The Real Effect of the Initial Enforcement of Insider Trading Laws[☆]

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This version: October 2012

Abstract
We find strong evidence that the enforcement of insider trading laws have a real effect on the economy. In particular, the sensitivity of corporate investment to stock prices increases significantly after the initial enforcement of insider trading laws using data from 45 countries over 1982-2003. Further analysis suggests that the main reason underlying this increase is that after the enforcement stock prices provide more information to guide managers' investment decisions. Finally, we find that the improvement in firm operating performance after the enforcement is positively associated with the increase in the investment-to-price sensitivity. Overall, our results tend to support the managerial learning hypothesis.

Keywords: Enforcement; Insider trading laws; Investment; Managerial learning; Market frictions; Real effect.

JEL Classification: D83, G15, G31, K22.

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1. Introduction

Whether or not insider trading should be regulated has been the subject of a long-lasting debate in the literatures of economics, finance, and law (Bainbridge, 2000). This debate has intensified in the wake of the recent financial crisis and insider trading scandals (Boudreaux, 2009; Economist, 2011). One important question is whether and how insider trading regulation affects real investment efficiency (e.g., Ausubel, 1990; Leland, 1992; Khanna, Slezak, and Bradley, 1994; Bernhardt, Hollifield, and Hughson, 1995). Answering this question is important for a comprehensive assessment of the economic consequences of insider trading regulation. The reason is that real investment is the ultimate driver of value creation and critically determines a nation's success in global competition (Thurow, 1993).

Proponents of the regulation argue that insider trading distorts real investment efficiency. Insider trading deters information acquisitions and trading by outside investors, leading to lower price efficiency (Fishman and Hagerty, 1992; Fernandes and Ferreira, 2009). As a result, managers have less information to guide efficient resource allocation (Khanna et al., 1994; Dow and Gorton, 1997; Subrahmanyam and Titman, 1999; Chen, Goldstein, and Jiang, 2007). Low price efficiency also exacerbates market frictions due to adverse selection and moral hazard, exaggerating firms' financing constraints (Myers and Majluf, 1984; Edmans, 2009; Ferreira et al., 2011). Insider trading directly distorts the incentives of managers and large shareholders, leading to more severe agency problems (Manove, 1989; Bebchuk and Fershtman, 1990; Maug, 2002; Cohen, Malloy, and Pomorski, 2012). As will be discussed in more detail in Section 2, one prediction of the above arguments is that insider trading restrictions should be associated with increases in the investment-to-price sensitivity and firm operating performance. Moreover, the increases in the two measures should be positively correlated.

Opponents of the regulation, however, view insider trading as an efficient way for managers to convey their private information to the market. Therefore, prohibiting insider trading reduces stock price efficiency (Haw, Hu, Lee, and Wu, 2012) and hurts real investment efficiency (Leland, 1992; Bernhardt et al., 1995). Restricting insider trading also reduces investment efficiency by directly distorting managers' incentives. This is because insider trading serves as an important mechanism to motivate managers to acquire and develop private information to guide their investment decisions (e.g., Manne, 1966; Carlton and Fischel, 1983), and to take risks that would benefit shareholders (Bebchuk and Fershtman, 1994; Roulstone, 2003). Therefore, how insider trading regulation affects real investment efficiency is difficult to quantify ex ante and needs to be resolved empirically.

However, there is very little empirical research on how insider trading regulation affects real investment efficiency. One line of the research examines how insider trading regulation affects the intensity and profitability of insider trading (e.g., Seyhun, 1992; Hillier and Marshall, 2002; Lee, Lemmon, Li, and Sequeria, 2011). This line of investigation helps us understand the effect of insider trading regulation on value redistribution between insiders and outside investors, but does not examine how it affects value creation. In an influential study, Bhattacharya and Daouk (2002) find that insider trading regulation reduces the country-level cost of equity. However, it is not clear to what extent corporate investments are affected because firms can potentially resort to non-equity funds (e.g., bank loans and public debt) to finance their projects. Two recent studies have documented that the initial enforcement of insider trading laws increases analysts' activities (Bushman, Piotroski, and Smith, 2005) and price informativeness (Fernandes and Ferreira, 2009). Nevertheless, as pointed out by Dow and Gorton (1997) and Bond, Edmans, and Goldstein (2011), informational efficiency of stock prices is neither a

sufficient nor necessary condition for investment efficiency.[1] Hence, it is still unclear how insider trading regulation affects corporate investment behavior.

In this paper, we attempt to address this issue by investigating the relation between the initial enforcement of insider trading laws (hereafter the enforcement) and the sensitivity of corporate investment to stock prices. Our sample includes 175,968 firm-year observations from 24,149 firms in 45 countries over 1982-2003. In our pooled-sample firm-level regression analysis, we find that the investment-to-price sensitivity increases significantly after the enforcement. The result remains even after controlling for country-level legal rules that protect investors who buy securities from share issuance (La Porta, Lopez-de-Silanes, and Shleifer, 2006) and that pertains directly to the problem of corporate self-dealing (La Porta, Lopez-de-Silanes, Shleifer, and Vishny, 1998; Djankov, La Porta, Lopez-de-Silanes, and Shleifer, 2008). Adjusting for the time trend in the investment-to-price sensitivity does not change the results. We conduct a series of sensitivity tests and find that the results are robust to various model specifications, sample selections, and measures of investment.

To substantiate the pooled sample results, we conduct an event-window analysis. In particular, we investigate the change in the investment-to-price sensitivity in the six-year window (i.e., years [-2,+3]) around the initial enforcement year (year 0). We continue to find a significant increase in the investment-to-price sensitivity after the enforcement. More importantly, we observe a significant jump in the investment-to-price sensitivity immediately

[1] For example, in the model of Dow and Gorton (1997), it is possible that investors do not produce information and managers do not make investment (because the unconditional net present value (NPV) is negative). In this case, while stock prices are efficient because they correctly reflect that investment will not be made, investment is not efficient because there is no information to help managers find out projects with positive NPV. Even if stock prices reveal positive NPV projects, investment may still be inefficient because there is no mechanism through which shareholders can force managers to take a particular action (Bond et al., 2011). Therefore, stock price efficiency may not lead to investment efficiency. On the other hand, Dow and Gorton (1997) point out that a banking system can serve as an alternative mechanism for the efficient allocation of investment resources so that stock price efficiency is not necessary for investment efficiency.

after the enforcement year (i.e., year +1, see Figure 1), but no significant change in either the pre-enforcement period (i.e., years -1 and 0) or the post-enforcement period (i.e., years +2 and +3). This finding further increases our confidence that the increase in the investment-to-price sensitivity is attributable to the enforcement. Finally, we repeat firm-level analysis using country-year specific estimates of the investment-to-price sensitivity and find similar results.

We then examine the underlying reasons for the increased investment-to-price sensitivity after the enforcement. One reason could be that the enforcement enables stock prices to provide more information to guide managers' investment decisions (the managerial learning hypothesis). Alternatively, the increase may be because the enforcement reduces the market frictions due to adverse selection and/or moral hazard (the market frictions hypothesis). It is also possible that both effects are responsible. As the first attempt to distinguish between the two hypotheses, we investigate the cross-sectional variations in the change of the investment-to-price sensitivity. More specifically, we test whether the change is associated with price informativeness, incentives of insiders to expropriate outside investors, and financing constraints.

The managerial learning hypothesis predicts that the increase in the investment-to-price sensitivity should be more pronounced when the enforcement results in a larger increase in price informativeness (Fernandes and Ferreira, 2009). We construct two proxies for price informativeness, namely the price non-synchronicity suggested by Morck, Yeung, and Yu (2000) and the measure of information-based trading proposed by Llorente, Michaely, Saar, and Wang (2002). In the pooled sample regression, we find that the effect of the enforcement is higher when the price non-synchronicity after the enforcement is higher. We find similar results using Llorente et al.'s (2002) measure of price informativeness, although the results are statistically insignificant. We further conduct a more precise analysis by linking the *change* in the

investment-to-price sensitivity in the event window to the corresponding *change* in price informativeness. We find a significant and positive association between price informativeness and the investment-to-price sensitivity using both measures, consistent with the managerial learning hypothesis.

The managerial learning hypothesis also predicts a negative association between the effect of the enforcement and the change in *public* information after the enforcement (e.g., Jayaraman, 2012). This is because an increase in *public* information reduces the amount of *private* information that managers can extract from the stock prices (Gao and Liang, 2011; Maffett, 2012). We construct three proxies for the public information quality, namely, the absolute value of discretionary accruals, the earnings opacity measure suggested by Bhattacharya, Daouk, and Welker (2003), and coverage by sell-side analysts (Bushman et al., 2005). We find results consistent with the learning hypothesis using earnings opacity and analyst coverage, while the result based on discretionary accruals is insignificant.

One of the predictions suggested by the market frictions hypothesis is that if the increase in the investment-to-price sensitivity after the enforcement is attributed to mitigating moral hazard problems (i.e., agency problems), we should observe a stronger effect in firms where insiders have stronger incentives to expropriate outside investors. We partition the sample based on the wedge between the control rights and the cash flow rights of the controlling shareholder (Claessens, Djankov, and Lang, 2000; Faccio and Lang, 2002). In both the pooled sample and the event-window analyses, although the sample size is significantly reduced, we still find a significant increase in the investment-to-price sensitivity after the enforcement in firms with lower wedges, but not in ones with higher wedges. The evidence is not consistent with the notion that the increase in the investment-to-price sensitivity is due to agency problems.

We also examine whether the increase in the investment-to-price sensitivity occur because the enforcement relaxes financing constraints associated with adverse selection. To do so, we partition the sample into equal-sized quartiles based on Whited and Wu's (2006) index which measures financial constraints. In both the pooled sample and event-window regressions we find a significant increase in the investment-to-price sensitivity in all quartiles except the most financially constrained one. We then link the effect of the enforcement to actual external financing activities. In the pooled sample regression, we find evidence that the effect of the enforcement is stronger for firms that raise equity capital after the enforcement but not for firms that raise debt capital. When we combine equity and debt financing together, the effect of total external financing is positive but only marginally significant. However, in the event-window analysis, we do not find any significant association between the *change* in external financing and the *change* in the investment-to-price sensitivity. These results in general appear to be inconsistent with the market frictions hypothesis.

We then examine the change in the sensitivity of the investment to sales growth after the enforcement. By construction, sales growth is a *public* signal that does not include stock prices, and thus the investment-to-sales growth sensitivity cannot capture any managerial learning effect. Any significant change in this sensitivity after the enforcement is most likely due to the change in market frictions. We find that in general the association between the enforcement and the investment-to-sales growth sensitivity is insignificant. This evidence is inconsistent with the market frictions hypothesis.

In general, our evidence suggests that the primary reason underlying the increase in the investment-to-price sensitivity after the enforcement is that stock prices contain more information to guide managers' investment decisions after the enforcement. If managers do

extract more information from stock prices and improve investment efficiency, we should observe an improvement in investment performance after the enforcement, and the improvement should be positively associated with the increase in the investment-to-price sensitivity. We find evidence consistent with this prediction. Specifically, in both the pooled sample and the event-window analyses, the *changes* in returns on assets and sales growth after the enforcement are positively associated with the *change* in the investment-to-price sensitivity.

The rest of the paper is organized as follows. Section 2 presents different arguments that link the enforcement to the investment-to-price sensitivity. Section 3 describes the data and research design issues. Section 4 shows the results of the regression analysis on the association between the enforcement and the investment-to-price sensitivity. Section 5 presents additional analyses attempting to identify the underlying reasons for the increase in the investment-to-price sensitivity. Section 6 examines the effect of the enforcement on firm operating performance. Finally, Section 7 concludes the paper and highlights the contributions.

2. The links between the initial enforcement of insider trading laws and the investment-to-price sensitivity

As mentioned in the introduction, the current literature has not reached a consensus on how insider trading regulation affects investment efficiency in general and the investment-to-price sensitivity in particular. In this section we discuss the arguments that predict a positive or a negative association between the enforcement and the investment-to-price sensitivity.

2.1. The positive association

The sensitivity of investment to stock prices is determined by two factors: the informativeness of stock prices about investment opportunities (Chen et al., 2007), and the adjustment costs driven by technological factors (Tobin, 1969) as well as market frictions due to

adverse selection and moral hazard (Wurgler, 2000). Insider trading regulation may increase the investment-to-price sensitivity by improving price informativeness and/or reducing market frictions. We detail these arguments below.

2.1.1. The managerial learning hypothesis

The managerial learning hypothesis predicts that insider trading regulation increases the investment-to-price sensitivity because it enables stock prices to provide more *new* information to managers to guide their investment decisions. The idea that managers can extract useful information from stock prices to help with making corporate decisions goes back to Hayek (1945) and is further developed in the recent finance literature. See, for example, Dow and Gorton (1997), Subrahmanyam and Titman (1999), Dow and Rahi (2003), Luo (2005), Foucault and Gehrig (2008), Dow, Goldstein, and Gümbel (2010), and Bond, Goldstein, and Gümbel (2011).[2] Consistent with this notion, Chen, Goldstein, and Jiang (2007) find that investment is more sensitive to stock prices when stock prices are more informative. Bakke and Whited (2010) find similar results after correcting for the measurement errors in the stock prices. Fishman and Hagerty (1992) point out that restricting insider trading increases the incentives of investors to acquire information.[3] The model by Khanna et al. (1994) also suggests that prohibiting insider

[2] The existence of the managerial learning effect does not require the market to have more information than managers. The assumption needed is that managers do not have perfect information about every decision-relevant factor, and therefore the market may possess some incremental information useful to them (Bond et al., 2011). This assumption can be true for several reasons. First, optimal decision making may depend not only on the internal information available to the firm, but also on the external information of which outside investors may know better (Luo, 2005; Bond et al., 2011). Second, corporate managers might be inefficient in collecting some information that exists within the scope of the firm (Rajan and Zingales, 2003; Gao and Liang 2011). Finally, while the impact of information from each individual speculator might be tiny, the market aggregates all information from a large population of speculators so that the aggregated impact can be significant (Subrahmanyam and Titman, 1999).

[3] On the one hand, restricting insider trading reduces the amount of information incorporated into stock prices through trades by insiders, which decreases price informativeness. On the other hand, the prohibition increases the incentives of outside investors to acquire information and trade, because the potential gains of doing so are higher. In addition, the restriction removes insiders' monopoly power of information so that information is more evenly distributed among traders, which results in a more competitive market and further increases the incentives to trade.

trading encourages outside investors to collect more information, which helps managers allocate their resources. Brunnermeier (2005) shows that information leakage through insider trading may reduce price informativeness in the long run. Consistent with this prediction, Bushman et al. (2005) find increased analyst activities after the enforcement. Fernandes and Ferreira (2009) find that stock prices become more informative in the developed countries after the enforcement. To the extent that stock prices provide more *new* information to managers after the enforcement, managers rely more on stock prices to guide resources allocation. Therefore, investment becomes more sensitive to stock prices after the enforcement.

2.1.2. The market frictions hypothesis

The market frictions hypothesis predicts that the enforcement reduces market frictions driven by adverse selection and/or moral hazard and therefore increases the investment-to-price sensitivity (Wurgler, 2000). First, insider trading regulation can directly mitigate moral hazard problems (i.e., agency problems) associated with insider trading. Manove (1989) and Ausubel (1990) suggest that insiders can expropriate outside investors by trading on foreknowledge about investment outcomes. Anticipating this, investors will distort stock investment to protect themselves. Bebchuk and Fershtman (1990) argue that managers may distort investment decisions in order to increase insider trading profits. Maug (2002) suggests that managers have incentives to give early warnings about adverse development to large shareholders when insider trading is allowed. This information sharing induces large shareholders to sell stocks and refrains them from intervening with managers' bad investment decisions.

Both forces increase price informativeness. Fishman and Hagerty (1992) show that under certain conditions, the second force dominates the first so that restricting insider trading results in a net increase in price informativeness.

Second, insider trading restrictions may increase price informativeness by mitigating adverse selection and/or moral hazard problems.[4] More informative stock prices reflect the consequences of managers' decisions more accurately. Therefore, investors' monitoring ability is higher because they can evaluate managers' performance more accurately and/or design more efficient incentive contracts (Dow and Gorton, 1997; Edmans, 2009; Ferreira, Ferreira, and Raposo, 2011). Faure-Grimaud and Gromb (2004) show that more informative stock prices motivate insiders to engage in value-enhancing activities. More informative stock prices also reduce information asymmetry between managers and outside investors and thus alleviate adverse selection problems in external financing (Myers and Majluf, 1984). Consistent with this notion, Bhattacharya and Daouk (2002) find that the enforcement decreases the country-level cost of equity and Fernandes and Ferreira (2009) find that the cost of equity is negatively associated with price informativeness. Sunder (2004) finds that more informative equity prices are associated with a lower cost of debt. In sum, the enforcement may relax financing constraints by alleviating adverse selection and/or moral hazard, leading to a higher investment-to-price sensitivity (Hubbard, 1998; Wurgler, 2000; Kusnadi, Titman, and Wei, 2010; McLean, Zhang, and Zhao, 2012).[5]

While the above two hypotheses represent different mechanisms through which the enforcement may affect the investment-to-price sensitivity, they are not mutually exclusive. For

[4] The managerial learning hypothesis requires that stock prices contain prospective information about investment opportunities that is *new* to managers, but the market frictions hypothesis does not. Adverse selection is reduced even if the prospective information contained in stock prices is not *new* to managers. Retrospective information about the consequences of managers' past decisions improves the monitoring function of the market (Dow and Gorton, 1997).

[5] The enforcement of insider trading laws may signal an overall increase in the level of enforcement of securities laws and property rights (Jayaraman, 2012, Djankov et al., 2008). The literature emphasizes the importance of the quality of the legal enforcement in investor protection (e.g., La Porta et al., 1998; Morck et al., 2000). Bhattacharya and Daouk (2009) argue that sometimes no law is better than a good law that is not enforced. This means that the enforcement may be associated with smaller market frictions because it signals better overall legal protection.

example, stock prices after the enforcement may contain more prospective information to guide managers' investment decisions as well as more retrospective information that could be used to monitor the managers (Dow and Gorton, 1997). In addition, reduced market frictions may also enhance the ability and incentives of the managers to respond to the information contained in stock prices (Chen, Goldstein, and Jiang, 2007; Kau, Linck, and Rubin, 2008; Jiang, Kim, and Pang, 2011).

Both the managerial learning hypothesis and the market frictions hypothesis suggest that the enforcement enhances investment efficiency and leads to improved firm operating performance. Moreover, the improvement in firm operating performance after the enforcement should be positively correlated with the increase in the investment-to-price sensitivity.

2.2. The negative association

The literature also provides arguments that the restriction of insider trading reduces the investment-to-price sensitivity by exaggerating market frictions. First, insider trading is an important channel for managers to transmit their private information to the market (Piotroski and Roulstone, 2004). Prohibiting insider trading reduces stock price efficiency and, in turn, increases market frictions. As pointed out by Leland (1992), insider trading resolves uncertainty and makes external financing easier. Bernhardt, Hollifield, and Hughson (1995) also suggest that insider trading improves investment decision efficiency by revealing their private information on productive opportunities in stock prices.[6] These studies do not consider the deterrence effect of insider trading (Fishman and Hagerty, 1992). However, removing the deterrence effect from insider trading regulation may not necessarily result in more informative stock prices, because

[6] Berndardt et al. (1995) also point out that anticipation of insider trading after investment is made can distort outside investors' equity investment, a conclusion similar to that of Ausubel (1990).

the information acquired by outside investors may not necessarily be enough to offset the loss of information from insider trading. Consistent with this notion, Fernandes and Ferreira (2009) find that there is no increase or even a decrease in price informativeness after the enforcement in the emerging markets. Haw et al. (2012) find that the enforcement is associated with stock prices that are less informative about future earnings.

Second, insider trading can be an efficient way to motivate managers to acquire and develop private information to guide their investment decisions (Manne, 1966; Carlton and Fischel, 1983). In addition, insider trading can motivate risk-averse managers to take risky projects that would benefit shareholders (Bebchuk and Fershtman, 1994). Consistent with this notion, Roulstone (2003) finds that firms that voluntarily adopt insider trading restrictions also make a greater use of incentive compensation. Thus, insider trading regulation may adversely affect the managers' incentives.

The above mixed theoretical arguments motivate the following empirical question: Does the initial enforcement of insider trading laws affect the investment-to-price sensitivity?

3. Research design

3.1. Data, sample selection process, and summary statistics

We collect all firm-year observations between 1982 and 2003 in 45 countries from Worldscope. Following the literature, we delete firms in financial industries (SIC codes between 6000 and 6999). We also delete firms with total assets or market value of equity below $10 million.[7] Finally, we delete observations with missing investment, Tobin's *Q*, and operating cash flow values (see the Appendix for detailed definitions). Our final sample consists of 175,968

[7] We repeat all tests excluding firms with total assets or market value of equity below $100 million. The results are qualitatively similar.

firm-year observations from 24,149 firms in 45 countries. Among them, 153,066 firm-year observations (19,713 firms) come from 23 developed markets or countries, and 22,902 firm-year observations (4,436 firms) come from 22 emerging markets or countries. Table 1 shows the sample distribution across countries, the year of the initial enforcement, and the first year in which the country adopted its insider trading laws.

[Insert Table 1 here]

Table 2 presents the summary statistics for the main variables used in this study. Panels A, B, and C show the summary statistics for the pooled sample, the pre-enforcement period, and the post-enforcement period, respectively. The mean (median) value of total investment (INVEST) of the pooled sample is 0.074 (0.042). There is no discernible difference in the magnitude of INVEST between the pre- and the post-enforcement periods. More specifically, the mean (median) value of INVEST is 0.075 (0.044) in the pre-enforcement period and 0.074 (0.042) in the post-enforcement period. As will be discussed in detail later, we adjust the investment variable in order to control the time trend in the investment-to-price and investment-to-cash flow sensitivities. The mean value of adjusted investment (Adj.INVEST) is slightly higher in the pre-enforcement period (0.023) than in the post-enforcement period (0.020). However, the median values show an opposite pattern (-0.002 versus 0.007). The average operating cash flow (CF) is slightly lower in the post-enforcement period than in the pre-enforcement period. Tobin's *Q* (Q) is higher in the post-enforcement period than in the pre-enforcement period, consistent with the findings of Bhattacharya and Daouk (2002) that the cost of equity is reduced after the enforcement.

[Insert Table 2 here]

3.2. Model specifications

We estimate the following regression to examine the association between the enforcement and the investment-to-price sensitivity:

$$INVEST_{c,f,t} = \mu_c + \mu_i + \mu_t + a_1 ITENF_{c,t\text{-}1} + b_1 Q_{c,f,t\text{-}1} + b_2 Q_{c,f,t\text{-}1} \times ITENF_{c,t\text{-}1} + b_3 Q_{c,f,t\text{-}1} \times PROTECT_c + c_1 CF_{c,f,t} + c_2 CF_{c,f,t} \times ITENF_{c,t\text{-}1} + \varepsilon_{c,f,t}, \quad (1)$$

where c, f, and t are indicators for country, firm, and year, respectively and μ_c, μ_i and μ_t are the fixed effects of country, industry, and year. We use 2-digit SIC codes to define industry. INVEST is defined as the sum of the change in net property, plant and equipment, the change in inventories, and R&D expenditures, scaled by lagged total assets (McLean et al, 2012). ITENF is a dummy variable that equals one for the years after (including the year of) the initial enforcement of insider trading laws, and zero otherwise.[8] Q is the natural logarithm of Tobin's Q, measured as the market value of equity minus the book value of equity plus the book value of total assets, scaled by the book value of total assets (Baker, Stein, and Wurgler, 2003; Foucault and Frésard, 2012). CF is operating cash flow, defined as net income before extraordinary items plus depreciation and amortization, scaled by lagged total assets.

Although the objective of this study is to examine the effect of the enforcement on the investment-to-price sensitivity, we allow the investment-to-cash flow sensitivity to vary across the pre- and post-enforcement periods. Investment is sensitive to operating cash flow because of financing constraints (Hubbard, 1988). The investment-to-cash flow sensitivity may also change after the enforcement because financing constraints are relaxed. Alternatively, the cash flow variable may contain information about investment opportunities (Alti, 2003). Therefore, Q and CF are two signals for investment opportunities. To the extent that the enforcement increases the informativeness of one signal, i.e., Q (Fernandes and Ferreira, 2009), the sensitivity of

[8] By using $ITENF_{c,t\text{-}1}$ rather than $ITENF_{c,t}$ in the regression, we implicitly assume that investment decisions are made at the beginning of each year.

investment to the other signal, i.e., CF, may also change. Restricting the coefficient of CF to be the same in the pre- and post-enforcement periods can potentially bias the estimate of the investment-to-price sensitivity (b_2).

The enforcement of insider trading laws is associated with other investor protection mechanisms and investor protection mechanisms are associated with the investment-to-price sensitivity (Wurgler, 2000; Kusnadi et al, 2010; McLean et al., 2012). We therefore control for the effect of investor protection mechanisms in the slope of Q. Prior studies show that four indices of investor protection are highly correlated with financial market development and the investment-to-price sensitivity: (1) the anti-director rights index (La Porta et al., 1998), (2) the anti-self-dealing index (Djankov et al., 2008), and (3) the disclosure requirement as well as (4) liability standard indices (La Porta et al., 2006). Note that none of these four indices directly measure the effects of insider trading regulation.[9] Since these four indices are highly correlated, including all them in one regression may create severe multicollinearity problems. Hence, we create a composite index, denoted as PROTECT, that aggregates the four indices.[10]

An important concern in estimating the effect of the insider trading laws enforcement is the confounding effect of the time trend in the investment-to-price sensitivity. That is, in the absence of the enforcement, there might be a time trend in the investment-to-price (β_t) and

[9] The disclosure requirement index and the liability standard index from La Porta et al. (2006) deal with regulations on the issuance of new equity, i.e., transactions in the primary markets. However, insider trading regulation mainly concerns transactions in the secondary markets. The anti-director rights index (La Porta et al., 1998; Spamann, 2010) mainly captures investor protection derived from corporate laws that govern the corporate decision making process. The anti-self-dealing index (Djankov et al., 2008) measures the hurdle that controlling shareholders must pass in order to get away from corporate self-dealing (or tunneling) transactions. Even if the four legal protection indexes mentioned above indirectly capture the effects of insider trading regulation, controlling for them in our regressions biases against finding any significant effect of the enforcement of insider trading laws.

[10] The anti-director rights index has a different scale (from 0 to 5) from the other three indices (from 0 to 1) and the disclosure requirement index and the liability standard index both capture legal protection in equity issuance. We therefore divide the anti-director rights index by 5 and divide the disclosure requirement index and the liability standard index by 2 before aggregating them. Using each individual index or the principal component obtained from the four indices does not change our conclusion qualitatively. Replacing the anti-director rights index by the revised index from Spamann (2010) does not qualitatively change our conclusions either.

investment-to-cash flow (η_t) sensitivities. In other words, in regression (1) $b_1=b_0+\beta_t$ and $c_1=c_0+\eta_t$. This trend in the investment-to-price sensitivity (β_t) could be increasing, possibly due to the improvement in financial market development and globalization unrelated to the enforcement. Failing to control for this trend may bias towards finding a positive estimate of b_2. Therefore, we make an explicit attempt to control for the potential trend effect. Inserting $b_1=b_0+\beta_t$ and $c_1=c_0+\eta_t$ into regression (1), simple algebra suggests that we can estimate the following regression to obtain an estimate of the effect of the enforcement on the trend-adjusted investment-to-price sensitivity:

$$\begin{aligned} Adj.INVEST_{c,f,t} = {} & \mu_c + \mu_i + \mu_t + a_1 ITENF_{c,t\text{-}1} + b_0 Q_{c,f,t\text{-}1} + b_2 Q_{c,f,t\text{-}1} \times ITENF_{c,t\text{-}1} + \\ & b_3 Q_{c,f,t\text{-}1} \times PROTECT_c + c_0 CF_{c,f,t} + c_2 CF_{c,f,t} \times ITENF_{c,t\text{-}1} + \varepsilon_{c,f,t}, \end{aligned} \tag{2}$$

where $\text{Adj.INVEST}_{c,f,t} = \text{INVEST}_{c,f,t} - \beta_t \text{Q}_{c,f,t\text{-}1} - \eta_t \text{CF}_{c,f,t}$. Following prior literature (Bushman et al., 2005; Fernandes and Ferreira, 2009), we use the data for the six countries (Brazil, Canada, France, Singapore, the U.K. and the U.S.) whose initial enforcement of insider trading laws occurred before 1982 to estimate the time trend in the investment-to-price (β_t) and investment-to-cash flow (η_t) sensitivities. Specifically, in each year, we estimate the following cross-sectional regression using the observations from the six countries:

$$INVEST_{c,f,t} = \beta_t Q_{c,f,t\text{-}1} + \eta_t CF_{c,f,t\text{-}1} + \mu_c + \mu_i + \varepsilon_{c,f,t}. \tag{3}$$

Effectively, the estimate of β_t obtained from regression (3) captures $b_0+b_2+b_3\times$Avg.PROTECT, where Avg.PROTECT is the average value of *PROTECT* from the six countries. As a result, the estimate of b_0 in regression (2) effectively captures $-b_2-b_3\times$Avg.PROTECT, which is the difference in the investment-to-price sensitivity driven by both the enforcement and the difference in the average value of the PROTECT index between the

remaining 39 countries and the six countries used to estimate the trend. However, the interpretation of the estimate of b_2 in regression (2) should not be affected.

4. The relation between the initial enforcement of insider trading laws and the investment-to-price sensitivity

4.1. Pooled sample regressions

Table 3 presents the results of the pooled sample regressions. The result in Column (1) does not include PROTECT. We find a significant and positive coefficient of Q×ITENF (coeff. = 0.073; t = 19.88). The coefficient of Q is also significantly positive (coeff. = 0.013; t = 3.72). Column (2) shows the results of regression model (1). The coefficient of *Q×ITENF* is positive (0.050) and highly significant (t = 19.22). This suggests that corporate investment is more sensitive to stock prices after the enforcement. Consistent with McLean et al. (2012), the coefficient of Q×PROTECT is also significantly positive (coeff. = 0.041; t = 19.22). Investment is positively associated with operating cash flow in the pre-enforcement period; the coefficient of CF is positive and highly significant (coeff. = 0.576; t = 24.16). However, the sensitivity is decreased after the enforcement, as the significantly negative coefficient of CF×ITENF shows (coeff. = -0.446; t = -18.03).[11]

Column (3) includes the effect of the existence of insider trading laws (ITEXIST) by adding ITEXIST, Q×ITEXIST, and CF×ITEXIST to the regression. ITEXIST is a dummy variable that equals one for the years after the initial adoption of insider trading laws, and zero

[11] After including the interaction term Q×PROTECT, the coefficient of Q (coeff. = -0.059; t = -11.61) should not be interpreted as the average investment-to-price sensitivity in the pre-enforcement period. To obtain an estimate of the average investment-to-price sensitivity in the pre-enforcement period, we should also consider the mean value of PROTECT. The mean of PROTECT in the pre-enforcement period is 1.65 (untabulated), suggesting an average investment-to-price sensitivity of 0.008 (0.041×1.65-0.059; p-value = 0.012) in the pre-enforcement period, which is still positive albeit lower than the corresponding number in Column (1).

otherwise. This specification aims at examining whether it is the existence or the enforcement of insider trading laws that affects managers' investment behavior. Consistent with prior literature that insider trading laws are not effective until they are enforced, the coefficient of Q×ITEXIST is insignificant (coeff. = -0.005; t = -0.70).

Columns (4) and (5) repeat the analyses in Columns (2) and (3) using adjusted investment (Adj.INVEST) as the dependent variable. The sample size is reduced significantly (from 175,968 to 84,365) because we exclude the observations in the six countries used to estimate the time trend in the investment-to-price and investment-to-cash flow sensitivities. We find qualitatively similar results. In particular, in Column (4) the coefficient of Q×ITENF is still positive and significant at the 1% level (t = 4.56), although the magnitude of the coefficient (0.017) is smaller than that of the coefficient (0.050) in Column (2). In Column (5), the coefficient of Q×ITENF continues to be significantly positive (coeff. = 0.021; t = 4.80) after controlling for the effect of the existence of insider trading laws. Overall, the evidence suggests that the initial enforcement of insider trading laws increases the investment-to-price sensitivity even after controlling for the time trend.[12]

[Insert Table 3 here]

4.2. Robustness tests

[12] We also observe that the coefficients of Q in both Columns (4) and (5) are negative, and are even more negative than those in Columns (2) and (3). As explained in Section 3, the coefficients of Q do not measure the absolute investment-to-price sensitivity in countries before the enforcement. They capture both the difference in the investment-to-price sensitivity driven by the enforcement and that driven by the variation in PROTECT between the remaining 39 countries and the six countries used to estimate the time trend of the investment-to-price sensitivity. Given that the investor protection level is larger in the six countries (mean PROTECT = 2.54, untabulated) than in the remaining countries (mean PROTECT = 1.77, untabulated), it is not surprising to find a more negative coefficient. The coefficient of Q×PROTECT becomes insignificant in all three regressions after excluding the six countries and using adjusted investment as the dependent variable. However, this is not necessarily inconsistent with Kusnadi et al. (2010) and McLean et al. (2012). This might suggest that the effect of investor protection on the investment-to-price sensitivity is mainly driven by the difference between the six countries and the remaining 39 ones, rather than the variations within the remaining 39 countries.

We conduct a series of sensitivity tests to check the robustness of the baseline regressions. The results are reported in Table 4. Panel A shows the results of alternative model specifications and sample selections. The first five columns use INVEST as the dependent variable and the remaining five columns use Adj.INVEST. Columns (1) and (6) estimate the Fama and MacBeth (1973) regression. The result is similar when INVEST is the dependent variable (Column (1)) but weaker when Adj.INVEST is the dependent variable (Column (6)) compared to baseline results reported in Table 3. Columns (2) and (7) estimate the firm-fixed effects regressions and the results are qualitatively similar. We also test whether the results are sensitive to exclusion of the Asian financial crisis period (1997-1998) as this period is unusual and many countries began enforcement in 1995 and 1996 anyway. The results are reported in Columns (3) and (8). We find that the coefficients of Q×ITENF are actually slightly larger when we exclude these two years. Columns (4) and (9) exclude the observations from the U.S., the U.K., Japan, Canada, France, and Germany as these countries account for a disproportionately large fraction of firm-year observations. While the sample size is reduced substantially, the coefficients of Q×ITENF continue to be positive and significant at the 1% level. Columns (5) and (10) cluster the standard errors by country and the results are again qualitatively similar.

[Insert Table 4 here]

Panel B reports the results using alternative measures of investment. Columns (1) to (4) use raw investment as the dependent variable and Columns (5) to (8) use trend-adjusted investment. We find that the results are robust regardless of whether we consider investment in property, plant and equipment (PPE) (Columns (1) and (5)), or investment in property, plant and equipment, and R&D expenditures (PPE and R&D) (Columns (2) and (6)). We obtain similar

results when we scale investment (INVEST) by current total assets (Columns (3) and (7)), and by lagged property, plant and equipment (Columns (4) and (8)).

4.3. Changes in the investment-to-price sensitivity around the initial enforcement year

One limitation of the above pooled sample analysis is that the sample includes data in the periods far after the initial enforcement and allows other confounding factors to take effect. To mitigate this concern, we examine the change in the investment-to-price sensitivity in a relatively short window around the initial enforcement year. If we observe a significant increase in the investment-to-price sensitivity coincident with the initial enforcement year, we are more confident that the increase in the investment-to-price sensitivity is attributed to the initial enforcement of insider trading laws.

Toward this end, we restrict the sample to include only those observations from year -2 to year +3, where year 0 is the initial enforcement year. We further require that each country has at least one firm-year observation in both the pre-enforcement period (i.e., years [-2, 0]) and the post-enforcement period (i.e., years [+1,+3]).[13] The selection procedure reduces the sample size to 19,293 firm-year observations (5,023 firms) from 24 countries. Table 5 presents the results. Column (1) uses raw investment (INVEST) as the dependent variable. The coefficient of Q is insignificant (coeff. = 0.002; t = 0.15) and the coefficient of Q×PROTECT is also insignificant (coeff. = -0.006; t = -0.94), suggesting that in the three years before the enforcement, corporate investment does not respond to stock prices. The coefficient of Q×ITENF is significant (coeff. = 0.043; t = 6.09), suggesting that managers rely more on information in stock prices to make their investment decisions after the enforcement. The magnitude of coefficient (0.043) is also

[13] Note that investment in year 0 is associated with the stock prices at the year of year -1.

comparable to that reported in Column (2) of Table 3. Column (2) of Table 5 uses trend-adjusted investment (Adj.INVEST) as the dependent variable and finds similar results. In particular, the coefficient of Q×ITENF has a similar magnitude (0.039) and significance level (t = 5.67) to those in Column (1). The results regarding the change in the investment-to-cash flow sensitivity are mixed. The coefficient of CF×ITENF is significantly negative in Column (1) (coeff. = -0.168; t = -3.56) but insignificant in Column (2) (coeff. = -0.061; t = -1.30).

[Insert Table 5 here]

To further investigate the timing of the change in the investment-to-price sensitivity, we replace the dummy variable ITENF by a series of indicators for each event year. Specifically, we estimate the following regression model:

$$INVEST_{c,f,t}(Adj.INVEST_{c,f,t}) = \sum_{t=-2}^{+3} a_t YEAR_{c,f,t} + bQ_{c,f,t-1} + \eta Q_{c,f,t-1} \times PROTECT_c + \sum_{t=-1}^{+3} b_t YEAR_{c,f,t} \times Q_{c,f,t-1} + cCF_{c,f,t-1} + \sum_{t=-1}^{+3} c_t YEAR_{c,f,t} \times CF_{c,f,t} + \mu_c + \mu_i + \mu_t + \varepsilon_{c,f,t} \quad (4)$$

where $YEAR_{c,f,t}$ (t = -1 to +3) is a dummy variable that equals one for firm f in country c in year t relative to the initial enforcement year, and zero otherwise. The investment-to-price sensitivity in year -2 is measured by coefficient b and serves as the benchmark. The changes in the investment-to-price sensitivity over years -1 to +3 relative to year -2 are captured by b_t ($t \in$[-1,+3]; b_{-2} is set to 0). The results are plotted in Figure 1. Panel A plots the estimates of b_t ($t \in$[-1,+3]) using INVEST as the dependent variable (i.e., the *raw* investment-to-price sensitivity). Panel B plots the estimates of b_t ($t \in$[-1,+3]) using Adj.INVEST as the dependent variable (i.e., the *adjusted* investment-to-price sensitivity). The patterns are similar so we only focus on Panel B. Compared with year -2, there is no significant change in the trend-adjusted investment-to-price sensitivity in year -1 (b_{-1}) (p-value = 0.986, unreported) or year 0 (b_0) (p-value = 0.493,

unreported). However, we observe a significant jump in the trend-adjusted investment-to-price sensitivity in year +1 (b_1). The unreported test shows that the difference between b_0 and b_1 is significant (p-value = 0.0002). In years +2 and +3, the trend-adjusted investment-to-price sensitivity continues to increase, albeit at a lower rate than in year +1. Further tests (unreported) show that the difference between b_2 and b_1 is not significant (p-value = 0.897), neither is the difference between b_3 and b_2 (p-value = 0.345).

[Insert Figure 1 here]

While different countries initiated their enforcement in different years, many did so within a relatively short period (from 1993 to 1996). As a result, the event-time analysis in Table 5 and Figure 1 may result in erroneous inferences if there is a large time-series variation in the investment-to-price sensitivity and our trend adjustments do not adequately control for this variation. To address this concern, we also benchmark the coefficient estimates in Table 5 and Figure 1 against the historical time-series variations in the corresponding coefficients and the inference is qualitatively similar.[14]

In sum, the results in Table 5 and Figure 1 show that the significant increase in the investment-to-price sensitivity occurs immediately after the enforcement. This further increases

[14] Specifically, for each country whose initial enforcement occurred after 1982 or did not enforce its insider trading laws until 2003 (39 countries), we randomly draw a year t ($t \notin$[T-3, T+3], where T is the actual initial enforcement year) as a pseudo enforcement year (t could be different for different countries). Thus, we create a set of pseudo enforcement year data for the 39 countries. We then conduct a pseudo-event-window analysis using the data between years t-2 and t+3. In particular, we repeat the regression in Table 5 and Figure 1 using adjusted investment as the dependent variable. The period from year t-2 to year t is defined as the pre-pseudo-enforcement period, and the period from year t+1 to year t+3 is defined as the post-pseudo-enforcement period. We require that $t \notin$[T-3, T+3] so that the sample period used in the pseudo-event-window analysis does not overlap with the actual initial enforcement year. We conduct the random sampling and the pseudo-event-window test 1,000 times. We then benchmark the coefficient estimate of Q×ITENF in Table 5 and the coefficient estimate of $Q \times YEAR_1 – Q \times YEAR_0$ in Figure 1 against the corresponding empirical distribution obtained from the 1,000 pseudo-event-window analyses. Our conclusion is not qualitatively changed. In particular, the 95% confidence interval (i.e., 2.5th and 97.5th percentiles of the distribution) for the coefficient estimate of Q×ITENF is [-0.027, 0.037] and that for $Q \times YEAR_1 – Q \times YEAR_0$ is [-0.035, 0.039]. The coefficient estimate of Q×ITENF in Column (2) of Table 5 is 0.039, and the coefficient estimate of $Q \times YEAR_1 – Q \times YEAR_0$ in Panel B of Figure 1 is 0.042 (0.034+0.008). Both fall outside the corresponding 95% confidence interval.

our confidence that the increase in the investment-to-price sensitivity is attributable to the initial enforcement of insider trading laws.

4.4. Country-level analysis

Since the literature has been debating about whether the analysis should be conducted at the country level or the firm level (Holderness, 2008), to further check the robustness of our results, we also conduct a country-level analysis. The country-level tests are performed in a two-step regression analysis. In the first step, for each country and each year with at least 50 firm observations, we regress the investment variable on Q and CF, controlling for the industry-fixed effects. We require at least 50 observations in order to maintain a reasonable degree of freedom so that the country-year estimates of the investment-to-price sensitivity would not be too noisy. [15] To obtain a meaningful degree of freedom, we replace the 2-digit SIC codes with the 12 industry classifications from Kenneth French's website to control for the industry-fixed effects. In the second stage, we regress the country-year specific estimates of the investment-to-price sensitivity on ITENF. Table 6 shows the results. The raw (trend-adjusted) investment-to-price sensitivity is obtained by using INVEST (Adj.INVEST) in the first-stage regressions. Columns (1) and (2) include all country-year observations with at least 50 firms. Columns (3) and (4) exclude the six countries used to estimate the time trend in the investment-to-price sensitivity. We find that the results are consistent with those of the firm-level analyses (reported in Table 3). In particular, the coefficients of ITENF are all significant at the 1% level except Column (4) (t = 1.82) which is significant at the 5% level. The coefficients of Q×ITENF are slightly smaller but are comparable

[15] Alternatively, we do not impose any requirement on the number of observations in each country-year regression, but estimate a weighted least squares (WLS) regression in the second stage, where the weight is the inverse of the standard error of the country-year specific estimates of the investment-to-price sensitivity. The results are qualitatively similar.

to those in the corresponding firm-level analysis. Columns (5) to (8) restrict the sample to the event window (years -2 to +3) around the initial enforcement year. The results are also qualitatively similar, although when country-fixed effects are included (Columns (6) and (8)) the coefficients of ITENF are no longer significant (t = 1.63 and 1.52, respectively). This could be due to the much smaller sample size (reduced from 474 or 362 to 102).

[Insert Table 6 here]

4.5. The economic significance of the effect of the enforcement

The magnitude of the coefficient of Q×ITENF is 0.050 in Column (2) of Table 3, which suggests that after the enforcement, a one-standard-deviation increase in Q (0.527) is associated with an increase in INVEST by 0.026 (0.527×0.050). This magnitude is economically significant as it accounts for 16% of the standard deviation of INVEST in the pooled sample (0.026/0.165). Alternatively, one could use the effect of PROTECT on the investment-to-price sensitivity as a benchmark. The results suggest that the magnitude of the effect of the enforcement is at least 40% (0.050/(0.041×3)) of that of the effect of PROTECT.[16]

The above analysis is likely to overstate the effect of the enforcement because the regression does not control for the time trend in the investment-to-price sensitivity. The results in Column (4) of Table 3 are obtained after controlling for the time trend. The coefficient estimate of Q×ITENF is 0.017, suggesting that a one-standard-deviation change in Q (0.527) explains 6% of the standard deviation of Adj.INVEST (0.527×0.017/0.149) after the enforcement. To analyze the effect of the enforcement relative to that of PROTECT, recall that the coefficient of Q in Column (4) captures the effect of the insider trading law enforcement of the six countries (Brazil, Canada, France, Singapore, the U.K., and the U.S.) as well as the effect of PROTECT of the six

[16] Note that by construction, the maximum variation in PROTECT in our sample is 3.

countries. Assuming that the effects of the enforcement are the same between the six countries and the remaining 39 countries, the difference in the investment-to-price sensitivity driven by the difference in PROTECT between the six countries and the remaining 39 countries is 0.056 (0.073-0.017). The enforcement increases the investment-to-price sensitivity by 30% (0.017/0.056) against this benchmark.[17] This also suggests that the magnitude of the enforcement effect on investment efficiency is economically significant. Parallel analysis using the coefficient estimates in Tables 5 and 6 allows us to draw qualitatively similar conclusions.

Thus far, we have established a robust association between the enforcement of insider trading laws and the investment-to-price sensitivity. In the next section, we attempt to understand the mechanisms through which the enforcement increases the investment-to-price sensitivity.[18]

5. The mechanisms through which the enforcement increases the investment-to-price sensitivity

As discussed in Section 2, there are two potential mechanisms through which the enforcement increases the investment-to-price sensitivity. First, the enforcement encourages outside investors to acquire and trade on *private* information and, in turn, managers learn more from the stock prices to guide their investment decisions (the managerial learning hypothesis). Second, the enforcement decreases market frictions due to adverse selection and/or moral hazard. As a result, the investment-to-price sensitivity increases because financing constraints are

[17] Most of the observations used to estimate the time trend in the investment-to-price sensitivity are from the developed markets (except Brazil). Untabulated results show that the effect of the enforcement is stronger in the developed markets than in the developing markets. Therefore, assuming that the enforcement has the homogenous effect around the world understates its economic significance because the denominator is overstated.

[18] There is also some evidence suggesting a decrease in the investment-to-cash flow sensitivity after the enforcement. However, the results are sensitive to model specifications. In addition, in contrast to a significant jump in the investment-to-price sensitivity immediately after the enforcement, we do not find an immediate fall in the investment-to-cash flow sensitivity. We leave the investigation of the effect of the enforcement on the investment-to-cash flow sensitivity for future research.

relaxed and managers have more incentives to make value-maximizing investment decisions (the market frictions hypothesis). While these two mechanisms are not mutually exclusive, it is interesting to distinguish between them. In this section we attempt to examine to what extent the effect of the enforcement can be explained by each hypothesis.

5.1. The managerial learning hypothesis: Cross-sectional evidence

One additional prediction by the managerial learning hypothesis is that the effect of the enforcement increases if stock prices incorporate investors' exclusive *private* information about firms' investment opportunities. However, we are not able to directly measure the amount of this exclusive private information, i.e., the information that is *new* to managers. We therefore take a step back and test whether the effect of the enforcement is associated with proxies for the amount of *private* information contained in stock prices. One underlying assumption is that when the stock prices contain more private information, they are likely to contain more information *new* to managers, and therefore managers are more likely to learn from the stock prices. Chen et al. (2007) and Foucault and Frésard (2012) take a similar approach.

We construct two proxies for investors' private information. The first proxy is price non-synchronicity, denoted as NSYNCH. A detailed definition is given in the Appendix. NSYNCH measures the relative idiosyncratic volatility of stock prices. Stock prices are likely to contain more private information when there are more informed trades based on firm-specific information, which leads to higher relative idiosyncratic volatility (Morck et al., 2000). Durnev et al. (2003) show that NSYNCH is positively associated with the ability of stock prices to predict future earnings. French and Roll (1986) and Roll (1988) suggest that a high NSYNCH is most likely driven by trades based on investors' private information. Durnev et al. (2004) and

Chen et al. (2007) suggest that a high NSYNCH is associated with more efficient investment decisions.

Our second proxy is the information-based trading measure suggested by Llorente, Michaely, Saar, and Wang. (2002), denoted as LMSW. The Appendix gives a detailed definition. LMSW measures the effect of trading volume on autocorrelation of individual stock prices. Assume that investors trade to share risks or to speculate on private information. Llorente et al. (2002) develop a model showing that returns generated by risk-sharing trades tend to reverse themselves, while returns generated by speculative trades tend to continue themselves. Thus, the larger the effect of trading volume, or the higher the value of LMSW, the more likely it is that the trades come from speculations based on private information. We align NSYNCH and LMSW measured for year t-1 with Q measured at the end of year t-1. [19]

Like Foucault and Frésard (2012), we interact ITENF and Q×ITENF with the proxy for private information contained in stock prices (PROXY = NSYNCH or LMSW) in our pooled sample regression. The managerial learning hypothesis predicts a positive coefficient of Q×ITENF×PROXY. To ease presentation, we only report the results of regressions using Adj.INVEST. Using INVEST gives qualitatively similar results. Columns (1) and (2) of Table 7 show the results. Consistent with the managerial learning hypothesis, the coefficient of Q×ITENF×NSYNCH is significantly positive (coeff. = 0.006; t = 5.17). The coefficient of Q×ITENF×LMSW is also positive (coeff. = 0.003), although not significant (t = 1.56).

[Insert Table 7 here]

The pooled sample regressions implicitly use the average value of PROXY of all firms in the pre-enforcement period as the benchmark to gauge the change in PROXY for each firm after

[19] Note that we use PROXY to measure the quality of Q. Therefore, we measure PROXY in the same period in which Q is measured.

the enforcement. In the event-window analysis, we replace PROXY with the *change* in PROXY in an attempt to conduct a more precise analysis. The change in PROXY for each firm is measured as the mean value of PROXY in the 3-year period after the enforcement (i.e., years [0,+2]) minus the mean value in the 3-year period before the enforcement (i.e., years [-3,-1]). A firm must have at least one observation in both the pre-enforcement and post-enforcement periods in order for us to compute the change in PROXY. Columns (4) and (5) in Table 7 report the results. The results are similar to those obtained from the pooled sample analysis. Specifically, the coefficient of Q×ITENF×ΔNSYNCH is significantly positive (coeff. = 0.008; *t* = 2.15), and so is the coefficient of Q×ITENF×ΔLMSW (coeff. = 0.017; *t* = 2.01). The difference-in-differences tests provide further support for the managerial learning hypothesis.

The above interpretation is based on the assumption that NSYNCH and LMSW capture investors' private information contained in stock prices. Admittedly, NSYNCH can be driven by public firm-specific information. Jin and Myers (2006) and Hutton, Marcus, and Tehranian (2009) find a positive association between transparency and NSYNCH. If NSYNCH captures public firm-specific information (e.g., public disclosure), the results in Columns (1) and (6) of Table 7 actually contradict the prediction by the managerial learning hypothesis. Because public information crowds out private information in stock prices, the more information managers disclose to the market, the less likely they can glean new information from stock prices (Gao and Liang, 2011).[20] Since the existing studies find that public information quality is also improved after the enforcement (Bushman et al., 2005; Jayaraman, 2012), we examine how the effect of the enforcement is associated with proxies for public information quality.

[20] In this case, the increased investment-to-price sensitivity might be because investors can better predict the investment decisions already made by managers (e.g., Foucault and Frésard, 2012). However, the results regarding the moderating effect of the three proxies of public information analyzed below reject this argument.

We construct three proxies for the quality of public information. The first proxy is the absolute value of discretionary accruals (ABSDAC). The second proxy is earnings opacity (OPACITY), as developed by Bhattacharya et al. (2003). Low values of these two variables indicate high quality financial reporting and thus high quality public information. Prior studies have shown that high quality disclosure and financial reporting are associated with less informed trading based on private information (Brown and Hillegist, 2007; Jayaraman, 2007; Maffett, 2012). The third proxy is the number of sell-side analysts following a firm (NAF). The information produced by sell-side analysts (i.e., earnings forecast reports) is typically public information. Prior literature has shown that a higher analyst coverage is associated with a lower probability of informed trading (Easley et al., 1998). Chan and Hameed (2006) find that analyst coverage is negatively associated with the amount of firm-specific information incorporated into stock prices, suggesting that analysts are primarily producing and disseminating market-wide and industry-wide information. The detailed definitions of ABSDAC, OPACITY, and NAF are shown in the Appendix.

Similarly, we align the three measures of public information in year t-1 with Q measured at the end of year t-1. As high quality public information reduces the extent to which managers can learn from stock prices, the managerial learning hypothesis predicts a positive moderating effect of ABSDAC and OPACITY (and ΔABSDAC and ΔOPACITY), and a negative moderating effect of NAF and ΔNAF.[21] Column (3) of Table 7 shows that the coefficient of Q×ITENF×ABSDAC is positive, albeit insignificant, in the pooled sample regression (coeff. = 0.024; t = 0.93). The coefficient of Q×ITENF×ΔABSDAC in Column (8) is also insignificantly

[21] High quality public disclosures of scheduled announcements (e.g., earnings announcements) may trigger more private search (Demski and Feltham, 1994). However, this argument does not apply to our setting because disclosures of investment are usually not scheduled in advance.

positive in the event-window regression (coeff. = 0.073; t = 0.66). Columns (4) and (9) use OPACITY and ΔOPACITY respectively as the conditioning variable. Column (4) shows that the coefficient of Q×ITENF×OPACITY is positive (coeff. = 0.002), although insignificant (t = 1.56). Column (9) finds a significant and positive coefficient of Q×ITENF×ΔOPACITY (coeff. = 0.018; t = 4.98).[22] Columns (5) and (10) use NAF and ΔNAF respectively as the moderating variable. The coefficient of Q×ITENF×NAF in Column (5) is negative and significant (coeff. = -0.001; t = -2.57). The event-window analysis reported in Column (10) finds a negative and significant coefficient of Q×ITENF×ΔNAF (coeff. = -0.002; t = -2.24). We conclude that the above results in general provide moderate support for the managerial learning hypothesis.

The next question is to what extent are the above results also consistent with the market frictions hypothesis? Both private information and public information in stock prices can facilitate the monitoring function of the market. The effect of private information on the cost of external financing is less clear. On the one hand, private information increases the precision with which stock prices reflect fundamentals. This results in a lower cost of external financing because it reduces the uncertainty and investors' information disadvantage against managers (Myers and Majluf, 1984). On the other hand, private information reduces liquidity by exaggerating adverse selection in the secondary market, which, in turn, increases the cost of capital in the primary market (Diamond and Verrecchia, 1991; Easley and O'Hara, 2004). However, public information unambiguously reduces the cost of external financing (e.g., Bhattacharya et al., 2003; Lee and Masulis, 2009). Therefore, while the results from NSYNCH

[22] Since OPACITY is measured at the country-year level, analysis at firm-level may not be appropriate. We repeat the analysis using the country-year specific investment-to-price sensitivity. In particular, for the pooled sample analysis, we regress the country-year estimate of the adjusted investment-to-price sensitivity on ITENF and an interaction term, ITENF×OPACITY, controlling for the country-fixed effects. The coefficient of ITENF×OPACITY is -0.002 (t = -0.76). For the event-window analysis, we regress the country-year estimate of the adjusted investment-to-price sensitivity on ITENF and an interaction term, ITENF×ΔOPACITY, controlling for the country-fixed effects. The coefficient of ITENF×ΔOPACITY is 0.021 (t = 1.64).

and LMSW may be potentially consistent with the market frictions hypothesis, the results from ABSDAC, OPACITY, and NAF are clearly not.

5.2. The market frictions hypothesis: Cross-sectional evidence

To further distinguish between the managerial learning hypothesis and the market frictions hypothesis, we examine several additional predictions by the market frictions hypothesis. First, if the increased investment-to-price sensitivity is due to reduced agency problems (i.e., moral hazard), we should find a more pronounced effect of the enforcement in firms with more severe agency problems. Second, if the enforcement increases the investment-to-price sensitivity by reducing ex ante adverse selection problems in external financing, we should find a stronger effect of the enforcement for firms that are more financially constrained.

Towards this end, we first partition the sample into two groups based on the wedge (WEDGE) between the control rights and the cash flow rights of the controlling shareholder. The data are obtained from Claessens et al. (2000) and Faccio and Lang (2002).[23] The first group includes all firms with WEDGE less than or equal to zero, and the second group contains all firms with positive WEDGE. Firms without controlling shareholders are deleted. The existing literature suggests that the second group of firms have higher incentives to expropriate minority shareholders and hence have more severe agency problems. That is, if the increase in the investment-to-price sensitivity after the enforcement is because the enforcement mitigates agency problems, we should find a stronger coefficient of Q×ITENF in the second group.

Panel A of Table 8 presents the results. The coefficients of Q×ITENF are positive and significant for firms with non-positive WEDGE (coeff. = 0.039 t = 2.66 in the pooled sample

[23] We assume that WEDGE does not change over our sample period.

regression; coeff. = 0.103, t = 3.82 in the event-window regression). However, the coefficients of Q×ITENF are insignificant for the firms with positive WEDGE, in both the pooled sample and the event-window analysis. The evidence is inconsistent with the notion that the enforcement increases the investment-to-price sensitivity by mitigating agency problems.[24] In contrast, the evidence might be consistent with the managerial learning hypothesis. The existing literature suggests that insiders whose interests are more aligned with those of outside investors are more willing to respond to the information contained in stock prices (Kau et al., 2008; Jiang et al., 2011).

[Insert Table 8 here]

Second, we partition the sample based on an ex ante measure of financing constraints. We measure financing constraints by the Whited and Wu (2006) index (WW-index). A low value of the WW-index suggests that firms are less financially constrained. In each country and each year, we partition the sample into four sub-groups based on the WW-index. If the increase in the investment-to-price sensitivity is due to the mitigated adverse selection problems in

[24] Another potential way to test the moral hazard hypothesis is to examine the association between other country-level legal protection mechanisms (e.g., PROTECT, which captures the legal protection in security issuance and against anti-self-dealing) and the effect of the enforcement. However, the prediction is not very clear. If insider trading regulation and other legal protection mechanisms are substitutes, the effect of the enforcement should be more pronounced in the countries with weaker investor protection. If insider trading restriction and other protection mechanisms are complementary, the opposite is predicted. If the enforcement represents an increase in the general quality of legal enforcement, the effect of the enforcement should be stronger in countries with better investor protection. For these reasons, we do not rely on this test to distinguish between the moral hazard hypothesis and the managerial learning hypothesis. Empirically, we find mixed results. In the pooled regression, we find a significantly positive association between PROTECT and the effect of the enforcement when we use INVEST as the dependent variable. However, the association becomes insignificant when we use Adj.INVEST as the dependent variable, and becomes negative in the event-time regression analysis. One possible reason might be that the effectiveness of the legal rules included in PROTECT depends on the quality of legal enforcement (La Porta et al., 1998; Morck et al., 2000), and in the pooled regressions of Adj.INVEST and the event-time regressions we exclude the countries with higher quality legal enforcement. Further analysis suggests that the negative association found in the event-time analysis is mainly driven by the emerging markets, which tend to have lower legal enforcement quality. We also replace PROTECT by an effective protection index, defined as the product of rule of law and PROTECT (Wurgler, 2000). We find that in the pooled regressions the effect of the enforcement is positively associated with the effective protection index regardless of whether we use INVEST or Adj.INVEST as the dependent variable. In the event-time regressions, however, we do not find a significant association between the effective protection index and the effect of the enforcement.

external financing, the coefficient of Q×ITENF should increase from the group of firms with the lowest value of the WW-index (least financially constrained) to the group of firms with the highest value of the WW-index (most financially constrained).

Panel B of Table 8 reports the results. The four columns on the left show the results of the pooled sample regressions. The coefficients of Q×ITENF are all significant and positive except for the firms with the highest values of the WW-index. The magnitudes of the coefficients also appear to be monotonically decreasing along the quartiles sorted by the WW-index, although the differences among the first three groups are not statistically significant. The results of the event-window analysis are similar. The coefficient of Q×ITENF is insignificant and even negative for the most financially constrained firms. All the other three less financially constrained groups find significant and positive coefficients of Q×ITENF. Thus, the evidence is not consistent with the notion that the enforcement increases the investment-to-price sensitivity because it alleviates financing constraints. Again, the evidence is potentially consistent with the managerial learning hypothesis in that less financially constrained firms are able to respond to information contained in stock prices more easily (Chen et al., 2007).[25]

Finally, we link the effect of the enforcement to the actual external financing activities. If the enforcement relaxes financing constraints by mitigating adverse selection and/or moral hazard problems, firms with better investment opportunities are more likely to raise external funds and invest more. Therefore, the increase in the investment-to-price sensitivity should be concentrated in the firms that raise external funds. However, learning only needs information transmission but not capital flows into the firms (Bond et al., 2011). Therefore, the managerial

[25] We also use the KZ-index (Kaplan and Zingales, 1997). The coefficients of Q×ITENF are insignificant for firms with the lowest and the highest values of the KZ-index, and are significantly positive for the middle two groups. The bottom line is that the effect of the enforcement does not increase with the KZ-index.

learning hypothesis predicts an increase in the investment-to-price sensitivity even in the absence of actual external fund raising.

Panel C of Table 8 shows the results. We consider equity issuance (Equity_ISSUE), debt issuance (Debt_ISSUE), and total issuance (Total_ISSUE) in the tests. The detailed definitions of these variables are outlined in the Appendix. Columns (1) to (3) estimate the pooled sample regressions. Column (1) finds a positive and significant coefficient of Q×ITENF×(Equity_ISSUE>0) (coeff. = 0.011; t = 3.62), suggesting a more pronounced effect of the enforcement in firms that raise external equity funds after the enforcement. The evidence seems to be consistent with the market frictions hypothesis. Column (2) examines the moderating effect of debt issuance and finds an insignificant coefficient of Q×ITENF×(Debt_ISSUE>0), inconsistent with the market frictions hypothesis. Firms may raise external equity or debt capital for the purpose of adjusting their capital structure but not investment. We therefore consider total external financing. Column (3) finds that the coefficient of Q×ITENF×(Total_ISSUE>0) is positive and significant at the 10% level (coeff. = 0.005; t = 1.64), weakly supporting the market frictions hypothesis.

Columns (4) to (6) conduct the event-window analysis and link the change in the investment-to-price sensitivity to the change in external funds raised between the post- and pre-enforcement periods. ΔEquity_ISSUE is defined as the mean amount of external equity funds raised in years [+1,+3] minus the mean equity funds raised in years [-2,0]. ΔDebt_ISSUE and ΔTotal_ISSUE are defined similarly. The results indicate that the effect of the enforcement is not associated with the change in external financing, regardless how we measure it. We therefore view that the results at most only weakly support the market frictions hypothesis. The coefficients of Q×ITENF are positive and significant in all regressions. The results suggest that

the enforcement increases the investment-to-price sensitivity even when firms do not raise external funds or the amount of external funds does not change after the enforcement. Since there is no capital flows into these firms, the change in the investment-to-price sensitivity is more likely due to information transmission. In other words, this result is consistent with the managerial learning hypothesis.

5.3. The effect of the enforcement on the sensitivity of investment to sales growth

Our final test is to examine the association between the enforcement and the sensitivity of investment to an investment signal that is not based on stock prices. Specifically, we test the change in the investment-to-sales growth (SGRW) after the enforcement. We define SGRW of year *t* as the change in the sales revenue of year *t* divided by the lagged total assets. We examine the sensitivity of investment in year *t* to SGRW of year *t*-1. Sales growth is widely used as a proxy for investment opportunities (e.g., Billett et al., 2007; Chari and Henry, 2008). By construction, it does not capture outside investors' private information as it is not based on stock prices. Since we measure SGRW at year t-1, the information contained in this variable is clearly known to managers when they make investment decisions. In sum, the investment-to-SGRW sensitivity does not capture any learning effect. If we find any increase in the investment-to-SGRW sensitivity after the enforcement, it is most likely due to the reduced market frictions.

Table 9 reports the results. Columns (1) to (4) estimate the pooled sample regressions. Columns (1) and (2) use INVEST as the dependent variable. In Column (1) we do not include Q in the regression. The coefficient of SGRW×PROTECT is significant and positive (coeff. = 0.034; t = 11.10), suggesting that the investment-to-SGRW sensitivity is affected by market frictions that are (inversely) measured by PROTECT. The coefficient of SGRW×ITENF is also

positive and significant (coeff. = 0.020; t = 3.86), appearing to support the market frictions hypothesis. However, the result is not robust. In particular, the coefficient of SGRW×ITENF becomes insignificant (coeff. = 0.000; t = 0.06) when we include Q in the regression, as reported in Column (2). The coefficient of SGRW×PROTECT continues to be positive and significant (coeff. = 0.019; t = 6.30). The significance of SGRW×ITENF also disappears when the dependent variable is adjusted for the time trend in the investment-to-SGRW and investment-to-cash flow sensitivities (Adj.INVEST2) in Column (3) and trend-adjusted sensitivities of investment to SGRW, Q, and CF (Adj.INVEST3) in Column (4). We use a similar method to the one for Adj.INVEST to compute these adjustments. The Appendix details the algorithm. The *t*-statistic of the coefficient of SGRW×ITENF is only 0.57 in Column (3) and 1.43 in Column (4). While the coefficients of SGRW×PROTECT are not significant in Columns (3) and (4), the coefficients of SGRW have become more negative than those reported in Columns (1) and (2). Given that on average the six countries that are used to estimate the time trend have better investor protection and less market frictions than the remaining countries, the more negative coefficients of SGRW also indicate that market frictions adversely affect the investment-to-SGRW sensitivity. Finally, the coefficients of Q×ITENF in Columns (2) and (4) continue to be positive and significant.

[Insert Table 9 here]

Columns (4) to (8) show the results of the event-window analysis. We find that none of the coefficients of SGRW×ITENF is significantly positive. Moreover, the coefficients of Q×ITENF in Columns (6) and (8) continue to be positive and significant. Together with the evidence presented in Tables 7 and 8, these results support the managerial learning hypothesis but not the market frictions hypothesis.

To summarize, the above evidence suggests that the primary mechanism through which the enforcement increases the investment-to-price sensitivity is managerial learning.

6. The relation between the enforcement and future accounting performance

If the increase in investment-to-price sensitivity after the enforcement reflects improved investment efficiency, we should observe an improvement in operating performance after the enforcement. In addition, this improvement should be correlated with the improvement in the investment-to-price sensitivity. We thus employ the following regression model to investigate the effect of the enforcement on firm operating performance:

$$PERFORMANCE_{c,f,t+n} = a_1 ITENF_{c,t} + a_2 ITENF_{c,t} \times \theta_c + b_1 \ln(TA_{c,f,t}) + b_2 LEV_{c,f,t} + b_3 CASH_{c,f,t} + b_5 PPE_{c,f,t} + \mu_f + \mu_t + \varepsilon_{c,f,t}, \quad (5)$$

where c, f, and t are indicators for country, firm, and year, respectively and μ_f and μ_t are firm and year fixed effects. *PERFORMANCE* is a proxy for future accounting performance. We use future return on assets (ROA) and sales growth (SGRW) to measure PERFORMANCE. Following Foucault and Frésard (2012), we include the following control variables: *ln*(TA) is the natural logarithm of the book value of total assets, *LEV* is leverage, *CASH* is cash and cash equivalent scaled by total assets, and *PPE* is net property, plant and equipment scaled by total assets. θ_c is the measure of the increase in the trend-adjusted investment-to-price sensitivity of country c after the enforcement. The caption of Table 10 details the estimation procedure of θ_c.

[Insert Table 10 here]

Columns (1) to (4) of Table 10 estimate the pooled sample regressions. Column (1) investigates the effect of ROA in year t+1, and Column (2) examines the average ROA over the years from t+1 to t+3. The results are consistent with the notion that the enforcement improves a

firm's operating performance by improving its investment decision quality. In particular, the coefficients of ITENF×θ are positive and significant at the 1% level in both columns. Columns (3) and (4) examine sales growth in year t+1 and over the years from t+1 to t+3. The results are similar. The coefficients of ITENF×θ are also significantly positive in both columns. Columns (5) to (8) repeat the tests but focus on the change in accounting performance over the short window around the enforcement year. We find similar results. The coefficients of ITENF×θ are all positive and significant at the 1% level.[26] Therefore, the results further support the argument that managers extract more information contained in stock prices to guide their investment decisions after the enforcement.[27]

7. Conclusion

We find a significant increase in the investment-to-price sensitivity after the initial enforcement of insider trading laws. This result is robust to various model specifications, sample selections, measures of investment, and units of observations in the analysis. In addition, we observe a significant jump in the investment-to-price sensitivity immediately after the enforcement. Further analysis reveals that the primary reason underlying the increase in the investment-to-price sensitivity is that stock prices provide more information to guide managers' investment decisions after the enforcement, i.e., more managerial learning. Finally, we find that the change in accounting performance after the enforcement is positively associated with the increase in the investment-to-price sensitivity. In contrast, the increase in investment-to-price

[26] We also follow Durnev (2012) and Foucault and Frésard (2012) to estimate the firm-level incremental investment-to-price sensitivity after the enforcement. The results are qualitatively similar.

[27] These results are inconsistent with the hypothesis that the increased investment-to-price sensitivity is because stock prices can better predict investment decisions already made. According to this hypothesis, investment decisions per se do not change. Therefore, the performance of investment should not have any systematic change. Even if there is any change, the change should not be associated with the change in the investment-to-price sensitivity after the enforcement.

sensitivity after the enforcement is in general not associated with market frictions that are due to agency problems (i.e., moral hazard) or financing constraints (i.e., adverse selection). Overall, our results appear to support the managerial learning hypothesis but are inconsistent with the market frictions hypothesis.

Our study makes several important contributions to the literature. First, to our best knowledge, our paper is the first large sample empirical study on the real effect of the enforcement of insider trading laws. Furthermore, we identify the specific mechanism through which the enforcement affects corporate investment behavior. Our evidence sheds light on the long-lasting analytical debates on the effect of insider trading regulation on corporate investment (e.g., Leland, 1992; Khanna et al., 1994; Bernhardt et al., 1995). Recent studies on the economic consequences of the enforcement of insider trading laws have focused on the financial market and the information side of the economy (e.g., Bhattacharya and Daouk, 2002; Bushman et al., 2005; Fernandes and Ferreira, 2009). While these important studies have implications for corporate investment behavior, it is not clear whether and through what exact mechanisms corporate investment behavior is affected. In sum, our study extends the literature by examining the effect of insider trading regulation on the real-side of the economy.

Second, we contribute to the empirical studies on how country-level legal, institutional and regulatory environments affect corporate investment and growth. Prior studies have examined the effects of legal protection (Wurgler, 2000; Kusnadi et al., 2010; McLean et al., 2012), financial development (Rajan and Zingales, 1998), financial liberalization (Bekaert et al., 2005), and accounting information and disclosure quality (Bushman et al., 2006). Our paper differs from these studies in two important aspects. First, none of them directly examines the real effect of insider trading regulation, one of the most important regulations on securities markets.

The fact that our results hold after controlling for proxies for country-level institutional factors (e.g., investor protection) suggests that insider trading regulation has an incremental effect. Second, these studies in general conclude that the country-level factors they examine affect corporate investment because these factors facilitate external financing and mitigate agency problems. Our paper suggests that insider trading regulation affects corporate investment primarily through the managerial learning channel.

Finally, our paper is related to the recent emerging research on the real-side implication of managerial learning from stock prices (e.g., Luo, 2005; Chen et al., 2007). More specifically, our study belongs to a series of recent studies examining the factors that affect managerial learning in an international setting. Durnev (2012) find political uncertainty driven by elections reduces managerial learning. Foucault and Frésard (2012) find increased managerial learning after firms are cross listed. Our study contributes to this line of research by documenting evidence on how insider trading regulation affects managerial learning in an international setting.

Appendix: Variable definitions

Variable name	Definition	Sources
Country-level variables		
ITENF	Dummy variable that equals one for the years after the year of initial enforcement of insider trading laws, and zero otherwise.	Bhattacharya and Daouk (2002)
ITEXIST	Dummy variable that equals one for the years after the year when insider trading laws were first instituted, and zero otherwise.	Bhattacharya and Daouk (2002)
PROTECT	Composite index of investor protection, defined as the average of (1) the anti-director-right index scaled by 5; (2) average of the disclosure requirement index and the liability standard index; and (3) the anti-self-dealing index.	La Porta et al. (1998); La Porta et al. (2006); Djankov et al. (2008)
OPACITY	Country-year specific measures of earnings opacity, following Bhattacharya et al. (2003). For each year and each country with at least 30 observations, we estimate the median accruals, loss avoidance, and earnings smoothness. Accruals is defined as (ΔCA-ΔCASH)+(-ΔCL-ΔSTD-ΔTP)+DEP, scaled by lagged total assets. CA is total current assets. CASH is cash and cash equivalent. CL is total current liabilities. STD is total short-term debt. TP is total tax payable. DEP is the expense of depreciation and amortization. Loss avoidance is defined as the ratio of the total number of firms reporting ROA between [-1%,0) divided by the total number of firms reporting ROA between [-1%,+1%]. ROA is defined as net income before extraordinary items, scaled by lagged total assets. Earnings smoothness is defined as minus one times the correlation between accruals and operating cash flow. Operating cash flow is defined as ROA minus accruals. We pool all country-year observations in our sample with all of the three measures and transform country-year median accruals, loss avoidance, and earnings smoothness into decile ranks. OPACITY is defined as the average of these three decile ranks.	Worldscope
Firm-level variables		
INVEST	Total investment, defined as the sum of the change in net property, plant, and equipment, the change in inventory, and R&D expenditures, scaled by lagged total assets.	Worldscope
Adj.INVEST	*INVEST* adjusted for the time trend in the investment-to-price and investment-to-cash flow sensitivities, defined as $INVEST_{c,f,t} - \beta_t \times Q_{c,f,t-1} - \eta_t \times CF_{c,f,t}$, where c, f, and t are indicators for country, firm, and year, respectively and β_t and η_t are estimated by the following annual regression using all observations in the six countries whose initial enforcement of insider trading laws occurred before 1982 (namely, Brazil, Canada, France, Singapore, the U.K., and the U.S.): $$INVEST_{c,f,t} = \beta_t Q_{c,f,t-1} + \eta_t CF_{c,f,t} + \text{country fixed effect} + 2\text{-digit SIC industry fixed effect} + \varepsilon_{c,f,t}$$	Worldscope
Adj.INVEST2	*INVEST* adjusted for the time trend in the investment-to-SGRW and investment-to-cash flow sensitivities, defined as $INVEST_{c,f,t} - \beta_t \times SGRW_{c,f,t-1} - \eta_t \times CF_{c,f,t}$, where c, f, and t are indicators for country, firm, and year, respectively and β_t and η_t are estimated by the following annual regression using all observations in the six countries whose initial enforcement of insider trading laws occurred before 1982 (namely, Brazil, Canada, France,	Worldscope

	Singapore, the U.K., and the U.S.): $INVEST_{c,f,t} = \beta_t SGRW_{c,f,t-1} + \eta_t CF_{c,f,t} + \text{country fixed effect} + \text{2 - digit SIC industry fixed effect} + \varepsilon_{c,f,t}$	
*Adj.INVEST*3	*INVEST* adjusted for the time trend in the investment-to-price, investment-to-SGRW, and investment-to-cash flow sensitivities, defined as $INVEST_{c,f,t} - \alpha_t \times Q_{c,f,t-1} - \beta_t \times SGRW_{c,f,t-1} - \eta_t \times CF_{c,f,t}$, where *c*, *f*, and *t* are indicators for country, firm, and year, respectively and α_t, β_t and η_t are estimated by the following annual regression using all observations in the six countries whose initial enforcement of insider trading laws occurred before 1982 (namely, Brazil, Canada, France, Singapore, the U.K., and the U.S.): $INVEST_{c,f,t} = \alpha_t Q_{c,f,t-1} + \beta_t SGRW_{c,f,t-1} + \eta_t CF_{c,f,t} + \text{country fixed effect} + \text{2 - digit SIC industry fixed effect} + \varepsilon_{c,f,t}$	Worldscope
Q	Natural logarithm of Tobin's *Q*, calculated as the market value of equity plus total assets minus the book value of total equity in year t-1, divided by the book value of total assets.	Worldscope
SGRW	Sales growth, defined as the change in the sales revenue divided by lagged total assets.	Worldscope
CF	Operating cash flow, defined as income before extraordinary items plus depreciation and amortization, scaled by lagged total assets.	Worldscope
NSYNCH	Natural logarithm transformed ($1-R^2$) from the following regression: $RET_{c,f,t} = \alpha + \beta_1 R^M_{c,t} + \beta_2 R^{US}_{c,t} + e_{c,f,t}$ where *c*, *f*, and *t* are indicators for country, firm, and week, respectively. *RET* is weekly returns denominated in U.S. dollars for firm *f*. $R^M_{c,t}$ is the value-weighted weekly market returns for country *c*, week *t*. The weekly return of the firm in question is excluded when constructing the market return. $R^{US}_{c,t}$ is the value-weighted weekly return of NYSE/AMEX/NASDAQ. We estimate the above regression for each firm in each fiscal year. We require at least 24 weekly observations in the regression. R^2 is the R squared in the regression.	Datastream
LMSW	Information-based trading developed by Llorente, Michaely, Saar, and Wang (2002), defined as the estimate of θ in the following regression: $RET_{c,f,t} = \alpha + \beta RET_{c,f,t-1} + \theta RET_{c,f,t-1} \times V_{c,f,t-1} + e_{c,f,t}$, where *c*, *f*, and *t* are indicators for country, firm, and month. *RET* is monthly returns denominated in U.S. dollars for firm *f*. We estimate the above regression for each firm in each fiscal year. *V* is the mean-adjusted logarithm of monthly turnover, where the mean is computed using the data for the previous 12 months. Following Llorente et al. (2002), the logarithm of turnover is defined as Log(turnover+0.00000255). We require 12 monthly observations in the regression. We use monthly observations because we do not have enough weekly or daily trading volume data for the mid-1990s.	Datastream

NAF	Number of analysts issuing earnings forecasts for the firm. For U.S. firms, NAF is set to zero if the data are missing. For non-US firms after year 1988, NAF is set to zero if the data are missing. We delete the observations before 1988 for non-U.S. firms.	I/B/E/S
ABSDAC	Absolute value of discretionary accruals. Discretionary accruals are defined as the residual term from the following country-year specific regression: $ACCR = a + b \times \Delta SALE + c \times PPE + \varepsilon$ *ACCR* is accruals, defined in *OPACITY*. $\Delta SALE$ is the change in sales divided by lagged total assets. *PPE* is net property, plant and equipment divided by lagged total assets.	Worldscope
WEDGE	Difference between the control rights and the cash flow rights of the largest shareholder.	http://jfe.rochester.edu/data.htm.
WW-index	Financing constraints index developed by Whited and Wu (2006).	Worldscope
Equity_ISSUE	External equity financing, defined as total equity issuance, scaled by lagged total assets. Total equity issuance is defined as the change in total book value of equity plus the change in deferred tax, minus the change in retained earnings.	Worldscope
Debt_ISSUE	External debt financing, defined as total debt issuance, scaled by lagged total assets. Total debt issuance is defined as the change in total debt.	Worldscope
Total_ISSUE	Sum of *Equity_ISSUE* and *Debt_ISSUE*.	Worldscope
ln(*TA*)	Firm size, defined as the natural logarithm of the book value of total assets.	Worldscope
LEV	Leverage, defined as total debt scaled by the book value of total assets.	Worldscope
CASH	Cash holding, defined as cash and cash equivalent scaled by the book value of total assets.	Worldscope
PPE	Net property, plant, and equipment scaled by the book value of total assets.	Worldscope
ROA	Operating income scaled by the lagged book value of total assets.	Worldscope

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Table 1. Sample distribution and country-level variables

This table shows the distribution of firm-year observations across the 45 countries included in this study. *IT enforcement year* is the year of the initial enforcement of insider trading laws. *IT existence year* is the year in which the insider trading laws were first instituted. Both variables are from Bhattacharya and Daouk (2002). ANTIDIR is the anti-director rights index from La Porta et al. (1998). DISCREQ and LIABSTD are the index of disclosure requirement and the index of liability standard from La Porta et al. (2006). ANTISD is the anti-self-dealing index from Djankov et al. (2008). N is the number of firm-year observations.

Country	N	IT enforcement year	IT existence year	Anti-director rights index (ANTIDIR)	Disclosure requirement index (DISCREQ)	Liability standard index (LIABSTD)	Anti-self-dealing index (ANTISD)
Developed markets							
Australia	3,485	1996	1991	4	0.75	0.66	0.76
Austria	946		1993	2	0.25	0.11	0.21
Belgium	1,248	1994	1990	0	0.42	0.44	0.54
Canada	7,045	1976	1966	5	0.92	1.00	0.64
Denmark	1,590	1996	1991	2	0.58	0.55	0.46
Finland	1,185	1993	1989	3	0.50	0.66	0.46
France	7,013	1975	1967	3	0.75	0.22	0.38
Germany	6,331	1995	1994	1	0.42	0.00	0.28
Greece	1,463	1996	1988	2	0.33	0.50	0.22
Hong Kong	3,255	1994	1991	5	0.92	0.66	0.96
Ireland	705		1990	4	0.67	0.44	0.79
Italy	2,424	1996	1991	1	0.67	0.22	0.42
Japan	29,294	1990	1988	4	0.75	0.66	0.50
Netherlands	2,176	1994	1989	2	0.50	0.89	0.20
New Zealand	630		1988	4	0.67	0.44	0.95
Norway	1,324	1990	1985	4	0.58	0.39	0.42
Portugal	607		1986	3	0.42	0.66	0.44
Singapore	2,201	1978	1973	4	1.00	0.66	1.00
Spain	1,670	1998	1994	4	0.50	0.66	0.37
Sweden	2,180	1990	1971	3	0.58	0.28	0.33

Switzerland	2,320	1995	1988	2	0.67	0.44	0.27
United Kingdom	16,073	1981	1980	5	0.83	0.66	0.95
United States	57,901	1961	1934	5	1.00	1.00	0.65
Emerging markets							
Argentina	287	1995	1991	4	0.50	0.22	0.34
Brazil	1,370	1978	1976	3	0.25	0.33	0.27
Chile	860	1996	1981	5	0.58	0.33	0.63
Colombia	155		1990	3	0.42	0.11	0.57
Egypt	47		1992	2	0.50	0.22	0.20
India	967	1998	1992	5	0.92	0.66	0.58
Indonesia	2,059	1996	1991	2	0.50	0.66	0.65
Israel	423	1989	1981	3	0.67	0.66	0.73
Jordan	24			1	0.67	0.22	0.16
South Korea	3,298	1988	1976	2	0.75	0.66	0.47
Malaysia	3,410	1996	1973	4	0.92	0.66	0.95
Mexico	916		1975	1	0.58	0.11	0.17
Pakistan	391		1995	5	0.58	0.39	0.41
Peru	231	1994	1991	3	0.33	0.66	0.45
Philippines	532		1982	3	0.83	1.00	0.22
South Africa	2,346		1989	5	0.83	0.66	0.81
Sri Lanka	68	1996	1987	3	0.75	0.39	0.39
Taiwan	3,218	1989	1988	3	0.75	0.66	0.56
Thailand	1,462	1993	1984	2	0.92	0.22	0.81
Turkey	698	1996	1981	2	0.50	0.22	0.43
Venezuela	95		1998	1	0.17	0.22	0.09
Zimbabwe	45			3	0.50	0.44	0.39

Table 2. Summary statistics of the main firm-level variables

This table shows the summary statistics for the main firm-level variables used in the paper. *INVEST* is total investment. *Adj.INVEST* is total investment (*INVEST*) adjusted for the time trend in the investment-to-price and investment-to-cash flow sensitivities. *Q* is the natural logarithm of Tobin's *Q*. *CF* is operating cash flow. *SGRW* is sales growth. Panels A, B, and C show the summary statistics for the pooled sample, the pre-enforcement period, and the post-enforcement period, respectively. N is the number of firm-year observations.

Panel A: The pooled sample

				Percentiles		
Variable	N	Mean	Std Dev	25%	50%	75%
$INVEST_t$	175,968	0.074	0.165	-0.014	0.042	0.125
$Adj.INVEST_t$	84,365	0.021	0.149	-0.060	0.005	0.074
Q_{t-1}	175,968	0.343	0.527	0.011	0.228	0.571
CF_t	175,968	0.077	0.120	0.036	0.081	0.134

Panel B: The pre-enforcement period

				Percentiles		
Variable	N	Mean	Std Dev	25%	50%	75%
$INVEST_t$	25,025	0.075	0.172	-0.021	0.044	0.136
$Adj.INVEST_t$	25,025	0.023	0.167	-0.071	-0.002	0.083
Q_{t-1}	25,025	0.292	0.446	0.017	0.208	0.503
CF_t	25,025	0.098	0.084	0.052	0.090	0.137

Panel C: The post-enforcement period

				Percentiles		
Variable	N	Mean	Std Dev	25%	50%	75%
$INVEST_t$	150,943	0.074	0.164	-0.013	0.042	0.123
$Adj.INVEST_t$	59,340	0.020	0.140	-0.055	0.007	0.072
Q_{t-1}	150,943	0.352	0.538	0.010	0.232	0.585
CF_t	150,943	0.074	0.125	0.032	0.079	0.133

Table 3. The association between the initial enforcement of insider trading laws and the investment-to-price sensitivity

This table shows the results of the following regression model:

$$INVEST_{c,f,t}\,(Adj.INVEST_{c,f,t}) = a_1 ITENF_{c,t-1} + b_1 Q_{c,f,t-1} + b_2 Q_{c,f,t-1} \times ITENF_{c,t-1} + b_3 Q_{c,f,t-1} \times ITEXIST_{c,t-1}$$
$$+ b_4 Q_{c,f,t-1} \times PROTECT_c + c_1 CF_{c,f,t} + c_2 CF_{c,f,t} \times ITENF_{c,t-1}$$
$$+ c_3 CF_{c,f,t} \times ITEXIST_{c,t-1} + \mu_c + \mu_i + \mu_t + \varepsilon_{c,f,t},$$

where c, f, and t are indicators for country, firm, and year, respectively and μ_c, μ_i and μ_t are fixed effects of country, industry (based on 2-digit SIC codes), and year, respectively. *INVEST* is total investment. *Adj.INVEST* is *INVEST* adjusted for the time trend in the investment-to-price and investment-to-cash flow sensitivities. *Q* is the natural logarithm of Tobin's *Q*. *CF* is operating cash flow. *ITENF* is a dummy variable that equals one in the years after the initial enforcement of insider trading laws, and zero otherwise. *ITEXIST* is a dummy variable that equals one in the period after the country has its insider trading laws instituted, and zero otherwise. *PROTECT* is the composite index of investor protection. See the Appendix for detailed definitions of variables. N is the number of firm-year observations. The *t*-statistics are reported in parentheses and are based on standard errors adjusted for heteroscedasticity and clustering at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	The dependent variable is *INVEST*			The dependent variable is *Adj.INVEST*	
Independent variable	(1)	(2)	(3)	(4)	(5)
ITENF	0.004	0.014***	0.017***	0.016***	0.021***
	(1.42)	(5.25)	(5.40)	(4.98)	(6.29)
ITEXIST			-0.005		-0.006
			(-1.05)		(-1.22)
Q	0.013***	-0.059***	-0.055***	-0.073***	-0.066***
	(3.72)	(-11.61)	(-8.14)	(-11.08)	(-8.45)
Q×*ITENF*	0.073***	0.050***	0.050***	0.017***	0.021***
	(19.88)	(13.47)	(11.45)	(4.56)	(4.80)
Q×*ITEXIST*			-0.005		-0.011
			(-0.70)		(-1.55)
Q×*PROTECT*		0.041***	0.041***	0.001	0.001
		(19.22)	(19.24)	(0.38)	(0.26)
CF	0.584***	0.576***	0.626***	0.335***	0.197***
	(24.50)	(24.16)	(15.47)	(14.05)	(4.91)
CF×*ITENF*	-0.457***	-0.446***	-0.415***	-0.087***	-0.157***
	(-18.44)	(-18.03)	(-14.80)	(-3.38)	(-5.34)
CF×*ITEXIST*			-0.081*		0.207***
			(-1.77)		(4.49)
Fixed effects of country, industry and year	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.139	0.144	0.144	0.112	0.113
N	175,968	175,968	175,968	84,365	84,365

Table 4. Robustness tests

This table shows the results of various robustness tests for the regressions in Columns (1) and (4) in Table 3. Panel A shows the results of various model specifications and sample selections. Columns (1) and (6) estimate the Fama and MacBeth (1973) regression. Columns (2) and (7) estimate the firm-fixed effects regressions. Columns (3) and (8) exclude observations in years 1997 and 1998. Columns (4) and (9) exclude all firm-year observations from Canada, France, Japan, Germany, the U.K., and the U.S. Columns (5) and (10) cluster the standard errors by country. Industry fixed effects are based on 2-digit SIC codes. *INVEST* is total investment. *Adj.INVEST* is *INVEST* adjusted for the time trend in the investment-to-price and investment-to-cash flow sensitivities. *Q* is the natural logarithm of Tobin's *Q*. *CF* is operating cash flow. *ITENF* is a dummy variable that equals one in the years after the initial enforcement of insider trading laws, and zero otherwise. *PROTECT* is the composite index of investor protection. See the Appendix for detailed definitions. N is the number of years in Columns (1) and (6) and the number of firm-year observations in other columns. The *t*-statistics in Columns (2) to (4) and Columns (7) to (9) are reported in parentheses and are based on standard errors adjusted for heteroscedasticity and clustering at the firm level. The *t*-statistics in Columns (1) and (6) are based on the Newey-West (1987) adjusted standard errors. The *t*-statistics in Columns (5) and (10) are based on standard errors adjusted for clustering at the country level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Alternative specifications

	Dependent variable is INVEST					Dependent variable is Adj.INVEST				
Independent variable	Fama-MacBeth regression (1)	Firm fixed effects regression (2)	Excluding Asian financial crisis (3)	Excluding influential countries (4)	Clustering standard errors by country (5)	Fama-MacBeth regression (6)	Firm fixed effects regression (7)	Excluding Asian financial crisis (8)	Excluding influential countries (9)	Clustering standard errors by country (10)
ITENF	0.010	0.035***	0.020***	0.020***	0.014	0.010	0.023***	0.014***	-0.003	0.016
	(1.28)	(9.66)	(7.18)	(4.83)	(1.50)	(1.29)	(5.73)	(4.06)	(-0.77)	(1.17)
Q	-0.005	-0.045***	-0.061***	0.005	-0.059***	-0.055***	-0.072***	-0.078***	-0.080***	-0.073***
	(-0.25)	(-6.49)	(-11.68)	(0.62)	(-2.68)	(-2.98)	(-7.64)	(-11.49)	(-10.30)	(-6.46)
Q×ITENF	0.058***	0.037***	0.052***	0.026***	0.050***	0.006	0.012**	0.022***	0.024***	0.017*
	(5.13)	(7.75)	(13.28)	(5.71)	(6.11)	(1.58)	(2.39)	(5.60)	(5.25)	(1.90)
Q×PROTECT	0.012	0.038***	0.041***	0.008**	0.041***	-0.007	0.004	0.003	0.004	0.001
	(0.93)	(13.04)	(18.61)	(2.09)	(3.97)	(-0.63)	(0.76)	(0.90)	(1.08)	(0.23)
CF	0.545***	0.825***	0.584***	0.550***	0.576***	0.275***	0.571***	0.335***	0.360***	0.335***
	(11.65)	(24.49)	(23.49)	(20.43)	(13.97)	(5.56)	(16.94)	(13.50)	(13.57)	(6.24)
CF×ITENF	-0.229***	-0.458***	-0.452***	-0.260***	-0.446***	-0.065	-0.158***	-0.106***	-0.097***	-0.087
	(-4.76)	(-13.31)	(-17.53)	(-8.67)	(-6.26)	(-1.43)	(-4.42)	(-3.92)	(-3.24)	(-1.61)
Country and industry effects	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Year effects	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Firm effects	No	Yes	No	No	No	No	Yes	No	No	No
Adjusted R^2	0.163	0.284	0.144	0.123	0.144	0.074	0.184	0.105	0.097	0.112
N	22	175,968	152,075	52,311	175,968	22	84,365	72,945	48,740	84,365

Panel B shows the results using alternative measures of investment. In Columns (1) and (5), investment is defined as the change in property, plant, and equipment scaled by lagged total assets (PPE). Columns (2) and (6) measure investment as the sum of the change in property, plant, and equipment, and R&D expenditures, scaled by lagged total assets (PPE + R&D). Columns (3) and (7) measure investment as the sum of the change in property, plant, and equipment, the change in inventories, and R&D expenditures, scaled by current total assets. Columns (4) and (8) measure investment as the sum of the change in property, plant, and equipment, the change in inventories, and R&D expenditures, scaled by lagged property, plant, and equipment. All regressions control for country, year, and industry (based on 2-digit SIC codes) effects. N is the number of firm-year observations. The *t*-statistics are based on standard errors adjusted for clustering at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel B: Alternative measures of investment

	Dependent variable is raw investment				Dependent variable is adjusted investment			
Variable	Investment in PPE (1)	Investment in PPE + R&D (2)	Scaled by current TA (3)	Scaled by lagged PPE (4)	Investment in PPE (5)	Investment in PPE + R&D (6)	Scaled by current TA (7)	Scaled by lagged PPE (8)
ITENF	0.011***	0.012***	0.011***	-0.024	0.016***	0.013***	0.013***	0.021
	(5.41)	(5.36)	(5.41)	(-1.11)	(6.36)	(4.84)	(5.29)	(0.86)
Q	-0.025***	-0.057***	-0.033***	-0.366***	-0.043***	-0.065***	-0.039***	-0.479***
	(-7.56)	(-13.07)	(-9.26)	(-7.89)	(-9.63)	(-11.85)	(-8.44)	(-8.11)
Q×ITENF	0.020***	0.039***	0.034***	0.543***	0.010***	0.008**	0.011***	0.109***
	(7.20)	(12.19)	(13.04)	(18.19)	(3.64)	(2.55)	(4.15)	(3.35)
Q×PROTECT	0.019***	0.039***	0.025***	0.272***	0.008***	0.004	-0.003	-0.070**
	(13.98)	(21.43)	(17.28)	(12.04)	(3.44)	(1.38)	(-1.26)	(-2.26)
CF	0.405***	0.406***	0.440***	1.917***	0.191***	0.284***	0.259***	1.885***
	(22.47)	(21.11)	(26.20)	(10.99)	(10.62)	(14.79)	(15.37)	(10.56)
CF×ITENF	-0.252***	-0.359***	-0.344***	-2.823***	-0.108***	-0.076***	-0.036**	0.112
	(-13.76)	(-17.87)	(-19.61)	(-15.04)	(-5.61)	(-3.60)	(-1.96)	(0.54)
Adjusted R^2	0.115	0.142	0.145	0.154	0.069	0.111	0.125	0.099
N	177,591	177,591	175,968	175,968	85,011	85,011	84,365	84,365

Table 5. Changes in the investment-to-price sensitivity around the initial enforcement of insider trading laws

This table shows the results of the following regression model:

$$INVEST_{c,f,t}\,(Adj.INVEST_{c,f,t}) = b_1 ITENF_{c,t} + b_2 Q_{c,f,t-1} + b_3 Q_{c,f,t-1} \times ITENF_{c,t-1} + c_1 CF_{c,f,t} + c_2 CF_{c,f,t} \times ITENF_{c,t-1} + \mu_c + \mu_i + \mu_t + \varepsilon_{c,f,t},$$

where c, f, and t are indicators for country, firm, and year, respectively and μ_c, μ_i and μ_t are fixed effects of country, industry (based on 2-digit SIC codes), and year, respectively. *INVEST* is total investment. *Adj.INVEST* is *INVEST* adjusted for the time trend in the investment-to-price and investment-to-cash flow sensitivities. *Q* is the natural logarithm of Tobin's *Q*. *CF* is operating cash flow. *ITENF* is a dummy variable that equals one in the years after the initial enforcement of insider trading laws, and zero otherwise. *PROTECT* is the composite index of investor protection. See the Appendix for detailed definitions. The sample includes all observations between year -2 and year +3 (i.e., $t \in [-2,+3]$) around the initial enforcement of insider trading laws. We require the country to have at least one firm-year observation in both the pre- (i.e., $t \in [-2,0]$) and post-initial (i.e., $t \in [+1,+3]$) enforcement periods . Year 0 is defined as the pre-enforcement period because investment in year 0 is associated with *Q* in year -1. N is the number of firm-year observations. The *t*-statistics are reported in parentheses and are based on standard errors adjusted for heteroscedasticity and clustering at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Independent variable	The dependent variable is *INVEST* (1)	The dependent variable is *Adj.INVEST* (2)
ITENF	-0.02***	-0.022***
	(-4.59)	(-5.11)
Q	0.002	-0.097***
	(0.15)	(-7.17)
Q×*ITENF*	0.043***	0.039***
	(6.09)	(5.67)
Q×*PROTECT*	-0.006	-0.005
	(-0.94)	(-0.71)
CF	0.559***	0.382***
	(12.97)	(8.77)
CF×*ITENF*	-0.168***	-0.061
	(-3.56)	(-1.30)
Country fixed effects	Yes	Yes
Industry fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Adjusted R^2	0.097	0.081
N	19,293	19,293

Table 6. Country-level analysis

This table shows the results of the two-step regressions. In the first step, the following annual regression is estimated for each country-year with at least 50 firm-year observations:

$$INVEST_{c,f,t}\,(Adj.INVEST_{c,f,t}) = b_{c,t}Q_{c,f,t-1} + c_{c,t}CF_{c,f,t} + \mu_{\mathrm{i}} + \varepsilon_{c,f,t},$$

where c, f, and t are indicators for country, firm, and year, respectively and μ_i is industry fixed effects (based on Fama and French's 12-industry classification). *INVEST* is total investment. *Adj.INVEST* is *INVEST* adjusted for the time trend in the investment-to-price and investment-to-cash flow sensitivities. *Q* is the natural logarithm of Tobin's *Q*. *CF* is operating cash flow. The raw investment-to-price sensitivity (raw $b_{c,t}$) is estimated using *INVEST*, and the trend-adjusted investment-to-price sensitivity (trend-adjusted $b_{c,t}$) is estimated using *Adj.INVEST*.

In the second step, the following regression is estimated:

$$b_{c,t} = \phi_0 + \phi_{\mathrm{i}} ITENF_{c,t} + \text{country effects} + \mu_{c,t},$$

where $b_{c,t}$ is the slope coefficient of *Q* estimated from the investment equation. *ITENF* is a dummy variable that equals one in the years after the initial enforcement of insider trading laws, and zero otherwise. *PROTECT* is the composite index of investor protection. See the Appendix for detailed definitions. The samples in Columns (1) and (2) include all country-year observations with at least 50 firms. Columns (3) and (4) exclude the six countries used to estimate the time trend in the investment-to-price sensitivity (Brazil, Canada, France, Singapore, the U.K., and the U.S.) Columns (5) to (8) only include observations between year -2 and year +3 (i.e., $t \in [-2,+3]$) around the initial enforcement of insider trading laws. We require a country to have at least one year with more than 50 firms in both the pre- (i.e., $t \in [-2,0]$) and post-initial enforcement (i.e., $t \in [+1,+3]$) periods . Year 0 is defined as the pre-enforcement period because investment in year 0 is associated with *Q* in year -1. The *t*-statistics are reported in parentheses and are based on standard errors adjusted for heteroscedasticity. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Pooled sample regressions				Event-window regressions			
	Raw investment-to-price sensitivity		Time trend-adjusted investment-to-price sensitivity		Raw investment-to-price sensitivity		Time trend-adjusted investment-to-price sensitivity	
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ITENF	0.033***	0.032***	0.021***	0.018*	0.027**	0.026	0.025**	0.025
	(5.04)	(3.44)	(3.17)	(1.82)	(2.46)	(1.63)	(2.03)	(1.52)
PROTECT	0.007		-0.002		0.015		0.016	
	(0.84)		(-0.30)		(1.08)		(1.06)	
Country effects	random	fixed	Random	fixed	random	fixed	random	fixed
Adjusted R^2	0.073	0.181	0.024	0.084	0.033	0.126	0.028	0.177
N	474	474	362	362	102	102	102	102

Table 7. Test results of the managerial learning hypothesis

The dependent variable is *Adj.INVEST*, which is total investment (*INVEST*) adjusted for the time trend in the investment-to-price and investment-to-cash flow sensitivities. *Q* is the natural logarithm of Tobin's Q. *CF* is operating cash flow. *ITENF* is a dummy variable that equals one in the years after the initial enforcement of insider trading laws, and zero otherwise. *PROTECT* is the composite index of investor protection. *NSYNCH* is the stock price non-synchronicity. *LMSW* is the information-based trading measure developed by Llorente, Michaely, Saar, and Wang (2002). *ABSDAC* is the absolute value of discretionary accruals. *OPACITY* is the earnings opacity measure of Bhattacharya et al. (2003). *NAF* is the number of analysts following the firm. Δ*NSYNCH*, Δ*LMSW*, Δ*ABSDAC*, Δ*OPACITY*, and Δ*NAF* are the differences in the average *NSYNCH*, *LMSW*, *ABSDAC*, *OPACITY*, and *NAF* between the pre-enforcement period (i.e., years [-3,-1]) and the post-enforcement period (i.e., years [0,+2]), respectively. See the Appendix for detailed definitions. The samples in the pooled sample regressions include all firm-year observations except the six countries used to estimate the time trend in the investment-to-price sensitivity. The samples in the event-window regressions include all firm-year observations between year -2 and year +3 (i.e., $t \in [-2,+3]$) around the initial enforcement of insider trading laws. We require each firm to have at least one observation in both the pre- (i.e., $t \in [-2,0]$) and post-enforcement (i.e., $t \in [+1,+3]$) periods. The *t*-statistics are reported in parentheses and are based on standard errors adjusted for heteroscedasticity and clustering at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Pooled sample regressions					Event-window regressions				
Independent variable	*PROXY* = $NSYNCH_{t-1}$ (1)	*PROXY* = $LMSW_{t-1}$ (2)	PROXY= $ABSDAC_{t-1}$ (3)	PROXY= $OPACITY_{t-1}$ (4)	*PROXY* = NAF_{t-1} (5)	*PROXY* = *ΔNSYNCH* (6)	*PROXY* = *ΔLMSW* (7)	PROXY= Δ*ABSDAC* (8)	PROXY= Δ*OPACITY* (9)	*PROXY* = *ΔNAF* (10)
ITENF	0.028***	0.002	0.024***	0.024***	0.026***	-0.019***	-0.052***	-0.049***	-0.051***	-0.023***
	(7.24)	(0.48)	(6.56)	(4.23)	(7.59)	(-3.79)	(-7.86)	(-9.24)	(-9.36)	(-5.45)
ITENF×PROXY	-0.006***	-0.000	0.038**	0.000	0.000***	-0.011***	-0.001	0.043	-0.004	0.004***
	(-11.99)	(-0.29)	(2.57)	(0.54)	(-3.32)	(-5.31)	(-0.28)	(0.82)	(-1.46)	(5.83)
Q	-0.077***	-0.091***	-0.080***	-0.082***	-0.073***	-0.100***	-0.074***	-0.095***	-0.074***	-0.096***
	(-9.50)	(-9.25)	(-10.16)	(-10.00)	(-11.00)	(-5.88)	(-3.88)	(-5.58)	(-4.77)	(-6.98)
Q×ITENF	0.008*	0.018***	0.012**	0.010	0.020***	0.036***	0.009	0.018**	0.027***	0.040***
	(1.65)	(2.82)	(2.47)	(1.49)	(4.90)	(4.43)	(0.88)	(2.02)	(2.68)	(5.81)
Q×ITENF×PROXY	0.006***	0.003	0.024	0.002	-0.001**	0.008**	0.017**	0.073	0.018***	-0.002**
	(5.17)	(1.56)	(0.93)	(1.56)	(-2.57)	(2.15)	(2.01)	(0.66)	(4.98)	(-2.24)
Q×PROTECT	0.002	0.006	0.004	0.007*	0.002	-0.002	-0.010	-0.004	-0.011	-0.006
	(0.56)	(1.47)	(1.18)	(1.79)	(0.60)	(-0.24)	(-1.13)	(-0.45)	(-1.44)	(-0.88)
CF	0.331***	0.372***	0.358***	0.362***	0.344***	0.374***	0.264***	0.373***	0.332***	0.386***
	(11.52)	(10.23)	(12.94)	(13.52)	(14.16)	(7.25)	(4.51)	(7.59)	(7.09)	(8.83)
CF×ITENF	-0.079***	-0.101***	-0.090***	-0.117***	-0.091***	-0.075	0.126*	0.069	0.085	-0.073
	(-2.56)	(-2.61)	(-3.02)	(-4.05)	(-3.43)	(-1.33)	(1.79)	(1.18)	(1.54)	(-1.54)
Fixed effects of country, industry, and year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.118	0.134	0.127	0.118	0.11	0.086	0.097	0.091	0.092	0.083
N	64,498	50,571	65,894	73,231	79,864	14,765	8,437	10,720	12,722	19,293

Table 8. Test results of the market frictions hypothesis

The dependent variable is *Adj.INVEST*, which is total investment (*INVEST*) adjusted for the time trend in the investment-to-price and investment-to-cash flow sensitivities. *Q* is the natural logarithm of Tobin's Q. *CF* is operating cash flow. *ITENF* is a dummy variable that equals one in the years after the initial enforcement of insider trading laws, and zero otherwise. *PROTECT* is the composite index of investor protection. *WEDGE* is wedge between the control rights and the cash flow rights of the controlling shareholder. The WW-index is the Whited and Wu (2006) index for financing constraints. *Equity_ISSUE*, *Debt_ISSUE*, and *Totoal_ISSUE* are the amount of equity issuance, debt issuance, and total issuance. Δ*Equity_ISSUE*, Δ*Debt_ISSUE*, and Δ*Total_ISSUE* are the changes in equity issuance, debt issuance, and total issuance between the post-enforcement period (i.e., t∈[+1,+3]) and the pre-enforcement period (i.e., t∈[-2,0]). The samples in the pooled sample regressions include all firm-year observations except for those used to estimate the time trend in the investment-to-price sensitivity. The samples in the event-window regressions include all observations between year -2 and year +3 (i.e., t∈[-2,+3]) around the initial enforcement of insider trading laws. We require a country to have at least one firm-year observation in both the pre- (i.e., t∈[-2,0]) and post-enforcement (i.e., *t*∈[+1,+3]) periods in Panels A and B. In Panel C we require each firm to have at least one observation in both the pre- and post-enforcement periods. The *t*-statistics are reported in parentheses and are based on standard errors adjusted for heteroscedasticity and clustering at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: The wedge between the voting rights and the cash flow rights of the controlling shareholder and the effect of the enforcement on the investment-to-price sensitivity

	Pooled sample regressions		Event-window regressions	
	WEDGE≤0	WEDGE>0	WEDGE≤0	WEDGE>0
Independent variable	(1)	(2)	(3)	(4)
ITENF	-0.029**	-0.024*	-0.059***	-0.048**
	(-2.57)	(-1.81)	(-2.65)	(-2.22)
Q	-0.112***	-0.041	-0.178***	-0.143**
	(-4.18)	(-0.94)	(-3.47)	(-2.29)
Q×*ITENF*	0.039***	-0.012	0.103***	0.053
	(2.66)	(-0.65)	(3.82)	(1.41)
Q×*PROTECT*	0.017	0.001	0.025	0.036
	(0.87)	(0.03)	(0.60)	(0.83)
CF	0.181***	0.310***	0.170	0.405***
	(2.81)	(3.22)	(1.59)	(2.86)
CF×*ITENF*	0.099	-0.054	0.221	-0.009
	(1.23)	(-0.49)	(1.61)	(-0.05)
Fixed effects of country, industry and year	Yes	Yes	Yes	Yes
Adjusted R^2	0.159	0.159	0.161	0.182
N	5,233	3,779	1,725	1,241

Panel B: Financing constraints and the effect of the enforcement on the investment-to-price sensitivity

	Pooled sample regressions				Event-window regressions			
Independent variable	Low WW-index (1)	Q2 (2)	Q3 (3)	High WW-index (4)	Low WW-index (5)	Q2 (6)	Q3 (7)	High WW-index (8)
ITENF	0.026***	0.015**	0.015**	0.012*	-0.010	-0.037**	-0.036**	-0.039**
	(4.45)	(2.40)	(2.35)	(1.87)	(-0.75)	(-2.58)	(-2.40)	(-2.40)
Q	-0.094***	-0.090***	-0.033**	-0.063***	-0.101***	-0.099***	-0.103***	-0.069**
	(-6.63)	(-6.34)	(-2.64)	(-5.37)	(-3.74)	(-3.72)	(-3.83)	(-2.38)
Q×ITENF	0.023**	0.020**	0.017**	0.008	0.050***	0.044***	0.045***	-0.006
	(2.56)	(2.40)	(2.24)	(1.17)	(3.10)	(3.12)	(2.81)	(-0.35)
Q×PROTECT	-0.005	0.006	-0.020***	0.003	-0.025*	-0.013	-0.008	0.003
	(-0.69)	(0.91)	(-3.29)	(0.45)	(-1.90)	(-0.96)	(-0.59)	(0.23)
CF	0.559***	0.410***	0.344***	0.279***	0.514***	0.324***	0.466***	0.261***
	(10.53)	(8.36)	(7.39)	(7.08)	(5.37)	(3.89)	(5.23)	(3.51)
CF×ITENF	-0.123**	-0.091*	-0.046	-0.098**	-0.072	0.060	-0.012	0.036
	(-2.11)	(-1.69)	(-0.91)	(-2.24)	(-0.62)	(0.61)	(-0.12)	(0.42)
Fixed effects of country, industry, and year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.164	0.133	0.119	0.104	0.169	0.120	0.116	0.092
N	18,838	18,508	18,671	18,990	4,194	4,119	4,157	4,230

Panel C: Actual external financing and the effect of the enforcement on the investment-to-price sensitivity

	Pooled sample regressions			Event-window regressions		
Independent variable	PROXY = Total_ISSUE$_t$>0 (1)	PROXY = Equity_ISSUE$_t$>0 (2)	PROXY = Debt_ISSUE$_t$>0 (3)	PROXY = ΔTotal_ISSUE (4)	PROXY = ΔEquity_ISSUE (5)	PROXY = ΔDebt_ISSUE (6)
ITENF	-0.036***	-0.013***	-0.022***	-0.035***	-0.041***	-0.038***
	(-9.42)	(-3.25)	(-5.71)	(-5.81)	(-6.71)	(-6.44)
ITENF×PROXY	0.102***	0.069***	0.088***	0.161***	0.157***	0.231***
	(75.21)	(49.07)	(63.28)	(7.88)	(5.23)	(7.96)
Q	-0.079***	-0.078***	-0.082***	-0.092***	-0.091***	-0.093***
	(-10.77)	(-10.59)	(-11.18)	(-4.79)	(-4.69)	(-4.83)
Q×ITENF	0.012**	0.013***	0.019***	0.021*	0.021*	0.020*
	(2.50)	(2.72)	(3.92)	(1.85)	(1.74)	(1.79)
Q×ITENF×PROXY	0.005*	0.011***	0.002	-0.006	-0.029	0.037
	(1.64)	(3.62)	(0.75)	(-0.14)	(-0.44)	(0.62)
Q×PROTECT	0.002	0.002	0.004	-0.006	-0.007	-0.005
	(0.71)	(0.61)	(1.14)	(-0.64)	(-0.71)	(-0.54)
CF	0.395***	0.394***	0.391***	0.328***	0.327***	0.333***
	(14.49)	(14.48)	(14.35)	(6.54)	(6.51)	(6.61)
CF×ITENF	-0.179***	-0.184***	-0.146***	0.047	0.081	0.044
	(-6.08)	(-6.23)	(-4.95)	(0.77)	(1.30)	(0.71)
Fixed effects of country, industry, and year	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.207	0.160	0.191	0.112	0.096	0.114
N	63,351	63,351	63,351	7,268	7,268	7,268

Table 9. The initial enforcement of insider trading laws and the sensitivity of investment to sales growth

Adj.INVEST is total investment (*INVEST*) adjusted for the time trend in the investment-to-price and investment-to-cash flow sensitivities. *Adj.INVEST2* is *INVEST* adjusted for the time trend in the investment-to-SGRW and investment-to-cash flow sensitivities. *Adj.INVEST*3 is *INVEST* adjusted for the time trend in the investment-to-SGRW, investment-to-price, and investment-to-cash flow sensitivities. *Q* is the natural logarithm of Tobin's Q. *SGRW* is sales growth. *CF* is operating cash flow. *ITENF* is a dummy variable that equals one in the years after the initial enforcement of insider trading laws, and zero otherwise. *PROTECT* is the composite index of investor protection. See the Appendix for detailed definitions. The samples in the pooled sample regressions include all observations except those used to estimate the time trend in the investment-to-price sensitivity. The samples in the event-window regressions include all observations between year -2 and year +3 (i.e., t∈[-2,+3]) around the initial enforcement of insider trading laws. We require a country to have at least one firm-year observation in both the pre- (i.e., t∈[-2,0]) and post-enforcement (i.e., *t*∈[+1,+3]) periods . The *t*-statistics are reported in parentheses and are based on standard errors adjusted for heteroscedasticity and clustering at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Pooled sample regressions				Event-window regressions			
Independent variable	INVEST (1)	INVEST (2)	Adj.INVEST2 (3)	Adj.INVEST3 (4)	INVEST (5)	INVEST (6)	Adj.INVEST2 (7)	Adj.INVEST3 (8)
ITENF	0.015***	0.015***	0.021***	0.019***	-0.021***	-0.025***	-0.033***	-0.031***
	(5.54)	(5.42)	(6.39)	(5.58)	(-2.69)	(-3.28)	(-4.16)	(-3.99)
SGRW	-0.024***	0.000	-0.063***	-0.039***	0.038**	0.034*	-0.071***	-0.037**
	(-3.48)	(-0.07)	(-7.34)	(-4.56)	(2.15)	(1.93)	(-4.01)	(-2.12)
SGRW×ITENF	0.020***	0.000	0.003	0.008	-0.013	-0.018	0.002	0.003
	(3.86)	(0.06)	(0.57)	(1.43)	(-1.09)	(-1.52)	(0.14)	(0.26)
SGRW×PROTECT	0.034***	0.019***	0.006	0.005	0.008	0.010	0.015	0.013
	(11.10)	(6.30)	(1.36)	(1.16)	(0.78)	(1.01)	(1.56)	(1.33)
Q		-0.051***		-0.058***		0.017		-0.073***
		(-9.62)		(-8.58)		(1.19)		(-5.01)
Q×ITENF		0.049***		0.011***		0.026***		0.020**
		(12.11)		(2.85)		(3.25)		(2.51)
Q×PROTECT		0.035***		-0.002		-0.011		-0.008
		(16.11)		(-0.65)		(-1.51)		(-1.16)
CF	0.583***	0.572***	0.310***	0.350***	0.461***	0.466***	0.268***	0.331***
	(24.57)	(22.69)	(12.70)	(13.96)	(11.15)	(9.87)	(6.34)	(6.83)
CF×ITENF	-0.437***	-0.456***	-0.035	-0.099***	-0.005	-0.047	0.086*	0.017
	(-17.52)	(-17.49)	(-1.31)	(-3.63)	(-0.10)	(-0.87)	(1.81)	(0.32)
Fixed effects of country, industry, and year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.097	0.146	0.103	0.119	0.129	0.131	0.093	0.110
N	163,941	163,941	78,014	78,014	17,518	17,518	17,518	17,518

Table 10. The association between the initial enforcement of insider trading laws and firm accounting performance

This table shows the results of the following regression model:

$$PERFORMANCE_{c,f,t+n} = a_1 ITENF_{c,t} + a_2 ITENF_{c,t} \times \theta_c + b_1 \ln(TA_{c,f,t}) + b_2 LEV_{c,f,t} + b_3 CASH_{c,f,t} + b_5 PPE_{c,f,t} + \mu_f + \mu_t + \varepsilon_{c,f,t},$$

where c, f, and t are indicators for country, firm, and year, respectively and μ_f and μ_t are firm and year fixed effects. *PERFORMANCE* is one of the proxies for future accounting performance. $ln(TA)$ is the natural logarithm of book value of total assets. *LEV* is leverage and *CASH* is cash and cash equivalent scaled by total assets. *PPE* is net property, plant, and equipment scaled by total assets. θ_c is the measure of the increase in the trend-adjusted investment-to-price sensitivity in country c after the initial enforcement of insider trading laws. In Columns (1) to (4), θ_c is estimated using the following regression:

$$Adj.INVEST_{c,f,t} = a_1 ITENF_{c,t-1} + b_0 Q_{c,f,t-1} + b_2 Q_{c,f,t-1} \times PROTECT_c + \sum_c \theta_c Q_{c,f,t-1} \times COUNTRY_c \times ITENF_{c,t-1} + c_1 CF_{c,f,t} + c_2 CF_{c,f,t} \times ITENF_{c,t-1} + \mu_c + \mu_i + \mu_t + \varepsilon_{c,f,t},$$

where Q is the natural logarithm of Tobin's Q. *CF* is operating cash flow. *Adj.INVEST* is total investment adjusted for the time trend in investment-to-price and investment-to-cash flow sensitivities. $COUNTRY_c$ is the indicator variable for country c. *ITENF* is a dummy variable that equals one in the years after the initial enforcement of insider trading laws, and zero otherwise. *PROTECT* is the composite index of investor protection. See the Appendix for detailed definitions. Industry fixed effects are based on the 2-digit SIC codes. The samples in Columns (1) to (4) include all observations except the six countries used to estimate the time trend in investment-to-price sensitivity.

In Columns (5)-(8), θ_c is estimated using the following regression:

$$Adj.INVEST_{c,f,t} = \sum_c a_c COUNTRY_c \times ITENF_{c,t-1} + \sum_c b_c Q_{c,f,t-1} \times COUNTRY_c + \sum_c \theta_c Q_{c,f,t-1} \times COUNTRY_c \times ITENF_{c,t-1} + c_1 CF_{c,f,t} + c_2 CF_{c,f,t} \times ITENF_{c,t-1} + \mu_c + \mu_i + \varepsilon_{c,f,t}.$$

The samples in Columns (5) to (8) include all observations between year -2 and year +3 (i.e., $t \in [-2,+3]$) around the initial enforcement of insider trading laws. We require a country to have at least one firm-year observation in both the pre- (i.e., $t \in [-2,0]$) and post-enforcement (i.e., $t \in [+1,+3]$) periods. The t-statistics are reported in parentheses and are based on standard errors adjusted for heteroscedasticity and clustering at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Samples including all firm-year observations except those in Brazil, Canada, France, Singapore, the U.K. and the U.S.				Samples including only observations in years [-2,+3] around the initial enforcement of insider trading laws			
Independent variable	ROA_{t+1} (1)	*Average ROA over* [t+1, t+3] (2)	$SGRW_{t+1}$ (3)	*Average SGRW over* [t+1, t+3] (4)	ROA_{t+1} (5)	*Average ROA over* [t+1, t+3] (6)	$SGRW_{t+1}$ (6)	*Average SGRW over* [t+1, t+3] (7)
ITENF	-0.003*	-0.004***	0.006	0.008	-0.004	-0.002	-0.006	0.025**
	(-1.83)	(-2.67)	(0.78)	(1.20)	(-1.47)	(-1.19)	(-0.36)	(2.13)
ITENF×θ_c	0.406***	0.347***	1.048***	0.895***	0.177***	0.099***	0.636***	0.399**
	(7.19)	(6.37)	(4.87)	(4.21)	(4.55)	(3.14)	(3.39)	(2.46)
ln(*TA*)	-0.029***	-0.030***	-0.130***	-0.166***	-0.051***	-0.040***	-0.238***	-0.274***
	(-18.88)	(-20.98)	(-21.96)	(-28.44)	(-12.52)	(-14.22)	(-11.03)	(-14.49)
LEV	-0.068***	-0.015***	0.009	0.000	-0.052***	-0.004	0.051	0.078*
	(-13.62)	(-3.33)	(0.53)	(0.02)	(-4.36)	(-0.47)	(0.96)	(1.68)
CASH/TA	0.054***	0.034***	0.017	0.058**	0.041**	0.022*	-0.010	0.072
	(7.52)	(5.46)	(0.62)	(2.27)	(2.38)	(1.70)	(-0.11)	(1.04)
PPE/TA	-0.002	0.010*	-0.076***	-0.105***	-0.041***	-0.005	-0.090	-0.103
	(-0.25)	(1.73)	(-2.93)	(-4.05)	(-2.64)	(-0.47)	(-0.99)	(-1.30)
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted. R^2	0.531	0.708	0.199	0.329	0.560	0.787	0.159	0.348
N	82,352	77,517	82,101	79,723	18,899	17,817	18,858	18,352

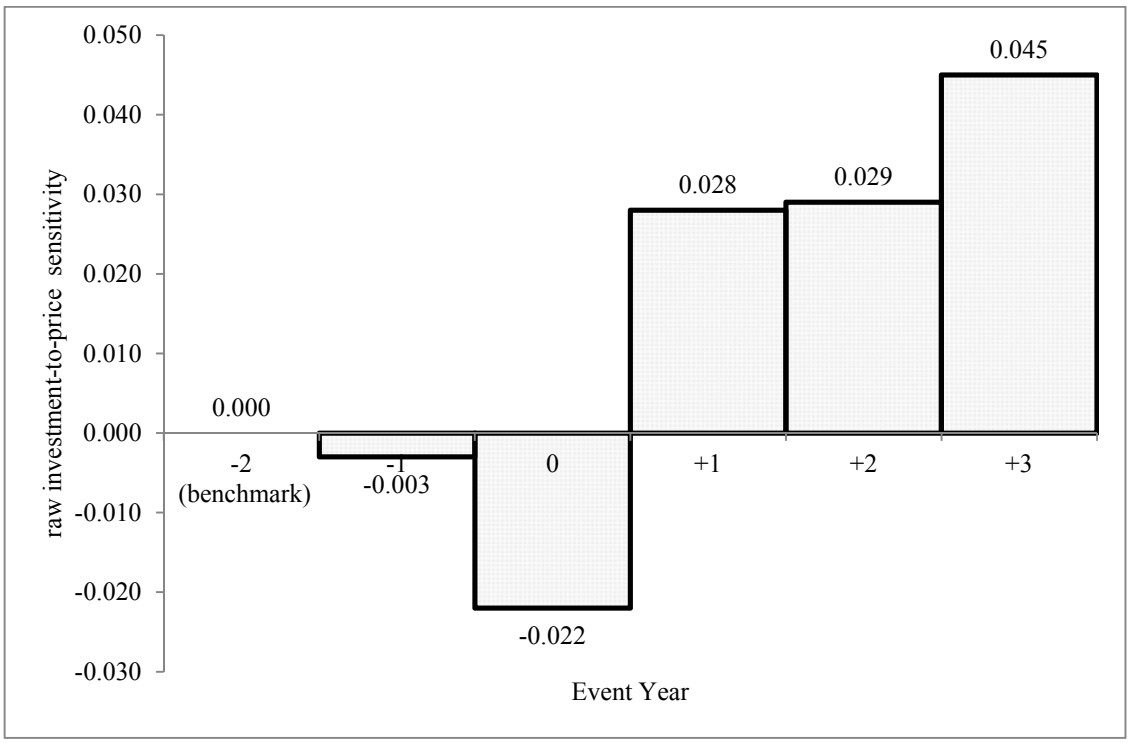
Figure 1. Plots of the investment-to-price sensitivity around the initial enforcement of insider trading laws

This figure plots the results of the following regression:

$$INVEST_{c,f,t}(Adj.INVEST_{c,f,t}) = \sum_{t=-1}^{+3} a_t YEAR_{c,f,t} + bQ_{c,f,t-1} + \sum_{t=-1}^{+3} b_t YEAR_{c,f,t} \times Q_{c,f,t-1} +$$
$$+ \eta Q_{c,f,t-1} \times PROTECT + cCF_{c,f,t} + \sum_{t=-1}^{+3} c_t YEAR_{c,f,t} \times CF_{c,f,t} + \mu_c + \mu_i + \mu_t + \varepsilon_{c,f,t},$$

where c, f, and t are indicators for country, firm, and event year, respectively and μ_c, μ_i and μ_t are fixed effects of country, industry (based on the 2-digit SIC code), and year, respectively. *INVEST* is total investment. *Adj.INVEST* is *INVEST* adjusted for the time trend in the investment-to-price and investment-to-cash flow sensitivities. *Q* is the natural logarithm of Tobin's *Q*. *CF* is operating cash flow. *PROTECT* is the composite index of investor protection. See the Appendix for detailed definitions. The sample includes all observations between year -2 and year +3 (i.e., $t \in [-2,+3]$) around the initial enforcement of insider trading laws. We require a country to have at least one firm-year observation in both the pre- (i.e., $t \in [-2,0]$) and post-enforcement (i.e., $t \in [+1,+3]$) periods . $YEAR_{c,f,t}$ is a dummy variable that equals one for year t relative to the year of the initial enforcement of insider trading laws. Panel A (Panel B) plots the estimates of coefficients b_t ($t \in [-2,+3]$), where b_{-2} is set to zero (the benchmark sensitivity), using *INVEST* (*Adj.INVEST*) as the dependent variable.

Panel A: The change in the *raw* investment-to-price sensitivity around the initial enforcement of insider trading laws



Panel B: The change in the *trend-adjusted* investment-to-price sensitivity around the initial enforcement of insider trading laws