# Insider Trading Restrictions and Insiders' Supply of Information: Evidence from Earnings Smoothing*

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## Abstract

We exploit the setting of first-time enforcement of insider trading laws to investigate the relationship between insider trading opportunities and insiders' supply of information. Insider trading opportunities motivate insiders to reduce supply of information by concealing firm performance, thereby increasing their information advantage over outsiders and resulting in higher insider trading profits. Using data from 40 countries over the 1988–2004 period, we find that reporting opacity, as captured by earnings smoothness, decreases significantly after the initial enforcement of insider trading laws in countries with strong legal institutions. The decrease in earnings smoothness is positively related to the strictness of insider trading laws. The decrease in earnings smoothness is also more pronounced for countries that have more persistent insider trading law enforcement and for countries that impose more severe penalty on insider trading cases. Further analyses show that the decrease in earnings smoothness following insider trading enforcement is concentrated among firms that are not closely held and among high growth firms. In addition to uncovering a channel through which insider trading restrictions affect the information environment, our evidence highlights the importance of country- and firm-level governance structures in determining the consequences of insider trading restrictions.



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*JEL Classification:* M41, M48, G18

## 1. Introduction

Central to the debate over insider trading regulations is the impact of the practice on economic efficiency (Dye, 1984; Ausubel, 1990; Leland, 1992). Some argue that, by helping to impound information into prices, insider trading improves the efficiency of financial markets and capital resource allocation (Manne, 1966a; Demsetz, 1969; Carlton and Fischel, 1983). [1] An opposing view is that insider trading undermines firms' information environment and economic efficiency. For example, insider trading can crowd out outside investors' information production by limiting the gains from information acquisition (Fishman and Hagerty, 1992; Khanna, Slezak, and Bradley, 1994; Fernandes and Ferreira, 2009).

This paper explores a different channel through which insider trading curtails the informational efficiency of financial markets: in addition to crowding out the *information acquisition of outsiders*, insider trading can affect the information environment by distorting the *information supply of insiders*.[2] Specifically, we hypothesize that insider trading opportunities motivate insiders to conceal firms' true performance by supplying low-quality, less transparent information for the purpose of increasing their informational advantage over outsiders and extracting greater trading profits.

Baiman and Verrecchia (1996) show theoretically that high quality financial disclosure, by reducing insiders' information advantage, decreases insider trading profits.

[1] Another body of literature argues that insider trading profits constitute a component of executive compensation that can improve corporate decision-making by aligning the interests of managers and shareholders (e.g., Manne, 1966b; Roulstone, 2003).

[2] Throughout this paper, the phrase "insider trading" refers to insider trades that are based on private information as opposed to those driven purely by portfolio diversification or liquidity needs.

Empirical evidence provided by Aboody et al. (2005) and Skaife et al. (2013) is consistent with this prediction. When making disclosure decisions, insiders likely take into account the negative relation between the quality of financial disclosure and insider trading profits. A number of law studies (e.g., Carlton and Fischel, 1983; Cox, 1986; Kraakman, 1991) argue that insider trading opportunities are likely to distort insiders' incentives to disclose information because the more insiders choose to withhold information, the more they stand to gain from insider trading. Benabou and Laroque (1992) theoretically model the effect of insider trading on insiders' incentives to manipulate information disclosure and suggest that restrictions on insider trading may be required to mitigate such incentives. Consistent with this reasoning, Cheng and Warfield (2005) and McVay et al. (2006) argue that insiders' stock trading provides incentives for earnings management. However, it is difficult to empirically test the theory and establish causality in a cross-sectional setting using data from the U.S. (McVay et al., 2006); the U.S. has seen no major shift in its insider trading regime in recent years because the insider trading laws were implemented (and enforced) as early as 1934 (1961).

In this study, we test the impact of insider trading opportunities on insiders' supply of information by exploiting the variation in insider trading opportunities provided by the unique setting of staggered initial insider trading law enforcement around the world. A country's first-time prosecution of insider trading offenders can signal a discrete increase in the probability of future prosecutions (Bhattacharya and Daouk, 2002). When enforced, insider trading laws can result in monetary penalties and possibly prison time for insiders. The threat is more likely considered real in countries with strong enforcement (Ball, 2001; Fernandes and Ferreira, 2009). By imposing significant legal consequences, initial insider

trading law enforcement in countries with strong enforcement lowers insiders' expectation about their ability to profit from their information advantage through insider trading. We thus hypothesize that this lowered expectation changes insiders' cost-benefit tradeoff when making disclosure decisions, leading to a higher equilibrium level of financial disclosure quality with less earnings management to conceal performance in countries with strong enforcement. We do not have a clear prediction for countries that lack strong enforcement, but we expect the initial enforcement to have different consequences in countries with strong compared to those with weak enforcement.[3]

Our sample includes 40 countries over the 1988–2004 period, during which many countries began to enforce laws restricting insider trading. The information we use on insider trading law enforcement is obtained by Bhattacharya and Daouk (2002) through a survey of 103 countries' securities regulators at the end of 1998. As these authors note, due to difficulties in extracting information from regulators, their data capture only whether a country had an insider trading law and whether there had been any prosecutions under that law, not the degree of consistency in or the intensity of its enforcement. Although these limitations may reduce the power of our tests, subsequent research discoveries using the same data (Bhattacharya and Daouk, 2002; Bushman et al., 2005; Fernandes and Ferreira,

[3] One may make the opposite prediction by arguing that, holding constant the strength of law enforcement, firms from countries with weak institutions and low quality financial reporting prior to the regulatory changes have greater room to improve. However, a large literature shows significant cross-country differences in legal institutions and suggests that the strength of legal institutions is positively related to the effectiveness of regulatory actions (e.g., La Porta et al., 1997, 1998). Following this literature, we conjecture that the enforcement of insider trading laws is more effective in countries with strong institutions and provide supporting evidence based on survey findings. Future research to further validate this conjecture is warranted.

2009) suggest that the data capture meaningful variations in the enforcement of insider trading.

Out of the 40 countries included in our sample, 23 started to enforce insider trading laws during the sample period. There is a wide distribution in the years in which initial enforcement occurred, ranging from 1989 (Israel) to 1998 (India and Spain). Therefore the data provide a setting of staggered events that may be helpful in drawing causal inferences (e.g., Bertrand and Mullainathan, 2003; Armstrong, Balakrishnan, and Cohen, 2012; Roberts and Whited, 2012).[4]

We use earnings smoothing to capture the extent to which insiders manage earnings to conceal firm performance from outside investors. In a cross-country setting, earnings smoothing is a commonly used measure of earnings management starting with Leuz et al. (2003). Leuz et al. (2003) argue that insiders (such as controlling owners or managers) smooth earnings to conceal firm economic performance from outsiders, therefore protecting their private benefits. Dechow et al. (2010), in reviewing the literature on earnings quality, note that the broad conclusion from the cross-country studies is that smoothing lowers earning quality. Lang et al. (2012) use earnings smoothing as a primary measure of reporting transparency. Drawing from the literature (e.g., Lang et al., 2003; Leuz et al., 2003; Bhattacharya et al., 2003; Lang et al., 2006; Barth et al., 2008; Lang et

[4] In studies that aim to examine the effect of a regulatory event (e.g., the Sarbanes-Oxley Act and Regulation Fair Disclosure), it is often difficult to identify control groups that are not affected by the events because all the firms are affected by the regulation at the same time. The setting of staggered events helps to overcome this difficulty; in this setting, for firms in each country undergoing insider trading law enforcement, the control group includes not only firms in countries that have never enforced their insider trading laws but also firms in those countries that have done so in a previous year or will enforce them in the future (Bertrand and Mullainathan, 2003; Armstrong, Balakrishnan, Cohen, 2012; Roberts and Whited, 2012).

al., 2012), we adopt two measures of earnings smoothing: variability of operating income to cash flows and correlation between accruals and cash flows.

In the empirical analysis, we start by following the existing literature (e.g., Leuz et al., 2003; Byard et al., 2011; Barth et al., 2012) and using the strength of legal institutions, as measured by indices developed by La Porta et al. (1997, 1998), to capture the strength of enforcement. Consistent with our hypothesis, our empirical analyses indicate that earnings smoothing decreases following the first-time enforcement of insider trading laws in countries with strong legal institutions. In contrast, we do not find a similar decrease of earnings smoothing in countries with weak legal institutions. In these analyses, we control for an array of firm- and country-level determinants of reporting quality and country, industry, and year fixed effects. Our results are robust to a number of alternative specifications. The evidence suggests that different aspects of a country's legal institution play complementary roles in determining managerial incentives and financial reporting outcomes (Ball, 2001).

To further link changes in earnings smoothing to the enforcement of insider trading laws, we examine how cross-country variation in the strictness of these laws affects the impact of insider trading law enforcement on reporting incentives. Wurgler (2000) suggests a complementary relation between the strength of laws and the strength of enforcement. Effective enforcement of more prohibitive insider trading laws likely has a stronger impact on insiders' expectation of their ability to profit from insider trading. Therefore, we predict that the decrease in earnings smoothing following effective insider trading law enforcement is more pronounced for countries with more prohibitive insider trading laws. Using a score of the stringency of insider trading laws developed by Beny

(2005) based on information from the 1990s, we find that the enforcement of more prohibitive laws leads to a more pronounced decrease in earnings smoothing in countries with strong legal institutions, consistent with our prediction.

Next, noting that the strength of legal institutions is a broad measure and may not accurately capture the strength of enforcement that is directly applicable to insider trading cases, we conduct a survey of our sample countries' financial regulators, stock exchanges, and law professors in order to obtain granular data on the strength of legal enforcement that is specific to insider trading cases. In the survey, we collect information regarding the persistence of enforcement and the strength of enforcement in terms of actually imposed penalty. Enforcement that is more persistent and/or imposes more severe penalty poses more credible threats and likely causes more pronounced changes in insider behavior. Using information from the survey and data sources identified by survey respondents, we are able to obtain detailed enforcement information for a small subsample of countries that have enforced insider trading laws. Empirical analysis finds that the reduction in earnings smoothing following insider trading law enforcement is more pronounced in countries with (1) more persistent enforcement, as indicated by the existence of additional enforcement cases during the five-year period following the initial enforcement, and (2) stronger enforcement, as indicated by more severe penalty *actually imposed* in the prosecution of these insider trading cases. The evidence suggests that stronger legal enforcement applied specifically to insider trading cases is associated with more pronounced changes in financial reporting (earnings smoothing in particular) incentives around initial insider trading law enforcement.

In developing our hypotheses based on the cost-benefit tradeoff in insiders' financial reporting decisions, an implicit assumption is that insider trading profits are a relevant factor, that is, extracting insider trading profits is a binding constraint. While the preceding empirical findings in support of our hypothesis suggest that on average this assumption holds, we expect that the binding constraint is more likely to hold in certain firms than others.

We expect insider trading restrictions to be less binding in closely held firms. Insiders of closely held firms have access to multiple rent-extracting opportunities beyond insider trading (e.g., related-party transactions and management buyouts) that benefit from opaque reporting.[5] The decision to withhold information is thus less likely to be affected by insider trading considerations alone. Additionally, in closely held firms, insider trading profits are likely driven less by the quality of financial disclosure than by other factors such as liquidity. Consequently, insider trading profits are a less important factor in the determination of financial disclosure quality in closely held firms. Insider trading law enforcement therefore would have a less pronounced impact on earnings smoothing in closely held firms. Our results are consistent with this prediction.

We also expect insider trading considerations to be more relevant to the financial disclosure decision of growth firms, where a large portion of firm value is accounted for by growth opportunities instead of assets in place. The nature of growth firms leads to a

[5] Insiders as referred to in this paper include both managers and controlling shareholders, who are shown to have significant influence on firm policies. We also review a number of books that discuss insider trading laws and find that, for the majority of the countries in our sample, the definition of insiders includes a reference to major shareholders. For those laws that do not specifically include major shareholders, they often use terms such as "any person who possesses inside information" (1988 Spanish Securities Market Act, as referred to by Stamp and Welsh, 1996, p. 209), which likely applies to major shareholders.

greater information advantage for the insiders over outsiders and deters external information production; hence outside investors of growth firms have to rely more on firm disclosure for information. This suggests that profiting from insider trading by providing low-quality financial disclosure is likely to be a more effective means of rent extraction for insiders of growth firms. Insider trading restrictions, by reducing the expected trading profits, therefore should have a stronger effect on the financial reporting decisions of growth firms. Again, we find evidence consistent with this prediction. Both tests help connect the change in earnings smoothing to the enforcement of insider trading laws as they show that firms exhibit predicted variations in their reporting response to insider trading restrictions.

As the last part of the analysis, we directly assess the extent to which the increase in country-level stock price informativeness following insider trading law enforcement as documented by Fernandes and Ferreira (2009) is attributable to the improvement in insiders' information supply, as captured by the decrease in earnings smoothing. Consistent with insider information supply being an important determinant of the informational efficiency of stock markets, we find that country-level stock price informativeness is positively associated with earnings smoothness. In addition, using mediation analysis (Hammersley, 2006; Lang et al., 2012), we find that our measure of earnings smoothing explains 37% of the impact of insider trading law enforcement on stock price informativeness. Interestingly, enforcement itself ceases to have a significant impact on stock price informativeness after we control for earnings smoothing. While this finding suggests that the results of Fernandes and Ferreira (2009) are driven by changes in

corporate insiders' information supply, we caution that our sample period and composition differ from theirs due to additional data requirements.

The literature (Bhattacharya and Daouk, 2002; Bushman, Piotroski, and Smith, 2005; Fernandes and Ferreira, 2009) uses the setting of staggered insider trading law enforcement to draw causal inferences, since it is less likely that some other events leading to the predicted changes occur in every case when different countries enforce insider trading laws at different times.[6] However, it is still an important issue whether concurrent confounding events, rather than the enforcement of insider trading laws, lead to the documented decrease in earnings smoothing. Our research design includes features that help mitigate this concern. We control for an array of firm- and country-level characteristics that can capture other macroeconomic changes and therefore help control for the impact of confounding events (Bekaert, Harvey, and Lundblad, 2005). Importantly, we show that the decrease in earnings smoothing in countries with strong legal institutions is a function of the stringency of insider trading laws, linking the change in financial reporting behavior more closely to insider trading law enforcement. This inference is further corroborated by additional analysis based on granular data that directly capture the persistence of insider trading enforcement and the penalty that is actually imposed in insider trading enforcement. Also, we find that the documented decrease in earnings smoothing varies with firm characteristics that are predicted to affect firms' reactions to the enforcement of insider trading laws. Potential confounding events have to explain these

[6] To affect the inferences in the setting of staggered events, the potential confounding events need to (1) coincide with the specific time of initial insider-trading law enforcement for each specific country, (2) occur in a significant number of our sample countries, and (3) have on average a positive effect on reporting quality.

systematic variations in order to be a plausible alternative explanation of our results. Lastly, we perform several analyses to directly identify possible confounding events, including searching the leading practitioner literature in countries with accessible data and surveying academics in each country about possible confounding events. While we cannot fully rule out the possibility that confounding events may affect our results, our tests suggest that they are unlikely to be the sole driver of our findings.

This study makes several contributions to the literature. First, we add to the ongoing debate over the costs and benefits of insider trading and the need for regulation by uncovering an important channel through which insider trading restrictions help to improve firms' information environment. We provide evidence on the impact of first-time insider trading law enforcement on insiders' supply of information, extending the line of research on the relation between insider trading and the information environment (e.g., Fernandes and Ferreira, 2009).

Second, this study furthers the understanding of the relation between insider trading and corporate financial reporting choices. Empirical evidence suggests that when managers engage in opportunistic reporting or fraud they tend to reap the benefits through insider trading (e.g., Summers and Sweeney, 1998; Beneish and Vargus, 2002; Cheng and Warfield, 2005; McVay, Nagar, and Tang, 2006; Karpoff, Lee, and Martin, 2008; Agrawal and Cooper, 2008). However, the role of insider trading in these cases is not clear. The positive association between insider trading and earnings management may indicate that insider trading provides incentives to distort corporate disclosure for the purpose of gaining a larger informational advantage over outsiders and boosting their own trading profits. Under this interpretation, insider trading opportunities induce earning manipulation and

undercut economic efficiency. However, an alternative interpretation of this association is that insiders passively trade on their knowledge of earnings management that is driven by other reasons (e.g., equity financing and executive compensation). In this scenario, insider trading could actually alert investors to earnings management on the part of insiders, thereby helping to impound information about earnings manipulations into stock prices and helping to correct the mispricing induced by such manipulations. With this interpretation, insider trading would improve the informational efficiency of financial markets. Although both interpretations are consistent with prior empirical findings, they have different regulatory implications. We exploit the unique setting of insider trading law enforcement and provide evidence in support of the former interpretation.[7]

Finally, we contribute to the literature on securities legislation and disclosure regulations by highlighting the importance of legal institutions and managerial incentives in determining the consequences of regulations. We show that country-level legal institutions cause variations in managerial incentives and thus variations in firms' response to regulations. Ball (2001) contends that simply transporting regulations from one economic environment to another can be unfruitful. Our results suggest that similar regulations, even when enforced, may have different consequences depending on the strength of enforcement.

While this study focuses on the impact of insider trading opportunities on mandatory reporting incentives, similar arguments can also be applied to voluntary

[7] Note that the two interpretations of the relation between insider trading and earnings management are not mutually exclusive. Our evidence does not negate the possibility that insiders benefit from a pre-existing information advantage through trading the stocks of their own firm.

disclosure, which is shown to be related to insider trading (e.g., Cheng and Lo, 2006). We believe that voluntary disclosure offers an interesting setting for testing our arguments. Future research examining voluntary disclosure would provide additional evidence on how insider trading opportunities affect management reporting. We also note that our measure of financial disclosure quality, earnings smoothing, is affected not only by deliberate manipulation but also by the underlying economics. We follow the literature and control for an array of firm- and country-level determinants of earnings smoothness. Yet smoothing needs to be interpreted with caution as a measure of earnings quality.

The remainder of the paper is organized as follows. Section 2 provides literature review and hypothesis development. Section 3 describes the research design adopted to test the impact of insider trading law enforcement on earnings smoothing. Section 4 presents the empirical evidence, and Section 5 concludes.

## 2. Hypothesis Development

Kyle (1985) demonstrates theoretically that insider trading profits increase with their information advantage over other investors. Empirical evidence (e.g. Bettis, Coles, and Lemmon, 2000; Aboody and Lev, 2000; Huddart and Ke, 2007) generally supports this prediction. On the other hand, theories (Diamond, 1985; Bushman, 1991; Lundholm, 1991) suggest that public disclosure reduces information asymmetry by providing investors equal access to information. A substantial literature empirically documents the negative relation between disclosure quality and information asymmetry (Welker, 1995; Healy, Hutton and Palepu, 1999; Leuz and Verrecchia, 2000; Heflin, Shaw, and Wild, 2005; Brown and Hillegeist, 2007).

By introducing financial disclosure into Kyle's (1985) model, Baiman and Verrecchia (1996) link these two strands of literature. They show that, when financial disclosure improves, insiders' information advantage decreases and insider trading profits decrease.[8] Consistent with the theoretical prediction of Baiman and Verrecchia (1996), a number of studies provide empirical evidence suggesting that higher reporting quality reduces insider trading profits in the United States.[9] Aboody et al. (2005) use a firm's exposure to an earnings quality factor in the context of a Fama-French three-factor model to measure the systematic component of information asymmetry that arises from low earnings quality. They find that both the frequency and the profitability of insider trades are positively correlated with the firm's exposure to the earnings quality factor. Skaife et al. (2013) find that insider trading profits, which take into account both the excess return following insider trades and the value of shares traded, are greater in firms with weak internal control relative to firms with effective control.[10,11]

[8] For insider trading to be profitable, the stock prices must eventually incorporate insiders' private information (at least partially). This can happen by two mechanisms. Stock prices can gradually reflect firms' true performance because of other information sources such as industry peers' reported performance and industry- and economy-wide information events (e.g., Piotroski and Roulstone, 2004; Thomas and Zhang, 2008; Durnev and Mangen, 2009). Insiders' private information can also be incorporated into stock prices when other traders observe the order flow (e.g., Kyle, 1985). Huddart, Hughes, and Levine (2001) further show that the after-the-fact disclosure of insider trades accelerates price discovery relative to Kyle (1985). Our investigation indicates that the after-the-fact disclosure of insider trades is a common requirement in our sample countries.

[9] This line of research is usually carried out in the U.S. because of data availability. However, the U.S. is well known for its early and relatively effective enforcement of insider trading laws, which likely works against finding strong evidence for opportunistic insider trading.

[10] While our measure of earnings quality is commonly used in the international setting, there is no direct evidence in the literature on how this measure is related to insider trading profits. In Section 4.2.4, we test the association between earnings smoothing and insider trading profits using U.S. data.

[11] Related studies (Summers and Sweeney, 1998; Beneish, 1999; Beneish and Vargus, 2002) also suggest that insiders systematically engage in insider trading to exploit the information asymmetry between insiders and outsiders due to low-quality financial reporting. To the extent that insiders

Hence, the more high-quality, transparent information insiders supply through financial reports, the lower are their information advantage and trading profits. When making disclosure decisions, insiders likely take into consideration this negative relation. A number of law studies (e.g., Carlton and Fischel, 1983; Cox, 1986; Kraakman, 1991) argue that insider trading opportunities are likely to distort insiders' incentives to disclose information because the more insiders choose to withhold information, the more they stand to gain from insider trading. Benabou and Laroque (1992) theoretically model the effect of insider trading on insiders' incentives to manipulate information disclosure. Their model suggests that such incentives exist even in an infinitely repeated game with learning by outside investors; they therefore suggest that restrictions on insider trading may be required to mitigate such incentives. There are two related empirical studies in accounting. Cheng and Warfield (2005) hypothesize and find evidence consistent with the argument that high equity incentives motivate managers to manage earnings to increase the value of their shares to be sold. McVay et al. (2006) show that the likelihood of reported earnings being managed to just meet consensus analyst forecasts is positively associated with subsequent stock sales for managers, and that this relation does not hold for nonmanager insiders. They conclude that stock sales provide an incentive for earnings management. However, as noted by McVay et al. (2006), it is difficult to test the theory and establish causality in the cross-sectional setting using insider trading data from the U.S.

Figure 1 provides a parsimonious description of the cost-benefit tradeoff facing insiders in the decision of information supply through financial reports. Consistent with

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trade more aggressively when they expect higher profits, this evidence is also consistent with the negative relation between information supply through financial reports and insider trading profits.

common assumptions, the marginal cost curve (the marginal benefit curve) slopes upward (downward), indicating increasing marginal costs of (decreasing marginal returns to) information supply. The equilibrium point is at the intersection of the two curves, where the marginal cost is equal to the marginal benefit. To corporate insiders, the costs of producing informative financial reports include, among other things, the forfeiture of insider trading profits because informative financial reports leads to a reduction in insiders' informational advantage over other investors (Benabou and Laroque, 1992). The benefits of informative financial reports include factors such as low cost of capital (e.g., Baiman and Verrecchia, 1996). Ideally, to identify the causal effect of insider trading opportunities on insiders' information supply, one needs to find a change in insider trading opportunities that shifts the whole marginal cost curve either upwards or downwards and is exogenous to firm-specific characteristics or incentives.

The staggered initial enforcement of insider trading laws around the world provides such a setting.[12] Several recent empirical studies use this setting to gauge the economic impact of insider trading. Bhattacharya and Daouk (2002) find a significant decrease in a country's cost of equity following the initial insider trading law enforcement. Focusing on the relation between insider trading and informational efficiency, Fernandes and Ferreira (2009) show that stock price informativeness increases after the initial enforcement of insider trading laws. Bushman, Piotroski, and Smith (2005) document an

[12] We focus on the impact of enforcement as opposed to the existence of laws because previous studies suggest that it is the enforcement, rather than the mere existence of insider trading laws, that deters insiders (Posen, 1991; Stamp and Welsh, 1996; Bhattacharya, Daouk, Jorgenson, and Kehr, 2000; Bhattacharya and Daouk, 2002, Fernandes and Ferreira, 2009).

increase in analyst following after initial enforcement, consistent with insider trading crowding out information acquisition prior to the enforcement.

The initial insider trading law enforcement in a country can signal a significant increase in the likelihood of future enforcement in that country. By increasing the probability of monetary penalties and jail time, enforcement lowers insiders' expectation about their ability to profit from their informational advantage through insider trading. Since, from insiders' perspective, a cost in providing informative financial reports is the forfeited insider trading profit, the initial insider trading law enforcement leads to an increase in the equilibrium level of information supply through financial reports. As illustrated in Figure 1, the initial insider trading law enforcement in a country moves the marginal cost curve downward (from the solid line to the dotted line), increasing the equilibrium information supply from IS* to IS'.[13] In other words, the existence of insider trading opportunities provides incentives for insiders to conceal information from outsiders through, for example, earnings management. Such incentives are weakened once insider trading opportunities are reduced, resulting in greater information supply by insiders, and in particular, less managed earnings that are more reflective of economic performance.

We expect the impact of initial insider trading law enforcement to be significant in countries with strong legal institutions. Holding constant the strength of law enforcement, firms from countries with weak institutions and low quality financial reporting prior to the

[13] Our prediction is based on the standard economic theories where insiders' trade-off of marginal costs and marginal benefits determines the equilibrium level of information supply through financial reports. However, under a different framework of analysis (for example, under the assumption that insiders must maintain the same level of profits), a possible prediction is that, following insider trading law enforcement, insiders choose a lower level of information supply.

regulatory changes may have greater room to improve. However, a large literature since La Porta et al. (1997, 1998) suggests that countries differ in the quality of enforcement and such differences have a significant impact on the functioning of financial markets and the economy and the effectiveness of regulatory actions. A strong legal regime can help ensure the persistence and strength of the enforcement of insider trading laws, strengthening the incentive effect of insider trading law enforcement. Enforcement matters in changing behavior because potential wrongdoers must believe that they will be prosecuted if they break the laws (Lopez-de-Silanes, 2003). In contrast, for countries with weak legal institutions, the enforcement of insider trading laws is less likely to be effective or persistent, in which case the impact of initial enforcement on insiders' expected costs of trading can be minimal. In the extreme case, if the threat of effective and persistent enforcement is not credible, insiders would not perceive a downward shift of the marginal cost curve as depicted in Figure 1, and the equilibrium information supply through financial reports would not decrease at all following the initial insider trading enforcement. Consistent with this argument, a number of studies (e.g., Daske et al., 2008; Byard et al., 2011; Barth et al., 2012; Christensen et al., 2013) show that firms from countries with weak legal institutions exhibit lesser response to regulatory changes.

Accordingly, we predict the impact of insider trading law enforcement on the supply of information to be significant in countries with strong legal enforcement. More specifically, we expect that insider trading law enforcement increases information supply by reducing insiders' earnings management to conceal firms' economic performance. We further expect the impact to be stronger in these countries than in those with weak enforcement. This leads to our first and second hypotheses (stated in alternative form):

H1: Ceteris paribus, initial insider trading law enforcement leads to a decrease in earnings management to conceal information in countries with strong enforcement.

H2: The decrease in earnings management to conceal information following initial insider trading law enforcement is more pronounced for countries with stronger enforcement.

Our predictions potentially reconcile Bushman et al. (2005) and Fernandes and Ferreira (2009). Although both studies point to an improvement in information environment following insider trading restrictions, Fernandes and Ferreira (2009) show the improvement in stock price informativeness to be concentrated in developed countries, while Bushman et al. (2005) find the increase in analyst following to be concentrated in emerging markets. As Fernandes and Ferreira (2009) note, an explanation for the different results may be that analyst activities do not produce significant firm-specific information and therefore more analyst following may not increase stock price informativeness.[14] If analyst activities do not explain Fernandes and Ferreira's (2009) findings, an important question that remains unanswered is exactly what changes around initial enforcement of insider trading laws and causes stock price informativeness to increase. We extend this literature by proposing insiders' supply of information as an important mechanism through which insider trading law enforcement affects the information environment.

In addition to enforcement, the strictness of the laws can also influence behavior. Wurgler (2000) suggests that the laws are less likely to be effective if a country has either strong but poorly enforced laws or strictly enforced but weak laws. Accordingly, Wurgler suggests a complementary relation between the strength of laws and the effectiveness of

[14] See Easley, O'Hara, and Paperman (1998), Piotroski and Roulstone (2004), and Chan and Hameed (2006) for more detailed discussions of analyst information production.

enforcement. Thus we expect the initial enforcement of insider trading laws to have a larger deterrent effect on insider trading, and consequently a larger incentive effect on insiders' information supply and earnings management, if the laws being effectively enforced are stricter in terms of scope and sanction. This leads to our last hypothesis.

H3: The decrease in earnings management to conceal information following initial insider trading law enforcement is more pronounced in countries that effectively enforce more stringent insider trading laws.

## 3. Research design

### *3.1. Measure of insiders' earnings management to conceal information*

We use earnings smoothing to capture insiders' earnings management to conceal information and therefore the level of their supply of information.[15] Leuz et al. (2003) argue that insiders (such as controlling owners or managers) can use earnings smoothing to conceal firm economic performance from outsiders, therefore protecting their private benefits. Dechow et al. (2010), in reviewing the literature on earnings quality, note that the broad conclusion from the cross-country studies is that smoothing lowers earning quality. Lang et al. (2012) use earnings smoothing as a primary measure of reporting transparency.

Drawing from the literature (e.g., Lang et al., 2003; Leuz et al., 2003; Bhattacharya et al., 2003; Lang et al., 2006; Barth et al., 2008; Lang et al., 2012), we examine two

[15] Another measure of reporting quality is the informativeness of earnings announcements. However, this measure is related to not only reporting quality but also to the intensity of insider trading and information leakage before earnings announcements (DeFond, Hung, and Trezevant, 2007; Bhattacharya et al., 2000). Since insider trading enforcement is likely associated with a significant reduction in insider trading and information leakage before earnings announcements, it would be difficult to draw unambiguous conclusions regarding the source of any change in earnings announcement informativeness surrounding insider trading enforcement.

commonly used measures of earnings smoothing: variability of operating income to cash flows and correlation between accruals and cash flows. The first (inverse) measure of earnings smoothing (*SMTH1*) is constructed based on the argument that the more firms use accruals to manage earnings, the smoother earnings will be relative to cash flows (e.g., Leuz et al., 2003; Lang et al., 2012). It is computed as the standard deviation of operating income divided by the standard deviation of cash flows from operations. A higher value for *SMTH1* indicates a lower level of earnings smoothing and therefore a lower level of earnings management and a higher level of information supply through financial reports. As in Lang et al. (2012), both earnings and cash flows are scaled by average total assets, and the standard deviations are calculated using rolling windows of five years, requiring a minimum of three years of data. Cash flow is computed indirectly by subtracting accruals from earnings, following the prior literature (e.g., Leuz et al., 2003), where accruals are defined as follows:

$Accruals_{it} = (\Delta Current\ Assets_{it} - \Delta Cash_{it}) - (\Delta Current\ Liabilities_{it} - \Delta Short\ Term\ Debt_{it} - \Delta Income\ Taxes\ Payable_{it}) - Depreciation_{it}$

If a firm does not report information on taxes payable or short-term debt, then the change in both variables is assumed to be zero.

The second (inverse) measure of earnings smoothing (*SMTH2*) is the Spearman correlation between accruals and cash flows (Leuz et al., 2003; Lang et al., 2006; Barth et al., 2008). In good times with high operating cash flows, managers can manipulate earnings by understating accruals and creating reserves for the future. These reserves will then be reversed to boost earnings in bad times when operating cash flows are low. Accruals and cash flows will be more negatively correlated when managers engage in earnings

smoothing. Thus, a higher value for *SMTH2* indicates a lower level of earnings smoothing, a lower level of earnings management, and therefore a higher level of information supply through financial reports. Similar to how we compute *SMTH1*, we calculate the correlation using rolling windows of five years, requiring a minimum of three and a maximum of five years of data. As in Lang et al. (2012), we combine the two measures of earnings smoothing by scaling them into percentile ranks, summing up the two ranks, and taking the average. The resulting variable, *SMTH*, is the aggregate measure of earnings smoothing. Higher values of *SMTH* correspond to less earnings smoothing and more information supply through financial reports.

The literature has established that accounting attributes are affected not only by managerial opportunism but also by firms' economic fundamentals such as size and growth. We adopt two approaches to control for the fundamentals that affect firms' operating environment and therefore earnings smoothing. Under the first approach, we use *SMTH* in the tests directly and control for factors that may affect earnings variability and the correlation between accruals and cash flows. Under the alternative approach, we construct a discretionary smoothing measure, *R_SMTH*, using the residual from the following regression: [16]

$$SMTH1_{it}\ (SMTH2_{it}) = \beta_0 + \beta_1\ SIZE_{it} + \beta_1 LEV_{it} + \beta_2\ BTM_{it} + \beta_3\ STD\_SALES_{it} + \beta_4 PCT\_LOSS_{it} + \beta_5\ OPCYCLE_{it} + \beta_6\ GROWTH_{it} + \beta_7\ OPLEV_{it} + \beta_8\ AVGCFO_{it} + Fixed\ Effects + \varepsilon_{it} \quad (1)$$

Following the literature (e.g., Lang et al., 2012), to isolate the discretionary portion of earnings smoothness, we control for firm size (*SIZE*), computed as the natural logarithm

[16] For space considerations, estimates of equations (1) to construct *R_SMTH* are not tabulated. They are available from the authors upon request.

of total assets measured in U.S. dollars; leverage (*LEV*), defined as long-term debt over total assets; book-to-market ratio (*BTM*); *STD_SALES*, the standard deviation of sales; *PCT_LOSS*, the percentage of loss years during the last five years; operating cycle (*OPCYCLE*), the logarithm of the number of days of accounts receivable and inventory; sales growth (*GROWTH*); *OPLEV*, net property, plant, and equipment over total assets; average cash flows (*AVGCFO*) during the past five years; and industry, year, and country fixed effects.[17] As in Lang et al. (2012), we estimate the regression for the two smoothing measures separately and obtain the residuals from each regression as the discretionary smoothing measures. We then convert the residuals into percentile ranks and take the average of the two measures as the aggregate discretionary smoothing measure, *R_SMTH*.

We conduct the tests using both *SMTH* and *R_SMTH* in our subsequent analyses. They yield the same inferences. For the sake of brevity, we report the main results using both measures and the results of additional analyses using *SMTH* only.

*3.2. Insider trading laws and insiders' earnings management to conceal information*

We examine the impact of initial enforcement of insider trading laws on insiders' earnings management to conceal information via earnings smoothing using the following regression:

$$SMTH_{it} = \beta_0 + \beta_1 ENFORCE_{it} + \sum\gamma_j Firm\text{-}level\ Controls + \sum\theta_k Country\text{-}level\ Controls + Fixed\ Effects + \varepsilon_{it} \quad (2)$$

*Enforce* is an indicator variable equal to one for country-years after the enforcement of insider trading laws and zero otherwise. After controlling for other factors that may affect

[17] All variable definitions are detailed in the appendix.

earnings smoothness, the coefficient on *ENFORCE* captures the impact of enforcement on *SMTH*. We do not have a prediction on the overall average effect of the initial enforcement on insiders' information supply as captured by the extent of earnings smoothing. Instead, we expect the impact of enforcement to vary with managerial incentives that are influenced by country- and firm-level governance mechanisms, including legal institutions and ownership structures.

Firm-level controls include all variables used in model (1) to capture the nondiscretionary portion of earnings smoothing. In addition, we control for mechanisms that can curb earnings management. We control for the quality of auditing (*AUD*) using an indicator variable that equals one if a firm's auditor is one of the Big Five accounting firms (PwC, KPMG, Arthur Andersen, Ernst & Young, and Deloitte & Touche) and zero otherwise. Some firms may have voluntarily adopted IFRS or U.S. GAAP in our sample period. Prior research finds that firms voluntarily adopting IFRS or U.S. GAAP experience an increase in the quality of financial reporting (Lang et al., 2003; Barth et al., 2008), although it is unclear whether the improvement results from firms' incentives to access the external capital markets or the adoption of a new set of accounting standards. We include an indicator variable equal to one for firms reporting under internationally recognized accounting standards (IFRS or U.S. GAAP). We classify firms into those following IFRS/U.S.GAAP accounting standards and those following domestic accounting standards based on the information provided by Worldscope.[18] Foreign firms cross-listed in the U.S.

[18] The coding is based on information in Worldscope field 07536, *Accounting standards followed*. Following the procedure used by Daske et al. (2007), if the Worldscope description is "IFRS," "International standards," "International standards and some EEC guidelines," "Local standards with EEC and IASC guidelines," "Local standards with OECD and IASC guidelines," "Local

are subject to greater U.S. oversight and more litigation exposure. Lang et al. (2003) find that foreign firms cross-listed in the U.S. exhibit higher reporting quality than their domestic peers. We thus include an indicator variable capturing U.S. cross-listing (*XLIST*) in the regression.

As in Bushman et al. (2005), we control for a number of country-level variables that can affect accounting attributes, including a country's per capita wealth ($LOG\_GDP_{it}$), annual growth in real GDP per capita ($GROWTH\_GDP_{it}$), the size of the domestic capital market ($LOG\_LISTED_{it}$), foreign direct investments ($FDI_{it}$), trade openness ($OPEN_{it}$), and whether a country's capital market is liberalized ($LIB_{it}$).[19]

Finally, industry, year, and country fixed effects are included in the regression. All continuous variables are winsorized at the top and bottom 1% to mitigate the undue impact of outliers. We estimate equation (1) pooling all firm-year observations from 1988 through 2004. We use robust standard errors clustered by country-year to draw inferences.

Building on equation (2), we expect that the impact of enforcing insider trading laws on insiders' supply of information varies with the strength of legal institutions. The more effective the enforcement of insider trading laws and other securities regulations, the better are managers prevented from extracting private benefits through opaque reporting and the more likely managers find that the benefits of providing transparent information outweigh the private costs of doing so. To examine the differential impact of enforcement

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standards with some IASC guidelines," we classify the observation as using IFRS; if the Worldscope description is "US standards (GAAP)" or "US GAAP reclassified from local standards," we classify the observation as using U.S. GAAP. Finally, all other cases, except when the applicable accounting standards are not disclosed, are classified as using local accounting standards.

[19] We do not separately control for time-invariant country-level variables, such as the legal origin and accounting standards. This is because all our regressions include country fixed effects, which subsume the impact of all such factors.

of insider trading laws in countries with strong and weak legal institutions, we estimate a variation of equation (2) by allowing the coefficient on *Enforce* to vary with the strength of legal institutions:

$$SMTH_{it} = \beta_0 + \beta_1 STRONG_{it}*ENFORCE_{it} + \beta_2 WEAK_{it}*ENFORCE_{it} + \sum\gamma_j Firm\text{-}level\ Control_j + \sum\theta_k Country\text{-}level\ Control_k + Fixed\ Effects + \varepsilon_{it} \quad (3)$$

The variable *STRONG* is set to one if a country has strong legal institutions, as captured by an above median rating of the efficiency of the judicial system, rule of law, and corruption, based on La Porta et al. (1998), and if the country is a developed market, and zero otherwise.[20] The variable *WEAK* is set to one if a country has a weaker enforcement structure, that is, if it does not have an above-median rating in any of the three dimensions, the efficiency of the judicial system, rule of law, and corruption, or if the country is an emerging market, and zero otherwise. Thus the coefficient on *STRONG*ENFORCE* captures the impact of enforcement in countries with strong legal institutions, while that on *WEAK*ENFORCE* captures the impact of enforcement in countries with weak legal institutions.[21] H1 predicts a positive coefficient on *STRONG*ENFORCE*—that is, it predicts an increase in insiders' information supply (a decrease in earnings smoothing) in countries with strong legal institutions following insider trading law enforcement. H2 predicts that the coefficient on *STRONG*ENFORCE* differs from that on *WEAK*ENFORCE*.

[20] La Porta et al.'s (1998) ratings of legal institutions are constructed such that a higher value corresponds to stronger legal environment. For example, a higher value for the rating of corruption indicates a lower degree of corruption.

[21] Although *STRONG* and *WEAK* are time-invariant measures, *ENFORCE* varies over time for the countries of interest. The interactive terms *STRONG*ENFORCE* and *WEAK*ENFORCE* therefore vary over time for the countries of interest and are not subsumed by the country fixed effects.

To link the changes in earnings smoothing more closely to the enforcement of insider trading laws, we examine how the strictness of insider trading laws influences the relation between enforcement and earnings smoothing. We estimate the following variation of equation (3):

$$SMTH_{it}=\beta_0+\beta_1 STRONG_{it}*ENFORCE_{it}+\beta_2 WEAK_{it}*ENFORCE_{it}+\beta_3 NOTSTRICT_{it}*STRONG_{it}*ENFORCE_{it} + \beta_4\ NOTSTRICT_{it}*WEAK_{it}*ENFORCE_{it} + \beta_5\ NOTSTRICT_{it}*STRONG_{it} + \sum\gamma_j\ Firm\text{-}level\ Control_j + \sum\theta_k\ Country\text{-}level\ Control_k + Fixed\ Effects + \varepsilon_{it} \quad (4)$$

The strictness of insider trading laws is measured following Beny (2005).[22] Beny (2005) constructs a score based on four dimensions capturing the scope and sanction of the laws: whether corporate insiders are prohibited from tipping outsiders about material nonpublic information and/or encouraging them to trade on such information for personal gain, whether outsiders are prohibited from trading on material nonpublic information that they have received from corporate insiders, whether potential monetary penalties for violating insider trading laws are proportional to insiders' trading profits, and whether violation of insider trading laws is a potential criminal offense. The resulting score ranges from one to four, with higher values indicating stricter laws. The variable *NOTSTRICT* is defined in two ways: it is either set equal to one if a country has less strict insider trading laws (a score of one or two) and zero for a country with stricter laws (a score of three or four), or set equal to the maximum score (four) minus the actual score. High values of *NOTSTRICT* thus represent less stringent laws. We expect the enforcement of insider trading laws to have a stronger impact on insiders' information supply when the laws are stricter (H3), predicting a negative coefficient on *NOTSTRICT*STRONG*ENFORCE*. It is unclear

[22] Beny's (2005) rating is based on information from Gaillard (1992) and Stamp and Welsh (1996).

whether, in countries with weak legal institutions, the stringency of laws would matter without effective enforcement.

## 4. Empirical results

### *4.1. Sample selection and descriptive statistics*

We obtain the country-level economic data from the World Development Indicators database and the legal structures data from La Porta et al. (1998). Our firm-level financial data are extracted from the Worldscope database. As the financial data necessary for computing accruals and cash flows and thus for measuring earnings smoothing are missing for many firms until the late 1980s, we start our sample period in 1988. Our sample ends in 2004, right before the year when most European firms are required to adopt the International Financial Accounting Standards (IFRS) by the European Union. A number of studies have documented that the adoption of IFRS is associated with higher reporting quality and lower information asymmetry (e.g., Daske et al., 2008; Landsman, Maydew, and Thornock, 2011). To ensure that our analysis captures the impact of enforcement of insider trading laws rather than that of IFRS adoption, we stop before the mandatory adoption of IFRS in the European Union. We also drop the year of enforcement to avoid contamination.

Bhattacharya and Daouk's (2002) data cover enforcement actions of insider trading laws up to 1998. Since we obtain the insider trading law enforcement data from Bhattacharya and Daouk (2002), we do not capture potential enforcement actions after 1998 in countries that have not enforced insider trading laws by the end of 1998. If some of these countries enforced insider trading laws between 1998 and 2004, the variable

*Enforce* can be miscoded for these countries during 1998–2004. We therefore exclude the post-1998 observations of countries that have not enforced insider trading laws by the end of 1998. Unreported analysis indicates that including these observations does not change our inferences.

Table 1 presents the distribution of the sample by country, the year of first enforcement of insider trading laws by country, and the descriptive statistics of variables capturing country-level economic development and legal institutions. Our sample covers 40 countries and 74,247 firm-year observations. During our sample period, 23 countries experienced first-time enforcement of insider trading laws. For these countries, the first enforcement year ranges from 1989 (Israel) to 1998 (India and Spain). Six countries enforced insider trading laws before our sample period starts. In terms of economic development, Switzerland has the highest level of average per capita GDP, while India has the lowest; Malaysia exhibits the highest average GDP growth in the sample period, while Venezuela shows the lowest. Finland has the smallest market as captured by the number of listed firms; the United States has the largest. To facilitate the interpretation of the interaction of *ENFORCE* and the strength of legal institutions in equation (3), all the legal institution variables are defined as dummy variables. Except for *Emerging*, a value of one for the legal institution variables—*EFF_JUD*, *RULE_LAW*, and *CORRUPTION*—indicates a stronger enforcement environment. Beny's (2005) score of insider trading laws is reported in the last column of Table 1.

**[Insert Table 1 here]**

Table 2 reports the descriptive statistics of firm-level variables. The mean and median of *SMTH1* resemble those reported in Lang et al. (2012) based on a slightly

different sample. The mean and median of our second measure of earnings smoothing, *SMTH2*, differs slightly from those in Lang et al. (2012). We use the Spearman correlation between accruals and cash flows, rather than the Pearson correlation, following most of the literature (e.g., Leuz et al., 2003; Lang et al., 2003; Barth et al., 2008). Our inferences are not sensitive to constructing the variable alternatively based on the Pearson correlation.

**[Insert Table 2 here]**

*4.2. Enforcement of insider trading laws and legal institutions*

4.2.1 Main results

Table 3 Panel A reports estimates of equations (2) and (3). All inferences are based on robust standard errors clustered at the country-year level. Column (1) reports estimation results of equation (2). The coefficient on *ENFORCE* is significantly positive with a *p*-value of 3.08%, suggesting that, on average, earnings smoothing decreases significantly after the initial enforcement of insider trading laws. Column (2) report estimates of equation (3), allowing the impact of enforcement to vary with the strength of legal environment. The coefficient on *STRONG*ENFORCE* is significantly positive at better than the 1% level, consistent with the prediction of H1. In contrast, the coefficient on *WEAK*ENFORCE* is not significantly different from zero, indicating no detectable decrease in earnings smoothing in countries with weak legal institutions. The difference between the two coefficients is significant at better than the 5% level, consistent with H2. The results suggest that the overall increase in information supply (decrease in earnings smoothing) shown in column (1) is driven by countries with strong legal institutions. The

firm-level monitoring variables—*AUD*, *ACCTSTD*, and *XLIST*—all load significantly with the expected sign.[23]

**[Insert Table 3 here]**

In Panel B, we estimate model (3) using each of the four sub-dimensions of legal environment—the efficiency of the judicial system (*EFF_JUD*), rule of law (*RULE_LAW*), corruption (*CORRUPTION*), and the stage of development (*EMERGING*)—to capture the strength of legal institutions. Across all four columns, the coefficient on *STRONG*ENFORCE* is positive and significant, indicating that earnings smoothing declines post first-time enforcement of insider trading laws in countries with strong legal institutions. In contrast, the coefficient on *WEAK*ENFORCE* is never significantly different from zero, indicating no decrease in earnings smoothing in countries with weak legal institutions. The difference between the two coefficients is significant in all cases except for column (4) with a p-value of 11.79%.

Panel C columns (1-2) reports the test results of H1 and H2 using the alternative earnings smoothing measure, *R_SMTH* (discretionary smoothness). *R_SMTH* is constructed based on the residuals from regression (1) and captures the discretionary portion of earnings smoothing. We estimate equation (3) using *R_SMTH* as the dependent variable with and without the firm-level control variables in equation (1). For the sake of

[23] If we drop country fixed effects and replace *STRONG*ENFORCE* and *WEAK*ENFORCE* with *STRONG*, *ENFORCE*, and *STRONG*ENFORCE*, the coefficients on the latter three variables are all positive. The results suggest that firms from countries with strong legal institutions exhibit higher reporting quality before the enforcement, consistent with Leuz et al. (2002), and that enforcement has a larger impact in these countries. Different from the main results, this test suggests that firms from countries with weak institutions also experience an increase in reporting quality. We believe that our main regression model is better specified since the model without country fixed effects is more likely to suffer from an omitted correlated variables problem.

brevity, we only report the coefficients on the variables of interest. In both cases, the coefficient on *STRONG*ENFORCE* is significantly positive, while that on *WEAK*ENFORCE* is insignificant, with the difference between the two highly significant, consistent with H1 and H2.

Our predictions focus on the discretionary portion of earnings smoothing and have no implications for the portion determined by fundamental economics. Yet, to strengthen our inferences on discretionary smoothing, we also estimate equation (3) using the predicted portion of earnings smoothing (*P_SMTH*, fundamental smoothness) based on equation (1) and report the results in columns (3–4) of Panel C. No matter whether the firm-level controls in equation (1) are included, there is no evidence that firms in countries with strong legal institutions experience an increase in *P_SMTH*. Neither is the coefficient on *STRONG*ENFORCE* significantly different from that on *WEAK*ENFORCE*. The results further validate our inferences from Panel A and Panel B with *SMTH* as the measure of earnings smoothing.

Finally, we adopt a specification alternative to model (3), allowing all the firm- and country-level control variables to have differential effects on *SMTH* for strong and weak enforcement countries. Specifically, we estimate the following equation:

$$SMTH_{it} = \beta_0 + \beta_1 STRONG_{it}*ENFORCE_{it} + \beta_2 WEAK_{it}*ENFORCE_{it} + \sum\gamma_j Firm\text{-}level\ Control_j + \sum\theta_k Country\text{-}level\ Control_k + \sum\gamma'_j STRONG*Firm\text{-}level\ Control_j + \sum\theta'_k STRONG*Country\text{-}level\ Control_k + Fixed\ Effects + \varepsilon_{it} \quad (3')$$

The results are presented in Table 3 Panel D. Again, we find strong evidence that in countries with strong legal institutions, earnings smoothing is attenuated post enforcement, consistent with our expectation that insider trading restrictions, together with a strict legal environment, motivate managers to increase information supply. In countries with weak

legal institutions, insider trading restrictions do not appear to provide sufficient incentives for managers to shy away from concealing information, as evidenced by the insignificant coefficient on *WEAK*ENFORCE*.

4.2.2 Event analysis

To strengthen the link between enforcement of insider trading laws and changes in earnings smoothing, we also perform an event analysis, focusing on shorter windows around the initial enforcement of insider trading laws in countries that enforced the laws before 1998. The results of this analysis are reported in Table 4.

**[Insert Table 4 here]**

Column (1) of Table 4 presents the estimation results of equation (3) over the [-5, 5] event window around the initial enforcement of insider trading laws. As in other tests, the enforcement year, (i.e., event year zero) is dropped. The sample is reduced to 17,829 observations. Again, the coefficient on *STRONG*ENFORCE* is positive and significant while that on *WEAK*ENFORCE* is insignificant, consistent with the inferences from the main analysis. Since we use rolling windows to construct our earnings smoothing measure, it is possible that the measurement of smoothing during the first several years post enforcement is contaminated. In column (2) of Table 4, we drop event year one, in addition to event year zero, and still examine the same event window as in column (1). In column (3), we exclude event years from zero to two. In all three columns, we obtain the same inferences—the initial enforcement of insider trading laws is followed by a decrease in earnings smoothing only in countries with strong legal institutions, and the impact differs significantly from that in countries with weak legal institutions. Again, the evidence points

to the importance of legal institutions in incentivizing managers to improve information supply in response to insider trading restrictions.

4.2.3 Robustness checks

We also conduct a number of robustness tests and report the results in Table 5. Three countries—Japan, the United Kingdom, and the United States—each account for more than 15% of our sample. To test the sensitivity of our results to the exclusion of these countries, we drop one country at a time and estimate equation (3). The results are reported in Panel A columns (1–3). In all three columns, the coefficient on *STRONG*ENFORCE* remains significantly positive and differs significantly from that on *WEAK*ENFORCE*.

**[Insert Table 5 here]**

As discussed in Section 4.2.2, it is possible that the measurement of smoothing during the first several years post enforcement is contaminated because of our use of rolling windows to construct the smoothing measure. In column (4), we drop event years zero to two, and the inferences are unchanged. In column (5), we drop firms that are cross-listed in the U.S. Again, our inferences remain the same.

4.2.4 Insider trading profits and earnings smoothing

Baiman and Verrecchia's (1996) prediction that insider trading profits decrease with reporting quality lays the theoretical foundation for our hypotheses. While prior research supports this prediction in various settings, we are not aware of direct evidence on how earnings smoothing relates insider trading profits. We therefore use U.S. insider trading information from Thomson Financial Insider Filing Data (TFN) to provide direct evidence on this association.

We extract trades of executives and directors over the period of 1990-2010 from TFN. As in prior research, we include only open-market transactions and require a minimum transaction price of $2 per share and 100 shares per transaction. We delete transactions with more shares than the CRSP daily volume and prices outside the bid-ask price range. We merge insider trades with Compustat to obtain financial data and CRSP to extract stock price data. Our analysis is based on the following model of insider trading profits:

$$Trading\ Profit_t = \alpha + \beta_1 R\_SMTH_{t-1} + \beta_2 BTM_{t-1} + \beta_3 MV_{t-1} + \beta_4 R\&D_{t-1} + \beta_5 LOSS_{t-1} + \beta_6 VOLATILITY_{t-1} + \beta_7 LOGANALYST_{t-1} + \beta_8 RET_{t-1} + \varepsilon_t \quad (5)$$

Following the literature (e.g., Huddart and Ke, 2007; Jagolinzer et al., 2011), we measure trade profitability as the intercept (or $\alpha$) from the following transaction-specific regression of daily returns on four common factors over the 180 days following each transaction:

$$R_i - R_f = \alpha + \beta_1 (R_{mkt} - R_f) + \beta_2 SMB + \beta_3 HML + \beta_4 UMD + \varepsilon$$

where $R_i$ is firm $i$'s daily stock return; $R_f$ is the daily risk-free interest rate; and $R_{mkt}$ is the CRSP value-weighted market return. *SMB*, *HML*, and *UMD* are size, book-to-market, and momentum factors. $Trading\ Profit_t$ measures potential gains following purchases and losses avoided following sales: it is equal to $\alpha$ for purchases and $-\alpha$ for sales. Prior research generally computes abnormal returns over a six-month horizon because insiders are penalized for profits earned on trades made fewer than 180 days after prior trades (Jagolinzer et al., 2011).

Our discretionary smoothing measure, *R_SMTH*, is constructed in the same way as described in the main analysis. Control variables include lagged book-to-market ratio ($BTM_{t-1}$) and logarithm of lagged market value of equity to capture firm size ($MV_{t-1}$).

Following prior research that finds insider trading profits to be increasing in information asymmetry, we include various controls of information asymmetry. Aboody and Lev (2000) document higher insider trading profits in firms with R&D activities. We define $R\&D_{t\text{-}1}$ as a dummy variable equal to one if a firm has positive R&D expenses. Following Huddart and Ke (2007) and Brochet (2010), we also include a loss dummy ($LOSS_{t\text{-}1}$). As in Ravina and Sapienza (2010), $VOLATILITY_{t\text{-}1}$ is computed as the variance of daily stock returns over the interval (-380, -20) before each trade. We also follow Huddart and Ke (2007) to include analyst following ($LOGANALYST_{t\text{-}1}$). Finally, we control for market-adjusted returns of the prior year ($RET_{t\text{-}1}$).

The estimation results are reported in Table 6. Standard errors are clustered by three-digit SIC industry and month. Consistent with our expectation, *R_SMTH* (higher *R_SMTH* corresponds to less earnings smoothing) is negatively correlated with insider trading profits, suggesting that insiders make larger profits when their firms engage more in earnings smoothing. The inference is unchanged when standard errors are clustered at the person level as in Ravina and Sapienza (2010).

**[Insert Table 6 here]**

4.2.5 Concurrent confounding events and alternative explanations

An important issue that potentially affects our interpretation of the results is confounding events. There can be other regulatory changes concurrent with the enforcement of insider trading restrictions. In this case, these regulatory changes, rather than insider trading law enforcement as we argue, may explain the decrease in earnings smoothing. The nature of our setting, a series of staggered events, mitigates the concern to some extent, as it is less likely that similar confounding events leading to a decrease in

earnings smoothness occur in every case when countries enforce insider trading laws at different times. Yet we conduct additional analyses to assess the impact of confounding events.

We directly explore the possibility of confounding events around the enforcement of insider trading laws using two approaches. First, we search for significant regulatory events in leading domestic law and accounting practitioner journals of English-speaking countries with accessible data (i.e., Australia and Hong Kong).[24] Second, we send out a survey to law and accounting faculty of the top universities in each country that enforces insider trading laws during our sample period, asking them to comment on (i) whether there were material regulatory changes affecting reporting quality during the three-year window around the enforcement of insider trading laws, (ii) if so, whether the event increases or decreases reporting quality, and (iii) the nature of the event and other related issues.[25] Our search of the literature in Australia and Hong Kong does not uncover any such concurrent events. As for the survey, we received responses from all countries with strong legal institutions but only a few replies from countries with weak legal institutions. We thus focus on two subsamples: (1) Australia and Hong Kong, where neither our own search nor the survey indicates any confounding events, and (2) countries with strong legal institutions where our survey does not indicate any confounding events. Since both samples include only countries with strong legal institutions, we estimate equation (2) to test whether there

[24] The list of journals and books we search includes *The Law Society Journal* (New South Wales, Australia), *Hong Kong Lawyer* (the official journal of the Law Society of Hong Kong), and *The Hong Kong Accountant* (a publication of the Hong Kong Society of Accountants).
[25] We selected up to four universities from each country and emailed the survey to at least five faculty members in each field (law and accounting) of each country. The survey is available from the authors upon request.

is a significant increase in information supply through financial reports (decrease in earnings smoothing) post enforcement.[26]

**[Insert Table 7 here]**

The results are reported in Table 7 Panel A. Column (1) reports the coefficient on *ENFORCE* for Australia and Hong Kong. It is significantly positive, indicating a decrease in earnings smoothing post insider trading law enforcement. Column (2) presents the result for the sample of countries with strong legal institutions, excluding Denmark and Belgium, where our survey respondents indicate confounding events.[27] Again, the coefficient on *ENFORCE* is significantly positive, consistent with our expectation. These results suggest that concurrent regulatory events are unlikely the sole driver of the change in accounting quality, as our prediction holds for the subsample without such events.

A recent study by Jayaraman (2012) may provide another alternative explanation for our results. He argues that the enforcement of insider trading laws is accompanied by an improvement in general contract enforcement, which in turn leads to an increase in the debt contracting demand for accounting conservatism (timely loss recognition). Using Khan and Watts' (2009) CSCORE to measure conservatism, he finds that conservatism increases following first-time enforcement of insider trading laws and that the increase is concentrated in firms with more debt.[28]

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[26] In this specification, the control group includes firms in those countries that had enforced their insider trading laws in a previous year or will do so in the future (Bertrand and Mullainathan, 2003; Armstrong, Balakrishnan, Cohen, 2012; Roberts and Whited, 2012).

[27] A Danish respondent indicates that the Danish Financial Statement Act was revised in 1996, right around the initial enforcement of insider trading laws. A respondent from Belgium reports governance reforms around the initial enforcement of insider trading laws and says the reforms may have helped improve reporting quality.

[28] We find that the correlation between our smoothing measure and CSCORE is significantly positive (0.083).

Conservatism captures an important aspect of reporting quality, and thus Jayaraman's findings closely relate to our study. However, different from Jayaraman's argument, our predictions are based on insider trading incentives that are more directly affected by law enforcement and do not rely on the debt contracting demand. To examine whether we, using earnings smoothing, capture a different aspect of financial reporting behavior that does not result from the debt contracting demand, we examine whether the decrease in earnings smoothing we document varies with the level of debt. Our arguments predict that, regardless of the level of debt, firms in countries with strong legal institutions experience a decrease in earnings smoothing as the insider trading law enforcement, by reducing insider trading opportunities, mitigates incentives to conceal information.

We conduct the tests using the following model:

$$SMTH_{it} = \beta_0 + \beta_1 STRONG_{it}*ENFORCE_{it} + \beta_2 WEAK_{it}*ENFORCE_{it} + \beta_3 HIGHLEV_{it}*STRONG_{it}*ENFORCE_{it} + \beta_4 HIGHLEV_{it}*WEAK_{it}*ENFORCE_{it} + \beta_5 HIGHLEV_{it} + \beta_6 HIGHLEV_{it}*STRONG_{it} + \sum\gamma_j Firm\text{-}level\ Control_j + \sum\theta_k Country\text{-}level\ Control_k + Fixed\ Effects + \varepsilon_{it} \quad (6)$$

The variable, *HIGHLEV*, captures the degree to which a firm relies on debt. We define it in several ways. First, we set it to one if long-term debt over total assets is above the median of the population and zero otherwise. The coefficient on *STRONG*ENFORCE* captures the impact of enforcement on *SMTH* for firms with less debt. The coefficient on *HIGHLEV*STRONG*ENFORCE* captures the incremental impact of enforcement on *SMTH* in firms with more debt from countries with strong legal institutions, whereas the coefficient on *HIGHLEV*WEAK*ENFORCE* captures the incremental impact of enforcement on *SMTH* in firms with more debt from countries with weak legal institutions. Second, *HIGHLEV* is defined as the decile ranking (0–9) of long-term debt over total

assets. Third, we define *HIGHLEV* as the percentile ranking (0–99) of the long-term debt ratio. Finally, we use the raw long-term debt ratio as *HIGHLEV*. Jayaraman's argument predicts a positive coefficient on *HIGHLEV*STRONG*ENFORCE*.

The estimation results are reported in Table 7 Panel B. The coefficient on *STRONG*ENFORCE* is significantly positive, regardless of the way we measure *HIGHLEV*, suggesting that firms with low debt in countries of strong legal institutions experience a significant decrease in earnings smoothing post enforcement. The coefficient on *HIGHLEV*STRONG*ENFORCE*, however, is insignificant in all four columns, providing no evidence that more leveraged firms exhibit a significantly greater decrease in earnings smoothness than their less leveraged counterparts.[29] Thus the results provide no support for the debt contracting demand driving the phenomenon we document. The results contrast with Jayaraman's (2012) finding that accounting conservatism increases only in heavily leveraged firms following enforcement of insider trading laws. The divergence suggests that (1) multiple factors affect managers' reporting response to the enforcement of insider trading laws and (2) the financial reporting system has multiple dimensions that are shaped by different incentives. Our understanding of financial disclosure quality and its determinants would be incomplete without evidence on the multiplicity of reporting incentives and their impact. Our analyses emphasize the direct impact of insider trading law enforcement on reporting choices via its effect on insider trading opportunities, complementing Jayaraman (2012).

[29] We also conduct the test measuring the debt contracting demand using country-level leverage. Our inferences are unchanged.

In summary, we find robust evidence that the enforcement of insider trading laws reduces earnings smoothing only in countries with strong legal institutions. While we cannot completely rule out the possibility that concurrent regulatory events contribute to the documented decrease in earnings smoothing, various analyses suggest that they are unlikely to be the sole driver of the results. Our findings highlight that managerial incentives, for example, those resulting from the legal environment, are important in determining the consequences of securities legislation.

*4.3. Enforcement of insider trading laws and strictness of insider trading laws*

We report the tests of the impact of the strictness of insider trading laws in Table 8. As the rating of insider trading laws is only available for a subset of our sample countries, we first estimate equation (3) with this subsample to verify that our main predictions (H1 and H2) are supported. The results are reported in column (1). Consistent with the results reported in Table 3, the coefficient on *STRONG*ENFORCE* is significantly positive and different from that on *WEAK*ENFORCE*, supporting H1 and H2. In columns (2) and (3), we allow the coefficient on *STRONG*ENFORCE* and *WEAK*ENFORCE* to vary with the strictness of the laws. In column (2), where *NOTSTRICT* is defined as a dummy variable, the coefficient on *STRONG*ENFORCE*, capturing the impact of enforcement on accounting quality in countries with strict laws and strong legal institutions, is significantly positive. Consistent with the complementary relation between the strength of laws and the effectiveness of enforcement (Wurgler, 2000), the coefficient on *NOTSTRICT*STRONG*ENFORCE* is significantly negative, indicating a reduction in the effects of enforcement on accounting quality in countries with strong legal institutions

when insider trading laws are not strict. The sum of *STRONG*ENFORCE* and *NOTSTRICT*STRONG*ENFORCE* is negative but insignificant at conventional levels (p-value = 0.2138), suggesting that there is no detectable change in earnings smoothing around insider trading law enforcement in countries with strong legal regimes when the laws are not restrictive. The coefficient on *NOTSTRICT*WEAK*ENFORCE* is also negative but insignificant at conventional levels. In column (3), *NOTSTRICT* takes values from zero to three, with low values corresponding to more stringent laws. The results are consistent with the previous specification, with the coefficient on *NOTSTRICT*STRONG*ENFORCE* being significantly negative.[30] The results help to link the decrease in earnings smoothing more closely to the enforcement of insider trading laws.

**[Insert Table 8 here]**

*4.4. Evidence from actual enforcement strength*

While our previous results based on country-level legal institutions support the prediction that the incentive effect of insider trading enforcement is positively related to the strength of enforcement, the proxies for the strength of legal institution based on La Porta et al. (1997, 1998) are broad measures and therefore may not accurately capture the strength of enforcement that is directly applicable to the enforcement of insider trading cases. In order to construct a more direct proxy for the strength of insider trading enforcement, we survey financial regulators, stock exchanges, and business law professors

[30] The sum of *STRONG* and *STRONG*NOTSTRICT* is never significantly negative for different values of *NOTSTRICT*.

from countries with the initial insider trading law enforcement during our sample period to collect more detailed enforcement information.

In the survey, we are particularly interested in information regarding the persistence of enforcement and penalty actually imposed in insider trading cases. Enforcement that is more persistent and/or imposes more severe penalty poses more credible threats and likely causes more pronounced changes in insider behavior. We ask whether each country has had any additional insider trading enforcement case during the five years subsequent to the year of initial enforcement, the number of additional insider trading enforcement cases, and the penalty involved in these cases.[31]

Through the survey we obtain information on the existence of additional insider trading enforcement cases during the five-year period following the year of initial enforcement from respondents from eight countries (Australia, Belgium, Denmark, Hong Kong, India, Israel, Malaysia, and Thailand). Three of the eight countries, India, Malaysia, and Thailand, did not have insider trading enforcement cases during the five-year period, while the other five did. We consider the five countries with subsequent enforcement cases as having persistent enforcement. In our main analysis, La Porta et al.'s indices identify four of the five countries as having strong enforcement (Australia, Belgium, Denmark, and Hong Kong) and all three countries without persistent enforcement as having weak enforcement.[32]

[31] The survey is available from the authors upon request.

[32] The response rate is similar for countries classified as having strong and weak legal institutions under La Porta et al. (1997, 1998), at 36% (4 out of 11) and 33 % (4 out of 12), respectively. Out of the eight responding countries, two responses (Belgium and Hong Kong, with the latter substantiated by information from court documents) are provided by financial regulators, while the remaining responses are from law professors. Regulators and academics may have different incentives and

Among the five countries with persistent enforcement, we were able to obtain detailed information regarding the enforcement outcome and imposed penalty from various sources as directed by the respondents for three countries (Australia, Denmark, and Hong Kong).[33] There are 11, 8, and 3 successful enforcement cases in Australia, Hong Kong, and Denmark, respectively, during the five-year period after the initial enforcement. Considering the relative size of the stock markets, the frequency of enforcement cases is comparable in the three countries. In terms of imposed penalty, Australia seems the toughest among the three. The maximum length of imprisonment is 3 years in Australia, whereas the maximum imprisonment is 6 months in Denmark and there is no record of imprisonment in Hong Kong. Executives convicted in Hong Kong are typically barred from being an executive or a director. The maximum fine imposed in Australia amounts to over A$60,000 and the maximum disgorgement is over A$1,000,000. The financial penalty in Hong Kong often involves disgorgement of profits earned or losses avoided plus 0.5 to 2 times of the insider trading benefits. The maximum fine in Hong Kong amounts to HK$10 million and the maximum disgorgement is around HK$7 million. No financial penalty information was found for Denmark cases. As Australia imposes both jail time and financial penalty on insider trading violations while Denmark and Hong Kong lack either imprisonment or financial penalty, we consider the enforcement in Australia to be more stringent than that in Denmark and Hong Kong.[34]

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expertise on the topic; however, it is unclear that this issue introduces a significant bias since academics are the primary source of information.

[33] Information about Australian cases is collected mostly from http://eresources.hcourt.gov.au/ and https://jade.io/t/home and that about Hong Kong cases is mostly from http://www.idt.gov.hk/.

[34] Hong Kong arguably imposes the most financial penalty among the three countries. Our inferences are unaffected if we classify both Australia and Hong Kong as countries with stringent enforcement.

Table 9 reports the empirical analysis based on the granular information obtained through the survey. We conduct the test using a model similar to equation (3) in column (1), allowing the effect of enforcement to differ with the strength of enforcement. We also estimate a model similar to equation (3') in column (2) including the interactions of the indicator variable for enforcement strength and control variables. Both specifications produce the same inferences. Panel A shows that, among the eight countries with information on the existence of follow-up enforcement of insider trading laws, the decrease in earnings smoothing is concentrated in the countries with persistent enforcement. Panel B compares the enforcement effect among the three countries where detailed information regarding the enforcement outcome and imposed penalty are available. The results reported in Panel B suggest that the decrease in earnings smoothing around initial insider trading law enforcement is more pronounced in Australia than in Denmark and Hong Kong. In summary, the evidence supports our prediction that the reduction in earnings smoothing following insider trading law enforcement is more pronounced in countries with (1) more persistent enforcement, as indicated by the existence of additional enforcement cases following the initial enforcement, and (2) stronger enforcement, as indicated by more severe penalty *actually imposed* in the prosecution of these insider trading cases.

**[Insert Table 9 here]**

*4.5. Firm-level cross-sectional tests*

In our discussion of the cost-benefit tradeoff in insiders' decision on financial disclosure quality, an implicit assumption is that extracting insider trading profits is a real binding constraint; that is, insider trading profits are an important (or relevant)

consideration in the determination of financial disclosure quality. While the preceding empirical findings in support of our hypotheses suggest that, on average, this assumption holds, we have reason to believe that this binding constraint is more likely to hold for firms with certain characteristics. In this section, we identify two such characteristics—ownership concentration and growth—and provide empirical evidence.

The binding constraint is likely to vary with ownership concentration for two reasons. First, insiders of closely held firms have access to multiple channels for rent extraction (e.g., related-party transactions and management buyouts) that benefit from opaque reporting. Prior research argues that a high ownership concentration is associated both with greater agency conflicts between insiders and outsiders and with more severe rent extraction and managerial entrenchment (Morck, Shleifer, and Vishny, 1988). Second, in closely held firms, insider trading profitability is likely to be driven less by reporting quality than by other factors such as liquidity constraints. Both reasons suggest that insider trading profits matter less in the determination of financial disclosure quality in closely held firms. As a result, a negative shock to insider trading opportunities, for example, insider trading law enforcement, would lead to a less pronounced improvement in financial disclosure quality in closely held firms. To test this prediction, we estimate the following equation using the sample of firms with nonmissing closely held shares data:[35]

$$SMTH_{it} = \beta_0 + \beta_1 STRONG_{it}*ENFORCE_{it} + \beta_2 WEAK_{it}*ENFORCE_{it} + \beta_3 CLOSE_{it}*STRONG_{it}*ENFORCE_{it} + \beta_4 CLOSE_{it}*WEAK_{it}*ENFORCE_{it} + \beta_5 CLOSE_{it} + \beta_6 CLOSE_{it}*STRONG_{it} + \sum \gamma_j Firm\text{-}level\ Control_j + \sum \theta_k Country\text{-}level\ Control_k + Fixed\ Effects + \varepsilon_{it} \quad (7)$$

[35] Following Kho, Stulz, and Warnock (2009), we also exclude observations with the percentage of insider ownership exceeding 100% or equal to zero.

The variable, *CLOSE*, captures the degree to which a firm is closely held. It is measured in several different ways. First, it is set to one if the percentage of closely held shares is above the median of the population and zero otherwise. The coefficient on *CLOSE*STRONG*ENFORCE* thus captures the incremental impact of enforcement on *SMTH* in more closely held firms relative to less closely held firms from countries with strong legal institutions, whereas the coefficient on *CLOSE*WEAK*ENFORCE* captures the incremental impact of enforcement on *SMTH* in more, relative to less, closely held firms from countries with weak legal institutions. We expect the coefficient on *CLOSE*STRONG*ENFORCE* to be negative. We do not have a clear prediction regarding the coefficient on *CLOSE*WEAK*ENFORCE*. When the legal institutions are weak, it is unclear whether firm-level ownership structures would have a significant influence on managers' reporting incentives. Second, *CLOSE* is defined as the decile ranking (0–9) of closely held shares percentage. Third, we define *CLOSE* as the percentile ranking (0–99) of closely held shares percentage. Finally, we use the raw closely held shares percentage as *CLOSE*. In all these alternative specifications, we expect the coefficient on *CLOSE*STRONG*ENFORCE* to be negative.

**[Insert Table 10 here]**

Table 10 reports the empirical results. Column (1) presents the results when *CLOSE* is defined as a dummy variable equal to one for more closely held firms. The coefficient on *STRONG*ENFORCE* captures the impact of initial enforcement on financial disclosure quality in less closely held firms from countries with strong legal institutions. It is significantly positive and different from that on *WEAK*ENFORCE*. As expected, the coefficient capturing the incremental impact of enforcement in more closely held firms

from countries with strong legal institutions, *CLOSE*STRONG*ENFORCE*, is significantly negative. The sum of the coefficient on *STRONG*ENFORCE* and that on *CLOSE*STRONG*ENFORCE* is still significantly positive. The results indicate that closely held firms in countries with strong legal institutions experience a decrease in earnings smoothing post enforcement but the increase is significantly smaller than that in less closely held firms. Neither the coefficient on *WEAK*ENFORCE* nor that on *CLOSE*WEAK*ENFORCE* is significant at conventional levels.

The results based on alternative definitions of *CLOSE* are reported in columns (2–4) of Table 10. Regardless of whether we define *CLOSE* as the decile ranking, the percentile ranking, or the raw percentage of closely held shares, the coefficient on *STRONG*ENFORCE* is always significantly positive, while that on *CLOSE*STRONG*ENFORCE* is significantly negative. The results convey the same message that closely held firms are less likely to respond to insider trading restrictions by supplying higher-quality information to the market.

The binding constraint is also likely to vary with firms' growth opportunities. Because the value of growth firms lies more in growth opportunities as opposed to assets in place, insiders have a greater informational advantage over outsiders. The nature of growth firms can deter external information production and make investors more dependent on firm disclosures. Insiders can extract more trading profits by providing low quality information. Therefore, for growth firms, insider trading profits are more likely to be an important consideration in insiders' disclosure decisions. In summary, the binding constraint is more likely to hold in growth firms, and we expect the decrease in earnings

smoothing following insider trading law enforcement to be more pronounced in such firms.[36] To test this prediction, we estimate the following equation:

$$SMTH_{it}=\beta_0+\beta_1 STRONG_{it}*ENFORCE_{it}+\beta_2 WEAK_{it}*ENFORCE_{it}+\beta_3 LOWGROW_{it}*STRONG_{it}*ENFORCE_{it} + \beta_4\ LOWGROW_{it}*WEAK_{it}*ENFORCE_{it} + \beta_5\ LOWGROW_{it}+ \beta_6\ LOWGROW_{it}*STRONG_{it}+ \sum\gamma_j\ Firm\text{-}level\ Control_j + \sum\theta_k\ Country\text{-}level\ Control_k + Fixed\ Effects + \varepsilon_{it} \quad (8)$$

The variable capturing low growth, *LOWGROW*, is constructed based on the average sales growth over the past five years. We define *LOWGROW* in four different ways. First, it is set to one if the average sales growth is below the median of the population and zero otherwise. The coefficient on *LOWGROW*STRONG*ENFORCE* thus captures the incremental impact of enforcement on *SMTH* in low growth relative to high growth firms from countries with strong legal institutions, whereas the coefficient on *LOWGROW*WEAK*ENFORCE* captures the incremental impact of enforcement on *SMTH* in low growth relative to high growth firms from countries with weak legal institutions. We expect the coefficient on *LOWGROW*STRONG*ENFORCE* to be negative. Second, *LOWGROW* is defined as the decile ranking of average sales growth, in descending order. Third, we define *LOWGROW* as the percentile ranking of average sales growth, in descending order. Finally, we use the negative of raw average sales growth as *LOWGROW*. In all these alternative specifications, we expect the coefficient on *LOWGROW*STRONG*ENFORCE* to be negative.

**[Insert Table 11 here]**

[36] In addition, because growth firms are more likely to access the capital markets, they benefit more from the reduction in the cost of capital resulting from high quality financial reporting (Baiman and Verrecchia, 1996; Lambert, Leuz, and Verrecchia, 2007). The benefits of responding to the insider trading restrictions by increasing reporting quality are greater for these firms. This also leads to the same empirical prediction.

We report the estimation results in Table 11. Column (1) presents the results when *LOWGROW* is defined as a dummy variable equal to one for firms with below-median growth. The coefficient on *STRONG*ENFORCE* captures the impact of initial enforcement on accounting quality in high growth firms from countries with strong legal institutions. It is significantly positive and different from that on *WEAK*ENFORCE*. As expected, the coefficient capturing the incremental impact of enforcement in low growth firms from countries with strong legal institutions, *LOWGROW*STRONG*ENFORCE*, is significantly negative, suggesting a significant reduction in the impact of enforcement when firms have lower growth and less incentive to improve their disclosure quality. Alternative specifications in columns (2–4) also yield the same inferences.

*4.6. Enforcement of insider trading laws, stock price informativeness, and insiders' information supply*

Fernandes and Ferreira (2009) find that stock price informativeness increases after the enforcement of insider trading laws and the effects are concentrated in developed countries. Motivated by their evidence, we explore the channel through which insider trading curtails the informational efficiency of financial markets. We argue that insider trading opportunities motivate insiders to supply low-quality information, resulting in lower informational efficiency of stock markets. This argument suggests that insiders' information supply through financial reports should help explain changes in stock price informativeness in the setup of Fernandes and Ferreira (2009).

Following Fernandes and Ferreira (2009), we use firm-specific stock return variation for each country to capture stock price informativeness. Specifically, as in

Fernandes and Ferreira (2009), we extract monthly stock return data from Datastream and estimate the following market model for each firm in each year:

$$r_{jt} = \alpha_j + \beta_j r_{mt} + e_{jt}$$

where $r_{jt}$ is the return of stock $j$ in month $t$ and $r_{mt}$ is the equal-weighted local market return. The measure of stock price informativeness, or firm-specific stock variation, is based on the $R^2$ of the above regression:

$$\psi_j = \log ((1- R^2_j)/ R^2_j)$$

The higher $\psi_j$ is, the greater the stock price informativeness. As in Fernandes and Ferreira (2009), we aggregate $\psi_j$ across firms for each country in each year and use the median $\psi_j$ as the dependent variable in the following country-level regression:

$$\Psi_{it} = \beta_0 + \beta_1 SMTH_{it} + \beta_2 DEVELOPED*ENFORCE_{it} + \beta_3 EMERGING*ENFORCE_{it} + \sum\gamma_j Median\ Firm\text{-}level\ Control_j + \sum\theta_k Country\text{-}level\ Control_k + Country\ Fixed\ Effects + \varepsilon_{it} \quad (9)$$

The explanatory variables are defined as before. To control for economic determinants of *SMTH*, we include the median of the firm-level controls in each country-year. For country-level controls, in addition to *LOG_GDP*, *LOG_LISTED*, and *LIB*, we also control for *IHERF*, the industry Herfindahl index; *FHERF*, the firm Herfindahl index; and *VGDP*, the variance of the annual GDP per capita growth. We expect a positive coefficient on $SMTH_{it}$ and *DEVELOPED*ENFORCE*.

We first estimate equation (9) without *SMTH* and report the results in Table 12 column (1). Consistent with Fernandes and Ferreira (2009), we find the coefficient on *DEVELOPED*ENFORCE* to be significantly positive, indicating that stock price informativeness increases post enforcement in developed countries. We then add our measure of information supply by insiders, *SMTH*, in the regression and report the results

in column (2). The coefficient on *SMTH* is significantly positive, suggesting that higher disclosure quality (less earnings smoothing) is associated with greater stock price informativeness. Interestingly, while the coefficient on *DEVELOPED*ENFORCE* remains positive, it ceases to be significant at conventional levels. The mediating effect of *SMTH* is 37% ( = (0.2032 – 0.1271)/0.2032 ) and is statistically significant based on the Sobel (1982) test.

**[Insert Table 12 here]**

The results in Table 12 suggest that the impact of enforcement on stock price informativeness documented in Fernandes and Ferreira (2009) is explained by changes in financial disclosure quality around the enforcement. While this finding highlights the impact of insiders' information supply on informational efficiency, we caution readers that our sample composition and period differ from those of Fernandes and Ferreira (2009) due to data limitations. These differences prevent us from drawing strong inferences from a comparison of our results and those of Fernandes and Ferreira (2009).

## 5. Conclusions

Exploiting the setting of first-time enforcement of insider trading laws around the world, we examine the impact of insider trading restrictions on insiders' incentives to smooth earnings in order to conceal firm fundamental performance from outsiders. Following the literature, we measure earnings smoothing using the variability of earnings relative to cash flows and the correlation between accruals and cash flows. Empirical analyses indicate that earnings smoothing decrease following a country's first-time enforcement of insider trading laws only in countries with strong legal institutions,

suggesting that a country's legal institutions play an important role in determining earnings quality. The decrease in earnings smoothing is more pronounced for countries that have more prohibitive insider trading laws. Last, for a subsample with data on enforcement actions after the initial enforcement, we find that the decrease in earnings smoothing is also more pronounced when insider trading enforcement are more persistent and when the actually imposed penalty is more severe in such enforcement.

This study contributes to the literature in several ways. First, we add to the literature on the costs and benefits of insider trading and the need for regulation. Uncovering an important channel through which insider trading restrictions benefit the information environment, our study extends research, such as Fernandes and Ferreira (2009) and Bushman et al. (2005), on the informational impact of insider trading restrictions. Specifically, insider trading law enforcement improves the quality of information supplied by corporate insiders and thus the information environment, in addition to increasing the information collection by outsiders, as shown by Bushman et al. (2005). We show that the improvement in information supply by insiders is concentrated in developed countries and countries with strong legal institutions, offering an explanation for Fernandes and Ferreira's (2009) finding of a larger increase of price informativeness in developed markets.

Second, this study illuminates the relation between insider trading and earnings management. While the literature has shown a positive correlation between earnings manipulation and insider trading, it is unclear whether insider trading motivates managers to distort corporate disclosures or whether managers trade passively to exploit their knowledge of the earnings management. With our unique setting of insider trading law

enforcements, we provide evidence consistent with the former interpretation and advance the understanding of the incentive effect of insider trading opportunities.

Finally, we extend the literature on securities and accounting regulation by highlighting the importance of managerial incentives in determining the consequences of regulations. We find that both country-level legal institutions and firm-level structures, such as ownership concentration, cause variations in managerial incentives and lead to differential responses to regulations.

While this study focuses on the impact of insider trading on mandatory reporting incentives, we believe that similar arguments can be applied to voluntary disclosure. High quality voluntary disclosure, similar to high quality mandatory disclosure, reduces insiders' information advantage and insider trading profits. Therefore, one may predict an improvement in voluntary disclosure quality following a negative shock to insider trading opportunities. This additional prediction is not mutually exclusive with the prediction tested in this paper. Although we cannot test this empirical prediction due to data availability, we believe that it offers a fruitful opportunity for future research.[37] We also note that our measure of financial disclosure quality, earnings smoothing, is affected not only by deliberate manipulation but also by the underlying economics. We follow the literature and control for an array of firm- and country-level determinants of earnings smoothness. Yet smoothing still must be interpreted with caution as a measure of earnings quality.

[37] Another potentially fruitful research direction is how the timing of insider trades changes around shocks to insider trading opportunities, because insiders can increase trading profits by strategically timing trades around disclosure events.

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**Appendix: Variable Definitions**

| Variable | Definition |
|---|---|
| *Measures of earnings smoothing* | |
| *SMTH* | Average of the percentile ranking of two earnings smoothing measures (ranging 0-1), the variability of earnings over cash flows (*SMTH1*) and the correlation between accruals and cash flows (*SMTH2*); a higher value indicates less earnings smoothing. |
| *SMTH1* | Standard deviation of operating income scaled by average total assets divided by standard deviation of cash flow from operations scaled by average total assets, estimated over rolling windows of five years, requiring at least three years of data; a higher value indicates less earnings smoothing. |
| *SMTH2* | Spearman correlation between accruals scaled by average total assets and cash flows from operations scaled by average total assets, estimated over rolling windows of five years, requiring at least three years of data; a higher value indicates less earnings smoothing. |
| *R_SMTH* | Average of the percentile ranking of the residuals from the following two regressions:<br>$SMTH1_{it} = \beta_0 + \beta_1 SIZE_{it} + \beta_1 LEV_{it} + \beta_2 BTM_{it} + \beta_3 STD\_SALES_{it} + \beta_4 PCT\_LOSS_{it} + \beta_5 OPCYCLE_{it} + \beta_6 GROWTH_{it} + \beta_7 OPLEV_{it} + \beta_8 AVGCFO_{it} + Fixed\ Effects + \varepsilon_{it}$<br>$SMTH2_{it} = \beta_0 + \beta_1 SIZE_{it} + \beta_1 LEV_{it} + \beta_2 BTM_{it} + \beta_3 STD\_SALES_{it} + \beta_4 PCT\_LOSS_{it} + \beta_5 OPCYCLE_{it} + \beta_6 GROWTH_{it} + \beta_7 OPLEV_{it} + \beta_8 AVGCFO_{it} + Fixed\ Effects + \varepsilon_{it}$ |
| *P_SMTH* | Average of the percentile ranking of the predicted values from the following two regressions:<br>$SMTH1_{it} = \beta_0 + \beta_1 SIZE_{it} + \beta_1 LEV_{it} + \beta_2 BTM_{it} + \beta_3 STD\_SALES_{it} + \beta_4 PCT\_LOSS_{it} + \beta_5 OPCYCLE_{it} + \beta_6 GROWTH_{it} + \beta_7 OPLEV_{it} + \beta_8 AVGCFO_{it} + Fixed\ Effects + \varepsilon_{it}$<br>$SMTH2_{it} = \beta_0 + \beta_1 SIZE_{it} + \beta_1 LEV_{it} + \beta_2 BTM_{it} + \beta_3 STD\_SALES_{it} + \beta_4 PCT\_LOSS_{it} + \beta_5 OPCYCLE_{it} + \beta_6 GROWTH_{it} + \beta_7 OPLEV_{it} + \beta_8 AVGCFO_{it} + Fixed\ Effects + \varepsilon_{it}$ |
| *Legal institution measures* | |
| *STRONG* | An indicator variable equal to one if a country has strong legal institutions, as captured by an above median rating of the efficiency of the judicial system, rule of law, and corruption, based on La Porta et al. (1998), and if the country is a developed market, and zero otherwise |
| *WEAK* | An indicator variable equal to one if a country has a weaker enforcement structure, that is, if it does not have an above-median rating in any of the three dimensions, the efficiency of the judicial |

| | |
|---|---|
| | system, rule of law, and corruption, or if the country is an emerging market, and zero otherwise |
| *EFF_JUD* | An indicator variable equal to one if the efficiency of a country's judicial system measured by La Porta et al. (1998) is above the median of the sample, and zero otherwise |
| *RULE_LAW* | An indicator variable equal to one if a country's rule of law measured by La Porta et al. (1998) is above the median of the sample, and zero otherwise |
| *CORRUPTION* | An indicator variable equal to one if a country's degree of corruption measured by La Porta et al. (1998) is above the median of the sample, and zero otherwise |
| *EMERGING* | An indicator variable equal to one if a country is an emerging market, and zero otherwise |
| *Country-level controls* | |
| *LOG_GDP* | The logarithm of a country's per capita GDP (source: World Development Indicators) |
| *GROWTH_GDP* | The annual growth in real GDP per capita (source: World Development Indicators) |
| *LOG_LISTED* | The logarithm of the number of domestic companies listed on the home country's stock exchange (source: World Development Indicators) |
| *FDI* | Net foreign direct investment divided by GDP (source: World Development Indicators) |
| *OPEN* | Trade openness, the sum of total imports and exports divided by GDP (source: World Development Indicators) |
| *LIBERAL* | An indicator variable that equals one for country-years after a country's capital market is liberalized, and zero otherwise |
| *Firm-level controls (Financial data are from Worldscope)* | |
| *SIZE* | The logarithm of total assets in U.S. dollars |
| *LEV* | Long-term debt over total assets |
| *STD_SALES* | Standard deviation of sales over the last five years |
| *PCT_LOSS* | Percentage of loss years over the last five years |
| *OPCYCLE* | The logarithm of days of accounts receivable plus inventory |
| *GROWTH* | Sales growth |
| *OPLEV* | Net property, plant, and equipment over total assets |
| *AVGCFO* | Average cash flow from operations scaled by average total assets over the last five years |
| *AUD* | An indicator variable equal to one if a firm is audited by one of the Big N auditors, and zero otherwise |

| | |
|---|---|
| *ACCTSTD* | An indicator variable equal to one if a firm reports under IFRS or U.S. GAAP, and zero otherwise |
| *XLIST* | An indicator variable equal to one if a firm is cross-listed in the U.S., and zero otherwise |
| *Variables used in the test of insider trading profits and earnings smoothing (Table 6)*<br>*Transaction data of executives and directors are extracted from Thomson Financial Insider Filing data (TFN), stock price data are from CRSP, and financial data are from Compustat.* | |
| *Trading Profit* | Equal to $\alpha$ (intercept) for insider purchases and $-\alpha$ for insider sales from the following transaction-specific regression of daily returns on four common factors over the 180 days following each transaction:<br>$R_i - R_f = \alpha + \beta_1 (R_{mkt} - R_f) + \beta_2 SMB + \beta_3 HML + \beta_4 UMD + \varepsilon$<br>where $R_i$ is firm $i$'s daily stock return, $R_f$ is the daily risk-free interest rate, and $R_{mkt}$ is the CRSP value-weighted market return. *SMB*, *HML*, and *UMD* are size, book-to-market, and momentum factors. |
| *BTM* | Book value of equity over market value of equity |
| *MV* | Logarithm of market value of equity |
| *R&D* | An indicator variable equal to one if a firm reports positive R&D expenses and zero otherwise |
| *LOSS* | An indicator variable equal to one if a firm reports negative earnings before extraordinary items, and zero otherwise |
| *VOLATILITY* | The variance of daily stock returns over the interval (-380, -20) before each trade |
| *LOGANALYST* | Logarithm of one plus the number of analysts following the firm |
| *RET* | Annual market adjusted stock returns |
| *Other variables* | |
| *ENFORCE* | An indicator variable equal to one after the initial enforcement of insider trading laws, and zero otherwise (source: Bhattacharya and Daouk, 2002) |
| *NOTSTRICT* | Defined in two different ways. First, it is set equal to one if Beny's (2005) score of insider trading laws is one or two, and zero if Beny's score is three or four. Second, it is set equal to the maximum value of Beny's score (four) minus the actual score for each country. |
| *PERSIST* | An indicator variable equal to one if there are inside trading law enforcement actions during the five-year window post the initial enforcement in a country, and zero if there is no such action. |
| *NO_PERSIST* | An indicator variable equal to one if there are no inside trading law enforcement actions during the five-year window post the initial enforcement in a country, and zero if there is such action. |
| *SEVERE_PENALTY* | An indicator variable equal to one for Australia, and zero for Denmark and Hong Kong. |
| *MILD_PENALTY* | An indicator variable equal to one for Denmark and Hong Kong, and zero for Australia. |

| | |
|---|---|
| *HIGHLEV* | Defined in four different ways in different specifications. First, it is set to one if *LEV* is above the median of the population, and zero otherwise. Second, it is defined as the decile ranking of *LEV*, in ascending order. Third, it is defined as the percentile ranking of *LEV*, in ascending order. Finally, it is set equal to *LEV*. |
| *CLOSE* | Defined in four different ways in different specifications. First, it is set to one if the percentage of closely held shares is above the median of the population, and zero otherwise. Second, it is defined as the decile ranking of closely held shares percentage, in ascending order. Third, it is defined as the percentile ranking of closely held shares percentage, in ascending order. Finally, it is set equal to closely held shares percentage. |
| *LOWGROW* | Defined in four different ways in different specifications. First, it is set to one if the average sales growth is below the median of the population, and zero otherwise. Second, it is defined as the decile ranking of average sales growth, in descending order. Third, it is defined as the percentile ranking of average sales growth, in descending order. Finally, it is set equal to the negative of raw average sales growth. |
| $\Psi$ | Median stock price informativeness of each country in each year, where stock price informativeness is computed as the logistic transformed relative firm-specific stock return variation estimated using the market model for each firm in each year |
| *IHERF* | Industry Herfindahl index calculated using two-digit SIC industry sales for each country in each year |
| *FHERF* | Firm Herfindahl index calculated using individual firm sales for each country in each year |
| *VGDP* | Variance of the annual GDP per capita growth estimated using a five-year rolling window for each country in each year |

**Table 1: Sample by country**

This table reports by country the number of firms, the year of initial enforcement of insider trading laws, the year of capital market liberalization, the medians of variables capturing economic development, and the value of variables capturing legal institutions and Beny's (2005) score of insider trading laws. Variables definitions are in the appendix.

| Country | *N* | *Enforce Year* | *Liberalization year* | *LOG_GDP* | *GROWTH_GDP* | *LOG_LISTED* | *FDI* | *OPEN* | *EFF_JUD* | *RULE_LAW* | *CORRUPTION* | *EMERGING* | *Beny's Score* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| ARGENTINA | 228 | 1995 | 1989 | 8.8819 | 5.5267 | 4.9127 | 0.0159 | 21.4678 | 0 | 0 | 0 | 1 | |
| AUSTRALIA | 2333 | 1996 | 1969 | 9.9141 | 3.9500 | 7.0817 | 0.0075 | 37.2158 | 1 | 1 | 1 | 0 | 3 |
| AUSTRIA | 289 | | 1969 | 10.1191 | 2.5391 | 4.6540 | 0.0010 | 72.4096 | 1 | 1 | 1 | 0 | 2 |
| BELGIUM | 163 | 1994 | 1969 | 10.3087 | 1.3669 | 5.4596 | 0.0270 | 148.1656 | 1 | 1 | 1 | 0 | 3 |
| BRAZIL | 816 | 1978 | 1991 | 8.1369 | 2.1500 | 6.2841 | 0.0139 | 19.2533 | 0 | 0 | 0 | 1 | 2 |
| CANADA | 1440 | 1976 | 1969 | 9.9508 | 2.8084 | 7.1428 | -0.0037 | 72.4816 | 1 | 1 | 1 | 0 | 4 |
| CHILE | 499 | 1996 | 1992 | 8.4151 | 5.8746 | 5.5451 | 0.0347 | 59.0738 | 0 | 0 | 0 | 1 | |
| COLOMBIA | 68 | | | 7.7328 | 3.4303 | 4.7274 | 0.0138 | 35.4972 | 0 | 0 | 0 | 1 | |
| DENMARK | 1119 | 1996 | 1969 | 10.3087 | 1.9754 | 5.4681 | -0.0019 | 71.6531 | 1 | 1 | 1 | 0 | 3 |
| FINLAND | 738 | 1993 | 1969 | 10.1258 | 3.6150 | 4.3567 | -0.0161 | 67.4981 | 1 | 1 | 1 | 0 | 3 |
| FRANCE | 4658 | 1975 | 1969 | 10.0590 | 2.2154 | 6.5309 | -0.0071 | 44.8775 | 0 | 1 | 1 | 0 | 4 |
| GERMANY | 4084 | 1995 | 1969 | 10.1495 | 2.0109 | 6.5191 | -0.0101 | 51.9412 | 1 | 1 | 1 | 0 | 3 |
| GREECE | 773 | 1996 | 1987 | 9.3899 | 3.4194 | 5.4116 | 0.0076 | 47.9016 | 0 | 0 | 0 | 1 | 2 |
| HONG KONG | 1677 | 1994 | 1969 | 10.1152 | 2.5559 | 6.7534 | 0.0152 | 282.0809 | 1 | 1 | 1 | 0 | 3 |
| INDIA | 1255 | 1998 | 1992 | 6.0536 | 6.1935 | 8.6394 | 0.0056 | 23.9850 | 0 | 0 | 0 | 1 | 2 |
| INDONESIA | 1145 | 1996 | 1989 | 6.8330 | 4.9200 | 5.6419 | 0.0007 | 55.9939 | 0 | 0 | 0 | 1 | 2 |
| IRELAND | 186 | | 1969 | 9.9203 | 8.4317 | 4.3820 | 0.0214 | 142.9933 | 1 | 0 | 1 | 0 | 3 |
| ISRAEL | 238 | 1989 | | 9.8232 | 3.7893 | 6.4646 | 0.0061 | 69.6501 | 1 | 0 | 1 | 1 | |
| ITALY | 1680 | 1996 | 1969 | 9.9502 | 1.5337 | 5.4931 | -0.0019 | 46.5884 | 0 | 1 | 0 | 0 | 3 |
| JAPAN | 12211 | 1990 | 1980 | 10.4077 | 1.5636 | 7.7553 | -0.0053 | 19.1248 | 1 | 1 | 1 | 0 | 2 |
| KOREA | 2166 | 1988 | 1992 | 9.2193 | 6.8716 | 6.8086 | -0.0007 | 62.1538 | 0 | 0 | 0 | 1 | 4 |
| MALAYSIA | 2788 | 1996 | 1988 | 8.2186 | 8.8851 | 6.4313 | 0.0429 | 185.6651 | 1 | 0 | 0 | 1 | 2 |
| MEXICO | 70 | | 1989 | 8.2184 | 4.2223 | 5.2883 | 0.0157 | 38.3921 | 0 | 0 | 0 | 1 | 1 |
| NETHERLANDS | 1455 | 1994 | 1969 | 10.1173 | 3.1163 | 5.3799 | -0.0238 | 113.9820 | 1 | 1 | 1 | 0 | 3 |

| | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| NEW ZEALAND | 185 | | 1984 | 9.4879 | 1.1211 | 4.9127 | 0.0291 | 55.0584 | 1 | 1 | 1 | 0 | |
| NORWAY | 734 | 1990 | 1969 | 10.4387 | 2.7866 | 5.0626 | -0.0043 | 71.4344 | 1 | 1 | 1 | 0 | 1 |
| PAKISTAN | 143 | | 1991 | 6.1249 | 4.2920 | 6.6117 | 0.0077 | 36.4925 | 0 | 0 | 0 | 1 | |
| PERU | 116 | 1994 | | 7.6597 | 4.0323 | 5.4424 | 0.0279 | 32.4890 | 0 | 0 | 0 | 1 | |
| PHILIPPINES | 44 | | 1991 | 6.8074 | 3.2520 | 5.2824 | 0.0152 | 77.2490 | 0 | 0 | 0 | 1 | 2 |
| PORTUGAL | 275 | | 1986 | 9.1929 | 4.2828 | 5.1930 | 0.0112 | 61.6696 | 0 | 1 | 0 | 1 | 3 |
| SINGAPORE | 922 | 1978 | 1969 | 9.9937 | 3.8195 | 6.1924 | 0.0697 | 382.9042 | 1 | 1 | 1 | 0 | 3 |
| SOUTH AFRICA | 537 | | | 8.1738 | 2.3949 | 6.5043 | -0.0022 | 44.8657 | 0 | 0 | 1 | 1 | 2 |
| SPAIN | 1018 | 1998 | 1978 | 9.6130 | 3.2668 | 6.0568 | 0.0056 | 46.7143 | 0 | 0 | 0 | 0 | 3 |
| SWEDEN | 1180 | 1990 | 1969 | 10.2615 | 2.7000 | 5.5013 | -0.0035 | 72.6180 | 1 | 1 | 1 | 0 | 3 |
| SWITZERLAND | 1219 | 1995 | 1969 | 10.5275 | 1.1908 | 5.4467 | -0.0334 | 74.8754 | 1 | 1 | 1 | 0 | 3 |
| THAILAND | 1561 | 1993 | 1987 | 7.6282 | 6.1227 | 5.9789 | 0.0235 | 98.2359 | 0 | 0 | 0 | 1 | 3 |
| TURKEY | 550 | 1996 | 1989 | 8.0235 | 6.1638 | 5.6240 | 0.0040 | 44.2426 | 0 | 0 | 0 | 1 | |
| UK | 12090 | 1981 | 1969 | 9.9507 | 2.8854 | 7.6275 | -0.0166 | 53.3048 | 1 | 1 | 1 | 0 | 3 |
| US | 11564 | 1961 | 1969 | 10.2714 | 3.5646 | 8.8161 | -0.0007 | 23.2971 | 1 | 1 | 1 | 0 | 4 |
| VENEZUELA | 30 | | 1990 | 8.0741 | 0.2847 | 4.5053 | 0.0182 | 52.2038 | 0 | 0 | 0 | 1 | |

**Table 2: Descriptive statistics**

This table reports the descriptive statistics of firm-level variables. Variable definitions are in the appendix.

| | Mean | P25 | Median | P75 | Std |
|---|---|---|---|---|---|
| *SMTH1* | 0.6227 | 0.2358 | 0.4424 | 0.7752 | 0.8166 |
| *SMTH2* | -0.7315 | -1.0000 | -0.9000 | -0.6000 | 0.3778 |
| *SMTH* | 0.5003 | 0.2828 | 0.4798 | 0.7121 | 0.2547 |
| *SIZE* | 12.5694 | 11.2567 | 12.4624 | 13.7813 | 1.9142 |
| *LEV* | 0.1415 | 0.0158 | 0.1017 | 0.2170 | 0.1511 |
| *BTM* | 0.8266 | 0.3669 | 0.6476 | 1.1061 | 1.1977 |
| *STD_SALES* | 0.1506 | 0.0562 | 0.1013 | 0.1838 | 0.1664 |
| *PCT_LOSS* | 0.2063 | 0 | 0 | 0.3333 | 0.2980 |
| *OPCYCLE* | 4.9483 | 4.5826 | 4.9889 | 5.3293 | 0.6821 |
| *GROWTH* | 0.1318 | -0.0347 | 0.0536 | 0.1677 | 0.5905 |
| *OPLEV* | 0.3421 | 0.1791 | 0.3089 | 0.4703 | 0.2118 |
| *AVGCFO* | 0.0840 | 0.0451 | 0.0915 | 0.1418 | 0.1447 |
| *AUD* | 0.6907 | 0 | 1 | 1 | 0.4622 |
| *ACCTSTD* | 0.2140 | 0 | 0 | 0 | 0.4102 |
| *XLIST* | 0.0257 | 0 | 0 | 0 | 0.1583 |

## Table 3: Enforcement of insider trading laws and earning smoothing

This table reports the results of testing H1 and H2.

**Panel A: Main results**

This table reports estimates of the following equations:

$SMTH_{it} = \beta_0 + \beta_1 ENFORCE_{it} + \sum\gamma_j Firm\text{-}level\ Controls + \sum\theta_k Country\text{-}level\ Controls + Fixed\ Effects + \varepsilon_{it}$

$SMTH_{it} = \beta_0 + \beta_1 STRONG_{it}*ENFORCE_{it} + \beta_2 WEAK_{it}*ENFORCE_{it} + \sum\gamma_j Firm\text{-}level\ Control_j + \sum\theta_k Country\text{-}level\ Control_k + Fixed\ Effects + \varepsilon_{it}$

Country, industry, and year fixed effects are included in the regressions. Column (1) reports the estimation results of model (2) and column (2) reports the estimates of model (3). Variable definitions are in the appendix. ***,**,* indicate that a coefficient is significantly different from zero at the 1%, 5%, and 10% levels based on two-tailed tests and robust standard errors clustered by country-year.

| | | (1) Average | | (2) Strong vs. Weak | |
|---|---|---|---|---|---|
| | Predicted | Estimate | Std Err | Estimate | Std Err |
| *ENFORCE* | | 0.0144** | 0.0067 | | |
| *STRONG*ENFORCE* | + | | | 0.0251*** | 0.0084 |
| *WEAK*ENFORCE* | | | | -0.0020 | 0.0089 |
| *SIZE* | | -0.0124*** | 0.0009 | -0.0121*** | 0.0009 |
| *LEV* | | -0.0695*** | 0.0075 | -0.0680*** | 0.0073 |
| *BTM* | | -0.0035*** | 0.0011 | -0.0044*** | 0.0010 |
| *STD_SALES* | | 0.0498*** | 0.0081 | 0.0479*** | 0.0077 |
| *PCT_LOSS* | | 0.1358*** | 0.0095 | 0.1310*** | 0.0100 |
| *OPCYCLE* | | 0.0009 | 0.0021 | -0.0007 | 0.0021 |
| *GROWTH* | | -0.0066** | 0.0028 | -0.0082*** | 0.0029 |
| *OPLEV* | | 0.1460*** | 0.0079 | 0.1491*** | 0.0078 |
| *AVGCFO* | | 0.1328*** | 0.0363 | 0.0896*** | 0.0344 |
| *AUD* | | 0.0042* | 0.0023 | 0.0050** | 0.0023 |
| *ACCTSTD* | | 0.0311*** | 0.0088 | 0.0302*** | 0.0086 |
| *XLIST* | | 0.0356*** | 0.0073 | 0.0350*** | 0.0073 |
| *LOG_GDP* | | 0.0343** | 0.0152 | 0.0326** | 0.0154 |
| *GROWTH_ GDP* | | -0.0015*** | 0.0006 | -0.0017*** | 0.0006 |
| *LOG_LISTED* | | -0.0176** | 0.0070 | -0.0139* | 0.0071 |
| *FDI* | | 0.0239 | 0.0401 | 0.0083 | 0.0386 |
| *OPEN* | | 0.0001 | 0.0002 | 0.0001 | 0.0001 |
| *LIB* | | -0.0431** | 0.0187 | -0.0403** | 0.0183 |
| N | | | 74247 | | 74247 |
| Adj. $R^2$ | | | 0.0961 | | 0.0943 |
| F-test | | | | | *p*-value |
| *Strong*Enforce= Weak*Enforce* | | | | | 0.0146 |

**Panel B: Dimensions of legal institutions**

This table reports estimates of the following equation:

$SMTH_{it} = \beta_0 + \beta_1$ *SUB_STRONG*ENFORCE*$_{it} + \beta_2$ *SUB_WEAK*ENFORCE*$_{it} + \sum\gamma_j$ *Firm-level Control*$_j + \sum\theta_k$ *Country-level Control*$_k$ + *Fixed Effects* + $\varepsilon_{it}$

The variable *SUB_STRONG* (*SUB_WEAK*) refers to the sub-dimensions of legal institutions, *EFF_JUD*, *RULE_LAW*, *CORRUPTION*, and *EMERGING*. *SUB_STRONG* (*SUB_WEAK*) is equal to one if *EFF_JUD* is equal to one (zero) in column (1), if *RULE_LAW* is equal to one (zero) in column (2), if *CORRPUTION* is equal to one (zero) in column (3), and if *EMERGING* is equal to zero (one) in column (4). Country, industry, and year fixed effects are included in all regressions. Variable definitions are in the appendix. ***,**,* indicate that a coefficient is significantly different from zero at the 1%, 5%, and 10% levels based on two-tailed tests and robust standard errors clustered by country-year.

| | | (1) *EFF_JUD* | | (2) *RULE_LAW* | | (3) *CORRUPTION* | | (4) *EMERGING* | |
|---|---|---|---|---|---|---|---|---|---|
| | Pred | Estimate | Std Err | Estimate | Std Err | Estimate | Std Err | Estimate | Std Err |
| *SUB_STRONG*ENFORCE* | + | 0.0256*** | 0.0082 | 0.0209*** | 0.0077 | 0.0251*** | 0.0084 | 0.0200*** | 0.0075 |
| *SUB_WEAK*ENFORCE* | | -0.0085 | 0.0074 | -0.0017 | 0.0117 | -0.0020 | 0.0089 | -0.0029 | 0.0132 |
| *SIZE* | | -0.0121*** | 0.0009 | -0.0121*** | 0.0009 | -0.0121*** | 0.0009 | -0.0122*** | 0.0009 |
| *LEV* | | -0.0681*** | 0.0073 | -0.0682*** | 0.0073 | -0.0680*** | 0.0073 | -0.0684*** | 0.0073 |
| *BTM* | | -0.0044*** | 0.0010 | -0.0043*** | 0.0010 | -0.0044*** | 0.0010 | -0.0043*** | 0.0010 |
| *STD_SALES* | | 0.0477*** | 0.0077 | 0.0480*** | 0.0077 | 0.0479*** | 0.0077 | 0.0480*** | 0.0077 |
| *PCT_LOSS* | | 0.1308*** | 0.0100 | 0.1310*** | 0.0100 | 0.1310*** | 0.0100 | 0.1311*** | 0.0101 |
| *OPCYCLE* | | -0.0009 | 0.0021 | -0.0007 | 0.0021 | -0.0007 | 0.0021 | -0.0007 | 0.0021 |
| *GROWTH* | | -0.0082*** | 0.0029 | -0.0082*** | 0.0029 | -0.0082*** | 0.0029 | -0.0082*** | 0.0029 |
| *OPLEV* | | 0.1488*** | 0.0078 | 0.1492*** | 0.0078 | 0.1491*** | 0.0078 | 0.1494*** | 0.0078 |
| *AVGCFO* | | 0.0895*** | 0.0344 | 0.0896*** | 0.0344 | 0.0896*** | 0.0344 | 0.0898*** | 0.0344 |
| *AUD* | | 0.0051** | 0.0023 | 0.0049** | 0.0023 | 0.0050** | 0.0023 | 0.0048** | 0.0023 |
| *ACCTSTD* | | 0.0301*** | 0.0086 | 0.0311*** | 0.0086 | 0.0302*** | 0.0086 | 0.0313*** | 0.0086 |
| *XLIST* | | 0.0350*** | 0.0073 | 0.0348*** | 0.0073 | 0.0350*** | 0.0073 | 0.0349*** | 0.0073 |
| *LOG_GDP* | | 0.0307** | 0.0153 | 0.0329** | 0.0156 | 0.0326** | 0.0154 | 0.0324** | 0.0156 |
| *GROWTH_ GDP* | | -0.0015*** | 0.0005 | -0.0017*** | 0.0006 | -0.0017*** | 0.0006 | -0.0017*** | 0.0006 |
| *LOG_LISTED* | | -0.0148** | 0.0072 | -0.0136* | 0.0072 | -0.0139* | 0.0071 | -0.0166** | 0.0073 |
| *FDI* | | 0.0202 | 0.0394 | 0.0081 | 0.0384 | 0.0083 | 0.0386 | 0.0123 | 0.0391 |
| *OPEN* | | 0.0000 | 0.0002 | 0.0001 | 0.0001 | 0.0001 | 0.0001 | 0.0001 | 0.0001 |
| *LIB* | | -0.0393** | 0.0184 | -0.0404** | 0.0183 | -0.0403** | 0.0183 | -0.0406** | 0.0185 |
| N | | | 74247 | | 74247 | | 74247 | | 74247 |
| Adj. $R^2$ | | | 0.0944 | | 0.0942 | | 0.0943 | | 0.0942 |
| F-test | | | *p*-value | | *p*-value | | *p*-value | | *p*-value |
| *Strong*Enforce= Weak*Enforce* | | | 0.0004 | | 0.0871 | | 0.0146 | | 0.1179 |

**Panel C: Residual vs. predicted smoothing**

This table reports estimates of the coefficients of interest in the following equation:
$R\_SMTH_{it}$ ($P\_SMTH_{it}$) = $\beta_0 + \beta_1$ $STRONG_{it}$*$ENFORCE_{it} + \beta_2$ $WEAK_{it}$*$ENFORCE_{it} + \sum\gamma_j$ *Firm-level* $Control_j + \sum\theta_k$ *Country-level* $Control_k$ + *Fixed Effects* + $\varepsilon_{it}$

The dependent variable is *R_SMTH* in columns (1-2). The dependent variable is *P_SMTH* in columns (3-4). Firm-level controls are included in columns (1) and (3) and excluded in columns (2) and (4). Country, industry, and year fixed effects are included in all regressions. Variable definitions are in the appendix. ***,**,* indicate that a coefficient is significantly different from zero at the 1%, 5%, and 10% levels based on two-tailed tests and robust standard errors clustered by country-year.

| | *R_SMTH* (Residual smoothness) | | | | *P_SMTH* (Fundamental smoothness) | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | | (2) | | (3) | | (4) | |
| | Estimate | Std Err | Estimate | Std Err | Estimate | Std Err | Estimate | Std Err |
| *STRONG*ENFORCE* | 0.0303*** | 0.0087 | 0.0322*** | 0.0086 | -0.0031 | 0.0022 | -0.0157 | 0.0100 |
| *WEAK*ENFORCE* | -0.0027 | 0.0089 | 0.0009 | 0.0086 | -0.0040 | 0.0028 | -0.0305** | 0.0145 |
| Firm-level controls | | Yes | | No | | Yes | | No |
| Country-level controls | | Yes | | Yes | | Yes | | Yes |
| N | | 74247 | | 74247 | | 74247 | | 74247 |
| Adj. $R^2$ | | 0.0533 | | 0.0374 | | 0.9361 | | 0.3587 |
| F-test | | *p*-value | | *p*-value | | *p*-value | | *p*-value |
| *Strong*Enforce= Weak*Enforce* | | 0.0038 | | 0.0046 | | 0.7800 | | 0.3440 |

**Panel D: Full interaction**

This table reports estimates of the coefficients of interest in the following equation:
$SMTH_{it}$ ($R\_SMTH_{it}$) = $\beta_0 + \beta_1$ $STRONG_{it}$*$ENFORCE_{it}$ + $\beta_2$ $WEAK_{it}$*$ENFORCE_{it}$ + $\sum\gamma_j$ *Firm-level Control*$_j$ + $\sum\theta_k$ *Country-level Control*$_k$ + $\sum\gamma'_j$ $STRONG_{it}$**Firm-level Control*$_j$ + $\sum\theta'_k$ $STRONG_{it}$**Country-level Control*$_k$ + *Fixed Effects* + $\varepsilon_{it}$
The dependent variable is *SMTH* in column (1) and *R_SMTH* in column (2). Country, industry, and year fixed effects are included in all regressions. Variable definitions are in the appendix. ***,**,* indicate that a coefficient is significantly different from zero at the 1%, 5%, and 10% levels based on two-tailed tests and robust standard errors.

| | (1) *SMTH* | | (2) *R_SMTH* | |
|---|---|---|---|---|
| | Estimate | Std Err | Estimate | Std Err |
| *STRONG*ENFORCE* | 0.0265*** | 0.0086 | 0.0309*** | 0.0087 |
| *WEAK*ENFORCE* | 0.0102 | 0.0088 | 0.0091 | 0.0086 |
| Firm-level controls | | Yes | | Yes |
| Country-level controls | | Yes | | Yes |
| N | | 74247 | | 74247 |
| Adj. $R^2$ | | 0.0979 | | 0.0568 |
| F-test | | *p*-value | | *p*-value |
| *Strong*Enforce= Weak*Enforce* | | 0.1667 | | 0.0653 |

**Table 4: Event analysis**

This table reports estimates of the following equation:

$SMTH_{it} = \beta_0 + \beta_1\ STRONG_{it}*ENFORCE_{it} + \beta_2\ WEAK_{it}*ENFORCE_{it} + \sum\gamma_j\ Firm\text{-}level\ Control_j + \sum\theta_k\ Country\text{-}level\ Control_k + Fixed\ Effects + \varepsilon_{it}$

The regression is estimated for the event window [-5, 5] around the initial enforcement of insider trading laws, excluding year zero in column (1), years zero and one in column (2), and years zero to two in column (3). Country, industry, and year fixed effects are included in all regressions. Variable definitions are in the appendix. ***,**,* indicate that a coefficient is significantly different from zero at the 1%, 5%, and 10% levels based on two-tailed tests and robust standard errors.

| | | (1) Exclude year 0 | | (2) Exclude year 0 and 1 | | (3) Exclude year 0, 1 and 2 | |
|---|---|---|---|---|---|---|---|
| | Pred. | Estimate | Std Err | Estimate | Std Err | Estimate | Std Err |
| *STRONG*ENFORCE* | + | 0.0336*** | 0.0126 | 0.0480*** | 0.0139 | 0.0568*** | 0.0210 |
| *WEAK*ENFORCE* | | 0.0061 | 0.0112 | 0.0173 | 0.0130 | 0.0243 | 0.0203 |
| *SIZE* | | -0.0102*** | 0.0016 | -0.0099*** | 0.0017 | -0.0096*** | 0.0018 |
| *LEV* | | -0.0591*** | 0.0141 | -0.0679*** | 0.0146 | -0.0636*** | 0.0158 |
| *BTM* | | 0.0049*** | 0.0016 | 0.0045*** | 0.0017 | 0.0053*** | 0.0018 |
| *STD_SALES* | | 0.0485*** | 0.0160 | 0.0515*** | 0.0172 | 0.0527*** | 0.0189 |
| *PCT_LOSS* | | 0.2011*** | 0.0106 | 0.2027*** | 0.0112 | 0.2028*** | 0.0121 |
| *OPCYCLE* | | 0.0048 | 0.0049 | 0.0046 | 0.0052 | 0.0055 | 0.0057 |
| *GROWTH* | | -0.0146*** | 0.0046 | -0.0148*** | 0.0048 | -0.0143*** | 0.0049 |
| *OPLEV* | | 0.1175*** | 0.0125 | 0.1198*** | 0.0135 | 0.1176*** | 0.0152 |
| *AVGCFO* | | 0.4264*** | 0.0337 | 0.4340*** | 0.0362 | 0.4367*** | 0.0389 |
| *AUD* | | 0.0224*** | 0.0052 | 0.0196*** | 0.0056 | 0.0172*** | 0.0057 |
| *ACCTSTD* | | 0.0336*** | 0.0111 | 0.0293*** | 0.0110 | 0.0245** | 0.0118 |
| *XLIST* | | 0.0370*** | 0.0118 | 0.0349*** | 0.0121 | 0.0307** | 0.0131 |
| *LOG_GDP* | | 0.0255 | 0.0306 | 0.0166 | 0.0361 | -0.0156 | 0.0306 |
| *GROWTH_ GDP* | | -0.0027*** | 0.0007 | -0.0027*** | 0.0008 | -0.0016* | 0.0009 |
| *LOG_LISTED* | | -0.0042 | 0.0100 | -0.0052 | 0.0102 | -0.0107 | 0.0103 |
| *FDI* | | -0.0024 | 0.1260 | -0.0206 | 0.1281 | -0.0311 | 0.1285 |
| *OPEN* | | -0.0001 | 0.0005 | 0.0000 | 0.0005 | 0.0000 | 0.0005 |
| *LIB* | | -0.0031 | 0.0344 | 0.0100 | 0.0373 | -0.0044 | 0.0312 |
| N | | | 17829 | | 15930 | | 13746 |
| Adj. $R^2$ | | | 0.1161 | | 0.1180 | | 0.1140 |
| F-test | | | *p*-value | | *p*-value | | *p*-value |
| *Strong*Enforce= Weak*Enforce* | | | 0.0158 | | 0.0092 | | 0.0084 |

**Table 5: Robustness Tests**

This table reports estimates of the coefficient of interest in the following equation:
$SMTH_{it} = \beta_0 + \beta_1 STRONG_{it}*ENFORCE_{it} + \beta_2 WEAK_{it}*ENFORCE_{it} + \sum\gamma_j Firm\text{-}level\ Control_j + \sum\theta_k Country\text{-}level\ Control_k + Fixed\ Effects + \varepsilon_{it}$
The regression is estimated for the sample excluding U.S. in column (1), excluding Japan in column (2), excluding U.K. in column (3), excluding event years zero to two in column (4), and excluding firms cross-listed in the U.S. in column (5). Country, industry, and year fixed effects are included in all regressions. Variable definitions are in the appendix. ***,**,* indicate that a coefficient is significantly different from zero at the 1%, 5%, and 10% levels based on two-tailed tests and robust standard errors.

| | (1) Exclude US | | (2) Exclude Japan | | (3) Exclude UK | | (4) Exclude (0,1,2) | | (5) Exclude cross-listed | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Estimate | Std Err | Estimate | Std Err | Estimate | Std Err | Estimate | Std Err | Estimate | Std Err |
| *STRONG*ENFORCE* | 0.0272*** | 0.0087 | 0.0332*** | 0.0075 | 0.0318*** | 0.0077 | 0.0291*** | 0.0090 | 0.0243*** | 0.0083 |
| *WEAK*ENFORCE* | -0.0017 | 0.0091 | -0.0029 | 0.0086 | 0.0030 | 0.0090 | -0.0007 | 0.0095 | 0.0029 | 0.0090 |
| Firm-level controls | | Yes | | Yes | | Yes | | Yes | | Yes |
| Country-level controls | | Yes | | Yes | | Yes | | Yes | | Yes |
| N | | 62683 | | 62036 | | 62157 | | 70164 | | 72337 |
| Adj. $R^2$ | | 0.0974 | | 0.0860 | | 0.0970 | | 0.0932 | | 0.0945 |
| F-test | | *p*-value | | *p*-value | | *p*-value | | *p*-value | | *p*-value |
| *Strong*Enforce= Weak*Enforce* | | 0.0085 | | 0.0006 | | 0.0066 | | 0.0119 | | 0.0542 |

## Table 6: Insider trading profits and earnings smoothing

This table reports estimates of the following equation:
$Trading\ Profit_t = \alpha + \beta_1\ R\_SMTH_{t-1} + \beta_2\ BTM_{t-1} + \beta_3\ MV_{t-1} + \beta_4\ R\&D_{t-1} + \beta_5\ LOSS_{t-1} + \beta_6\ VOLATILITY_{t-1} + \beta_7\ LOGANALYST_{t-1} + \beta_8\ RET_{t-1} + \varepsilon_t$
The regression is estimated using the sample of trades of executives and directors of U.S. firms over the period of 1990-2010. Variable definitions are in the appendix. ***,**,* indicate that a coefficient is significantly different from zero at the 1%, 5%, and 10% levels based on two-tailed tests and robust standard errors clustered by clustered by 3-digit SIC industry and month.

| | Estimate | Std Err |
|---|---|---|
| *R_SMTH* | -0.0229*** | 0.0080 |
| *BTM* | 0.0050 | 0.0036 |
| *MV* | -0.0022 | 0.0015 |
| *R&D* | -0.0017 | 0.0038 |
| *LOSS* | 0.0089 | 0.0068 |
| *VOLATILITY* | 8.3037*** | 1.5678 |
| *LOGANALYST* | -0.0026 | 0.0021 |
| | | |
| N | | 227,369 |
| $R^2$ | | 0.0070 |

**Table 7: Confounding events and alternative explanations**

**Panel A: Confounding regulatory events**

This table reports estimates of the coefficients of interest in the following equation:
$SMTH_{it} = \beta_0 + \beta_1\ STRONG_{it}*ENFORCE_{it} + \beta_2\ WEAK_{it}*ENFORCE_{it} + \sum\gamma_j\ Firm\text{-}level\ Control_j + \sum\theta_k\ Country\text{-}level\ Control_k + Fixed\ Effects + \varepsilon_{it}$
The regression is estimated for Australia and Hong Kong in column (1) and countries with strong legal institutions except for Denmark and Belgium in column (2). Country, industry, and year fixed effects are included in all regressions. Variable definitions are in the appendix. ***,**,* indicate that a coefficient is significantly different from zero at the 1%, 5%, and 10% levels based on two-tailed tests and robust standard errors.

| | (1) Australia & Hong Kong only | | (2) Excluding Denmark and Belgium | |
|---|---|---|---|---|
| | Estimate | Std Err | Estimate | Std Err |
| *ENFORCE* | 0.0920*** | 0.0093 | 0.0195** | 0.0095 |
| Firm-level controls | | Yes | | Yes |
| Country-level controls | | Yes | | Yes |
| N | | 4010 | | 52121 |
| Adj. $R^2$ | | 0.0784 | | 0.0941 |

**Panel B: Enforcement of insider trading laws and leverage**

This table reports estimates of coefficients of interest in the following equation:
$SMTH_{it} = \beta_0 + \beta_1\ STRONG_{it}*ENFORCE_{it} + \beta_2\ WEAK_{it}*ENFORCE_{it} + \beta_3\ HIGHLEV*STRONG_{it}*ENFORCE_{it} + \beta_4\ HIGHLEV_{it}*WEAK_{it}*ENFORCE_{it} + \beta_5\ HIGHLEV_{it} + \beta_6\ HIGHLEV_{it}*STRONG + \sum\gamma_j\ Firm\text{-}level\ Control_j + \sum\theta_k\ Country\text{-}level\ Control_k + Fixed\ Effects + \varepsilon_{it}$

The variable *HIGHLEV* is equal to a dummy variable set to one if a firm's leverage is above the median of the population in column (1), the decile ranking of leverage in column (2), the percentile ranking of leverage in column (3), and leverage, long-term debt over total assets, in column (4). Country, industry, and year fixed effects are included in all regressions. Variable definitions are in the appendix. ***,**,* indicate that a coefficient is significantly different from zero at the 1%, 5%, and 10% levels based on two-tailed tests and robust standard errors.

| | (1) Dummy | | (2) Decile | | (3) Percentile | | (4) Raw | |
|---|---|---|---|---|---|---|---|---|
| | Estimate | Std Err | Estimate | Std Err | Estimate | Std Err | Estimate | Std Err |
| *STRONG*ENFORCE* | 0.0269*** | 0.0097 | 0.0206* | 0.0113 | 0.0208* | 0.0118 | 0.0246** | 0.0247 |
| *WEAK*ENFORCE* | -0.0021 | 0.0100 | -0.0051 | 0.0118 | -0.0054 | 0.0123 | -0.0015 | -0.0014 |
| *HIGHLEV*STRONG*ENFORCE* | -0.0031 | 0.0077 | 0.0009 | 0.0013 | 0.0001 | 0.0001 | 0.0028 | 0.0023 |
| *HIGHLEV*WEAK*ENFORCE* | 0.0008 | 0.0082 | 0.0008 | 0.0015 | 0.0001 | 0.0002 | 0.0006 | 0.0003 |
| Firm-level controls | | Yes | | Yes | | Yes | | Yes |
| Country-level controls | | Yes | | Yes | | Yes | | Yes |
| N | | 74247 | | 74247 | | 74247 | | 74247 |
| Adj. $R^2$ | | 0.0943 | | 0.0943 | | 0.0943 | | 0.0944 |
| F-test | | *p*-value | | *p*-value | | *p*-value | | *p*-value |
| *Strong*Enforce= Weak*Enforce* | | 0.0225 | | 0.0918 | | 0.1020 | | 0.0432 |

## Table 8: Strictness of insider trading laws

This table reports estimates of the coefficients of interest in the following equation:
$SMTH_{it} = \beta_0 + \beta_1 STRONG_{it}*ENFORCE_{it} + \beta_2 WEAK_{it}*ENFORCE_{it} + \beta_3 NOTSTRICT*STRONG_{it}*ENFORCE_{it} + \beta_4 NOTSTRICT*WEAK_{it}*ENFORCE_{it} + \beta_5 NOTSTRICT*STRONG + \sum\gamma_j Firm\text{-}level\ Control_j + \sum\theta_k Country\text{-}level\ Control_k + Fixed\ Effects + \varepsilon_{it}$
Column (1) reports the base line results (without interactions involving *NOTSTRICT*) for the subsample with scores on law strictness. The variable *NOTSTRICT* is equal to one if Beny's (2005) score of insider trading laws is equal to one or two, and zero if the rating is equal to three or four in column (2) and equal to the maximum score (four) minus Beny's score in column (3). Country, industry, and year fixed effects are included in all regressions. Variable definitions are in the appendix. ***,**,* indicate that a coefficient is significantly different from zero at the 1%, 5%, and 10% levels based on two-tailed tests and robust standard errors.

| | (1) | | (2) | | (3) | |
|---|---|---|---|---|---|---|
| | Estimate | Std Err | Estimate | Std Err | Estimate | Std Err |
| *STRONG*ENFORCE* | 0.0272*** | 0.0086 | 0.0318*** | 0.0077 | 0.1072*** | 0.0392 |
| *WEAK*ENFORCE* | -0.0051 | 0.0076 | 0.0038 | 0.0096 | 0.0238 | 0.0214 |
| *NOTSTRICT*STRONG*ENFORCE* | | | -0.0831** | 0.0434 | -0.0757** | 0.0383 |
| *NOTSTRICT*WEAK*ENFORCE* | | | -0.0200 | 0.0143 | -0.0200 | 0.0143 |
| Firm-level controls | | | | Yes | | Yes |
| Country-level controls | | | | Yes | | Yes |
| N | | 57838 | | 57838 | | 57838 |
| Adj. $R^2$ | | 0.0971 | | 0.0974 | | 0.0974 |
| F-test | | *p*-value | | *p*-value | | *p*-value |
| *Strong*Enforce= Weak*Enforce* | | 0.0009 | | 0.0058 | | 0.0566 |

## Table 9: Enforcement strength: enforcement and penalty post the initial enforcement

**Panel A: Existence of insider trading enforcement cases post initial enforcement year**
This table reports estimates of the coefficients of interest in the following equation and its variation: $SMTH_{it} = \beta_0 + \beta_1 NO\_PERSIST*ENFORCE_{it} + \beta_2 PERSIST*ENFORCE_{it} + \sum\gamma_j Firm\text{-}level\ Control_j + \sum\theta_k Country\text{-}level\ Control_k + Fixed\ Effects + \varepsilon_{it}$

The variable *PERSIST* is equal to one if there are inside trading law enforcement actions during the five-year period post the initial enforcement in a country, and zero if there is no such action; *NO_PERSIST* is equal to one minus *PERSIST*. Post-enforcement observations from countries where information on the enforcement actions during the five-year period post the initial enforcement is not available are dropped. Column (1) reports the results from estimating the above equation. The interactions of *PERSIST* and firm- and country-level control variables are added in the regression in Column (2). Country, industry, and year fixed effects are included in all regressions. Variable definitions are in the appendix. ***,**,* indicate that a coefficient is significantly different from zero at the 1%, 5%, 10% level based on two-tailed tests and robust standard errors clustered by country-year.

| | (1) | | (2) | |
|---|---|---|---|---|
| | Estimate | Std Err | Estimate | Std Err |
| *NO_PERSIST*ENFORCE* | 0.0129 | 0.0182 | 0.0120 | 0.0187 |
| *PERSIST*ENFORCE* | 0.0624*** | 0.0143 | 0.1886** | 0.0912 |
| Firm-level controls | | Yes | | Yes |
| Country-level controls | | Yes | | Yes |
| N | | 18197 | | 18197 |
| Adj. $R^2$ | | 0.1094 | | 0.1135 |
| F-test | | *p*-value | | *p*-value |
| *NO_PERSIST*ENFORCE=PERSIST*ENFORCE* | | 0.0066 | | 0.0604 |

**Panel B: Actual penalty of insider trading enforcement cases**

This table reports estimates of the coefficients of interest in the following equation and its variation: $SMTH_{it} = \beta_0 + \beta_1\ MILD\_PENALTY*ENFORCE_{it} + \beta_2\ SEVERE\_PENALTY*ENFORCE_{it} + \sum\gamma_j\ Firm\text{-}level\ Control_j + \sum\theta_k\ Country\text{-}level\ Control_k + Fixed\ Effects + \varepsilon_{it}$

The variable *SEVERE_PENALTY* is set equal to one for Australia, and zero for Denmark and Hong Kong. *MILD_PENALTY* is equal to one minus *SEVERE_PENALTY*. Post-enforcement observations from countries where information on actual penalty in insider trading cases during the five-year period post the initial enforcement is not available are dropped. Column (1) reports the results from estimating the above equation. The interactions of *SEVERE_PENALTY* and firm- and country-level control variables are added in the regression in Column (2). Country, industry, and year fixed effects are included in all regressions. Variable definitions are in the appendix. ***,**,* indicate that a coefficient is significantly different from zero at the 1%, 5%, 10% level based on two-tailed tests and robust standard errors clustered by country-year.

| | (1) | | (2) | |
|---|---|---|---|---|
| | Estimate | Std Err | Estimate | Std Err |
| *MILD_PENALTY*ENFORCE* | 0.0355** | 0.0180 | 0.0397** | 0.0163 |
| *SEVERE_PENALTY*ENFORCE* | 0.0605*** | 0.0146 | 0.1557** | 0.0688 |
| Firm-level controls | | Yes | | Yes |
| Country-level controls | | Yes | | Yes |
| N | | 12965 | | 12965 |
| Adj. $R^2$ | | 0.1251 | | 0.1323 |
| F-test | | *p*-value | | *p*-value |
| *MILD_PENALTY*ENFORCE=SEVERE_PENALTY*ENFORCE* | | 0.0860 | | 0.0912 |

**Table 10: Enforcement of insider trading laws and ownership structure**

This table reports estimates of the coefficients of interest in the following equation:
$SMTH_{it} = \beta_0 + \beta_1 STRONG_{it}*ENFORCE_{it} + \beta_2 WEAK_{it}*ENFORCE_{it} + \beta_3 CLOSE*STRONG_{it}*ENFORCE_{it} + \beta_4 CLOSE*WEAK_{it}*ENFORCE_{it} + \beta_5 CLOSE + \beta_6 CLOSE*STRONG + \sum\gamma_j Firm\text{-}level\ Control_j + \sum\theta_k Country\text{-}level\ Control_k + Fixed\ Effects + \varepsilon_{it}$

The variable *CLOSE* is equal to a dummy variable set to one if a firm's percentage of closely held shares is above the median of the population in column (1), the decile ranking of closely held shares percentage in column (2), the percentile ranking of closely held shares percentage in column (3), and the percentage of closely held shares in column (4). Country, industry, and year fixed effects are included in all regressions. Variable definitions are in the appendix. ***,**,* indicate that a coefficient is significantly different from zero at the 1%, 5%, and 10% levels based on two-tailed tests and robust standard errors.

| | (1) Dummy | | (2) Decile | | (3) Percentile | | (4) Raw | |
|---|---|---|---|---|---|---|---|---|
| | Estimate | Std Err | Estimate | Std Err | Estimate | Std Err | Estimate | Std Err |
| *STRONG*ENFORCE* | 0.0544*** | 0.0109 | 0.0652*** | 0.0145 | 0.0689*** | 0.0154 | 0.0674*** | 0.0152 |
| *WEAK*ENFORCE* | 0.0210 | 0.0182 | 0.0273 | 0.0220 | 0.0267 | 0.0232 | 0.0263 | 0.0227 |
| *CLOSE*STRONG*ENFORCE* | -0.0265*** | 0.0101 | -0.0053** | 0.0021 | -0.0005*** | 0.0002 | -0.0006** | 0.0002 |
| *CLOSE*WEAK*ENFORCE* | -0.0132 | 0.0150 | -0.0027 | 0.0026 | -0.0002 | 0.0003 | -0.0002 | 0.0003 |
| Firm-level controls | | Yes | | Yes | | Yes | | Yes |
| Country-level controls | | Yes | | Yes | | Yes | | Yes |
| N | | 66625 | | 66625 | | 66625 | | 66625 |
| Adj. $R^2$ | | 0.0954 | | 0.0955 | | 0.0955 | | 0.0955 |
| F-test *Strong*Enforce= Weak*Enforce* | | *p*-value 0.1049 | | *p*-value 0.1456 | | *p*-value 0.1264 | | *p*-value 0.1283 |

## Table11: Enforcement of insider trading laws and growth

This table reports estimates of the following equations with measures of earnings smoothing as the dependent variable:

$SMTH_{it} = \beta_0 + \beta_1 STRONG_{it}*ENFORCE_{it} + \beta_2 WEAK_{it}*ENFORCE_{it} + \beta_3 LOWGROW*STRONG_{it}*ENFORCE_{it} + \beta_4 LOWGROW*WEAK_{it}*ENFORCE_{it} + \beta_5 LOWGROW + \beta_6 LOWGROW*STRONG + \sum\gamma_j Firm\text{-}level\ Control_j + \sum\theta_k Country\text{-}level\ Control_k + Fixed\ Effects + \varepsilon_{it}$

The variable *LOWGROW* is equal to a dummy variable set to one if a firm's average sales growth in the last 5 years is below the median of the population in column (1), the decile ranking of growth in column (2), in descending order, the percentile ranking of growth in column (3), in descending order, and the negative of the average growth in column (4). Country, industry, and year fixed effects are included in all regressions. Variable definitions are in the appendix. ***,**,* indicate that a coefficient is significantly different from zero at the 1%, 5%, and 10% levels based on two-tailed tests and robust standard errors.

| | (1) Dummy | | (2) Decile | | (3) Percentile | | (4) Raw | |
|---|---|---|---|---|---|---|---|---|
| | Estimate | Std Err | Estimate | Std Err | Estimate | Std Err | Estimate | Std Err |
| *STRONG*ENFORCE* | 0.0376*** | 0.0087 | 0.0515*** | 0.0100 | 0.0548*** | 0.0104 | 0.0203** | 0.0089 |
| *WEAK*ENFORCE* | -0.0013 | 0.0093 | 0.0089 | 0.0106 | 0.0105 | 0.0111 | -0.0168* | 0.0096 |
| *LOWGROW*STRONG*ENFORCE* | -0.0243*** | 0.0078 | -0.0057*** | 0.0015 | -0.0006*** | 0.0002 | -0.0361* | 0.0213 |
| *LOWGROW*WEAK*ENFORCE* | -0.0074 | 0.0098 | -0.0040** | 0.0018 | -0.0004** | 0.0002 | -0.0570*** | 0.0135 |
| Firm-level controls | | Yes | | Yes | | Yes | | Yes |
| Country-level controls | | Yes | | Yes | | Yes | | Yes |
| N | | 74247 | | 74247 | | 74247 | | 74247 |
| Adj. $R^2$ | | 0.0948 | | 0.0950 | | 0.0950 | | 0.0947 |
| F-test | | *p*-value | | *p*-value | | *p*-value | | *p*-value |
| *Strong*Enforce= Weak*Enforce* | | 0.0009 | | 0.0021 | | 0.0023 | | 0.0020 |

## Table 12: Stock price informativeness

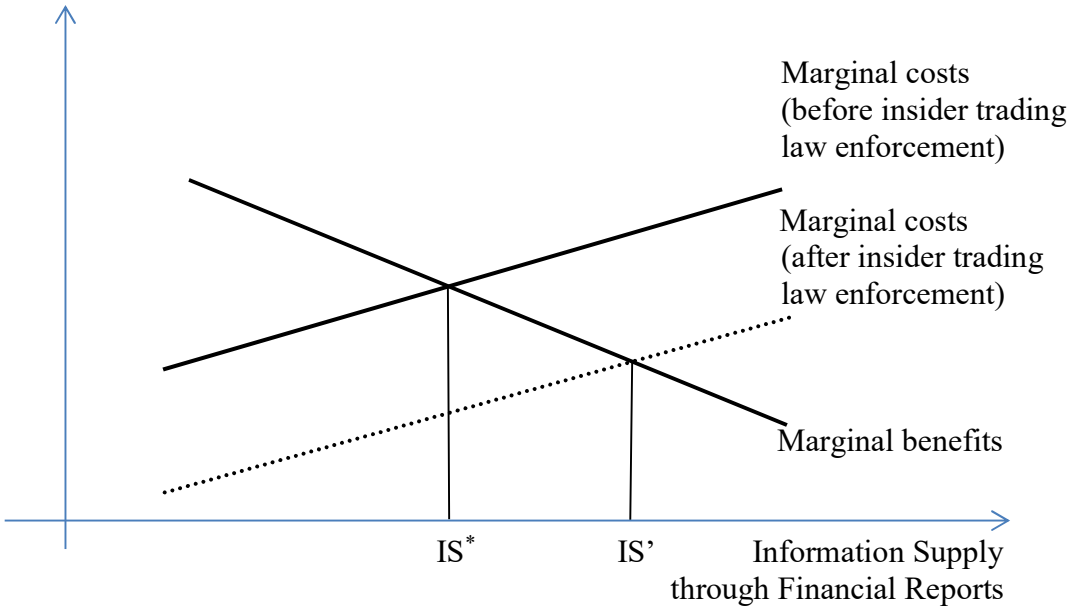
This table reports the estimation results of the following regression:

$\Psi_{it} = \beta_0 + \beta_1 SMTH_{it} + \beta_2 DEVELOPED*ENFORCE_{it} + \beta_3 EMERGING*ENFORCE_{it} + \sum \gamma_j Median\ Firm\text{-}level\ Control_j + \sum \theta_k Country\text{-}level\ Control_k + Country\ Fixed\ Effects + \varepsilon_{it}$

Country fixed effects are included. Variable definitions are in the appendix. ***,**,* indicate that a coefficient is significantly different from zero at the 1%, 5%, and 10% levels based on two-tailed tests and robust standard errors clustered by country-year.

| | (1) No SMTH | | (2) With SMTH | |
|---|---|---|---|---|
| | Estimate | Std Err | Estimate | Std Err |
| *SMTH* | | | 1.0670*** | 0.3732 |
| *DEVELOPED*ENFORCE* | 0.2032* | 0.1164 | 0.1271 | 0.1173 |
| *EMERGING*ENFORCE* | 0.1151 | 0.1645 | 0.1025 | 0.1610 |
| *LOG_GDP* | 0.4272** | 0.1807 | 0.3950** | 0.1770 |
| *LOG_LISTED* | 0.1389 | 0.1066 | 0.2123 | 0.1069 |
| *FHERF* | 0.3989 | 2.0100 | 0.5028 | 1.7786 |
| *IHERF* | 0.7423 | 1.0399 | 0.7433 | 1.0396 |
| *VGDP* | -0.0030 | 0.0032 | -0.0033 | 0.0031 |
| *LIB* | 0.2944 | 0.2158 | 0.2405 | 0.1965 |
| Controls for median firm characteristics | | Yes | | Yes |
| N | | 485 | | 485 |
| Adj. $R^2$ | | 0.5735 | | 0.5831 |

**Figure 1**




Upon the enforcement of insider trading laws, the marginal cost of information supply through financial reports decreases, resulting in a shift of the marginal cost curve from the solid line to the dotted line and an increase of the equilibrium level of information supply from IS* to IS'.