Journal of Accounting and Economics 53 (2012) 77–97

Contents lists available at SciVerse ScienceDirect

Journal of Accounting and Economics

journal homepage: www.elsevier.com/locate/jae

The effect of enforcement on timely loss recognition: Evidence from insider trading laws ☆

Sudarshan Jayaraman

Olin Business School, Washington University in St. Louis, Campus Box 1133, One Brookings Drive, St. Louis, MO 63130, United States

ARTICLE INFO

Article history:
Received 8 June 2010
Received in revised form
23 September 2011
Accepted 3 October 2011
Available online 12 October 2011

JEL classification:
M41
G15
K42

Keywords:
Timely loss recognition
Conservatism
Insider trading
Enforcement
Financial reporting

ABSTRACT

I use the first-time enforcement of insider trading laws in sixteen countries as a shock to enforcement and examine its influence on timely loss recognition (*TLR*). Consistent with greater enforcement increasing the usefulness of accounting information in contracts and thereby the demand for higher quality reporting, insider trading enforcement is associated with a significant increase in *TLR*. No such increase is detected in neighboring non-enforcing countries. In addition to documenting how shocks to enforcement influence financial reporting outcomes, this is also the first study to extend the Khan and Watts (2009) measure of accounting conservatism to a cross-country setting.

1. Introduction

I examine how first-time enforcement of insider trading laws affects the extent of timely loss recognition (*TLR*) in financial statements. A growing literature examines how cross-country variation in institutional characteristics shape financial reporting outcomes (e.g., Ball et al., 2000, 2003; Ball and Shivakumar, 2005; Bushman and Piotroski, 2006; Hail and Leuz, 2006). These studies find that variation in the demand for accounting information in contracts drives differences in the quality of financial reporting across countries. As the usefulness of accounting information in contracts depends on how well these contracts are enforced, the effectiveness of enforcement of securities laws is an important determinant of reporting quality.

I use the first-time enforcement of insider trading laws across sixteen countries as a shock to enforcement and examine its influence on *TLR*. Prior studies (e.g., Bekaert and Harvey, 1997, 2000; Bhattacharya and Daouk, 2002; Daouk et al., 2006) provide evidence that first-time enforcement of insider trading laws results in an overall increase in the level of enforcement of securities laws and property rights. Countries that enforce these laws for the first time follow it up with several initiatives designed to sustain the increased level of enforcement. These result in improvements in sovereign credit

☆ I thank S.P. Kothari (the editor) and Mark DeFond (the referee) for several helpful comments that have greatly improved the paper. Any errors are my own.

E-mail address: jayaraman@wustl.edu

0165-4101/$ - see front matter © 2011 Elsevier B.V. All rights reserved.
doi:10.1016/j.jacceco.2011.10.003

ratings and greater lending by foreign investors—the ones more likely to rely on financial statements for monitoring (Ball et al., 2000; Leuz et al., 2009).

Following studies that predict that improvements in enforcement increase the demand for higher quality financial information, I expect first-time insider trading enforcement to be associated with an increase in *TLR*. While Hail and Leuz (2006), Burgstahler et al. (2006) and Bushman and Piotroski (2006) provide evidence that enforcement is relevant for financial reporting outcomes and cost of capital, the cross-sectional design of these studies is likely to raise concerns about endogeneity and omitted variables. My study complements these studies by using a shock to enforcement and tracing its effect on reporting outcomes. The advantage of this design is that it uses each firm as its own control thus mitigating omitted variable bias.

Using the Khan and Watts (2009) firm-year measure of timely loss recognition (the *CSCORE*), I find strong evidence of an increase in *TLR* in the two years after first-time enforcement compared to the two years before. I also use a control sample of countries (based on matching each treatment country with a control country) that did not enforce insider trading laws and find no evidence of a change in *CSCORE* for these countries. These inferences are confirmed in a difference-in-difference design that compares changes in *CSCORE* in treatment firms with those in control firms. While the sensitivity of earnings to bad news is 40% higher than that to good news in enforcing countries even prior to enforcement, this differential sensitivity increases to 47% after first-time enforcement of insider trading laws (i.e., a relative change of 17%).

Next, I explore cross-sectional variation in the effect of insider trading enforcement. First, as the contracting demand for *TLR* depends on the use of accounting information in debt contracts, I expect post-enforcement *TLR* increases to be pronounced in firms that rely more on debt financing (Ball et al., 2008). Using the level of debt, I find that post-enforcement increases in *TLR* are indeed concentrated in firms with more debt. Second, I examine whether the presence of a strong internal monitor affects post-enforcement *TLR* increases. As firms with a strong monitor rely less on explicit contracting arrangements to mitigate agency conflicts (Bushman and Piotroski, 2006), I expect these firms to have smaller increases in *TLR*. Using ownership data from the sources in Claessens et al. (2000) and Faccio and Lang (2002), I find that *TLR* increases after first-time IT enforcement are restricted to firms without a majority shareholder. These results provide additional evidence in support of the contracting-based demand for *TLR*.

Third, I examine whether demand from equity markets influences the effect of IT enforcement on *TLR*. Lafond and Roychowdhury (2008) argue that agency conflicts between shareholders and managers are associated with differences in *TLR*. However, Ball et al. (2008) argue that equity markets are not the primary source of demand for *TLR*. Using equity issuances to capture the importance of equity markets, I find no evidence that post-enforcement increases in *TLR* are related to the importance of equity markets. It might seem counterintuitive that enforcement of insider trading laws, which pertain to trading behavior in securities markets, is associated with increases in *TLR* but that these increases are not related to the importance of equity markets. However, it should be noted that increases in *TLR* are driven by the heightened contracting-based demand for high quality financial reporting. My results suggest that this demand emanates from debt holders and not from equity holders.

While the above results are consistent with the effect of enforcement, they could also be consistent with several alternative explanations. I perform additional tests to rule these out. First, I verify that the results are not being driven by effects of IFRS adoption—both voluntary and mandatory. Second, I verify that my results are not due to improvements in corporate governance as enforcing countries start globally integrating their capital markets. Third, I include time-varying macroeconomic variables to mitigate concerns that changes in macroeconomic factors could be driving observed *TLR* changes and find that my results remain unaffected.

My results are also robust to several additional sensitivity tests. First, I include all available countries as controls (instead of one-to-one matching) and find robust results. Second, I examine whether IT enforcement potentially impacts other factors in the reporting environment. To do so, I examine changes in timely gain recognition (*GSCORE*). In contrast to the results for *CSCORE*, I find no change in *GSCORE* before versus after enforcement. Thus, IT enforcement appears to be related only to the incremental timeliness of bad news recognition and not to that of good news recognition. Third, I use the Basu (1997) model to measure *TLR* and find consistent results. Fourth, I allow the effect of country-level variables to vary over time and find robust results. Fifth, I verify that my results are not driven by any single country. Finally, I include firm and year fixed effects and find robust results.

My study offers several contributions to the literature. First, it uses first-time enforcement of insider trading laws to examine how *changes in* institutional features affect *changes in* financial reporting. This *changes* design treats every firm as its own control thereby mitigating omitted variable problems that affect cross-country comparisons. My results confirm predictions by Holthausen (2009) that enforcement has an important effect on financial reporting. Second, my study contributes to the literature on the debt-based contracting demand for *TLR* by documenting that post-enforcement increases in *TLR* are concentrated in firms that rely more on debt and not equity (see Ball et al., 2008). Third, my study contributes to the recent debate on the effectiveness of IFRS adoption. Studies such as Ball (1995, 2006), Leuz and Wysocki (2008), and Kothari et al. (2010) argue that convergence of financial reporting outcomes is unlikely to be achieved merely by adopting IFRS but by changing underlying institutional structures. My results provide corroborating evidence by documenting that within the subset of IFRS adopters, only those that enforce IT laws experience an increase in *TLR*. Fourth, my study answers the call of recent studies such as Leuz and Wysocki (2008) on the need to explore the effects of recent regulatory and enforcement changes in several countries on financial reporting outcomes. Finally, I extend the *CSCORE* measure of Khan and Watts (2009) to a cross-country setting.

The rest of the paper is as follows: Section 2 presents the motivation and hypotheses. Section 3 discusses the research design and Section 4 presents the results. Section 5 presents results of additional tests and Section 6 concludes.

2. Motivation and hypotheses

A growing literature examines how incentives resulting from cross-country variation in institutional characteristics shape financial reporting outcomes. Studies find that differences in the quality of financial reporting are driven by differences in the demand for accounting information in contracts and not necessarily from differences in mandated accounting standards. For example, Ball et al. (2000) show that common law countries where lending is more "arms-length" rely more on financial statements in their contracts and thus have more timely loss recognition than code law countries.[1] Similarly, Ball et al. (2003) document that financial reporting in four East Asian countries, purported to have high quality accounting standards, is of low quality due to low reporting incentives.

As the usefulness of accounting information in contracts depends on how well these contracts are enforced, the effectiveness of enforcement of securities laws is an important determinant of the quality of financial reporting. Hail and Leuz (2006) find that country-level implied costs of capital decrease with effective enforcement aspects of securities laws while Leuz et al. (2003), Burgstahler et al. (2006) and Bushman and Piotroski (2006) document the role of legal and judicial institutions on reporting outcomes.

Recent studies highlight the importance of enforcement and call for more research on its effects on financial reporting. For example, Leuz and Wysocki (2008) indicate that there are many opportunities to examine the optimal form and implementation of regulation and enforcement across markets and countries. Similarly, Guay and Verrecchia (2006) state that links between enforcement and accounting information in contracts are critical to our understanding of how institutions influence accounting reports and call for more direct research along these lines. Holthausen (2009, pp. 453–456) opines "…At this point, I do not think we have very strong evidence to help us fully understand the importance of enforcement with respect to financial reporting outcomes". He also notes that "…one place for future research would be to try to investigate the effects of changes in enforcement on reporting outcomes…looking for places where enforcement changed and other institutions were held constant might lead us to a further understanding on the role of enforcement".

2.1. First-time enforcement of insider trading laws

Examining the effect of enforcement on reporting outcomes is far from trivial. Scholars in the area of law such as Mahoney (2009) and Coffee (2007) state that legal indices based on enforcement rules "on the books" are likely to miss large differences in legal institutions because coding substantive and procedural rules fails to capture how the law is actually used. Coffee (2007, p. 264) states that in order to understand the role of enforcement on economic outcomes "…it is useful to focus on a specific form of illegal behavior that is contrary to law in virtually all countries, and yet appears to occur systematically. Insider trading satisfies both of these conditions. It has been criminalized by virtually all jurisdictions with securities markets. Yet, it persists. Thus, it supplies an ideal context in which to examine relative enforcement intensity."

Motivated by the above studies, I use first-time enforcement of insider trading laws across sixteen countries as a shock to enforcement. Prior studies find that first-time enforcement is followed by several initiatives taken by countries to increase the overall level of property rights. Bekaert and Harvey (1997, 2000) provide a detailed analysis of financial, economic and political events in several emerging markets and find that countries follow up first-time insider trading enforcement with reforms to achieve a sustained improvement in enforcement of laws and to attract foreign investment.[2] For example, after enforcing insider trading laws for the first time in 1996, Chile introduced a series of reforms to its financial sector including easing up reserve requirements for foreign direct investments and substantially reforming its banking regulations. Similar initiatives were undertaken in other countries such as India (which followed up insider trading enforcement by signing an agreement with the U.S. SEC to share information and help in the enforcement of their respective countries' securities laws) and Argentina (which implemented a series of initiatives to remove entry barriers and branching restrictions on the banking sector after enforcing insider trading laws).

Consistent with these reforms improving countries' borrowing ability, Bhattacharya and Daouk (2002) find that first-time enforcement has a positive effect on country credit ratings. Similarly, Daouk et al. (2006) find that first-time enforcement results in a significant increase in lending by foreign investors—the ones more likely to rely on public financial statements to monitor borrowers (Ball et al., 2000; Leuz et al., 2009). Thus, the beneficial effects of insider trading enforcement are not isolated to the stock market but appear to ease financing constraints of firms more generally. These initiatives result in increases in borrowing in general, and in borrowing from foreign investors in particular. It is pertinent to note that first-time enforcement is not likely to be exogenous as countries will enforce these laws when they expect the associated benefits to be the largest. Bhattacharya and Daouk (2009) perform a detailed analysis of when countries enforce insider trading laws and find that first-time enforcement occurs when the quality of bureaucracy improves and as a response to increasing corruption.

[1] See Watts (1977, 2003a, b), Watts and Zimmerman (1986), Ball (2001), Holthausen and Watts (2001) and Ball and Shivakumar (2005) on the role of timely loss recognition in alleviating agency conflicts.

[2] A detailed list of events for each country is available at http://www.duke.edu/~charvey/Country_risk/couindex.htm.

Following prior studies such as Ball et al. and Leuz et al. who argue that arms-length investors demand higher quality financial reporting information to mitigate agency conflicts, I expect first-time enforcement of insider trading laws to be associated with an increase in the quality of financial reporting. I use timely loss recognition (*TLR*) as the key construct that encapsulates financial reporting quality. My first hypothesis (in the alternative) is as follows:

H1. Timely loss recognition increases after first-time enforcement of insider trading laws.

2.2. Role of contracting-based demand

As pointed out by studies such as Ball and Shivakumar (2005), the contracting explanation for *TLR* appeals to the role of accounting in supporting debt contracts, where *TLR* grants the providers of debt capital a mechanism to monitor managers and to respond to declining credit worthiness in a timely manner. Thus, if greater enforcement of insider trading laws increases the relevance of accounting numbers in financial contracts and thus the demand for *TLR*, then this increase is likely to be pronounced in environments with greater reliance on debt contracts. This leads to my second hypothesis:

H2. The effect of first-time enforcement of insider trading laws on timely loss recognition is stronger in firms with higher contracting demands.

2.3. Strong internal monitors

Bushman and Piotroski (2006) find that the presence of a strong internal monitor reduces the need for explicit contracting arrangements, and thus the demand for *TLR*. They use the average percentage of common shares owned by the three largest shareholders in the 10 largest domestic firms as the measure of the importance of internal monitoring and find that these countries are characterized by lower *TLR*. Following Bushman and Piotroski (2006), I examine whether a strong internal monitor weakens the effect of insider trading enforcement on *TLR*. As firms with a strong internal monitor are less likely to use explicit contracting arrangements to reduce agency conflicts, I expect post-enforcement increases in *TLR* to be pronounced in firms without a strong internal monitor. This leads to my third hypothesis:

H3. The effect of first-time enforcement of insider trading laws on timely loss recognition is stronger in firms without a strong internal monitor.

2.4. Role of equity markets

Finally, I examine whether greater demand from equity market participants drives post-enforcement increases in *TLR*. On one hand, studies such as Lafond and Roychowdhury (2008) find that *TLR* is higher in firms with lower managerial equity ownership suggesting that agency conflicts between shareholders and managers are associated with differences in *TLR*. On the other hand, Ball et al. (2008) argue that equity markets are not the primary source of demand for *TLR*, because given the total information in the stock price, the proportion from one source or another seems a second-order concern to the equity market. My fourth (non-directional) prediction is as follows:

H4. Post-enforcement increases in TLR are unrelated to the importance of equity markets.

3. Research design and data

I now describe the empirical proxies, regression specifications and samples used.

3.1. Timely loss recognition (TLR)

Following prior studies, I define *TLR* (or conditional conservatism) as the extent to which earnings reflect bad news during the period faster than good news. I measure *TLR* using the Khan and Watts (2009) firm-year *CSCORE* measure.[3] The *CSCORE* draws on the Basu (1997) measure of asymmetric timeliness to estimate a firm-year measure of conservatism. Following Khan and Watts (2009), I specify the following Basu (1997) specification and estimate it individually for each country:

$$EARN_{i,t} = \beta_1 + \beta_2 NEG_{i,t} + \beta_3 RET_{i,t} + \beta_4 RET \times NEG_{i,t} + \varepsilon \quad (1)$$

where $EARN_{i,t}$ is the annual earnings of firm i in year t, $NEG_{i,t}$ is an indicator that equals one if $RET_{i,t}$ is negative and zero otherwise, $RET_{i,t}$ denotes buy-and-hold return over the year. The coefficient on $RET_{i,t}$ (β_3) measures the timeliness of earnings with respect to good news (positive returns); while the coefficient on $RET \times NEG_{i,t}$ (β_4) captures the incremental timeliness of earnings with respect to bad news (negative returns). The coefficient on β_4 measures the extent of *TLR*.

[3] Sensitivity tests (discussed in Section 5.3) verify that the inferences are robust to using the Basu (1997) measure.

To estimate timeliness with which accounting reflects both good news and bad news at the firm-year level, Khan and Watts (2009) specify that both the timeliness of good news (referred to as the *GSCORE*) and the incremental timeliness of bad news (*CSCORE*) are linear functions of firm-specific characteristics. In other words,

$$GSCORE_{i,t} = \beta_3 = \mu_1 + \mu_2 SIZE + \mu_3 MB + \mu_4 LEV \tag{2}$$

$$CSCORE_{i,t} = \beta_4 = \lambda_1 + \lambda_2 SIZE + \lambda_3 MB + \lambda_4 LEV \tag{3}$$

CSCORE is the firm-year measure of conservatism, or incremental bad news timeliness while *GSCORE* is the firm-year measure of good news timeliness. Although Eq. (1) is estimated by country, both *CSCORE* and *GSCORE* vary across firms as well as over time within each country through cross-sectional variation in the firm-year characteristics (i.e., size, market-to-book and leverage). I retain countries with at least 20 observations to estimate Eq. (1).[4] Following Khan and Watts (2009), I use the following regression model to estimate *CSCORE* and *GSCORE*:

$$\begin{aligned} EARN = {} & \beta_1 + \beta_2 NEG + RET(\mu_1 + \mu_2 SIZE + \mu_3 MB + \mu_4 LEV) + RET \times NEG(\lambda_1 + \lambda_2 SIZE + \lambda_3 MB + \lambda_4 LEV) \\ & + (\delta_1 SIZE + \delta_2 MB + \delta_3 LEV + \delta_4 NEG \times SIZE + \delta_5 NEG \times MB + \delta_6 NEG \times LEV) + \varepsilon \end{aligned} \tag{4}$$

3.2. Insider trading enforcement

To examine changes in *CSCORE* around enforcement of insider trading laws, I follow Bertrand and Mullainathan (1999, 2003) and implement a difference-in-difference specification, where I examine the change in *CSCORE* between the pre and the post enforcement period for treatment firms (i.e., those in countries that enforce insider trading laws) and compare these changes to those for control firms (i.e., firms in countries that do not enforce these laws). To do so, I define two indicator variables. The first is *POST* which denotes the pre versus post enforcement periods. *POST* takes the value of 1 for observations in the post-enforcement period and 0 in the pre-enforcement period. I define the post-enforcement period as two years after passage of insider trading laws (excluding the year of enforcement).[5] Similarly, I define the pre-enforcement period as the two years before the year of passage.

The second indictor is *IT* which denotes treatment versus control countries. *IT* takes the value of 1 for countries that enforce insider trading laws during the sample period (i.e., treatment countries) and 0 for the countries that do not enforce these laws during this period (i.e., control countries). To examine the differential effect of enforcement of insider trading laws on treatment firms versus control firms, I interact *POST* with *IT*. The coefficient on $POST \times IT$ captures the incremental effect of enforcement on *CSCORE* for treatment firms relative to control firms.

3.3. Control variables

I include three sets of control variables. First, following Callen et al. (2010), I include annual sales growth (*GROWTH*) and research and development scaled by total sales (*R&D*) to control for growth opportunities, the investment cycle (*INVCYCLE*) measured as depreciation divided by total assets and stock price volatility (*VOL*) to capture firm-specific uncertainty.[6] In addition, I include return on equity (*ROE*) and market to book (*MB*) as additional controls. The second set pertains to firm-level controls for voluntary IFRS adoption. Following Daske et al. (2009), I employ a binary variable (*IFRS*) to indicate voluntary IFRS adoption. *IFRS* takes the value of 1 if the "Accounting Standards" field in Global Vantage indicates that the firm has adopted international standards. In addition, I include firm-level variables likely to capture incentives to provide high quality financial information. Following Daske et al., I include log of total assets (*LNASSETS*) to control for firm size, stock return (*RET*) to capture performance, and foreign income as a percentage of sales (*FOREIGN*) to capture international operations. Further, I include industry effects defined at the 4 digit SIC code level to capture growth opportunities.[7] I also include an indicator variable to denote the presence of a Big Eight auditor (*BIG8*).

The third set of controls is country-level factors to capture variables correlated with the country's decision to enforce insider trading laws. I include time-varying factors such as the level of per capita GDP (*GDP*), the ratio of equity market capitalization to GDP (*EQMKTCAP*), per capita GDP growth (*GDPGROWTH*) and the annual rate of inflation (*INFLATION*). To capture differences in access to capital, I include the Djankov et al. (2007) time-varying measure of creditor rights (*CREDRIGHTS*). I include the amount of net foreign direct investment inflows (*FDI*) to control for differences in external financing.[8] Finally, I include time-invariant institutional factors using legal-origin based fixed effects (i.e., English, French, and Scandinavian origin). The omitted group is German. All variables are winsorized at the 1% and 99% tails and are

[4] Results are significant at less than 1% level (one-sided) when the restriction is increased to 100 observations.

[5] Results are robust to including the year of enforcement in the post-enforcement period, suggesting that the stricter enforcement regime was (at least in part) anticipated.

[6] While Callen et al. also include firm age, I exclude it because firm age cannot be reliably computed for my sample firms as Global Vantage data are available only from 1992 onwards.

[7] While Daske et al. include ownership concentration, I exclude it because these data are not available for a majority of my sample firms. However, results are robust to including it and are discussed in Section 5.5.

[8] I also interact the country-level variables with *POST* to allow their effect on *CSCORE* to vary between the pre and the post enforcement periods. Results are robust to this specification and are discussed in Section 5.4.

defined in Appendix A. The regression specification is:

$$\begin{aligned} CSCORE = {} & \eta_0 + \eta_1 POST + \eta_2 IT + \eta_3 POST \times IT + \eta_4 ROE + \eta_5 MB + \eta_6 GROWTH \\ & + \eta_7 R\&D + \eta_8 INVCYCLE + \eta_9 VOL + \eta_{10} IFRS + \eta_{11} BIG8 + \eta_{12} LNASSETS \\ & + \eta_{13} RET + \eta_{14} FOREIGN + \eta_{15} GDP + \eta_{16} EQMKTCAP + \eta_{17} GDPGROWTH \\ & + \eta_{18} INFLATION + \eta_{19} CREDRIGHTS + \eta_{20} FDI + \sum Industry + \sum LEGOR + \varepsilon \end{aligned} \quad (5)$$

where *LEGOR* refers to the legal origin. The robust standard errors are clustered by firm to mitigate concerns about serial correlation. Hypothesis *H1* predicts that $\eta_3 > 0$.

3.4. Sample

My data come from four main sources. Data on first-time insider trading enforcement are from Bhattacharya and Daouk (2002, pp. 80–84). Data on accounting income and dividends are from the Global Vantage Industrial/Commercial (IC) file while stock price data are from the Global Vantage Issue file. Data on firm-level ownership structure are from the sources in Claessens et al. (2000) and Faccio and Lang (2002). I use two samples for the empirical analyses. The first (used to estimate *CSCORE*) is based on available data for all countries with at least 20 observations between the years 1992 and 2001. I require at least 20 observations to reliably estimate *CSCORE* and *GSCORE*. This sample comprises of 85,955 firm-year observations across 47 countries. The second sample uses data from two years before and two years after the year of insider trading enforcement and comprises of 10,164 firm-year observations for 27 countries (16 countries and 11 control countries).

4. Results

4.1. Validating the Khan and Watts (2009) measure

As this is the first study to use the Khan and Watts firm-year measure of *CSCORE* in an international context, I first verify that this measure is comparable to the country-level design based on Basu (1997) used in prior studies (e.g., Ball et al. 2000).[9] To do so, I compare the properties of *CSCORE* and *GSCORE* across countries with different legal origins. Ball et al. find that firms in common law regimes are timelier with respect to the recognition of bad news than those in code law countries. On the other hand, firms in code law countries are quicker to recognize good news than their counterparts in common law countries. Table 1 presents results of the validation. Panel A presents descriptive statistics of the sample. This sample comprises of 85,955 observations across 47 countries between the years 1992 and 2001. In Panel B, I split the sample based on legal origin and present the mean and median values of *CSCORE* and *GSCORE* for each sub-sample. Common law firms have a higher mean of *CSCORE* (0.099) compared to code law firms (0.003). These differences are highly significant. Further, the median *GSCORE* is consistently lower for common law firms (0.002) compared to code law firms (0.020). These differences are also statistically significant. Overall, results based on *CSCORE* and *GSCORE* are consistent with those based on the Basu model. Appendix B presents additional analyses to validate the use of the Khan and Watts model in my cross-country setting.

4.2. Insider trading enforcement—event-study

4.2.1. Treatment countries and control countries

Bhattacharya and Daouk (2002) find that 29 countries enforce insider trading laws for the first time since 1990. As Global Vantage data start in 1992 and I need two years of pre-enforcement data, 7 countries with pre-1994 enforcement dates (Czech Republic, Finland, Japan, Norway, Poland, Sweden and Thailand) are excluded. Six additional countries (Bangladesh, Hungary, Oman, Peru, Sri Lanka and Slovenia) are dropped due to insufficient data in both the pre and post periods. The final sample consists of 5,704 observations from 16 treatment countries with enforcement dates between 1994 and 1998 and with available data for the pre and post periods on Global Vantage.

Panel A of Table 2 presents a list of these 16 countries—Argentina, Australia, Belgium, Chile, Denmark, Germany, Greece, Hong Kong, India, Indonesia, Italy, Malaysia, Netherlands, Spain, Switzerland and Turkey. The most frequent enforcement year is 1996 (8 countries), followed by 1994 and 1995 (3 countries each) and finally 1998 (2 countries). In terms of individual countries, the most observations are from Malaysia (990) followed by Germany (918) and Australia (743). To select control countries, I match each enforcing country with a country in the same region, but which has not enforced inside trading laws. This one-to-one matching allows me to unambiguously assign an event date to the control country. Control countries are presented under the column "Matched country" and are of two kinds—(i) those that have not enforced insider trading laws till the end of the sample period (i.e., 2000) and (ii) those that have enforced these laws before the start of the sample period (i.e., 1992). The column entitled "Actual enforcement year" indicates the year of

[9] I also examine whether the association between *CSCORE* and firm-level characteristics is similar to that shown in Khan and Watts (2009). These are discussed in Appendix B.

Table 1
Validating the Khan and Watts (2009) measures of *CSCORE* and *GSCORE*.

The sample is based on Global Vantage data for the period 1992 to 2001 and comprises of 85,955 firm-year observations across 47 countries with at least 20 observations per country. Classification of countries into code law and common law is based on the classification of LaPorta et al. (1997).

Panel A: List of countries

Country	Obs.	Legal origin	Country	Obs.	Legal origin
Argentina	119	Code	Mexico	325	Code
Australia	1,879	Common	Netherlands	1,032	Code
Austria	482	Code	New Zealand	270	Common
Belgium	434	Code	Norway	588	Code
Brazil	477	Code	Pakistan	179	Common
Canada	3,923	Common	Peru	56	Code
Chile	384	Code	Philippines	465	Code
China	319	Code	Poland	133	Code
Colombia	84	Code	Portugal	213	Code
Czech Republic	70	Code	Russia	46	Code
Denmark	621	Code	Singapore	1,431	Common
Finland	464	Code	Slovakia	25	Code
France	2,865	Code	South Africa	545	Common
Germany	3,052	Code	South Korea	550	Code
Greece	235	Code	Spain	809	Code
Hong Kong	705	Common	Sweden	910	Code
Hungary	58	Code	Switzerland	923	Code
India	1,166	Common	Taiwan	739	Code
Indonesia	992	Code	Thailand	1,738	Common
Ireland	158	Common	Turkey	194	Code
Israel	182	Common	United Kingdom	7,289	Common
Italy	814	Code	United States	26,141	Common
Japan	19,039	Code	Venezuela	33	Code
Malaysia	2,799	Common			
			Total	**85,955**	

Panel B: Comparison of *CSCORE* and *GSCORE* across legal origins

The sample is based on Global Vantage data for the period 1992 to 2001 and comprises of 85,955 firm-year observations across 47 countries with at least 20 observations per country. Classification of countries into common law and code law is based as per LaPorta et al. (1997). *CSCORE* and *GSCORE* indicate the Khan and Watts (2009) firm-year measures of incremental timeliness of bad news recognition and timeliness of good news recognition, respectively.

	Common law *(N=48,405)*		**Code law** *(N=37,550)*		**Difference (Common − Code)**	
	Mean	**Median**	**Mean**	**Median**	***t*-test**	***W*-test**
CSCORE	0.099	0.094	0.003	0.014	64.05	66.66
GSCORE	0.040	0.002	0.056	0.020	−26.67	−52.37

enforcement for control countries that fall in the latter group. Countries in the former group are denoted by "N/E" (Not Enforced).

For example, Argentina which enforced insider trading laws in 1995 is matched with Brazil that enforced these laws in 1978. Similarly, Denmark (enforcement year of 1996) is matched with Austria which has not enforced its insider trading laws until 2000. There are some instances where the same control country is matched with more than one treatment country. This happens if all the treatment countries are in the same region and have the same enforcement year. For example, both Malaysia and Indonesia enforced insider trading laws for the first time in 1996 and are matched with Philippines which has not enforced its insider trading laws until the end of the sample period. Similarly, both Germany and Switzerland (enforcement year of 1995) have been matched with United Kingdom which enforced insider trading laws in 1981. The control sample comprises of 4,460 firm-year observations. The overall sample has 10,164 firm-year observations for 27 countries (16 treatment and 11 control countries) between 1992 and 2000. The indicator *IT* takes the value 1 for treatment countries and 0 for control countries.

4.2.2. Descriptive statistics

Panel B of Table 2 presents descriptive statistics for treatment and control samples and by pre and post periods. The mean *CSCORE* for the treatment sample increases from −0.003 to 0.024 between the pre and post periods. The medians are both positive and increase from 0.003 to 0.010. In contrast, both the mean and median *GSCORE* decrease, with the latter being significant. Turning to the control sample, both the mean and median *CSCORE* are negative and also increase between the pre and post periods. This univariate evidence should, however, be interpreted cautiously as several firm and

Table 2
Insider trading enforcement dates.

Insider trading enforcement dates are obtained from Bhattacharya and Daouk (2002, pp. 80–84). The sample is based on 10,164 firm-year observations across 27 countries (16 countries that enforced insider trading laws during the sample period (1992–2000) and 11 control countries that did not). "Enforcement Year" represents the year of the first-time enforcement of insider trading laws. "Matched country" represents the country that is in the same geographical area as the enforcing country but did not itself enforce these laws during the sample period. The sample of matched countries include those that have not enforced insider trading laws until the end of the sample period as well as those that enforced these laws before the start of the sample period. "Actual enforcement year" indicates the year of insider trading enforcement for control countries that have enforced these laws before the start of the sample period. "N/E" indicates that the control country has not enforced insider trading laws until the end of the sample period.

Panel A: First-time insider trading enforcement countries and enforcement dates

IT country	Obs.	Enforcement year	Matched country	Actual enforcement year	Obs.
Argentina	30	1995	Brazil	1978	107
Australia	743	1996	New Zealand	N/E	83
Belgium	104	1994	France	1975	679
Chile	108	1996	Mexico	N/E	131
Denmark	260	1996	Austria	N/E	192
Germany	918	1995	United Kingdom	1981	2483
Greece	58	1996	Austria	N/E	192
Hong Kong	190	1994	Singapore	1978	364
India	639	1998	Pakistan	N/E	96
Indonesia	338	1996	Philippines	N/E	153
Italy	312	1996	Austria	N/E	192
Malaysia	990	1996	Philippines	N/E	153
Netherlands	286	1994	France	1975	679
Spain	385	1998	Portugal	N/E	109
Switzerland	279	1995	United Kingdom	1981	2483
Turkey	64	1996	Israel	1989	63
Total	**5,704**				**4,460**

Panel B: Descriptive statistics—By treatment and control groups and pre and post periods

Means (medians) are tabulated without (within) brackets. All variables are defined in Appendix A. *** and ** indicate significance at the 1% and 5% level, respectively.

	Treatment countries *(N=5,704)*			**Control countries** *(N=4,460)*		
	Pre	**Post**	**Diff.**	**Pre**	**Post**	**Diff.**
CSCORE	−0.003	0.024	0.027***	−0.162	−0.137	0.025***
	(0.003)	(0.010)	(0.007)***	(−0.137)	(−0.119)	(0.018)**
GSCORE	0.065	0.063	−0.002	0.159	0.148	−0.011***
	(0.061)	(0.052)	(−0.009)***	(0.166)	(0.155)	(−0.011)***
Firm-level controls						
ROE	0.032	0.016	−0.016**	0.018	0.020	0.002
	(0.046)	(0.045)	(−0.001)	(0.052)	(0.058)	(0.006)***
MB	54.952	58.968	4.016	11.080	7.537	−3.543
	(0.971)	(0.811)	(−0.160)***	(0.967)	(0.882)	(−0.085)***
GROWTH	0.153	0.130	−0.023	0.133	0.125	−0.008
	(0.067)	(0.061)	(−0.006)**	(0.058)	(0.062)	(0.004)
R&D	0.004	0.004	0.000	0.005	0.007	0.002
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
INVCYCLE	0.046	0.043	−0.003***	0.043	0.041	−0.002
	(0.040)	(0.037)	(−0.003)***	(0.039)	(0.037)	(−0.002)**
VOL	2.374	2.429	0.055	1.129	1.058	−0.071
	(1.777)	(1.669)	(−0.108)	(0.246)	(0.323)	(0.077)**
BIG8	0.674	0.656	−0.018	0.675	0.694	0.019
	(1.000)	(1.000)	(0.000)	(1.000)	(1.000)	(0.000)
LNASSETS	7.342	7.206	−0.136**	6.096	6.087	−0.009
	(7.157)	(7.014)	(−0.143)**	(5.814)	(5.947)	(0.133)
RET	0.196	0.132	−0.064***	0.300	0.217	−0.083***
	(0.069)	(0.000)	(−0.069)***	(0.158)	(0.061)	(−0.097)***
FOREIGN	0.000	−0.002	−0.002***	0.000	−0.001	−0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
IFRS	0.033	0.036	0.003	0.033	0.034	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Country-level controls						
GDP	14.258	13.426	−0.832***	18.211	18.499	0.288
	(17.758)	(18.010)	(0.252)***	(19.781)	(21.704)	(1.923)***
EQMKTCAP	0.824	0.754	−0.070***	0.936	1.056	0.120***

Table 2 (*continued*)

Panel B: Descriptive statistics—By treatment and control groups and pre and post periods

Means (medians) are tabulated without (within) brackets. All variables are defined in Appendix A. *** and ** indicate significance at the 1% and 5% level, respectively.

	Treatment countries (*N=5,704*)			**Control countries** (*N=4,460*)		
	Pre	**Post**	**Diff.**	**Pre**	**Post**	**Diff.**
	(0.402)	(0.653)	(0.251)***	(1.161)	(1.458)	(0.297)***
GDPGROWTH	4.327	1.696	−2.631***	3.159	3.175	0.016
	(4.054)	(3.116)	(−0.938)***	(2.661)	(3.103)	(0.442)***
INFLATION	4.092	7.078	2.986***	4.828	3.926	−0.902***
	(3.459)	(2.585)	(−0.874)***	(2.136)	(2.892)	(0.756)***
CREDRIGHTS	2.670	2.556	−0.114***	3.001	2.729	−0.272***
	(3.000)	(3.000)	(0.000)***	(4.000)	(4.000)	(0.000)***
FDI	2.022	2.081	−0.059	1.780	3.203	1.423***
	(1.546)	(1.638)	(0.092)	(1.608)	(2.295)	(0.687)***

Panel C: Changes in *CSCORE* between the pre and post period—by country

This table presents changes in *CSCORE* by country. Detailed variable definitions are in Appendix A. *** and ** indicate significance at the 1% and 5% level, respectively.

	Pre-period			**Post-period**			**Diff.**	
	Obs.	**Mean**	**Median**	**Obs.**	**Mean**	**Median**	**Mean**	**Median**
Treatment								
Argentina	6	0.020	0.021	24	0.047	0.051	0.027***	0.031***
Australia	288	0.016	0.021	455	0.041	0.047	0.025***	0.026***
Belgium	28	−0.047	−0.045	76	0.006	0.006	0.054***	0.051***
Chile	20	0.023	0.047	88	0.019	0.002	−0.004***	−0.045***
Denmark	94	−0.009	−0.001	166	0.015	0.024	0.023***	0.025***
Germany	358	−0.001	0.007	560	0.027	0.037	0.028***	0.030***
Greece	4	−0.028	−0.027	54	−0.009	−0.018	0.019***	0.009***
Hong Kong	69	−0.017	−0.013	121	0.003	0.007	0.020***	0.020***
India	177	−0.018	−0.015	462	0.009	0.014	0.027***	0.029***
Indonesia	84	0.046	0.072	254	0.070	0.077	0.024***	0.004***
Italy	90	0.001	0.013	222	0.039	0.045	0.038***	0.032***
Malaysia	301	0.009	0.009	689	0.017	0.024	0.007***	0.014***
Netherlands	121	0.003	0.008	165	0.032	0.037	0.029***	0.029***
Spain	186	−0.024	−0.048	199	0.024	0.032	0.049***	0.080***
Switzerland	87	−0.071	−0.067	192	−0.026	−0.015	0.046***	0.052***
Turkey	9	0.048	0.019	55	0.100	0.105	0.052***	0.086***
Overall	**1,922**	**−0.003**	**0.003**	**3,782**	**0.024**	**0.010**	**0.027*****	**0.007*****
Control								
Austria	65	−0.444	−0.455	127	−0.392	−0.396	0.052***	0.059**
Brazil	38	0.025	0.010	69	−0.034	0.003	−0.059***	−0.006***
France	213	−0.432	−0.426	466	−0.311	−0.294	0.121***	0.132***
Israel	9	0.024	−0.032	54	−0.174	−0.200	−0.198***	−0.168***
Mexico	44	−0.147	−0.154	87	0.037	0.001	0.184***	0.155***
New Zealand	25	−0.054	−0.031	58	−0.068	−0.061	−0.014***	−0.029***
Pakistan	11	0.008	0.028	85	0.068	0.072	0.060***	0.044**
Philippines	10	0.055	0.084	143	0.054	0.033	−0.001***	−0.051***
Portugal	64	−0.255	−0.254	45	−0.213	−0.258	0.042***	−0.003***
Singapore	95	−0.011	−0.021	269	−0.030	−0.023	−0.019***	−0.002***
UK	984	−0.110	−0.082	1,499	−0.125	−0.105	−0.014***	−0.023***
Overall	**1,558**	**−0.162**	**−0.137**	**2,902**	**−0.137**	**−0.119**	**0.025*****	**0.018*****

country-level factors are also changing during the sample period. The values of *CSCORE* are much lower and the distribution more skewed for this sample of 27 countries than the larger sample of Table 1.

The control variables are divided into two categories—firm-level and country-level. Sample firms are growing, as can be seen by the values of sales growth (*GROWTH*) that vary between 12.5% and 15.3% for the entire sample. Big 8 auditors audit around two-thirds of the sample while 3.3% of the sample firms have voluntarily adopted IFRS standards. These adoption numbers are similar to those reported by Daske et al. (2009). The mean GDP growth rate is decreasing in the treatment countries (−2.63%) while it is increasing marginally for the control countries. Similarly, net FDI inflows are decreasing marginally in treatment countries but increasing in control countries.

Table 3
Effect of insider trading enforcement on timely loss recognition.

The dependent variable is *CSCORE*. *POST* is an indicator variable that takes 1 during the post enforcement period and 0 for the pre period. *IT* is an indicator which denotes insider trading enforcement countries. Detailed variable definitions are in Appendix A. *** and ** indicate significance at the 1% and 5% level, respectively. Significance levels are based on a one-sided test for *POST* (for treatment firms) and *POST* × *IT* and on a two-sided test for all other variables.

	Treatment countries (1)		Treatment countries (2)		Control countries (3)		Entire sample (4)	
	Coeff.	*t*-stat	Coeff.	*t*-stat	Coeff.	*t*-stat	Coeff.	*t*-stat
Intercept	0.296***	7.10	0.507***	8.62	0.687***	8.18	0.278***	6.64
POST	0.025***	5.10	0.023***	4.71	0.004	0.58	0.011	1.74
IT							0.150***	13.86
POST × *IT*							0.021***	2.52
ROE	0.062***	3.57	0.060***	3.43	0.000	−0.01	0.038**	2.37
MB	0.000***	5.80	0.000***	5.81	0.000***	−3.29	0.000	1.79
GROWTH	−0.007	−1.29	−0.003	−0.56	0.009	1.03	0.005	0.99
R&D	−1.050***	−3.35	−0.942***	−3.05	−0.303	−1.40	−0.600***	−2.99
INVCYCLE	−0.069	−0.60	0.046	0.40	0.438**	2.17	0.142	1.25
VOL	−0.011***	−3.50	−0.013***	−4.05	0.006	0.86	−0.008***	−2.83
IFRS	−0.131***	−4.59	−0.124***	−4.37	0.001	0.05	−0.087***	−3.96
BIG8	−0.003	−0.47	−0.003	−0.47	−0.014	−1.30	−0.009	−1.33
LNASSETS	−0.013***	−5.39	−0.018***	−6.39	−0.087***	−19.89	−0.045***	−17.51
RET	−0.004	−0.90	−0.005	−1.16	−0.023***	−3.85	−0.004	−1.09
FOREIGN	−1.014***	−3.63	−0.789***	−2.78	−0.906	−1.37	−0.325	−1.04
GDP	0.450	0.92	−0.320	−0.56	−20.666***	−14.09	−5.858***	−10.00
EQMKTCAP	−0.007	−1.37	−0.039***	−4.53	0.012	0.63	0.002	0.24
GDPGROWTH	0.003***	5.57	0.001**	2.37	0.005***	2.70	0.008***	11.64
INFLATION	0.000	1.82	0.000	−0.11	0.002**	2.19	0.002***	5.46
CREDRIGHTS	−0.060***	−6.84	−0.083***	−8.01	−0.017***	−2.57	−0.010	−1.85
FDI			0.010***	2.77	0.014***	6.60	0.007***	4.38
Industry effects	Yes		Yes		Yes		Yes	
Institution effects	No		No		No		Yes	
Adj. R^2	0.18		0.19		0.58		0.35	
Obs.	5,704		5,478		4,460		9,938	

Panel C provides country-wise evidence of changes in *CSCORE* between the pre and the post periods. There is a positive and significant increase in *CSCORE* for most of the treatment countries, indicating that the results are unlikely to be driven by any single country. In terms of the control countries, most experience an insignificant change in *CSCORE*, with some experiencing a significant increase while others a significant decrease.

4.2.3. The effect of insider trading enforcement on timely loss recognition

Table 3 presents results of the effect of insider trading enforcement on *CSCORE*. The first two models present results for enforcing countries. Model 1 excludes *FDI* as these data are not available for all treatment countries while Model 2 includes it. Model 3 pertains to control firms and Model 4 presents results for the entire sample. The coefficient on *POST* is positive and significant at the 1% level (based on a one-sided test) in models (1) and (2), indicating that insider trading enforcement is associated with a significant increase in *CSCORE*.[10] These results are consistent with hypothesis *H1*. On the other hand, the coefficient on *POST* is insignificant in Model 3, suggesting that non-enforcing countries do not appear to experience an increase in *TLR*. The above inferences are confirmed in the difference-in-difference design of Model 4. The coefficient on *POST* is insignificant while that on *POST* × *IT* is positive and significant (at the 1% level, one-sided), indicating a strong increase in *CSCORE* for treatment firms.

To provide an idea of economic magnitude, I first examine how the sensitivity of earnings to good news and bad news varies by *CSCORE*. To do so, I estimate the Basu (1997) model and interact each variable with *CSCORE*.[11] Not surprisingly, the incremental sensitivity of earnings to bad news relative to good news is increasing in *CSCORE* (i.e., the coefficient on *RET* × *NEG* × *CSCORE* is positive and significant). Given pre-enforcement *CSCORE* of 0.15 (the coefficient on *IT*), a 20% positive stock return increases earnings in enforcing countries by 6.26% while a 20% negative stock return decreases earnings by 8.78%. Thus, earnings are more sensitive to bad news than good news by 40%. The value of *CSCORE* after

[10] Bushman and Piotroski (2006) also examine the relation between insider trading enforcement and *TLR*. Using a sample of 38 countries which includes enforcers and non-enforcers, they interact the enforcement indicator with the effectiveness of the legal/judicial system and find an insignificant coefficient on this interaction. There are several differences between my design and theirs. First, I use the firm-year *CSCORE* measure which allows me to control for several firm-level factors and other incentives that vary around this time. Second, I examine *changes* in *TLR* using a difference-in-difference design. Third, I conduct cross-sectional tests to identify what drives *TLR* increases.

[11] The coefficients on the intercept, *NEG*, *RET* and *RET* × *NEG* are 0.030, 0.012, 0.160 and −0.089 respectively while those on *CSCORE*, *NEG* × *CSCORE*, *RET* × *CSCORE* and *RET* × *NEG* × *CSCORE* are 0.013, 0.116, −0.045 and 0.470 respectively.

enforcement increases to 0.171 (i.e., *IT* plus *POST* × *IT*). A 20% positive return now increases earnings by 6.27% while a 20% negative return reduces earnings by 9.23%. Earnings are more now sensitive to bad news than to good news by 47%. Thus, insider trading enforcement increases the incremental timeliness of earnings to bad news from 40% to 47% (a relative change of 17%).

4.2.4. The effect of mandatory IFRS adoption

Before turning to cross-sectional variation tests, I examine whether my results are driven by other confounding factors that might be occurring during the sample period. For example, it is possible that firms in sample countries are increasing the quality of their financial reporting in anticipation of mandatory IFRS adoption. In this case, it is the anticipation effects of mandatory IFRS adoption that would be driving observed changes in *TLR*.

To examine this possibility, I follow Daske et al. (2008, pp. 1100–1102) and classify my sample countries into whether or not they adopt IFRS by the year 2005. Panel A of Table 4 presents the classification of the sample countries into IFRS adopters and non-adopters. There are a couple of noteworthy features here. First, a number of countries that enforce insider trading laws during the sample period also mandatorily adopt IFRS. Second, not all enforcers adopt IFRS and similarly, not all IFRS adopters enforce insider trading laws. This is the feature that I use to identify whether increases in *CSCORE* are being driven by IFRS adoption.

To do so, I define an indicator variable *MAND_IFRS* to denote whether the country adopts IFRS. I then split the sample based on those that adopt IFRS (*MAND_IFRS*=1) and those that do not (*MAND_IFRS*=0). The advantage of splitting the sample is that it allows the coefficients on all variables to differ between the two sub-samples. As there are countries that enforce insider trading laws as well as those that do not within each sub-sample, I am able to examine the effect of insider trading enforcement holding the effect of IFRS adoption constant.

Here is an example to clarify the identification strategy. Both Hong Kong and Singapore mandatorily adopt IFRS by 2005. However, Hong Kong enforces insider trading laws during the sample period (in 1994) while Singapore enforced it much before the start of the sample period (1978). Similarly, while both Chile and Mexico have not adopted IFRS, Chile enforced insider trading laws in 1996 while Mexico has not until 2000. Thus, by examining the effect of insider trading enforcement on *TLR* within sub-samples of IFRS adopters and non-adopters, I hold the effect of IFRS adoption constant, thereby isolating the effect of insider trading enforcement.

Panel B of Table 4 presents results based on Model 4 of Table 3. In the *MAND_IFRS*=1 sub-sample, the coefficient on *POST* × *IT* is positive (0.035) and significant at the 1% level (one-sided). Further, the coefficient on *POST* is insignificant. This indicates that within the sub-sample of IFRS adopters, only those that enforce insider trading laws experience an increase in *TLR*. There is no detectable increase in countries that do not enforce these laws. Turning to the sub-sample of IFRS non-adopters (i.e., *MAND_IFRS*=0), the coefficient on *POST* × *IT* is again positive (0.058) and significant at the 5% level (one-sided). Thus, even in the sub-sample of IFRS non-adopters, insider trading enforcement is associated with a significant increase in *TLR*.

Based on the estimates in footnote 11, earnings are more sensitive to bad news than to good news by 61% for IFRS adopters in the pre-enforcement period (a 20% positive return increases earnings by 6.29% while a 20% negative return reduces earnings by 10.11%). This incremental sensitivity increases to 72% after enforcement. For IFRS non-adopters, the incremental sensitivity of bad news increases from 16% in the pre-enforcement period to 35% in the post period. Overall, these results suggest that while some enforcing countries also adopt IFRS, the effects on *TLR* appear to be due to insider trading enforcement and not IFRS adoption.

4.2.5. The effect of financial market liberalization

Another interpretation of the results is that they are driven by overall changes in corporate governance during the sample period. For example, mid-1990s was the period when most emerging markets were opening up their economies. Thus, it could be that the changing governance environment in these economies in conjunction with capital market integration might be responsible for the observed changes in *TLR*.

To examine this possibility, I consider the role of financial market liberalization on my results. Studies such as Bekaert et al. (2005) and Henry (2000) find that countries that liberalize their financial markets experience significant economic growth and also receive significant inflows of foreign capital. Given that foreign investors rely on publicly available financial statements in making and monitoring their investments (e.g., Ball et al. 2000), it could be that the increase in *TLR* is a response to financial market liberalization and associated foreign capital inflows and not enforcement of insider trading laws. To examine this interpretation, I gather financial market liberalization dates from Bekaert et al. (2005, pp. 42–47). In addition to the formal date of regulatory change, Bekaert et al. also present two dates that correspond to the "first sign" of financial liberalization—the date of the first American Depository Receipt (ADR) and the launch date of the first country fund. Bekaert et al. argue that these other events could either pre-date or follow the official liberalization date.

Panel A of Table 5 presents liberalization dates for the treatment countries (except Hong Kong for which liberalization dates are not available) and for the control countries. Rows that are marked "Full" indicate that the country has fully liberalized its financial markets before the start of the sample period. Here again, one can see that a number of countries that enforce insider trading laws during the sample period also liberalize their financial markets during this period. Further, not all insider trading enforcers liberalize their financial markets and not all financial market liberalizers enforce

Table 4
Are the results confounded by mandatory IFRS adoption?

Data on mandatory International Financial Reporting Standards (IFRS) adoption are from Daske et al. (2008, pp. 1100–1102). *MAND_IFRS* is an indicator variable that denotes whether or not the country has mandatorily adopted IFRS on or before 2005.

Panel A: Countries that mandatorily adopted IFRS on or before 2005

Countries	Enforcement Year	Mandatory IFRS adoption	*MAND_IFRS*
IT Countries			
Argentina	1995	No	0
Australia	1996	Yes	1
Belgium	1994	Yes	1
Chile	1996	No	0
Denmark	1996	Yes	1
Germany	1995	Yes	1
Greece	1996	Yes	1
Hong Kong	1994	Yes	1
India	1998	No	0
Indonesia	1996	No	0
Italy	1996	Yes	1
Malaysia	1996	No	0
Netherlands	1994	Yes	1
Spain	1998	Yes	1
Switzerland	1995	Yes	1
Turkey	1996	No	0
Control countries			
Austria	1996	Yes	1
Brazil	1995	No	0
France	1994	Yes	1
Israel	1996	No	0
Mexico	1996	No	0
New Zealand	1996	No	0
Pakistan	1998	No	0
Philippines	1996	Yes	1
Portugal	1998	Yes	1
Singapore	1994	Yes	1
United Kingdom	1995	Yes	1

Panel B: Relation between insider trading enforcement and *CSCORE* within each group

The dependent variable is *CSCORE*. *MAND_IFRS* is an indicator variable that denotes whether or not the country mandatorily adopted IFRS on or before 2005. Detailed variable definitions are in Appendix A. *** and ** indicate significance at the 1% and 5% level, respectively. Significance levels are based on a one-sided test for *POST* × *IT* and on a two-sided test for all other variables.

	MAND_IFRS = 1		*MAND_IFRS* = 0	
	Coeff.	*t*-stat	Coeff.	*t*-stat
Intercept	0.193***	3.62	0.379***	5.35
POST	0.009	1.22	−0.062**	−2.11
IT	0.212***	20.17	0.077	1.80
POST × *IT*	0.035***	2.70	0.058**	1.99
ROE	0.045**	2.12	0.027	1.59
MB	0.000***	5.69	0.000***	−5.05
GROWTH	0.006	0.80	0.006	1.05
R&D	−0.749***	−3.68	−0.428	−0.78
INVCYCLE	0.471***	3.87	−0.512**	−2.43
VOL	0.008**	2.22	−0.014***	−2.77
IFRS	−0.061***	−2.91	0.017	0.35
BIG8	−0.036***	−4.49	0.005	0.61
LNASSETS	−0.049***	−16.44	−0.032***	−8.45
RET	−0.009	−1.77	−0.010	−1.94
FOREIGN	−0.181	−0.32	−0.185	−0.61
GDP	−18.581***	−10.20	6.885	1.52
EQMKTCAP	0.093***	8.03	0.017	1.36
GDPGROWTH	0.020***	7.86	0.005***	6.49
INFLATION	0.015***	4.58	0.000	−0.22
CREDRIGHTS	−0.030***	−4.82	−0.006	−0.45
FDI	0.004	1.71	−0.035***	−6.98
Industry effects	Yes		Yes	
Institution effects	Yes		Yes	
Adj. R^2	0.49		0.29	
Observations	7,289		2,649	

Table 5
Examining the effects of financial market liberalization.

Data on financial liberalization dates are from Bekaert et al. (2005, pp. 42–47). Rows marked "Full" indicate that the country has fully liberalized its financial markets before the start of the sample period. "First ADR" and "First Country fund" indicate the year of the first American Depository Receipt (ADR) and the launch date of the first country fund, respectively. *LIBDUM* is an indicator to denote the occurrence of financial liberalization during the sample period.

Panel A: Financial market liberalization dates

Countries	Enforcement Year	Financial Liberalization	First ADR	First Country fund	*LIBDUM*
IT Countries					
Argentina	1995	1989	1991	1991	1
Australia	1996	Full			0
Belgium	1994	Full			0
Chile	1996	1992	1990	1989	1
Denmark	1996	Full			0
Germany	1995	Full			0
Greece	1996	1987	1988	1988	1
Hong Kong	1994	–	–	–	–
India	1998	1992	1992	1986	1
Indonesia	1996	1989	1991	1989	1
Italy	1996	Full			0
Malaysia	1996	1988	1992	1987	1
Netherlands	1994	Full			0
Spain	1998	1985	1988		1
Switzerland	1995	Full			0
Turkey	1996	1989	1990	1989	1
Control countries					
Austria	1996	Full			0
Brazil	1995	1991	1992	1992	1
France	1994	Full			0
Israel	1996	1993	1987	1992	1
Mexico	1996	1989	1989	1981	1
New Zealand	1996	1987	1983		1
Pakistan	1998	1991	1994	1991	1
Philippines	1996	1991	1991	1987	1
Portugal	1998	1986	1990	1987	1
Singapore	1994	Full			0
United Kingdom	1995	Full			0

Panel B: Relation between insider trading enforcement and *CSCORE* within each group

The dependent variable is *CSCORE*. *LIBDUM* is an indicator to denote the occurrence of financial liberalization. Detailed variable definitions are in Appendix A. *** and ** indicate significance at the 1% and 5% level, respectively. Significance levels are based on a one-sided test for *POST* × *IT* and on a two-sided test for all other variables.

	LIBDUM = 1		*LIBDUM* = 0	
	Coeff.	*t*-stat	Coeff.	*t*-stat
Intercept	0.056	0.75	0.467***	8.48
POST	−0.024	−0.90	−0.003	−0.49
IT	0.046	1.41	0.199***	18.44
POST × *IT*	0.064***	2.36	0.038***	3.34
ROE	0.052***	2.78	−0.024	−1.61
MB	0.000	−1.35	0.000***	10.20
GROWTH	0.004	0.66	−0.015**	−2.51
R&D	−0.063	−0.13	−0.595***	−2.88
INVCYCLE	0.134	0.61	0.215**	1.97
VOL	−0.027***	−5.82	0.022***	5.79
IFRS	−0.037	−0.97	−0.020	−1.02
BIG8	−0.021**	−2.01	−0.013	−1.83
LNASSETS	−0.005	−1.21	−0.075***	−30.18
RET	0.001	0.14	−0.013***	−2.95
FOREIGN	−0.927***	−2.91	−0.773	−1.57
GDP	7.766***	3.96	−9.559***	−3.15
EQMKTCAP	0.019	1.33	0.014	0.96
GDPGROWTH	0.004***	4.94	0.002	1.28
INFLATION	0.001**	1.98	0.017***	5.33
CREDRIGHTS	0.006	0.49	−0.039***	−6.33
FDI	−0.014***	−2.82	0.013***	6.90
Industry effects	Yes		Yes	
Institution effects	Yes		Yes	
Adj. R^2	0.22		0.66	
Observations	3,318		6,620	

insider trading laws. I define an indicator variable *LIBDUM* to denote whether the country liberalized its financial markets during the sample period. Similar to the tests for IFRS adoption, I split the sample based on countries that liberalize their markets during the sample period (*LIBDUM*=1) and those that do not (*LIBDUM*=0).

Panel B of Table 5 presents results of the above tests. In the *LIBDUM*=1 sub-sample, the coefficient on *POST*×*IT* is positive (0.064) and significant at the 1% level (one-sided). The coefficient on *POST* (denoting changes in *TLR* for non-enforcers) is insignificant. This indicates that within the sub-sample of financial market liberalizers, only those that enforce insider trading laws experience an increase in *TLR*. There is no detectable increase in non-enforcing countries. Turning to non-liberalizers (i.e., *LIBDUM*=0), the coefficient on *POST*×*IT* is again positive and significant. Thus, even in the sub-sample of non-liberalizers, enforcement of insider trading laws is associated with an increase in *TLR*.

There, is, however, significant variation in the economic effects of enforcement across the sub-samples. Insider trading enforcement increases the incremental sensitivity of bad news relative to good news for liberalizers from 5% to 27% and for non-liberalizers from 57% to 69% (based on estimates from footnote 11). The reason is that pre-enforcement levels of *TLR* are much higher for non-liberalizers (as most of these countries have already liberalized their financial markets and stepped up financial reporting quality). More importantly, these results suggest that the effects of IT enforcement are not driven by financial market integration.

4.3. Cross-sectional variation tests

In this section, I explore cross-sectional variation in the effects of IT enforcement.

4.3.1. Debt contracting intensity (LEV)

To examine hypothesis *H2*, which is that enforcement effects of insider trading laws are stronger in firms with higher intensity of debt contracting, I use the level of debt (*LEV*) as the measure of contracting intensity. In particular, I split the sample based on the median value of *LEV* and examine changes in *CSCORE* for each sub-sample. I define an indicator variable *HIGHLEV* to denote firms with high (i.e., above median) values of *LEV*. As post-enforcement increases in *TLR* are expected to be pronounced for firms with high reliance on debt contracts, I expect the coefficient on *POST*×*IT* to be stronger in firms with high debt contract intensity (*HIGHLEV*=1) than in those with low debt contract intensity (*HIGHLEV*=0). Results are presented in Panel A of Table 6.

Consistent with hypothesis *H2*, the coefficient on *POST*×*IT* is positive (0.024) and significant at the 5% level (one-sided) in the *HIGHLEV*=1 sample while it is negative (−0.001) and insignificant (*t*-stat=−0.11) in the *HIGHLEV*=0 sub-sample. The chi-square test of the difference in the coefficient on *POST*×*IT* between the subsamples is 3.81 and is significant at the 5% level. Further, the coefficient on *POST*×*IT* for the *HIGHLEV*=1 sub-sample is similar to that in Table 3 suggesting that the results in Table 3 are driven by high leverage firms.

4.3.2. Strong internal monitors (LARGESHARE)

To test hypothesis *H3*, which is that the effect of insider trading enforcement on *TLR* is stronger in firms without a strong internal monitor, I use firm-level ownership data from the sources in Claessens et al. (2000) and Faccio and Lang (2002).[12] Claessens et al. hand-collect ownership data for firms in 9 East Asian countries (Hong Kong, Indonesia, Japan, Korea, Malaysia, Philippines, Singapore, Taiwan and Thailand) and find a significant amount of ownership concentration in these firms. Similarly, Faccio and Lang collect firm-level ownership data for a large cross-section of firms from 13 Western European countries (Austria, Belgium, Finland, France, Germany, Ireland, Italy, Norway, Portugal, Spain, Sweden, Switzerland and the UK) and also find that a vast majority of the firms are controlled by large shareholders. I exclude firms where the largest shareholder belongs to a family-group, as the theory I am testing pertains to large shareholders who play a stewardship role. Merging these data with my sample leaves a sample size of 1,996 firm-year observations. To examine how the presence of a strong internal monitor affects insider trading enforcement effects on *TLR*, I create an indicator variable *LARGESHARE* to denote firms where the largest shareholder holds more than 50% of the cash-flow rights of the firm. I expect post-enforcement increases in *TLR* to be pronounced in firms without a strong internal monitor, i.e., in firms where *LARGESHARE*=0.

To test this prediction, I split the sample into two groups based on *LARGESHARE* and examine differences in the coefficient on *POST*×*IT* between the two sub-groups (i.e., *LARGESHARE*=0 and *LARGESHARE*=1). Results are presented in the first two specifications of Panel B of Table 6. The coefficient on *POST*×*IT* is negative (−0.018) and insignificant (*t*-stat is −0.41) in the *LARGESHARE*=1 group while it is positive (0.097) and significant at the 1% level (one-sided) (*t*-stat=3.44) in the *LARGESHARE*=0 group. The coefficient on *POST*×*IT* is significantly different at the 5% level (one-sided) between the two groups.

To ensure that the above results are not being driven by differences in sample size, I re-estimate these results based on the quartiles as cutoffs. In particular, I define *LARGESHARE*=1 when cash flow rights are higher than the third quartile (48.35%) and *LARGESHARE*=0 when cash flow rights are lower than the first quartile (13.3%). The sample sizes are 518 and 509 observations, respectively. The second set of specifications in Panel B present these results. The coefficient on *POST*×*IT*

[12] These data are available on the *Journal of Financial Economics* website.

Table 6
Cross sectional-variation in the effect of insider trading enforcement on *CSCORE*.

Panel A: Role of contract intensity (*HIGHLEV*)

The dependent variable is *CSCORE*. *HIGHLEV* is an indicator variable that denotes firms with above median level of debt. Detailed variable definitions are in Appendix A. *** and ** indicate significance at the 1% and 5% level, respectively. Significance levels are based on a one-sided test for *POST* × *IT* and on a two-sided test for all other variables.

	HIGHLEV = 1		*HIGHLEV* = 0	
	Coeff.	*t*-stat	Coeff.	*t*-stat
Intercept	0.328***	5.19	0.326***	6.96
POST	−0.001	−0.15	0.013	1.49
IT	0.171***	10.30	0.130***	9.52
POST × *IT*	0.024**	1.92	−0.001	−0.11
ROE	0.055***	3.03	−0.039	−1.94
MB	−0.125***	−9.20	0.000	1.58
GROWTH	0.005	0.53	0.008	1.68
R&D	−0.968***	−3.04	−0.297	−1.36
INVCYCLE	0.220	1.16	0.041	0.32
VOL	−0.016***	−3.83	0.012***	3.97
IFRS	−0.070***	−2.65	−0.079**	−2.53
BIG8	−0.001	−0.12	−0.007	−0.97
LNASSETS	−0.055***	−13.73	−0.051***	−17.98
RET	0.017***	2.99	−0.015***	−4.06
FOREIGN	−1.075**	−2.12	0.443	1.28
GDP	−7.922***	−8.06	−5.120***	−6.93
EQMKTCAP	0.040***	3.34	−0.027***	−3.09
GDPGROWTH	0.016***	13.03	0.005***	6.27
INFLATION	0.008***	7.15	0.001***	3.33
CREDRIGHTS	−0.007	−0.95	0.003	0.38
FDI	0.009***	3.89	0.004**	1.98
χ^2 of difference of *POST* × *IT*			3.81	
p value of difference			0.025	
Industry & institution effects		Yes		Yes
Adj. R^2		0.45		0.46
Observations		4,974		4,964

Panel B: Presence of a large shareholder (*LARGESHARE*)

The sample is based on 1,996 firm-year observations with non-missing large shareholder data. The dependent variable is *CSCORE* which is the measure of conservatism. *LARGESHARE* is an indicator variable that denotes firms with a large shareholder. Detailed variable definitions are in Appendix A. *** and ** indicate significance at the 1% and 5% level, respectively. Significance is based on one-sided test for *POST* × *IT* and two-sided test for all other variables.

	High versus low (50% ownership)				**High versus low (Quartiles)**			
	LARGESHARE = 1		*LARGESHARE* = 0		*LARGESHARE* = 1		*LARGESHARE* = 0	
	Coeff.	*t*-stat	Coeff.	*t*-stat	Coeff.	*t*-stat	Coeff.	*t*-stat
Intercept	−0.628***	−3.27	−0.429***	−3.09	−0.638***	−3.27	0.294	0.91
POST	−0.003	−0.10	−0.057***	−4.25	−0.006	−0.20	−0.046**	−2.31
IT	0.270***	4.62	0.049	0.85	0.262***	4.58	0.016	0.17
POST × *IT*	−0.018	−0.41	0.097***	3.44	−0.017	−0.40	0.144***	2.40
ROE	−0.072	−1.37	0.054	1.43	−0.072	−1.35	0.054	1.31
MB	0.000**	2.24	0.000	0.47	0.000**	2.19	0.001***	3.70
GROWTH	0.037	1.22	−0.021	−1.65	0.034	1.13	−0.069***	−3.03
R&D	−0.786	−1.31	0.089	0.19	−0.778	−1.31	−0.874	−1.84
INVCYCLE	0.247	0.70	0.411	1.13	0.130	0.38	1.373	1.30
VOL	−0.047***	−3.01	−0.013	−1.13	−0.044***	−2.86	−0.028	−0.86
IFRS	−0.095	−1.63	−0.075	−1.30	−0.095	−1.61	−0.146	−0.91
BIG8	−0.038	−1.51	−0.012	−0.63	−0.037	−1.47	−0.022	−0.59
LNASSETS	−0.027**	−1.98	−0.054***	−5.60	−0.026	−1.89	−0.079***	−4.14
RET	−0.013	−0.83	−0.026***	−2.80	−0.015	−0.92	−0.016	−1.16
FOREIGN	−1.046	−0.78	1.163	1.10	−1.013	−0.74	−2.699	−1.93
GDP	3.968	0.72	−8.726**	−2.57	3.244	0.60	−2.030	−0.38
EQMKTCAP	0.132***	3.34	0.175***	5.77	0.136***	3.46	0.044	0.85
GDPGROWTH	−0.005	−1.51	0.002	0.84	−0.005	−1.46	0.007**	1.98
INFLATION	−0.003**	−2.01	0.001	0.68	−0.003	−1.89	0.007**	2.09
CREDRIGHTS	0.054	1.91	0.112***	6.13	0.058***	2.01	0.014	0.20
FDI	0.005	0.50	0.024***	6.59	0.006	0.54	0.028***	4.33

Table 6 (*continued*)

Panel B: Presence of a large shareholder (*LARGESHARE*)

The sample is based on 1,996 firm-year observations with non-missing large shareholder data. The dependent variable is *CSCORE* which is the measure of conservatism. *LARGESHARE* is an indicator variable that denotes firms with a large shareholder. Detailed variable definitions are in Appendix A. *** and ** indicate significance at the 1% and 5% level, respectively. Significance is based on one-sided test for *POST* × *IT* and two-sided test for all other variables.

	High versus low (50% ownership)				**High versus low (Quartiles)**			
	LARGESHARE = 1		*LARGESHARE* = 0		*LARGESHARE* = 1		*LARGESHARE* = 0	
	Coeff.	*t*-stat	Coeff.	*t*-stat	Coeff.	*t*-stat	Coeff.	*t*-stat
χ^2 of difference of *POST* × *IT*		4.64				8.13		
p value of difference		0.016				0.001		
Industry & institution effects	Yes		Yes		Yes		Yes	
Adj. R^2	0.74		0.58		0.74		0.82	
Observations	510		1,486		518		509	

Panel C: Role of equity markets

The dependent variable is *CSCORE*. *ISSUERS* is an indicator variable that denotes firms with above median equity issuances in the subsequent year. Detailed variable definitions are in Appendix A. *** and ** indicate significance at the 1% and 5% level, respectively. Significance levels are based on a one-sided test for *POST* × *IT* and on a two-sided test for all other variables.

	ISSUERS = 1		*ISSUERS* = 0	
	Coeff.	*t*-stat	Coeff.	*t*-stat
Intercept	0.707***	10.94	0.017	0.36
POST	0.005	0.70	−0.002	−0.16
IT	0.125***	8.88	0.115***	7.17
POST × *IT*	0.041***	3.44	0.029**	1.98
ROE	0.022	0.69	0.038**	2.30
MB	0.000	1.86	0.000	1.62
GROWTH	0.006	0.72	0.003	0.49
R&D	−0.382	−1.70	−0.720**	−2.31
INVCYCLE	0.130	0.79	0.173	1.34
VOL	0.001	0.12	−0.018***	−5.51
IFRS	−0.071***	−3.16	−0.062**	−2.07
BIG8	−0.008	−0.80	−0.002	−0.25
LNASSETS	−0.067***	−18.97	−0.026***	−8.86
RET	−0.014**	−2.50	0.002	0.43
FOREIGN	−0.639	−1.02	−0.609	−1.75
GDP	−10.369***	−12.08	−2.647***	−3.62
EQMKTCAP	−0.043***	−3.57	0.025***	2.85
GDPGROWTH	0.007***	5.31	0.007***	9.64
INFLATION	0.000	0.70	0.001***	5.06
CREDRIGHTS	−0.004	−0.51	0.001	0.14
FDI	0.019***	8.14	−0.005**	−2.17
χ^2 of difference of *POST* × *IT*		0.56		
p value of difference		0.454		
Industry & institution effects	Yes		Yes	
Adj. R^2	0.51		0.26	
Observations	4,291		5,647	

remains negative and insignificant in the large shareholder sub-sample and positive and significant in the non-large shareholder sub-sample. Here too, the difference in the coefficient on *POST* × *IT* is statistically significant between the two sub-samples. These results are consistent with hypothesis *H3* and indicate that post-enforcement increases in *TLR* are concentrated in firms with a lower incidence of strong internal monitors, which rely more on explicit contracting arrangements to alleviate agency conflicts.

In unreported tests, I examine whether leverage and large shareholding each have a unique marginal effect. To do so, I employ indicator variables to denote sub-samples where the contracting and monitoring demands are expected to be dominant (i.e., *HIGHLEV* and *LOWLEV* and *HIGHSHARE* and *LOWSHARE*) and interact each of these indicators with *POST* × *IT*. I find that *POST* × *IT* is positive and significant only in the subset of firms with high leverage *and* low large shareholding, indicating that both forces have an independent effect. The marginal effect of large shareholding is around 40% larger than that of leverage. These results are robust to using majority block holding as well as the upper and lower quartiles as cutoffs.

4.3.3. Role of equity markets

To examine hypothesis *H4*, I use future equity issuances by firms as the measure of equity market demand for *TLR*.[13] I define *EQISS* as the percentage change in the number of shares outstanding in the subsequent year and define *ISSUERS* as an indicator variable to denote high versus low equity issuers. I split the sample based on whether *ISSUERS*=1 or *ISSUERS*=0 and examine differences in the coefficient on *POST* × *IT* between these two sub-samples. If post-enforcement increases in *TLR* are driven by equity markets, I expect the coefficient on *POST* × *IT* to be pronounced in the *ISSUERS*=1 sub-sample.

Panel C of Table 6 presents the above results. The coefficient on *POST* × *IT* is positive (0.041) and significant at the 1% level (one-sided) in the *ISSUERS*=1 sub-sample and similarly positive (0.029) and significant at the 5% level (one-sided) in *ISSUERS*=0 sub-sample. Further, the difference in the coefficient on *POST* × *IT* between the two groups is not significant (*p*-value is 0.454). The economic magnitudes of the effect are fairly similar across the two sub-samples. The incremental sensitivity of bad news increases from 32% to 46% for issuers and from 29% to 38% for non-issuers. Overall, I find no evidence that post-enforcement increases in *TLR* are driven by equity markets, consistent with Ball et al. (2008).

5. Additional robustness tests

5.1. Alternate control samples

In this section, I examine sensitivity of my results to using alternate control samples. The earlier results are based on selecting one control country per treatment country. In this section, I select control countries in two alternate ways. First, I randomly assign enforcement dates to all non-enforcing countries in the sample. This creates a control sample of 25,853 firm-year observations across 25 countries. Second, I assign 1996 which is the most common insider trading enforcement year to all the control countries. This procedure yields a control sample of 26,925 firm-year observations across 24 countries. Results (unreported) based on using these alternate control samples are similar to those tabulated. In particular, the coefficient on *POST* × *IT* remains positive and significant at the 1% level in each case.[14] Further, the increases in *CSCORE* are restricted to firms with more debt and in those without a strong internal monitor.

5.2. Timely gain recognition (GSCORE)

It is possible that insider trading enforcement impacts other factors in the reporting environment, such as the information content of earnings. If so, the reported change in *TLR* could be an artifact of a change in the way earnings map into stock prices. To examine this possibility, I examine changes in the timeliness of recognition of good news in earnings using *GSCORE*. In unreported tests, I find that the coefficient on *POST* × *IT* is insignificant. These results are not consistent with an overall change in the mapping between earnings and stock prices, but rather suggest that results are specific to changes in the speed with which bad news is recognized in earnings. Further, I include *GSCORE* as an explanatory variable in the *CSCORE* regressions and find that the coefficient on *POST* × *IT* remains positive and significant.

5.3. Using the Basu (1997) model

I examine the robustness of my results to using the Basu (1997) model. I augment the Basu model by interacting *RET*, *NEG* and *RET* × *NEG* with *POST*, *IT* and *POST* × *IT*. The relevant coefficient is *RET* × *NEG* × *POST* × *IT* which captures how the incremental timeliness of bad news recognition changes after enforcement. Consistent with inferences using *CSCORE*, the coefficient on *RET* × *NEG* × *POST* × *IT* is positive and significant at the 5% level. To mitigate the concern that this specification does not control for firm and country-level factors, I first orthogonalize *EARN* with respect to firm-level and country-level controls and then use the residual as the dependent variable in the augmented Basu model. Results from this specification are also robust to those reported. In particular, the coefficient on *RET* × *NEG* × *POST* × *IT* remains positive and significant at the 5% level. Further, these results are driven by firms with above median debt and those without an internal monitor.

5.4. Interaction of POST with country-level variables

Next, I allow the effect of country-level variables to change between the pre and the post periods. To do so, I interact *POST* with *GDP*, *EQMKTCAP*, *GDPGROWTH*, *INFLATION*, *CREDRIGHTS* and *FDI*. In unreported tests, I find that *POST* × *IT* remains significant at the 5% level. Further, it is significant only in firms with more debt and without a large shareholder.

[13] I do not use country-level variables such as the ratio of equity-market cap to GDP as these are used as controls.

[14] All results are available upon request.

5.5. Including ownership concentration

Daske et al. (2009) include ownership concentration as one of the determinants of voluntarily IFRS adoption. I do not include it because these data are not available for a majority of my sample firms. In this section, I examine whether my results hold in the smaller sub-set of 5,126 observations with ownership data. I find that results remain significant when ownership concentration is included as a control. In particular, the coefficient on $POST \times IT$ remains positive and significant at the 1% level and this effect is restricted to firms with above median debt.

5.6. Are results driven by any one country?

I examine whether the results for the pooled sample are being driven by any particular country. To do so, I delete each country (both treatment and control) individually from the sample and estimate the primary regression. I find that the coefficient on $POST \times IT$ remains positive and significant in every instance.

5.7. Firm and year fixed effects

I examine the sensitivity of my results to including firm and year fixed effects. As the year fixed effects subsume the *POST* indicator and the firm fixed effects subsume *IT*, I drop these from the regression. In unreported tests, I find that the coefficient on $POST \times IT$ remains positive and significant at the 1% level, providing assurance that the results are unlikely to be driven by time-invariant firm-level or country-level characteristics. Further, these results are restricted to firms with above median debt and those without a large shareholder.

6. Conclusion

I use first-time enforcement of insider trading laws across 16 countries as a shock to enforcement of securities laws and examine its effect on timely loss recognition (*TLR*). Consistent with increased enforcement increasing the usefulness of accounting information in financial contracts and thereby the demand for high quality information, I find that first-time enforcement increases the extent of *TLR*. My results confirm predictions by Holthausen (2009) that enforcement of laws is likely to have an important effect on financial reporting outcomes and complements evidence in prior studies on the effect of institutional features on reporting outcomes (e.g., Ball et al., 2000, 2003; Leuz et al., 2003; Bushman and Piotroski, 2006).

Consistent with the contracting demand for *TLR*, post enforcement increases are concentrated in firms with high reliance on debt. Further, there are no detectable increases in *TLR* in firms with a strong internal monitor as these firms do not need to rely on explicit contracting arrangements to alleviate agency conflicts. I also find that equity markets do not seem to be the primary driver of post-enforcement increases in *TLR*.

My results are robust to several sensitivity tests and suggest that higher enforcement of securities laws increases the usefulness of accounting information in financial contracts and thus the demand for *TLR*. However, as pointed out by Bushman and Piotroski (2006), strong courts can facilitate securities-related litigation and thus the influence of insider trading enforcement on *TLR* might also be consistent with a litigation-based demand.

Appendix A. Variable definitions

See Table A1.

Table A1

Variable name	Definition	Data items	Source
BIG8	Big 8 auditor	Indicator variable that denotes whether the firm is audited by a Big 8 auditor (data item auop)	Global Vantage Fundamentals
CFRIGHTS	Cash flow rights	Cash flow rights of the largest shareholder	Claessens et al. (2000); Faccio and Lang (2002)
CREDRIGHTS	Creditor rights index	The time-varying measure of creditor rights	Djankov et al. (2007)
CSCORE	Firm-year measure of conservatism or incremental bad news timeliness	Eqs. (1) and (3)	
EARN	Accounting income	Net income (IC data 32) scaled by opening market value of equity; calculated as price (data item prcadj) times number of outstanding shares (data item shradj), adjusted for stock splits and dividends using the Global Vantage adjustment factor. Minority interest (data 27) is added to *EARN* for German firms.	Global Vantage Issue

Table A1 (*continued*)

Variable name	Definition	Data items	Source
EQISS	Equity issuance indicator	Equity issuances in the subsequent year; computed as the percentage change in the number of shares outstanding	Global Vantage Issue
EQMKTCAP	Equity market capitalization	Market capitalization of listed companies (% of GDP)	World Development Indicators (WDI) database of the World Bank
FDI	Foreign direct investment	Foreign direct investment, net inflows (% of GDP)	
FOREIGN	Proportion of foreign income	Foreign income (data item 56) divided by total sales (data item 1)	Global Vantage Fundamentals
GDP	Per capita GDP	GDP per capita (constant 2000 US$)	WDI
GDPGROWTH	Per capita GDP growth	GDP growth (annual %)	WDI
GROWTH	Sales growth	Annual percentage change in total sales (data item 1)	Global Vantage Fundamentals
GSCORE	Firm-year measure of good news timeliness	Eqs. (1) and (2)	
IFRS	Voluntary *IFRS* adoption indicator	Indicator variable based on the "Accounting Standards" field (data item astd) that takes 1 for codes "DA", "DI", "DT", "DU", "MU" or "US".	Global Vantage Fundamentals
INFLATION	Annual inflation	Inflation, GDP deflator (annual %)	WDI
INVCYCLE	Length of the investment cycle	Annual depreciation (data item 11) divided by total assets	Global Vantage Fundamentals
LEV	Leverage	Total debt (data item 106) scaled by market value	Global Vantage Fundamentals
LIBDUM	Financial market liberalization	Indicator that denotes that the country undertook financial liberalization during the sample period	Bekaert et al. (2005, pp. 41–47).
LNASSETS	Firm size	Log of total assets (data item 89)	Global Vantage Fundamentals
MAND_IFRS	Mandatory IFRS adoption	Indicator variable that denotes whether or not the country mandatorily adopted IFRS on or before 2005	Daske et al. (2008, pp. 1100–1102)
MB	Market-to-book	The ratio of market value of assets (market value of equity plus book value of debt) divided by book value of assets (data item 89)	Global Vantage Fundamentals and Issue
R&D	Research and development	Research and development expense (data item 52) divided by sales. Missing values of *R&D* are set to zero.	Global Vantage Fundamentals
RET	Stock return	Annual change in data item prcadj	Global Vantage Issue
ROE	Return on equity	Net income divided by market value of equity	Global Vantage Fundamentals and Issue
VOL	Stock volatility	Annual standard deviation of monthly stock prices	Global Vantage Issue

Appendix B. Validating the Khan and Watts (2009) model: additional tests

In this section, I perform additional analyses to further verify the appropriateness of the Khan and Watts (2009) model in my setting. To do so, I follow Khan and Watts (2009, Table 5) and examine how the coefficients on the Basu (1997) regression vary based on values of *CSCORE*. To do so, I first create deciles annually based on *CSCORE* and estimate the Basu (1997) regression for each decile. Khan and Watts (2009) find that the coefficient on $RET \times NEG$ in the Basu specification is increasing in deciles of *CSCORE*. Further, they find no association between *CSCORE* deciles and the coefficients on *RET*. Below are the coefficients on the Basu model estimated for my larger sample of 47 countries using a robust regression. While those based on an OLS regression are similar to those reported below, the robust regression mitigates the influence of outliers. I also present the relevant estimates from Table 5 of Khan and Watts (2009, p. 139) to facilitation comparison (Table B1).

Similar to Khan and Watts, I find that the coefficient on $RET \times NEG$ is increasing in *CSCORE* deciles in my sample. Further, consistent with their results, I also find that the coefficient on *RET* is uncorrelated with *CSCORE*. These results provide confidence of the applicability of the Khan and Watts model to my setting.

Next, I follow Khan and Watts (2009, Tables 6 and 7, p. 140) and examine the association between *CSCORE* decile and several firm-characteristics. These are return on assets (*ROA*), standard deviation and skewness of *ROA*, non-operating

Table B1

CSCORE deciles	Khan and Watts (2009, p. 139)				My sample			
	Intercept	*NEG*	*RET*	*RET × NEG*	*Intercept*	*NEG*	*RET*	*RET × NEG*
1	0.083	−0.018	−0.025	0.147	0.065	−0.010	0.009	0.039
2	0.089	−0.017	−0.008	0.127	0.048	−0.006	0.033	0.014
3	0.091	−0.016	−0.007	0.140	0.050	−0.013	0.007	0.042
4	0.094	−0.022	0.000	0.139	0.050	−0.016	0.004	0.042
5	0.098	−0.022	−0.004	0.172	0.052	−0.016	0.006	0.047
6	0.097	−0.022	−0.001	0.196	0.052	−0.013	0.016	0.043
7	0.087	−0.009	0.014	0.214	0.057	−0.012	0.028	0.047
8	0.088	−0.016	0.010	0.236	0.062	−0.017	0.028	0.070
9	0.083	−0.026	−0.002	0.263	0.064	−0.015	0.027	0.112
10	0.062	−0.037	−0.015	0.337	0.052	0.001	0.063	0.209
Rank. corr.			0.382	0.915[c]			0.479	0.952[c]
Hi − Lo			0.010	0.191[c]			0.054	0.171[c]
(Pred. sign)			(−)	(+)			(−)	(+)

Table B2

CSCORE deciles	*ROA*	*ROA* (Std. dev)	*ROA* (Skew)	*NOACC*	*NOACC* (Std. dev)	*NOACC* (Skew)	*CSCORE*	*GSCORE*	*MB*	*SIZE*	*LEV*	*INVCYCLE*	*ILLIQ*	*VOL*
1	0.05	0.10	−4.58	0.12	0.14	−4.87	−0.34	0.21	1.08	7.82	0.14	0.04	0.01	0.10
2	0.04	0.09	−4.05	0.10	0.10	−2.97	−0.13	0.04	1.06	8.43	0.13	0.04	0.01	0.10
3	0.03	0.10	−4.73	0.09	0.14	−4.41	−0.06	0.02	0.99	8.03	0.10	0.03	0.00	0.11
4	0.02	0.11	−4.46	0.09	0.15	−4.44	−0.01	0.01	0.91	7.57	0.09	0.03	0.01	0.12
5	0.03	0.10	−4.40	0.09	0.14	−3.22	0.03	0.00	0.89	7.42	0.10	0.03	0.01	0.11
6	0.02	0.12	−4.34	0.08	0.17	−3.55	0.07	0.00	0.81	7.49	0.14	0.03	0.02	0.11
7	0.02	0.13	−3.88	0.08	0.17	−3.20	0.12	0.00	0.79	6.30	0.14	0.03	0.05	0.12
8	0.02	0.17	−3.30	0.07	0.18	−3.03	0.18	0.00	0.79	5.63	0.17	0.04	0.12	0.12
9	0.02	0.20	−2.69	0.05	0.23	−2.54	0.28	0.00	0.84	4.79	0.16	0.04	0.27	0.13
10	0.01	0.25	−2.11	0.02	0.28	−2.05	0.46	0.00	0.66	3.49	0.27	0.04	1.67	0.16
Rank. corr.	−0.98[c]	0.95[c]	0.85[c]	−1.00[c]	0.89[c]	0.72[b]		−0.95[c]	−0.92[c]	−0.95[c]	0.76[b]	0.25	0.87[c]	0.92[c]
Hi − Lo	−0.05[c]			−0.09[c]			0.80[c]	−0.21[c]	−0.42[c]	−4.33[c]	0.13[c]	0.00[c]	1.65[c]	0.06[c]
(Pred. sign)	(−)	(+)	(−)	(−)	(+)	(−)	(+)	(−)	(−)	(−)	(+)	(−)	(+)	(+)

accruals (*NOACC*), standard deviation and skewness of *NOACC*, market-to-book (*MB*), firm size (*SIZE*), leverage (*LEV*), illiquidity (*ILLIQ*) and volatility (*VOL*). These are presented in Table B2.

Consistent with the predictions in Khan and Watts, *ROA* and *NOACC* are decreasing in *CSCORE*. Further, *GSCORE*, *MB* and *SIZE* are decreasing while *LEV* and *VOL* are increasing in deciles of *CSCORE*. As PIN data are not available for foreign firms and bid-ask spread data are very noisy (Lesmond 2005), I use the Amihud (2002) illiquidity measure (*ILLIQ*). Higher values of *ILLIQ* indicate that a small amount of trading volume generates a large price movement—indicating greater illiquidity. Consistent with the inferences based on PIN and bid-ask spreads, illiquidity is increasing in *CSCORE*. The main inconsistency pertains to investment cycle where Khan and Watts hypothesize an inverse association while I find a positive (albeit economically miniscule) association. This could be due to differences in depreciation rules across countries. Overall, the association between *CSCORE* and firm characteristics in my cross-country panel mirrors those shown by Khan and Watts for U.S. firms.

References

Amihud, Y., 2002. Illiquidity and stock returns: cross-section and time-series effects. Journal of Financial Markets 5, 31–56.

Ball, R., 1995. Making accounting more international: why, how, and how far will it go? Journal of Applied Corporate Finance 8, 19–29

Ball, R., 2001. Infrastructure requirements for an economically efficient system of public financial reporting and disclosure. Brookings—Wharton Papers on Financial Services 127–169.

Ball, R., 2006. International financial reporting standards (IFRS): pros and cons for investors. Accounting and Business Research 36, 5–27.

Ball, R., Kothari, S.P., Robin, A., 2000. The effect of international institutional factors on properties of accounting earnings. Journal of Accounting & Economics 29, 1–51.

Ball, R., Robin, A., Sadka, G., 2008. Is financial reporting shaped by equity markets or by debt markets? An international study of timeliness and conservatism. Review of Accounting Studies 13, 168–205.

Ball, R., Robin, A., Wu, J.S., 2003. Incentives versus standards: properties of accounting income in four east Asian countries. Journal of Accounting & Economics 36 (1–3), 235–270.

Ball, R., Shivakumar, L., 2005. Earnings quality in U.K. private firms: comparative loss recognition timeliness. Journal of Accounting & Economics 39, 83–128.

Basu, S., 1997. The conservatism principle and the asymmetric timeliness of earnings. Journal of Accounting & Economics 24 (1), 3–37.

Bekaert, G., Harvey, C.R., 1997. Emerging equity market volatility. Journal of Financial Economics 43 (1), 29–77.
Bekaert, G., Harvey, C.R., 2000. Foreign speculators and emerging equity markets. Journal of Finance 55 (2), 565–613.
Bekaert, G., Harvey, C.R., Lundblad, C., 2005. Does financial liberalization spur growth? Journal of Financial Economics 77 (1), 3–55.
Bertrand, M., Mullainathan, S., 1999. Is there discretion in wage setting? A test using takeover legislation. RAND Journal of Economics 30, 535–554.
Bertrand, M., Mullainathan., S., 2003. Enjoying the quiet life? Corporate governance and managerial preferences. Journal of Political Economy 111 (5), 1043–1075.
Bhattacharya, U., Daouk, H., 2002. The world price of insider trading. Journal of Finance 57 (1), 75–108.
Bhattacharya, U., Daouk, H., 2009. When no law is better than a good law. Review of Finance 13 (4), 577–627.
Burgstahler, D.C., Hail, L., Leuz, C., 2006. The importance of reporting incentives: earnings management in European private and public firms. Accounting Review 81 (5), 983–1016.
Bushman, R.M., Piotroski, J.D., 2006. Financial reporting incentives for conservative accounting: the influence of legal and political institutions. Journal of Accounting & Economics 42 (1–2), 107–148.
Callen, J.L., Guan, Y., Qiu, J., 2010. The Market for Corporate Control and Accounting Conservatism. University of Toronto Working Paper.
Claessens, S., Djankov, S., Lang, L.H.P., 2000. The separation of ownership and control in East Asian Corporations. Journal of Financial Economics 58, 81–112.
Coffee, J.C., 2007. Law and the market: the impact of enforcement. University of Pennsylvania Law Review 156 (2), 229–311.
Daouk, H., Lee, C.M.C., Ng, D., 2006. Capital market governance: how do security laws affect market performance? Journal of Corporate Finance 12 (3), 560–593.
Daske, H., Hail, L., Leuz, C., Verdi, R., 2008. Mandatory IFRS reporting around the world: early evidence on the economic consequences. Journal of Accounting Research 46 (5), 1085–1142.
Daske, Holger, Hail, L., Leuz, C., Verdi, R., 2009. Adopting a Label: Heterogeneity in the Economic Consequences of IFRS Adoptions. University of Mannheim Working Paper.
Djankov, S., McLiesh, C., Shleifer, A., 2007. Private credit in 129 countries. Journal of Financial Economics 84 (2), 299–329.
Faccio, Mara, Lang, Larry H.P., 2002. The ultimate ownership of Western European Corporations. Journal of Financial Economics 65 (3), 365–395.
Guay, W., Verrecchia., R., 2006. Discussion of an economic framework for conservative accounting and Bushman and Piotroski (2006). Journal of Accounting & Economics 42 (1–2), 149–165.
Hail, L., Leuz., C., 2006. International differences in the cost of equity capital: Do legal institutions and securities regulation matter? Journal of Accounting Research 44, 485–531.
Henry, P.B., 2000. Do stock market liberalizations cause investment booms? Journal of Financial Economics 58 (1–2), 301–334.
Holthausen, R.W., 2009. Accounting standards, financial reporting outcomes, and enforcement. Journal of Accounting Research 47 (2), 447–458.
Holthausen, R.W., Watts, R.L., 2001. The relevance of the value-relevance literature for financial accounting standard setting. Journal of Accounting & Economics 31 (1–3), 3–75.
Khan, M., Watts, R., 2009. Estimation and empirical properties of a firm-year measure of accounting conservatism. Journal of Accounting & Economics 48 (2–3), 132–150.
Kothari, S.P., Ramanna, K., Skinner, D., 2010. Implications for GAAP from an analysis of positive research in accounting. Journal of Accounting & Economics 50 (2–3), 246–286.
Lafond, R., Roychowdhury, S., 2008. Managerial ownership and accounting conservatism. Journal of Accounting Research 46 (1), 101–135.
LaPorta, R., LopezDeSilanes, F., Shleifer, A., Vishny, R.M., 1997. Legal determinants of external finance. Journal of Finance 52 (3), 1131–1150.
Lesmond, D.A., 2005. Liquidity of emerging markets. Journal of Financial Economics 77 (2), 411–452.
Leuz, C., Lins, K.V., Warnock, F.E., 2009. Do foreigners invest less in poorly governed firms? Review of Financial Studies 22 (8), 3245–3285.
Leuz, C., Nanda, D., Wysocki., P.D., 2003. Earnings management and investor protection: an international comparison. Journal of Financial Economics 69 (3), 505–527.
Leuz, C., Wysocki, P., 2008. Economic Consequences of Financial Reporting and Disclosure Regulation: A Review and Suggestions for Future Research. University of Chicago Working Paper.
Mahoney, P.G., 2009. The development of securities law in the United States. Journal of Accounting Research 47 (2), 325–347.
Watts, R., 1977. Corporate financial statements, a product of the market and political processes. Australian Journal of Management (April), 53–75.
Watts, R., 2003a. Conservatism in accounting, Part I: explanations and implications. Accounting Horizons 17 (3), 207–221.
Watts, R., 2003b. Conservatism in accounting, Part II: evidence and research opportunities. Accounting Horizons 17, 4.
Watts, R.L., Zimmerman, J.L., 1986. Positive Accounting Theory. Prentice-Hall, Englewood Cliffs, NJ.