001	-0.026	0.007	0.006	0.021	-0.001	-0.001	-0.026	0.007	0.006*	0.021	-0.001
	(-0.605)	(-1.438)	(1.345)	(1.673)	(0.425)	(-0.086)	(-0.593)	(-1.417)	(1.353)	(1.695)	(0.430)	(-0.084)
SIZE	0.002**	-0.055***	0.004	-	-	-	0.002**	-0.055***	0.004	-	-	-
	(1.979)	(-5.611)	(0.715)				(1.989)	(-5.602)	(0.702)			
LEV	0.000	0.000	0	-	-	-	0.000	-0.003	0.000	-	-	-
	(-0.258)	(-1.057)	(-0.574)				(-0.254)	(-1.023)	(-0.553)			
ROA	0.059***	0.296*	0.029	-	-	-	0.058***	0.284*	0.026	-	-	-
	(2.773)	(1.779)	(0.535)				(2.742)	(1.707)	(0.480)			
COSTANT	-0.024**	2.418***	0.882***	-0.006	0.803***	0.657***	-0.026***	2.382***	0.877***	-0.006	0.803***	0.658***
	(-2.055)	(11.759)	(10.108)	(-0.589)	(6.794)	(17.969)	(-2.191)	(11.441)	(10.285)	(-0.592)	(6.792)	(17.961)
Test on coeff.												
GOOD=BAD	-	-	-	-	-	-	-0.45	2.23	3.06	1.98	-2.06	3.01
Year fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,557	10,557	10,557	1,545	1,545	1,545	10,557	10,557	10,557	1,545	1,545	1,545
R-squared	0.561	0.161	0.095	0.572	0.023	0.118	0.561	0.161	0.095	0.572	0.023	0.023

Table 3.8 reports estimated coefficients and reported t-statics based on robust standard errors that are clustered at firm level (in parentheses) from the estimation of model (2) on BKLS metrics. Columns 1-3 and columns7-9 employ as control sample the worldwide population of non cross-listed firms, while colums 4-6 and columns 10-12 use a control sample obtained with a propensity score matching

See APPENDIX III.B for variable definitions.

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

We now examine whether the firm information environment changes after a remediation of previously disclosed internal control deficiencies. UP captures the difference in the change in the properties of the firm information environment between cross-listed firms that remediate to an internal control deficiency and control firms. We use two control samples: cross-listed firms that never disclose internal control deficiencies (table 3.9, columns 1-3) and all non-cross-listed firms (Table 3.9, columns 4-6).

Table 3.9
Conditional analysis

	(1)	(2)	(3)	(4)	(5)	(6)
	Control sample: cross-listed firms never disclosing ICD			*Control sample: non cross-listed firms*		
	ΔACC	ΔDISP	ΔFOLL	ΔACC	ΔDISP	ΔFOLL
UP	1.102***	-0.819**	0.199	1.497***	-0.917***	0.169
	(3.153)	(-2.250)	(1.046)	(2.525)	(-3.904)	(0.897)
ΔEAR	0.000	0.000	0.000	0.000***	-0.000*	0.000
	(0.386)	(0.043)	(-0.115)	(3.746)	(-1.870)	(-0.367)
ΔDISP	-14.093***	-	1.595	-1.570***	-	0.396**
	(-2.628)		(1.469)	(-2.588)		(2.198)
ΔSD_ROA	0.34	0.307	-1.350*	0.006	-0.399***	-0.21
	(0.385)	(0.533)	(-1.706)	(0.101)	(-3.382)	(-1.355)
ΔSIZE	0.000	0.000	0.000	0.000	0.000	-0.000**
	(1.136)	(-0.925)	(-0.847)	(-0.470)	(-1.346)	(-2.514)
ΔROA	-0.535*	-0.065	0.215	0.124**	-0.221***	-0.056
	(-1.690)	(-0.323)	(0.758)	(2.113)	(-4.274)	(-0.719)
ΔLEV	-0.039***	0.049***	0.000	-0.013***	0.002	-0.002
	(-2.726)	(2.361)	(0.033)	(-2.982)	(1.023)	(-0.491)
COSTANT	-0.013	0.008	0.008	0.012	-0.008	0.098
	(-0.700)	(0.672)	(0.490)	(0.205)	(-0.153)	(0.663)
Observations	511	511	511	6,108	6,108	6,108
R-squared	0.334	0.173	0.016	0.536	0.131	0.074

Table 3.9 reports estimated coefficients and reported t-statics based on robust standard errors that are clustered at firm level (in parentheses) from the estimation of model (3). Columns 1-3 employ as control sample cross-listed firms that never disclose internal control deficiencies. Colums 4-6 employ as control sample non-cross-listed firms.
See APPENDIX III.B for variable definitions.
***, ** and * denote significance at 1% , 5% and 10% levels (two-tailed), respectively

We find that firms that remediate to previously disclosed internal control deficiencies experience an increase in the quality of the firm information environment relative of the control samples. If we consider as a control sample cross-listed firms that never disclose internal control deficiencies (columns 1-3) we find a positive association with change in accuracy (1.102, $p < 0.001$), a negative association with the change in dispersion (-0.819, $p < 0.050$) and no association with the change in analyst following (0.199, $p = 1.046$). These results are consistent across the two control groups.

Our last set of analysis explore whether the association between the quality of the internal controls and firm information environment depends on the level of enforcement of the country in which the cross-listed firm is domiciled. We expect that the difference in the benefits on the information environment between firms disclosing and not disclosing internal control deficiencies are stronger for firms domiciled in weak legal environment countries. Table 3.10, columns (1)-(3) confirm that cross-listing effects are stronger for firms from weak legal environment countries. Across the models, the dummy variable XLIST is associated with an higher quality of the firm information environment but the interaction between cross-listing and the level of enforcement (XLIST×LAW) is significant but goes in the opposite direction. Results are unchanged when we consider the propensity score matched sample (columns (4)-(6)). In columns (7)-(9) we explore whether the relationship between the successful (mimicking) adoption of stricter rules in terms of internal controls on firm information environment is associated with the strength of the legal environment. We find that only cross-listed firms not disclosing internal control deficiencies domiciled in weak legal enforcement country experience cross-listing benefits in term of forecast accuracy (GOOD: 0.004, $p < 0.001$); while cross-listed firms from strong legal environment countries do not get information benefits (GOOD+GOOD×LAW = 0.000, $p < 0.945$).

Table 3.10

Regression by home country legal characteristics

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
			Full Sample			*PSM*			*Full Sample*			*PSM*	
		ACC	DISP	FOLL	ACC	DISP	FOLL	ACC	DISP	FOLL	ACC	DISP	FOLL
LAW	β_1	0.003***	-0.003**	-0.039	0.008**	-0.003	-1.168***	0.003***	-0.003**	-0.023	0.007**	-0.003	-1.182***
		(4.423)	(-2.146)	(-0.413)	(2.553)	(-0.503)	(-6.516)	(4.393)	(-2.109)	(-0.240)	(2.462)	(-0.508)	(-6.656)
XLIST	β_2	0.005***	-0.014***	0.325***	0.008**	-0.012***	0.342***	-	-	-	-	-	-
		(3.215)	(-4.367)	(6.846)	(2.275)	(2.751)	(5.650)						
XLIST*LAW	β_3	-0.004**	0.005*	-0.200*	-0.009**	0.001	-0.159	-	-	-	-	-	-
		(-2.170)	(1.817)	(-1.965)	(-2.234)	(0.201)	(-1.156)						
GOOD	β_2	-	-	-	-	-	-	0.004***	-0.013***	0.323***	0.007**	-0.010**	0.332***
								(3.024)	(-4.105)	(7.292)	(2.005)	(-2.561)	(5.860)
BAD	β_3	-	-	-	-	-	-	-0.002	-0.005	0.028	0.000	-0.005	0.032
								(-0.749)	(-1.404)	(0.299)	(0.047)	(-1.587)	(0.336)
GOOD*LAW	β_4	-	-	-	-	-	-	-0.004*	0.006**	-0.163*	-0.008*	0.002	-0.121
								(-1.869)	(1.986)	(-1.698)	(-1.961)	(0.288)	(-0.976)
BAD*LAW	β_5	-	-	-	-	-	-	-0.001	-0.002	-0.18	-0.003	-0.002	-0.219
								(-0.238)	(-0.454)	(-0.726)	(-0.535)	(-0.424)	(-0.819)
LOSS		-0.020***	0.045***	-0.053	-0.026***	0.042***	-0.005	-0.020***	0.045***	-0.054	-0.026**	0.039***	-0.007
		(-7.338)	(5.599)	(-1.422)	(-2.582)	(4.506)	(-0.053)	(-7.336)	(5.597)	(-1.428)	(-2.573)	(4.501)	(-0.077)
SD'(ROA)		-0.006	0. 132***	0.495***	0.009	0.041***	0.389**	-0.006	0.131***	0.498***	0.009	0.039**	0.410**
		(-0.817)	(3.588)	(2.693)	(0.660)	(4.512)	(2.337)	(-0.804)	(3.573)	(2.718)	(0.729)	(2.015)	(2.642)
DISP		-0.723***	-	-0.934***	-0.700***	-	-0.845**	-0.724***	-	-0.940***	-0.701***	-	-0.878**
		(-18.651)		(-5.176)	(-7.706)		(-2.034)	(-18.653)		(-5.221)	(-7.708)		(-2.114)
ΔEAR		-0.000*	0.000***	0.000*	-0.000	0.000	0.000***	-0.000*	0.000***	0.000*	-0.000	0.000	0.000**
		(-1.896)	(7.130)	(1.925)	(-0.259)	(0.576)	(3.080)	(-1.909)	(7.143)	(1.890)	(-0.318)	(0.568)	(2.666)
SKEW		0.000	0.000	0.051***	0.001	-0.001	0.010	0.005	-0.0005	0.052***	0.001	-0.001	0.012
		(0.779)	(-0.484)	(2.803)	(0.319)	(-0.471)	(0.442)	(0.792)	(-0.495)	(2.810)	(0.307)	(-0.450)	(0.505)
SIZE		0.000	0.003***	0.374***	-	-	-	0.001	0.003***	0.374***	-	-	-
		(0.304)	(3.116)	(34.128)				(0.398)	(3.071)	(34.251)			
LEV		-0.000***	0.000	0.000	-	-	-	-0.000***	0.000	-0.001	-	-	-
		(-2.709)	(-0.102)	(-1.159)				(-2.708)	(-0.102)	(-1.152)			
ROA		0.002	-0.018	0.671**	-	-	-	0.002	-0.018	0.666**	-	-	-
		(0.471)	(-1.296)	(2.507)				(0.464)	(-1.296)	(2.497)			
COSTANT		-0.012**	0.006	0.101	-0.011**	0.015*	-1.130***	-0.0120**	0.007	0.095	-0.010**	0.017*	-1.122***
		(-1.970)	(0.934)	(0.439)	(-2.008)	(1.654)	(-8.143)	(-2.000)	(0.997)	(0.377)	(-1.977)	(1.728)	(-8.139)
Test on coeff.													
$\beta_2+\beta_3=0$		-0.45	-1.72	3.56	-0.25	-1.97	2.56	-	-	-	-	-	-
$\beta_2+\beta_4=0$		-	-	-	-	-	-	-0.08	-1.69	4.56	-0.12	-1.63	3.71
$\beta_3+\beta_5=0$		-	-	-	-	-	-	-1.08	-1.84	-0.45	-0.82	-1.31	-0.05
$\beta_2=\beta_3$		-	-	-	-	-	-	3.27	-2.86	4.30	4.12	-1.11	5.91
$\beta_2+\beta_4=\beta_3+\beta_5$		-	-	-	-	-	-	0.56	-0.89	1.34	0.01	-0.89	1.46
Year fe		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fe		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country fe		No	No	No	No	No	No	No	No	No	No	No	No
Observations		10,822	10,822	10,822	1,562	1,562	1,562	10,822	10,822	10,822	1,562	1,562	1,562
R-squared		0.514	0.069	0.564	0.548	0.062	0.672	0.514	0.069	0.564	0.548	0.061	0.673

Table 3.10 reports estimated coefficients and reported t-statics based on robust standard errors that are clustered at firm level (in parentheses) from the estimation of model (4). Columns (1)-(3) and columns (7)-(9) employs as control sample all non-cross-listed firms, while columns (4)-(6) and (10)-(12) a propensity score matched sample.

See APPENDIX III.B for variable definitions.

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

In addition, cross-listed firms from lax legal environment countries that disclose internal control deficiencies lose information environment benefits in term of forecast accuracy (BAD: -0.002; $p > 0.600$; GOOD $\neq$ BAD, $p < 0.001$). Results on dispersion and analyst following are similar. Columns (10)-(12) provide essentialy the same results using the propensity score matched sample. This evidence suggests that information provided through SOX302 is useful especially for firms that come from countries where investors are poorly protected. On the other side, for cross-listed firms that come from strong legal environment countries, where the information environment is supposed to be already rich these disclosures seem to be not as relevant as it is for the latter. Un-tabulated results show that the higher quality of the firm information environment is achieved through an increase of the precision of common information and it does not depend on the characteristics of the legal environment.

3.5. Robustness checks

Over-representation of some countries

Japan and United Kingdom account for about the 20 percent and 10 percent, respectively, of the total sample. We verify whether these two countries drive our results by estimating all the models without firms domiciled in these countries. Results are basically unchanged. We perform the same analyses using only European countries: also in this case results remain unchanged.

Measurement issues

The variables ACC and DISP are scaled by the closing price as the end of the year. Another scaling factors widely used in analyst literature is the absolute value of the earnings per share (i.e.

EPS). Using EPS as scaling factor do not affect the results. We also consider both longer (i.e. from the earnings announcement date of year t-1 to the earnings announcement date of year t) and shorter (i.e. from the closing date of year t to the earnings announcement of year t+1) forecasting windows. Also in this case results are consistent with those reported. Previous literature provides several proxies of the level of the enforcement in a country. In our main analyses, we consider the average score of the four dimensions Government Effectiveness, Regulatory Quality, Rule of Law, and Control of Corruption (Kaufman et al., 2007) for the year 2005. Rather than considering the sum of the four measures to partition the sample we consider the four variables one by one. Also in these cases, our results are unchanged.

3.6. Conclusions

Extant research documents an enhancement in the firm information environment for firms cross-listed in the U.S.This paper disputes the underlying premise that cross-listing *per se* enhances the quality of the firm information environment, arguing that it depends on an *effective* commitment to achieve higher levels of corporate transparency of cross-listed firms. As research setting, we use the adoption of the Section 302 of the Sarbanes-Oxley act that requires disclosing any discovered internal control deficiencies on internal controls over financial reporting. Using this research setting, we examine whether firm information environment benefits following cross-listed vanish when cross-listed firms mimic the adoption of effective internal controls

Our result shows that cross-listing is associated with an increase in the quality of the information environment only for firms that have effectively adopted stricter internal controls over financial reporting. We also find that a better information environment for cross-listed firms is achieved

through an increase in the higher precision of common information. On the other side, our evidence shows that cross-listed firms disclosing internal control deficiencies do not enjoy a better information environment and do not differentiate themselves, in terms of firm information environment, from their domestic peers. We also find that cross-listed firms that remediate to internal control deficiencies, experience an improvement in the quality of the information environment. We finally show that the association between the properties of the firm information environment and the effective adoption of stricter internal controls depends on the level of enforcement of the country in which the cross-listed firm is domiciled. We find that the difference in the benefits on the information environment between firms disclosing and not disclosing internal control deficiencies are stronger for firms domiciled in weak legal environment countries. Overall, our findings support the idea that the quality of the firm information environment increases following cross-listing only when cross-listed firms *effectively* commit themselves to higher levels of corporate transparency, and not merely in name just mimicking the adoption of stricter rules.

Appendix III.A: BKLS metrics

Barron et al. (1998) develop a model in which N financial analysts forecast expected earnings per share (y). Each analyst information set encompass a part of public (with precision h) information and a private (idiosyncratic) signal $z_i = y + \varepsilon_i$. Each ε_i is independent of all other variables and follows a normal distribution: $\varepsilon_i \sim N(0,1/s)$. In forming forecasts, each analyst assigns a weight to her common and private information according to their respective precision (h or s). To model the precision of common and private information, the model require the following assumption to hold: (1) analyst issued unbiased forecasts; (2) earnings forecast do not determine earnings realizations; (3) all analysts' idiosyncratic information is of equal precision; (4) forecast error are normally distributed. If these conditions hold, then the precision of public and private information could be expressed in term of the expected square error in the mean forecast (SE), expected forecast dispersion (D), and the number of analyst following (N) as follow:

$$H = (SE - D/N)/[(1 - 1/N)\,D + SE]^2 \quad \text{(A.1)}$$

$$S = D/[(1 - 1/N)\,D + SE]^2 \quad \text{(A.2)}$$

Analyst consensus is the ratio between the precision of analyst public information (H) and the precision of the total information (i.e. CONS= H/H+S). The Barron et al.'s model is based on unconditional expectations of dispersion and error in the mean forecast, but only realizations of these variables are available to construct proxies. We calculate for each firm *i* the ex-post realized forecast dispersion (D_{it}) and squared error in the mean forecast (SE_{it}) as follow:

$$\widehat{SE}_{it} = (A_{it} + \overline{FE_{it}})$$

$$\widehat{D}_{it} = \frac{1}{N_{it}-1}\sum_{i=1}^{N}(FE_{jit} - \overline{FE_{it}})$$

where:

FE_{jit}= is the forecast of year t annual earnings per share for firm i from analyst j from the 11th month of the fiscal year to three days before the annual earnings announcement;

$\overline{FE_{it}}$ = is the median of the individual FE_{jit} forecasts of year t annual earnings per share for firm i from the 11th month of the fiscal year to three days before the annual earnings announcement;

N_{it} = is the observed number of analyst j forecasts of year t annual earnings per share for firm i from the 11th month of the fiscal year to three days before the annual earnings announcement;

$\widehat{SE}_{it}$= is the estimated of square error in the mean forecast $\overline{FE_{it}}$ for firm i;

A_{it} = is the actual annual earnings per share for firm i in year t;

$\widehat{D}_{it}$ = is the estimated of dispersion from the observed forecasts for firm i.

We scale both $\widehat{D}_{it}$ and $\widehat{D}_{it}$ for the closing price as the end of year t. Next, we substitute them, within the number of analyst (N_{it}) into equations A.1 and A.2. to calculate H, S, and CONS. The Barron et al.'s model relies on the assumption that forecast accuracy and dispersion are related to the public and idiosyncratic components of bias in individual analyst forecast. On the one side, error in the mean forecast reflects error in the public information available to all the analysts that they use to make their forecast, on the other side forecast dispersion is related only to uniquely private (i.e. idiosyncratic) information upon which each individual analyst base her forecast.

Appendix III.B: Variable definition

Variable	Definition
XLIST	Binary variable equals to one if a firm is cross-listed, zero otherwise.
GOOD	Binary variable equals to one if a cross-listed firm discloses no internal control deficiency in year t, zero otherwise.
BAD	Binary variable equals to one if a cross-listed firm discloses an internal control deficiency in year t, zero otherwise.
UP	Binary variable equals to one if a cross-listed firm discloses an internal control deficiency in year *t-1* and no internal control deficiency in year *t.*
ACC	Analyst forecast accuracy computed as $ACCURACY_{it}$ = |Actual $Earnings_{it}$ – Median $Forecast_{it}$|/Stock $Price_{it,}$,
DISP	Analyst forecast dispersion computed as the Standard Deviation of Forecasts/Stock Price.
FOLL	Analyst following computed as the logarithm of the total number of analysts who issue at least one annual forecast for a given firm-year
H	Average precision of analyst public information
S	Average precision of analyst private information
CONS	Analyst consensus computed as H/(H+S)
LAW	Binary variable equals to one if a firm is incorporate in a country that is the above the sample median of the summation between the following legal environment variables taken from Kaufmann et al. (2007) for the year 2005: (1) Government Effectiveness; (2) Regulatory Quality; (3) Rule of Law; (4) Control of Corruption, zero otherwise.
LOSS	Binary variable equals to one if a firm actual earnings per share is less than zero, zero otherwise
σROA	Standard deviation of the return on assets over the past five years
ΔEAR	Absolute value of the difference between the current year's earnings per share and the last year's earnings per share
SKEW	Skewness of earnings over the past five years
SIZE	Natural logarithm of the total assets as the beginning of the year
LEV	Ratio between total debts and total assets as the beginning of the year
ROA	Ratio between net income and total assets as the beginning of the year

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