Do Insider Trading Laws Reduce Stock Price Crash Risk? International Evidence

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Abstract

Using a large sample of 48 countries over the period of 1982-2006, we document empirical evidence that initial enforcement of insider trading laws in a country significantly reduces stock price crash risk. We find that the negative effect of the insider trading law enforcement on stock price crash risk is more pronounced in countries with poor quality of institutional infrastructures in terms of investor protection, financial disclosure requirements, financial market liberalization, or product market competition. Our findings are in line with the following view: the enforcement of insider trading laws makes their trading on private information costly and risky. This in turn constrains corporate insiders' incentives and abilities to conceal adverse information and/or engage in suboptimal over-investments (e.g., empire building), thereby reducing stock price crash risk. .

1. Introduction

Extant literature has identified various causes of crashes in asset prices in the U.S. market.[1] On the other hand, although previous research highlights the important role of institutional infrastructures in asset pricing and firm valuation, the underlying factors contributing to cross-country differences in stock price crash risk remain largely unexplored. In this paper, we fill this gap by examining whether and how legal prohibitions against insider trading in a country affect firms' stock price crash risk around the world.

Prior studies provide evidence that corporate insiders trade profitably on private information before a variety of economically significant events (Seyhun, 1990; John and Lang, 1991; Lee, Mikkelson, and Partch, 1992; Ke, Huddart, and Petronib, 2003).[2] Insider trading gains represent an important form of compensation for top executives (Kato and Hebner, 1997; Roulstone, 2003; Denis and Xu, 2013). For example, Seyhun (1992) shows that in the later 1980s, top-level insiders earned abnormal returns of 9% during the year following open-market trading. Related research also indicates that since executives are able to gain profits from superior information through insider trading, they are less likely to make efforts to increase firm value (Levmore, 1982; Manove, 1989; Ausubel, 1990).

Building on prior literature, we expect that restrictions on insider trading reduce stock price crash risk. The rationale runs as follows. First, the capacity of corporate insiders (e.g., managers) to profitably exploit private information and realize significant trading profits incentivizes them to withhold adverse information from public disclosure, leading to a severe overvaluation of

[1] See Chen, Hong, and Stein (2001), Jin and Myers (2006), Hutton, Marcus, and Tehranian (2009), and Kim, Li, and Zhang (2011a, b) among others. We review the related literature in Section 2.

[2] Informational advantage driven insider trading is widely documented in several studies, e.g., Jaffe (1974), Seyhun (1986), Rozeff and Zaman (1988), and Cheng, Nagar, and Rajan (2007), although insiders' liquidity needs and portfolio rebalancing partially explain the motives of some insider trades.

share price.[3] However, managers are only able to withhold the bad news up to a point. When the accumulated negative information reaches the upper limit, a sudden release of the hidden bad news engenders bubble bursting and a stock price crash (Jin and Myers, 2006; Hutton, Marcus, and Tehranian, 2009). Legal restrictions on insider trading make managers' opportunistic trading on private information costly and risky and attenuate managerial incentives and abilities to withhold bad news,[4] thereby lowering stock price crash risk.

Second, insider trading motivates managers to divert corporate resources for their private gains. In an environment of information opaqueness or poor protection of minority shareholder rights, outside stakeholders, including incumbent shareholders and the board of directors, are not able to take timely corrective interventions in managers' poor decision-making. The failure to identify unprofitable projects in an early stage and to force the managers to take abandonment actions in a timely manner is likely to eventually bring about stock price crashes: if money-losing projects are kept alive, managers have incentives to conceal bad performance, contributing to asset overvaluation. When the overvalued asset prices burst, extremely negative returns realize or stock price crashes occur (Bleck and Liu, 2007). Insider trading restrictions require that managers at least partially absorb costs associated with keeping money-losing projects by

[3] While previous studies indicate that information advantage drives insiders to sell stocks before the disclosure of bad news and to purchase stocks before the disclosure of good news, our study focuses particularly on bad news rather than on good news. This is because previous research suggests that corporate insiders such as top managers, in general, have incentives to withhold or delay reporting bad news but to accelerate the release of good news in a timely manner. For example, Kothari, Shu, and Wysocki (2009) find evidence that various incentives, such as career concerns, motivate managers to withhold bad news and overstate financial performance. Ball (2001, 2009) posits that nonfinancial motives, such as empire building and maintaining the esteem of one's peers, also provide strong incentives for managers to conceal bad performance. In contrast, managers have incentives to informally disclose or leak good news prior to the public disclosure of the news (Kothari, Shu, and Wysocki, 2009). Therefore, we conjecture that the effect of legal restrictions against insider trading is concentrated mainly on constraining managerial tendency from withholding bad news (rather than good news).

[4] Consistent with the costly insider trading argument, Han, Jagannathan, and Krishnamurthy (2013) document that opportunistic sales of stocks by corporate insiders prior to stock price crash increase the likelihood of a class action lawsuit being filed against the firm in the US. Anecdotal evidence also supports the view. For instance, *Fortune* (July 31, 2012) reported that in July 2012, Zynga, a NASDAQ listed firm, was hit by a class action lawsuit, alleging that its senior executives dumped shares before a stock price crash. Zynga executives sold their stocks at $12 before its shares fell to $6 on May 28, 2013, when initial investors were allowed to sell their shares.

limiting their trading gains from selling *ex ante* (Fernandes and Ferreira, 2009), and thus weaken managers' tendency to engage in inefficient investments, which in turn contributes to lowering the likelihood that stock price crashes occur.

Using a large sample of 48 countries over the period of 1982-2006, during which the majority of our sample countries started to enforce laws restricting insider trading, we employ a difference-in-difference approach to test whether crash risk of firms' stock prices in a country changes before and after the initial enforcement of insider trading laws. We find strong evidence that firms' stock price crash risk significantly declines after the initial enforcement of insider trading laws. Event study analysis also reveals a decline in stock price crash risk around the enforcement year, but the effect is weaker and less significant than that estimated in the baseline regression analysis, suggesting that the effect of insider trading enforcement on reducing crash risk is long lasting. The economic impact is also significant. Specifically, in our baseline regression analysis, stock price crash risk declines, on average, by more than a half of their standard deviations in countries that enforce insider trading laws, compared with the level of crash risk in countries that have yet not enforced insider trading laws.[5]

We next examine how the negative effect of the insider trading law enforcement on stock price crash risk varies across countries, depending on several institutional characteristics. We find that the effect of insider trading restrictions is more pronounced in countries where: 1) outside investors' rights are poorly protected; 2) more information disclosure regarding firm-specific information is mandatorily required; 3) the financial market has not yet been liberalized; or 4) the product market is less competitive. These results further support our argument that legal actions against insider trading discourage managerial opportunism such as bad news hoarding, resource diversion, and inefficient investments.

[5] Similar inference is drawn based on the event study analysis.

It is possible that the observed association between stock price crash risk and insider trading restrictions is driven by some omitted variables. First, restrictions on insider trading change analyst coverage and stock price informativeness (Bushman, Piotroski, and Smith, 2005; Fernandes and Ferreira, 2009). We find that our results are robust to the inclusion of these two variables. Second, stock market liquidity that influences risk-sharing among investors improves after the enforcement of insider trading laws (Bhattacharya and Daouk, 2002), and a national security law is often enforced simultaneously with other macroeconomic policies that can also affect managerial behaviors. We thus control for stock market liquidity and actual financial and trade flows, i.e., foreign direct investment and openness of a country's product market. Our results are robust to the inclusion of these variables. Finally, since short selling restrictions deter bad news from being incorporated into stock prices (Diamond and Verrecchia, 1987; Beber and Pagano, 2013), thereby increasing firms' stock price crash risk, we include an indicator to denote the feasibility of short selling to isolate its effect from that of insider trading law enforcement on crash risk. Our results remain unchanged after we control for the effect of short selling restrictions.

A reverse causality concern may also arise if a country enforces its insider trading laws in response to an increased likelihood of stock market crashes. We then follow Bertrand and Mullainathan (2003) to examine the dynamics of stock price crash risk surrounding the insider trading enforcement events. Our findings indicate that the reduction in crash risk is initiated only after rather than before the enforcement of insider trading laws. These findings suggest a causal effect of insider trading restrictions on stock price crash risk across countries.

Our paper contributes to the existing literature in three ways. First, our study draws new insight into the growing research on asset price crashes. While this strand of research underlines

the importance of managerial incentives of hiding bad news from public disclosure in determining the probability of stock price crashes, these studies rely mainly on the U.S. markets, leaving attributes to cross-country differences in stock price crash risk largely unexplored. To the best of our knowledge, our study is the first to show how insider trading regulations influence stock price crash risk in a cross-country context.

Second, a major challenge in the empirical literature on stock price crash risk is that crash risk is likely to be endogenously determined by firm or market characteristics, including insider trading.[6] As a result, it is difficult to establish a causal effect of insider trading on stock crashes. Our research alleviates this concern by exploiting the legal shifts in insider trading regimes as a natural experiment. In particular, we conduct tests using initial enforcement of insider trading laws as an exogenous shock to insider trading, and compare the change of stock price crash risk across countries with and without the insider trading law enforcement before and after the regime shifts.

Finally, our work extends the literature by providing one alternative explanation, complementary to Fernandes and Ferreira (2009), for why the enforcement of insider trading laws reduces the cost of equity, as documented in Bhattacharya and Daouk (2002, 2009).[7] Our results imply that the cost of capital declines after the insider trading enforcement, particularly in countries where institutional infrastructures are of poor quality, because the enforcement reduces the risk of asset price crashes, a risk that cannot be reduced through diversification (Sunder, 2010; Chang, Christoffersen, and Jacobs, 2013; Kozhan, Neuberger, and Schneider, 2013).

[6] In addition, as pointed out by Bhattacharya and Daouk (2002), studies using firm-level insider trading data to examine its effects are likely to suffer from a selection bias sine the only source of data concerning illegal trades is confidential and the data publicly available only reflects part of insiders' trading activities.

[7] Fernandes and Ferreira (2009) show that the enforcement of insider trading laws significantly improves stock price informativeness, but the improvement of price informativeness is concentrated in countries with high quality of institutional infrastructures. Their findings from the perspective of price informativeness, therefore, as discussed by themselves, cannot fully explain for the negative association between insider trading restrictions and the cost of capital.

The remainder of the paper is organized as follows. Section 2 provides a review of related literature. Section 3 describes the sample and variables. Section 4 presents the main empirical results, and Section 5 reports the effects of cross-sectional heterogeneity. Results of tests on omitted variables and reverse causality and additional results of sensitivity tests are documented in Section 6. Section 7 summarizes and concludes.

2. Related Literature

By examining the impact of insider trading law enforcement on stock price crash risk, we bring together two different strands of literature. First, our paper contributes to the literature on stock price crash risk.[8] Motivated by the model of agency conflicts in Jin and Myers (2006), recent empirical studies focus on firm-specific internal factors, particularly, financial reporting opacity, which potentially drive stock price crashes. Hutton, Marcus, and Tehranian (HMT, 2009) test the Jin-Myers prediction that information opaqueness increases the occurrence of stock price crashes in the future, using firm-level data. Specifically, HMT provide evidence that information opaqueness, measured by three-year moving sum of absolute abnormal accruals, enables managers to hide unfavorable information about firm performance and to accumulate this negative information over time up to a certain point unless external monitoring is efficacious, which in turn increases stock price crash risk in the future. DeFond et al. (2012) provide evidence that the mandatory IFRS adoption in the European Union decreases stock price crash risk by increasing information transparency. In another study, Kim, Li, and Zhang (2011a) show that aggressive tax strategies and planning create managerial incentives to obfuscate financial reporting, which in turn provides managers with a means to conceal negative information that

[8] Early studies that identify the sources of stock price crashes focus on external factors such as volatility feedback (French, Schwert, and Stambaugh, 1987; Campbell and Hentschel, 1992) and investor heterogeneity (Hong and Stein, 2003).

increases crash risk. Furthermore, a recent work by Kim and Zhang (2013) predict and find that accounting conservatism constrains managers' incentives to conceal bad news by requiring more timely recognition of bad news as losses than good news as gains. Also, Kim, Yeung, and Zhou (2013) predict and find that the presence of internal control weakness increases stock price crash risk by facilitating managers' bad news hoarding behaviors.

Different from the aforementioned literature that focuses on the impact of firm-level reporting opacity on stock price crash risk, our paper examines a hitherto unexplored question of whether and how country-level regulations on insider trading, which limit insiders' incentives, opportunities, and abilities to make profits via private information-based trading, influence stock price crash risk at the firm level. By doing so, our study extends and complements Kim, Li, and Zhang (2011b) who find that CFOs granted with more option-based compensation are likely to engage more aggressively in bad news hoarding and over-investments in unprofitable projects, which in turn exacerbates future crash risk. Our study is also related to Hong, Kim, and Welker (2013) who show that agency conflicts derived from the divergence between ownership and control lead to a higher likelihood of stock price crashes, using an international sample of dual-class firms from 19 countries. While Hong, Kim, and Welker (2013) and our study analyze different firm-specific factors driving stock price crashes, both studies address a similar issue, that is, how managerial incentives, opportunities and abilities for extracting private gains, whether exacerbated by the control-ownership wedge or constrained by the insider trading laws enforcement, eventually lead to the occurrence of extreme negative outliers in stock return distributions or stock price crashes.

Second, our paper adds to the insider trading literature, particularly, that analyzes the economic consequences of insider trading. The debate on costs and benefits of insider trading is

still ongoing. On one hand, the view in favor of insider trading claims that as insiders' private information is incorporated into stock prices via their trading, insider trading enhances the ability of stock prices to capture a firm's true underlying value (Manne, 1966; Carlton and Fischel, 1982). Furthermore, a few studies argue that insider trading plays an important role in contracting with corporate insiders (Bebchuk and Fershtman, 1994; Roulstone, 2003; Denis and Xu, 2013) since substantial benefits derived from insider trading serves as a supplementary incentive for corporate insiders to engage more actively in value-creating activities. This implies that insider trading is beneficial or its prohibition is costly in that it creates incentives for insiders to compensate for their opportunity losses arising from not being able to trade *ex ante* on their private information.

On the other hand, the argument against insider trading is based on the fact that insider trading reduces the gains available to outside investors by making the private information acquisition costly (Fishman and Hagerty, 1992; Khanna, Slezak, and Bradley, 1994), which in turn discourages outside investors to participate in stock markets and deteriorates stock market liquidity. Consistent with this view, Bhattacharya et al. (2000) provide evidence that insider trading by Mexican companies causes stock prices to fully reflect corporate news before its public release, suggesting that insiders enjoy trading gains based on their private information at the expense of outside investors who rely on public information for their trading decisions. Along this line of research, several studies provide supportive evidence for the benefits of insider trading prohibitions. For instance, Bushman, Piotroski, and Smith (2006) find that restrictions on insider trading encourage information acquisition by outsiders through an increase in analyst coverage. Fernandes and Ferreira (2009) find that stock price becomes more informative after a

country enforces its insider trading laws, thereby lowering the cost of equity capital (Bhattacharya and Daouk, 2002).

In addition, executives are able to appropriate part of returns to corporate investments through insider trading at the expenses of outsider shareholders, which adversely affects the efficiency of corporate investments (Levmore, 1982; Manove, 1989; Ausubel, 1990).[9] Also, to the extent that insider trading prohibitions attract more informed investors to the market and facilitate information-based trading in the market, stock prices become more informative and are likely to contain information about firms' future prospect, of which insiders are unaware. In such an environment, corporate insiders can also learn more about future investment opportunity when insider trading is prohibited than when it is allowed. In line with this argument, Chen et al. (2012) provide evidence that the initial enforcement of insider trading laws enhances the sensitivity of corporate investment to stock prices, which eventually improves firms' operating performance.

Our study adds to the above debate about the benefit and cost sides of insider trading by showing that restrictions on insider trading are beneficial to shareholders as these restrictions weaken managerial incentives to withhold negative information within a firm, and discourage managers to divert corporate resources to inefficient investments, which in turn reduces the likelihood of observing extreme negative outliers in firm-specific return distribution or simply stock price crash risk.

Our paper is also related to Marin and Olivier (2008), who theoretically and empirically demonstrate why insider selling can lead to stock price crashes in the U.S. market. In their model,

[9] For example, Manove (1989) theoretically shows that in the presence of pervasive insider trading, outside investors who have no access to inside information are mostly like to be offered overvalued shares. Sophisticated investors taking account of adverse selection are unwilling to support corporate investments. Consequently, corporate investments tend to fall below the optimal level.

shareholdings by insiders are constrained by certain "floor" value. When outside investors are provided with a signal derived from insiders' selling that they are in possession of bad news, the uncertainty of how bad the news really is can increase the likelihood of stock price crashes.[10] Different from Marin and Olivier (2008), our study uses *exogenous* changes in insider trading regulations in an international setting. This research setting allows us to observe more cross-country difference in the insider trading regulatory environments and to establish a meaningful causal effect of insider trading restriction on stock price crash risk. It is difficult to make a meaningful inference about the causal effect in a single country setting because the firm-level relation between inside trading and bad news hoarding (and thus crash risk) within a single country is likely to be endogenous.

3. Sample, Measurement of Major Variables, and Descriptive Statistics

3.1. Sample and Data

We collect data on stock prices, stock returns and, exchange rates from Datastream, and extract financial statement variables from Worldscope, for all companies from 48 countries over the period from 1982 to 2006.[11] We then exclude firm-years with negative sales (Fernandes and Ferreira, 2009) and firms with less than 26 weeks of stock trading data in a given year (Kim, Li, and Zhang, 2011b). The years of enactment and enforcement of insider trading laws are obtained from Bhattacharya and Daouk (2002).

Moreover, we extract macroeconomic data for each country from World Bank WDI database and IMF International Financial Statistics. Other data sources include IBES where we obtain the analyst coverage data, and Bekaert, Harvey, and Lundblad (2005), Djankov et al. (2008), and La

[10] According to Marin and Olivier's (2008) theory, intense insider purchases does not generate a jump in the stock price since there is no "ceiling" constraint on insiders' holdings.
[11] Our sample selection of 48 countries is based on Fernandes and Ferreira (2009). We end our sample period in 2006 because the inside trading law data is only available until 2006 according to Denis and Xu (2013).

Porta, Lopez-De-Silanes, and Shleifer (2006), where we obtain the country-level financial liberalization year, anti-self-dealing index, and disclosure index, respectively.

3.2. Measuring stock price crash risk

To estimate stock price crash risk, we first calculate firm-specific weekly returns for each firm in a given year. Specifically, we use weekly stock return data (Wednesday to Wednesday) to estimate the following expanded market model that regresses stock returns of firms on the local and U.S. weekly market index returns from week t-2 to t+2, including two lag weeks and two lead weeks, as in Jin and Myers (2006):

$$\begin{aligned} r_{i,t} = {} & \alpha_i + \beta_{1,i} r_{m,j,t} + \beta_{2,i}[r_{US,t} + EX_{j,t}] + \beta_{3,i} r_{m,j,t-1} + \beta_{4,i}[r_{US,t-1} + EX_{j,t-1}] \\ & + \beta_{5,i} r_{m,j,t-2} + \beta_{6,i}[r_{US,t-2} + EX_{j,t-2}] + \beta_{7,i} r_{m,j,t+1} + \beta_{8,i}[r_{US,t+1} + EX_{j,t+1}] \\ & + \beta_{9,i} r_{m,j,t+2} + \beta_{10,i}[r_{US,t+2} + EX_{j,t+2}] + \varepsilon_{i,t} \end{aligned} \quad (1)$$

where, $r_{i,t}$ is the return on stock i in week t (in market j); $r_{m,j,t}$ is the MSCI country-specific market index return or the country index return compiled by Datastream in week t; $r_{US,t}$ is the U.S. market index return (a proxy for the global market); $EX_{j,t}$ is the change in country j's U.S. dollar exchange rate; and $\varepsilon_{i,t}$ represents unspecified factors. The expression $r_{US,t} + EX_{j,t}$ translates U.S. market returns into local currency units. We allow for nonsynchronous trading by including lead and lag terms for the market index returns (Dimson, 1979). The firm-specific weekly return for firm i in week t, denoted by $W_{i,t}$, is defined as the natural logarithm of one plus the residual return ($\varepsilon_{i,t}$) from the above equation.

Following prior studies, we measure stock price crash risk in two different ways (e.g., Chen, Hong, and Stein, 2001; Kim, Li, and Zhang, 2011a, b; DeFond et al., 2012).[12] Our first measure

[12] In our country-level analysis, we do not use an alternative measure of the likelihood of crashes, which is defined as an indicator variable for firms experiencing one or more crash events during a year, because the aggregation of the firm level *dichotomous* measure to the country level has less power in detecting the hypothesized association between insider trading law and crash risk compared to the other two *continuous* measures of crash risk. In addition,

of crash risk is negative conditional firm-specific weakly return skewness (*NCSKEW*). Specifically, we calculate *NCSKEW* for a given firm in a fiscal year by taking the negative of the third moment of firm-specific weekly returns, $W_{i,t}$, during the same fiscal year period and dividing it by the standard deviation of firm-specific weekly returns raised to the third power. The negative sign creates a variable that increases as the return distribution becomes increasingly negatively skewed. For each firm *i* in year *t*, we obtain *NCSKEW* as follows:

$$NCSKEW_{i,t} = -[n(n-1)^{3/2}\sum W_{i,t}^3]/[(n-1)(n-2)(\sum W_{i,t}^2)^{3/2}] \quad (2)$$

Our second measure of stock price crash risk is the down-to-up volatility of crash risk, denoted by *DUVOL*. Specifically, for each firm in a given fiscal year, we separate out all weeks with firm-specific weekly returns ($W_{i,t}$) below the annual mean ("down" weeks) from those with firm-specific returns above the annual mean ("up" weeks), and calculate the standard deviation for each of these subsamples separately. We then compute the *DUVOL* measure using the natural logarithm of the ratio of the standard deviation on down weeks to the standard deviation on up weeks. For each firm *i* in year *t*, *DUVOL* is computed as:

$$DUVOL_{i,t} = Ln[\sqrt{\sum_{Down} W_{i,t}^2/(n_d-1)} \,/\, \sqrt{\sum_{Up} W_{i,t}^2/(n_u-1)}] \quad (3)$$

where n_d and n_u are the number of down and up weeks, respectively.

To conduct a country-level analysis, we aggregate the two crash risk measures across firms for each country in each year. We then use the mean values of crash risk measures as the main dependent variables. To alleviate the time-trend effect on crash risk, we trend-adjust the crash risk variables for our analysis. Specifically, we use six countries, i.e., Brazil, Canada, France, Singapore, UK, and US, that have enforced their insider trading laws prior to 1982, the start of

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the indicator measure computes the left tail risk independently of the right tail risk and thus does not capture the asymmetry in the return distribution.

our sample period, as the benchmark to adjust for the time trend. The trend-adjusted *NCSKEW* and *DUVOL* are defined as the raw *NCSKEW* and *DUVOL* in each country-year less their average values in the same year reported by the benchmark countries, respectively.

3.3. Country-level control variables

To isolate the effect of insider trading law enforcement on stock price crash risk, we control for an array of country-level characteristics that are known to have an impact on crash risk according to previous studies. We first control for a few macroeconomic conditions that may be associated with crash risk in a country based on previous literature, e.g., Povel, Singh, and Winton (2007) and Barro and Ursúa (2009). Specifically, we include the log of a country's gross domestic product per capita in U.S. dollars (*GDP*) and the variance of annual GDP per capita growth in the past five years (*VGDP*) as proxies for the level of economic development and macroeconomic risk, respectively.

Giroud and Mueller (2010, 2011) find that product market competition plays an important role in disciplining managers' discretionary behaviors. Accordingly, we control for the industry Herfindahl index (*IHERF*) calculated using two-digit SIC industry sales for each country in each year and the firm Herfindahl index (*FHERF*) calculated using individual firm sales for each country in each year. Also included is the log of the number of listed firms in each country in a given year (*NSTOCK*) because stock price crash risk is like to be correlated with stock market size. Finally, we include an indicator (*LIB*) to control for the potential effect financial market liberalization in a country on stock price crash risk. Specifically, *LIB* takes that the value of one in the year of the country's official financial liberalization and thereafter, and zero otherwise.

3.4. Descriptive statistics

Table I presents summary statistics of all variables used in this study by country. There are a

total of 963 country-year observations in the 48 sample countries. Although insider trading laws were enacted in all 48 countries, only 6 countries enforced their insider trading laws before our sample period. Out of the 48 sample countries, 29 countries enforced their insider trading laws during the sample period, and 13 countries never enforced their insider trading laws by the sample-period end of 2006.

[Insert Table I here]

The mean (median) values of the two country-level raw (unadjusted) crash risk measures, *NCSKEW_raw* and *DUVOL_raw*, are -0.064 (-0.066) and -0.077 (-0.080), respectively. Furthermore, we observe that the mean value of *NCSKEW_raw* (*DUVOL_raw*) varies widely across countries ranging from a minimum value of -0.190 (-0.145) in Italy to a maximum value of 0.148 (0.042) in Venezuela, with a standard deviation of 0.142 (0.083). The similar inference is drawn upon the trend-adjusted crash risk variables. However, due to the exclusion of the 6 benchmark countries that we use to trend-adjust the crash risk variables, the sample size is reduced to 823 country-year observations. The statistics of other variables are comparable to those in previous studies, (e.g., Jin and Myers, 2006; Fernandes and Ferreira, 2009).

4. Main Results

4.1. Baseline regressions

We employ a difference-in-differences approach to test the change of stock price crash risk across countries with and without the enforcement of insider trading laws before and after the enforcement as follows:

$$Crash_{i,t} = b_0 + b_1\, ENFORCE_{i,t} + Controls_{i,t} + \mu_{i,t} \quad (4)$$

where $Crash_{i,t}$ refers to the two trend-adjusted stock price crash risk measures (*NCSKEW* and *DUVOL*) for country *i* in year *t*. Our key explanatory variable of interest, *ENFORCE*, is a

dummy variable that takes the value of one in the year of the country's first enforcement case on insider trading and thereafter, and zero otherwise. $Controls_{i,t}$ represents the set of control variables defined in Section 3.3. The standard errors of the estimated coefficients allow for clustering of observations by country.

[Insert Table II here]

The results of our baseline regressions using Eq. (4) are presented in Table II. The first two columns of Table II report the results of the ordinary least squares (OLS) pooling regressions without any controls. We find that *ENFORCE* is negatively and significantly associated with both measures of crash risk, *NCSKEW* and *DUVOL*, with *t*-statistics of -2.78 and -3.24, respectively. In columns (3) and (4), we control for country fixed effects and find that the coefficients on *ENFORCE* is still negative and highly significant at less than the 1% level, suggesting that the results in columns (1) and (2) are not simply driven by countries' time-invariant characteristics. In fact, we observe that both the magnitude and the statistical significance of the coefficients on *ENFORCE* increase after country fixed effects are included in the regressions.

In columns (5) and (6), we further control for more country-level dynamic variables, namely, GDP per capita in U.S. dollars (*GDP*), GDP variance (*VGDP*), the industry and firm Herfindahl index (*IHERF* and *FHERF*), the number of listed firms (*NSTOCK*), and the financial liberalization indicator (*LIB*). The results confirm a significant negative relation between stock price crash risk and the enforcement of insider trading laws. In terms of economic significance, after the enforcement of insider trading laws, crash risk measures, *NCSKEW* and *DUVOL*, declines by 0.074 and 0.041, which are approximately 53% and 49% of their standard deviations,

compared with those in countries that have never enforced their insider trading laws.[13] Hence the economic impact of the legal restriction on insider trading is material. Taken together, the baseline regression results in Table II are consistent with our prediction that firms' stock price crash risk declines after initial enforcement of insider trading laws.

With regard to the country-level controls, we find that *GDP* is negatively, but the variance of GDP growth is positively, related to crash risk, indicating that a country's economic development and stability have a negative impact on the probability of firms' stock price crashes. We also find that countries with higher Herfindahl concentration indices (*FHERF*) are associated with higher crash risk, suggesting that firms' stock prices are more likely to crash in countries where the economy is dominated by a few large companies.

4.2. Event study

While the baseline regression analysis above shows that firms' stock price crash risk becomes significantly lower after a country initially enforces its insider trading laws, this analysis does not focus directly on the changes in firms' crash risk around the enforcement events. In this section, we perform an event study analysis to compare the average levels of stock price crash risk three years before and three years after a country's initial enforcement of insider trading laws.[14] Our analysis in this section is based on a reduced sample of 25 countries that initially enforced their insider trading laws during the sample period.[15]

[Insert Table III and Figure 1 here]

[13] The inference is drawn upon the regression coefficients on *ENFORCE* reported in columns (5) and (6). As shown in Table I, the standard deviations of *NCSKEW* and *DUVOL* across the country-year observations are 0.139 and 0.083, respectively. Since *ENFORCE* is a dummy variable, its coefficient captures the extent of crash risk decline before and after the enforcement.

[14] We include the year of the enforcement in our analysis, but our results are robust to the exclusion of this year.

[15] Among the 48 sample countries, 29 countries enforced their insider trading laws during the sample period. However, stock price data before the enforcement events are unavailable for Czech Republic, Israel, Peru, and Poland. Hence the sample for the event study is reduced to 25 countries.

We first conduct a univariate test and report the results in Table III. We find that 19 out of 25 countries experience a decline in stock price crash risk from three years before to three years after the enforcement of insider trading laws.[16] We also test the mean differences in our crash risk variables (*NCSKEW* and *DUVOL*) between the pre- and post-enforcement events and report the levels of significance in columns (6) and (7). The *t*-test indicates that *NCSKEW* and *DUVOL*, on average, become significantly lower around the insider trading law enforcement with *p*-values below 0.01. Figure 1 plots the data pattern in Table III.

[Insert Table IV here]

We then perform a multivariate regression analysis for the sample of 25 countries that experience the insider trading law enforcement during the sample period. Table IV presents the results. We find that the coefficients on *ENFORCE* are negative and significant across all six columns with *t*-values ranging from -2.02 to -2.58. The coefficients are similar to but are smaller and less significant than those reported in the baseline regression analysis in Table II. For example, in column (5) of Table IV, where *NCSKEW* is used as the dependent variable, the coefficient on *ENFORCE* is -0.062 with a *t*-statistic of -2.34, while it is -0.074 with a *t*-statistic of -3.60 in column (5) of Table II. The smaller and less significant effect of the enforcement in the event study setting suggests that the impact of insider trading restrictions on firms' stock price crash risk is not achieved immediately and that only part of the effect is realized in a limited period of three years after the enforcement. Collectively, the empirical evidence from the

[16] The results indicate that trend-adjusted stock price crash risk increases in 6 countries/regions, i.e., Chile, Hong Kong, Japan, Malaysia, Sweden, and Thailand, after the enforcement of insider trading laws. Except for Japan, whose stock market crashed in the same year as the enforcement event, the possible reason for the other three southeastern Asian countries such as Hong Kong, Malaysia, and Thailand is that these countries are likely to be affected by the Asian financial crisis that occurred following the initial enforcement of their insider trading laws in the late 1990s.

event study again strongly supports our conjecture that firms in a country are less likely to experience stock price crashes after this country starts to enforce its insider trading laws.

5. Role of Institutional Characteristics

In this section, we further explore how the effect of insider trading restriction on reducing stock price crash risk varies across countries, depending on different institutional characteristics. Our analyses focus on four dimensions of institutional characteristics, including investor protection, financial market liberalization, financial disclosure, and product market competition.[17] We report the results in Table V, where the regressions include all control variables in Eq. (4). For brevity, we do not tabulate the coefficients on these variables. Panel A of Table V shows the heterogeneity in results across countries using the baseline regressions and Panel B shows the results of tests conducted using the event study approach, respectively.

5.1. Investor protection

Insider trading is less likely to occur in countries where investors' rights are well protected. As a result, in countries with stronger investor right protection, the incremental effect of insider trading restrictions on preventing corporate insiders from withholding bad news and engaging in inefficient investments can be relatively small.[18] We hence expect that the effect of insider trading restrictions on stock price crash risk is more evident in countries with poorer investor protection than in countries with stronger investor protection.

[17] We also follow Fernandes and Ferreira (2009) and Bushman, Piotroski, and Smith (2005) by separating the total sample countries into groups of emerging and developed markets and examining the effect of the enforcement in the two groups. Empirical results show that the effect of insider trading restrictions on firms' stock price crash risk is negative and significant in both groups, and the effect between the two groups is not statistically different.

[18] Bushman, Piotroski, and Smith (2005) find that the enforcement of insider trading laws is associated with a smaller increase in analyst following than in countries with stronger investor protection. In contrast, Fernandes and Ferreira (2009) show that the improvement of stock price informativeness after the initial enforcement of insider trading laws is restricted only in countries where investors' private property rights are well protected.

We use the anti-self-dealing index (*ASDI*) created by Djankov et al. (2008) as a proxy for investor protection.[19] As argued by Djankov et al. (2008), *ASDI* captures the legal protection of minority shareholders against expropriation by corporate insiders. Therefore, *ASDI* is a desirable measure of investor protection in the context of our study.

[Insert Table V here]

We reestimate the regressions in Tables III and IV by including the interaction of *ASDI* and *ENFORCE*. Since the standalone effect of *ASDI* is absorbed by country fixed effects, we do not include *ASDI* itself as an independent control. The results are reported in columns (1) and (2) of Panels A and B in Table V. We find that the coefficients on the interaction term are positive and significant with *t*-statistics ranging from 2.61 to 3.01. The finding suggest that the effect of insider trading restrictions on reducing stock price crash risk is less pronounced in countries with stronger investor protection than in counties with poorer investor protection. More importantly, the finding that our results are dependent on the level of investor protection suggests that insider trading restrictions affect stock price crash risk at least partially through the channel of protecting outside investors from corporate insiders' expropriation.

5.2. Financial disclosure

Based on Jin and Myers' (2006) theoretical analysis, HMT empirically show that opaque firms are more prone to stock price crashes. To the extent that opaqueness incentivizes managers to hide unfavorable corporate information and invest sub-optimally, rules and regulations that require more financial disclosure can serve as a tool to effectively mitigate this agency problem.

[19] Note that the index of Djankov et al. (2008) is measured based on legal rules prevailing in 2003. Although prior studies suggest that investor protection institutions evolve very slowly over time, the long sample period of our study (1982-2006) may make the preexisting level of investor protection quite different from what is captured by Djankov et al.'s (2008) index. We therefore employ several alternative measures of investor protection, including the origin of laws and the comprehensive index of shareholder rights developed by La Porta et al. (1998). Untabulated results show that using alternative measures of investor protection does not qualitatively alter our results.

Given the lower chance of managerial misbehavior in a more transparent financial disclosure environment, we expect the negative impact of insider trading restrictions on crash risk to be stronger in countries with higher disclosure requirements. We use the disclosure requirements index (*DISC*) constructed by La Porta, Lopez-de-Silanes, and Shleifer (2006) as a proxy for disclosure transparency. In particular, this index measures the breadth of disclosure requirements regarding prospectus, compensation of corporate insiders (e.g., directors and key executives), ownership structure and inside ownership, contracts irregularity, and insider transactions.

We then reestimate the model in Eq. (4) after adding the interaction term of disclosure and enforcement indicators, i.e., *DISC*×*ENFORCE*. Since the country fixed effects take account of the independent effect of *DISC*, we do not include it as a separate control. Results reported in columns (5) and (6) of both Panels A and B show that the interaction of *DISC* and *ENFORCE* has a positive and significant association with both crash risk measures with *t*-statistics of 2.60 to 2.78. These results are consistent with the notion that more disclosure regarding key corporate insiders mitigates their incentives to trade on private information as well as the associated adverse effect on stock price crash risk.

5.3. Financial market liberalization

Liberalization of equity markets opens a country's financial market to the free flow of capital, which promotes the improvement of a country's corporate governance since foreign investors may demand better governance to protect their investments (Bekaert, Harvey, and Lundblad, 2005). Furthermore, as shown in Bushman, Piotroski, and Smith (2005), financial market liberalization also encourages analyst coverage, which improves transparency of firms' information environment. As a consequence, the disciplinary effect of insider trading restrictions on managers' bad news hoarding behavior and inefficient investment decision can be partially

substituted by the reform on financial market liberalization. We thus expect that the effect of insider trading restrictions on crash risk is less pronounced in countries where financial markets have already been liberalized.

We include the interaction of the financial liberalization indicator, *LIB*, and *ENFORCE* in Eq. (4) and reestimate the regressions. Consistent with our expectation, the results presented in columns (3) and (4) of Panels A and B indicate that the coefficients on the interaction term are all positive and significant with *t*-statistics from 1.95 to 4.64, suggesting that the enforcement of insider trading laws plays a more important role in countries where the financial markets are not yet liberalized.

5.4. Product market competition

It has been well documented in the literature that competition from product market can work as a substitutive mechanism to motivate managers to maximize long-term firm value by forcing poorly operating firms out of the business (e.g., Machlup, 1967). In line with this notion, Giroud and Mueller (2010, 2011) provide evidence that the value-enhancing role of the corporate control market is concentrated only in firms facing low product market competition, suggesting that product market competition serves as a substitute to existing corporate governance. Therefore, we expect that insider trading restrictions, as an important governance mechanism to constrain managers' self-dealing behavior, have a stronger impact on reducing crash risk in countries with a less competitive product market.

To test our conjecture, we include the interaction term of *ENFORCE* and the firm Herfindahl index, *FHERF*, as an additional explanatory variable in regressions.[20] The regression

[20] We also use the industry Herfindahl index, *IHERF*, as an alternative measure for product market competition and find similar results. However, since not all countries in our sample have a diverse industry sectors as U.S., *IHERF* may not be a good proxy for the product market competition in the context of our cross-country study.

results are presented in columns (7) and (8) in Panels A and B of Table V. We observe that except for the regression in the event study with *NCSKEW* as the dependent variable, the coefficient on *ENFORCE*×*FHERF* is negative and significant at less than the 5% level across the columns. This finding suggests the effect of initial enforcement of insider trading laws on reducing stock price crash risk is more pronounced in countries where the business is more likely to be concentrated in a few large companies (i.e., where the product market is less competitive).

6. Further Analysis

6.1. Controlling for potential omitted variables

While we have controlled for a standard set of variables that can affect stock price crash risk and insider trading regulations in our analysis based on previous studies, the negative association between the insider trading enforcement and crash risk can be driven by some potential omitted variables. To address this concern, we explicitly describe and control for these possible omitted variables and tabulate the results in Table VI.

[Insert Table VI here]

We first consider variables related to firms' information environment since managerial incentives that cause stock to be crash-prone are likely to be affected by the information environment. Prior literature documents a drastic change in a country's information environment after the country enforces its insider trading laws. For example, Fernandes and Ferreira (2009) find that initial enforcement of insider trading laws significantly improves stock price informativeness. Bushman, Piotroski, and Smith (2005) find that analyst following increases after the enforcement of insider trading laws. We hence control for the stock price informativeness and analyst coverage in regressions. Following Fernandes and Ferreira (2009), we define stock price informativeness (*INFORM*) as the log of the ratio of idiosyncratic volatility

(i.e., the variance of residuals in Eq. (1)) to total stock price volatility for each firm in each year. As in Bushman Piotroski, and Smith (2005), analyst coverage (*ANALYST*) is calculated as the log of mean number of analysts providing a forecast for each firm listed in the IBES for each country in each year.

The results are reported in columns (1) and (2) of Table VI. Consistent with Jin and Myers (2006), the coefficient on *INFORM* is negative but statistically insignificant. The effect of *ANALYST* on crash risk, though negative, is also statistically insignificant at conventional levels. More importantly, after controlling for the two variables related to firms' information environment, *ENFORCE* is still negatively and significantly associated with crash risk, suggesting that our main finding regarding the effect of the insider law enforcement is robust.

In addition to stock price informativeness, we consider earnings management as an alternative measure for information opacity.[21] As in HMT and Kim, Li, and Zhang (2011b), we measure earnings management (*DISACC*) using absolute value of discretionary accruals, where discretionary accruals are estimated from the modified Jones model (Dechow, Sloan, and Sweeney, 1995). We then average this firm-level variable across firms for each country in each year. Untabulated results show that our main findings are not affected by the inclusion of *DISACC* as an additional control.[22]

Second, we consider the potential impact of liquidity on our analysis. Liquidity in financial markets is a crucial factor that influences risk-sharing among investors and thus the cost of capital (e.g., Amihud and Mendelson, 1986; Brennan and Subrahmanyam, 1996). Bhattacharya and Daouk (2002) document that stock market liquidity is greatly enhanced after a country

[21] Zhang and Zhang (2012) find that first-time enforcement of insider trading laws limits corporate insiders' earnings management for the purpose of increasing their informational advantage to extract trading profits.

[22] Since discretionary accruals can only be estimated for industrial firms, we exclude financial firms when aggregating the firm-level discretionary accruals to the country level.

enforces its insider trading laws. We thus explicitly control for stock market liquidity in the regressions to examine whether our main results are driven by the omitted variable related to liquidity. Following Bhattacharya and Daouk (2002), we measure liquidity (*LIQ)* as the ratio of trading volume to market capitalization for each country in each year.[23]

Third, we also consider the possible effect of actual financial and trade flows. Although we control for the effect of financial market liberalization, other macroeconomic policies that affect actual financial and trade flows may also drive the observed relation in our main results. For example, the government in a country may enhance the openness of its capital and product market to attract foreign investors and enforce its insider trading laws to please these investors. Given that crash risk is negatively associated with the quality of corporate governance (Andreou et al., 2013), the negative effect of insider trading restrictions on crash risk is possibly driven by financial and trade flows. In our regressions, we thus control for two variables that capture this effect, i.e., market's trade openness (*OPEN*) and foreign direct investment (*FDI*). *OPEN* and *FDI* are defined, respectively, as the amount of foreign direct investment in a country scaled by the country's GDP and the sum of imports plus exports in a country scaled by the country's GDP.[24]

Finally, Diamond and Verrecchia (1987) and Beber and Pagano (2013) provide evidence that when short selling is allowed and practiced, negative information is incorporated into stock prices faster, which results in a reduction in stock price crash risk. Since a number of countries lift the bans on short selling during 1990s, to isolate the effect of insider trading law enforcement on crash risk, we include an indicator to denote the feasibility of short-selling (*SHORT*).

[23] In an untabulated test, we replace *LIQ* with detrened stock trading volume as a proxy for investor heterogeneity following Chen, Hong, and Stein (2001) and HMT, and find that the inclusion of detrened stock trading volume does not alter our results.

[24] As indicated earlier, the data of *FDI* and *OPEN* are collected from the World Bank WDI database. Since the database does not cover Taiwan, the sample size for this analysis is reduced to 41 countries.

Specifically, *SHORT* takes a value of one if short selling is infeasible in a country, and zero otherwise, based on Charoenrook and Daouk (2009).[25]

The results are reported in columns (3) and (4) of Table VI. We find that the effect of insider trading restriction on stock price crash risk is still negative and significant at less than the 1% level, indicating that the inclusion of liquidity, foreign direct investment and trade openness, and short selling restrictions does not affect our main findings. Moreover, we find that *LIQ* has a negative and significant impact on *NCSKEW* and *DUVOL*, suggesting that in markets with higher liquidity, firms are less likely to experience stock price crashes. Consistent with Diamond and Verrecchia (1987) and Beber and Pagano (2013), *SHORT* has a significant and positive effect on crash risk, suggesting that bans on short selling lead to an increase in stock price crash risk. However, foreign direct investment and trade openness are not significantly associated with crash risk.

In the last two columns of Table VI, we incorporate all potential omitted variables described above together with the main country-level control variables and country fixed effects in one regression based on Eq. (4). With these controls, the regression coefficients on *ENFORCE* remain negative and highly significant at less than the 1% level. Overall, we provide robust evidence in Table VI that the negative effect of insider trading enforcement on stock price crash risk is unlikely to be derived from an omitted-variable bias.

6.2. Test of reverse causality

Our main results show that there is a negative association between insider trading restrictions and stock price crash risk. This finding can be viewed as supportive evidence for our

[25] Charoenrook and Daouk (2005) construct two indicators to denote the legality and feasibility of short selling around the world, respectively. Our results are not affected if we include the other indicator that denotes the legality of short selling as an additional control. In most cases of our analysis, the effect of short selling feasibility dominates that of short selling legality.

argument that the initial enforcement of a country's insider trading laws constrains managers' incentives, opportunities, and abilities to withhold bad news by making their trading on inside information costly and risky. However, the finding is also consistent with the view that a country starts to enforce its insider trading laws to mitigate stock price crash risk, simply because the widespread insider trading in the country exacerbates stock price crash risk. If our main findings are driven by the reverse causation, we should observe that it is the increase in stock price crash risk that leads to the initial enforcement of insider trading laws, not vice versa.

[Insert Table VII here]

To address the above issue of potential reverse causality, we follow Bertrand and Mullainathan (2003) to examine the dynamics of stock price crash risk and its changes surrounding the enforcement events. Specifically, we construct five year indicators, namely, $YEAR_{t-2}$, $YEAR_{t-1}$, $YEAR_t$, $YEAR_{t+1}$, and $YEAR_{t+2}$, to indicate the relative years around the initial enforcement of insider trading laws, where *t* denotes the enforcement year of insider trading laws. We then replace *ENFORCE* with these five indicators in our baseline regressions.

The results are presented in Table VII. We find that the coefficients on $YEAR_{t-2}$ and $YEAR_{t-1}$ are statistically insignificant, suggesting that there is no significant increase in crash risk prior to the enforcement of insider trading laws. More importantly, we find that the coefficients on $YEAR_t$, $YEAR_{t+1}$, and $YEAR_{t+2}$ are negative and significant, indicating that from the year of initial enforcement onwards, firms' stock price crash risk starts to decline substantially. Interestingly, we find that the coefficients on $YEAR_t$, $YEAR_{t+1}$ and $YEAR_{t+2}$ are gradually and monotonically increasing, suggesting that the impact of insider trading enforcement on mitigating crash risk is long lasting.[26] These results also confirm our previous finding that the effect of insider trading

[26] As shown in column (1) of Table VII, where *NCSKEW* is the dependent variable of crash risk, the coefficient on $YEAR_{t+2}$ is twice as large as those on $YEAR_t$ and $YEAR_{t+1}$, suggesting that only approximately half of the effect is

restrictions is stronger and more significant in the baseline regression analysis than in the event study where three-year window before and after the enforcement event year is employed. Overall, our analysis suggests that it is insider trading enforcement that leads to a reduction in stock price crash risk, not vice versa.

6.3. Additional sensitivity tests

We conduct a variety of additional sensitivity tests to further verify the validity of our results. The results are reported in Table VIII. Specifically, we show that our main finding reported in Table II is robust to the following specifications: 1) using raw values instead of trend-adjusted values of *NCSKEW* and *DUVOL* as measures of stock price crash risk;[27] 2) using median rather than mean values of firm-level *NCSKEW* and *DUVOL* to construct the country-level stock price crash risk; 3) removing the country-years prior to 1990, as Fernandes and Ferreira (2009) point out that Datastream/Worldscope largely expanded its country coverage since 1990; 4) removing firm-years during the Asian financial crisis (1997-1998), a period that follows a significant number of enforcements occurred in 1995-1996; 5) excluding Japan, as this country experienced a market crash and enforced its insider trading laws in the same year; 6) excluding firms in financial industries (SIC: 6000-6999) when estimating country-level stock price crash risk variables; 7) excluding firms with total sales less than 10 million US dollars when estimating country-level stock price crash risk variables; 8) excluding American Depository Receipts (ADRs) and Global Depository Receipts (GDRs) when calculating country-level stock price crash risk; and 9) using country random effects instead of country fixed effects when estimating the regressions. As shown in Panels A through H of Table VIII, we find that the

realized in years t and t+1, and another half is realized in later years from three years after the enforcement. Similar inference is drawn if *DUVOL* is used as the crash risk variable.

[27] To control for the time trend, we include year dummies in the regressions for this test.

estimated coefficients of *ENFORCE* are all qualitatively identical to those reported in Table II, suggesting that our main regression results are robust to a variety of sensitivity checks.

[Insert Table VIII here]

We next conduct a firm-level analysis to further support the robustness of our main findings at the country level. Following previous studies, we include an array of firm-level control variables in regressions. Specifically, we control for detrened trading volume (*DTURN*) as a proxy for investor heterogeneity (Chen, Hong, and Stein, 2001) and lagged *NCSKEW* crash risk measure to capture the potential persistence of the third moment of stock returns. We also control for standard deviation of firm-specific weekly stock returns (*SIGMA*) and the average of firm-specific weekly stock returns (*RET*) over the fiscal year since Chen, Hong, and Stein (2001) find that more volatile stock and stocks with high past returns are more likely to crash. Following HMT and Kim, Li, and Zhang (2011a, b), we also include firm size (*SIZE*), market-to-book ratio (*MB*), financial leverage (*LEV*), operating performance measured as income before extraordinary items divided by lagged total assets (*ROA*), and the absolute value of discretionary accruals (*DISACC*) as defined in Section 6.1. Finally, we include industry and country fixed effects to capture the time-invariant industry and country characteristics.[28] All variables are lagged for one year, and the standard errors of the estimated coefficients allow for clustering of observations by country.

[Insert Table IX here]

Since *DISACC* can only be estimated for industrial firms, we separately analyze industrial firms and financial firms following DeFond et al. (2012) and report the results in columns (1) to (2) and columns (3) to (4) of Table IX, respectively. We find that *ENFORCE* has a negative and

[28] In an untabulated test, we replace industry and country fixed effects with firm fixed effects and find qualitatively similar results.

significant impact on stock price crash risk at less than the 5% level for both subsamples of industrial and financial firms. The signs of the coefficients on control variables are basically consistent with previous literature. The firm level analysis further supports the robustness of our findings at the country level.

Finally, though not tabulated for brevity, we also conduct a test to compare the effects of the existence versus the enforcement of insider trading laws on stock price crash risk. Specifically, we include a variable of insider trading law enactment (*EXIST*), together with the enforcement variable (*ENFORCE*) in our regressions. *EXIST* is an indicator variable that equals one in the year of a country's first enactment of insider trading laws and thereafter, and zero otherwise. With the inclusion of *EXIST*, the coefficient on *ENFOCE* remains negative and significant at less than the 1% level. However, the coefficient on *EXIST* is statistically insignificant, suggesting that the enforcement plays a dominant role, over and beyond the law enactment, in limiting managerial insider-trading incentives and reducing stock price crash risk. This is consistent with the finding of Bhattacharya and Daouk (2002, 2009) that it is the enforcement of insider trading laws rather than its enactment that matters.

7. Summary and Conclusion

In this study, we exploit cross-country and intertemporal variation in stock price crash risk using a large sample of 48 countries over the period of 1982-2006, during which the majority of our sample countries started to enforce their insider trading laws, to examine the effect of insider trading restrictions on stock price crash risk. We find strong evidence that stock price crash risk substantially declines after initial enforcement of insider trading restrictions. We interpret this finding as supportive evidence for our argument that insider trading restrictions impair managers'

incentives to withhold bad news from public disclosure by making their trading on inside information costly and risky.

We also investigate the role of institutional characteristics in determining the effect of insider trading restrictions on stock price crash risk. Briefly, our results show that the effect of the enforcement of insider trading laws on reducing crash risk is more pronounced for countries where investors' rights are poorly protected, less firm-specific information is required to be disclosed by law, the financial market has not yet been liberalized, or the product market is less competitive. Our findings are robust to a variety of tests on endogeneity issues and model specifications.

Collectively, our findings highlight the role of insider trading restrictions as an important legal regime in reducing crashes in asset prices. Furthermore, our study from the perspective of the third moment of stock returns (i.e., stock price crash risk) provides an explanation for the negative relation between cost of capital and insider trading enforcement documented in Bhattacharya and Daouk (2002), which complements the findings of Fernandes and Ferreira (2009) who provide similar evidence from the perspective of the second moment of stock returns (i.e., stock return variation).

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Table I: Summary statistics

The sample consists of firms jointly covered in Datastream and Worldscope between 1982 and 2006. *Starting* year is the first year when the data to calculate stock price crash risk in a country is available in the sample. *EXIST* year is the year of the country's initial enactment of insider trading law. *ENFORCE* year is the year of the country's first insider trading enforcement case. *NCSKEW_raw* is the mean value of negative skewness of firms-specific-weekly return for each country in each fiscal year. *DUVOL_raw* is the mean value of log of the ratio of the standard deviations of down-week to up-week firm-specific-weekly returns for each country in each fiscal year. *NCSKEW* and *DUVOL* are trend-adjusted *NCSKEW_raw* and *DUVOL_raw*, which are defined as *NCSKEW_raw* and *DUVOL_raw* less the average value of *NCSKEW_raw* and *DUVOL_raw* in the same fiscal year reported by the six benchmark countries that enforced insider trading restrictions prior to 1982. *GDP* is the log of the gross domestic product per capita in US dollars for each country in each year. *VGDP* is the sample standard deviation of the annual GDP per capita growth estimated using a five-year moving window for each country in each year. *IHERF* is the industry Herfindahl index calculated using two-digit SIC industry sales for each country in each year. *FHERF* is the firm Herfindahl index calculated using individual firm sales for each country in each year. *NSTOCK* is the log of the number of listed firms in each country in each year. *LIB* year is the year of the country's official financial liberalization. Dollar values are converted into 2000 constant US dollars using the GDP deflator.

Country	*#Obs*	*Starting year*	*EXIST year*	*ENFORCE year*	*NCSKEW_raw*	*DUVOL_raw*	*NCSKEW*	*DUVOL*	*GDP*	*VGDP*	*IHERF*	*FHERF*	*NSTOCK*	*LIB year*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Panel A: Summary statistics by country														
Argentina	18	1988	1991	1995	0.021	-0.026	0.067	0.044	8.89	6.48	0.17	0.12	3.50	1989
Australia	25	1982	1991	1996	0.010	-0.034	0.077	0.047	9.84	1.56	0.08	0.03	5.66	1969
Austria	25	1982	1993		-0.103	-0.086	-0.036	-0.005	9.93	1.23	0.12	0.08	4.05	1969
Belgium	25	1982	1990	1994	-0.106	-0.092	-0.039	-0.012	9.89	1.33	0.14	0.09	4.45	1969
Brazil	15	1992	1976	1978	-0.035	-0.073	-	-	8.21	2.19	0.18	0.10	4.73	1991
Canada	25	1982	1966	1976	-0.024	-0.057	-	-	9.93	1.98	0.06	0.02	6.25	1969
Chile	16	1991	1981	1996	-0.081	-0.078	-0.042	-0.011	8.49	2.52	0.10	0.04	4.72	1992
China	15	1992	1993		0.073	0.002	0.112	0.070	6.79	2.01	0.12	0.05	5.78	
Columbia	15	1992	1990		-0.064	-0.064	-0.025	0.003	7.85	2.22	0.18	0.08	3.31	1991
Czech Rep.	12	1995	1992	1993	0.114	0.025	0.136	0.083	8.69	2.52	0.20	0.09	3.70	
Denmark	25	1982	1991	1996	-0.110	-0.091	-0.043	-0.010	10.17	1.54	0.11	0.06	4.67	1969
Finland	20	1987	1989	1993	-0.030	-0.055	0.028	0.022	9.96	2.22	0.14	0.10	4.25	1969
France	25	1982	1967	1975	-0.088	-0.092	-	-	9.87	1.07	0.08	0.02	6.00	1969
Germany	25	1982	1994	1995	-0.080	-0.083	-0.012	-0.003	9.92	1.32	0.09	0.03	5.98	1969
Greece	19	1988	1988	1996	-0.079	-0.099	-0.025	-0.024	9.30	1.62	0.23	0.06	5.00	1987
Hong Kong	25	1982	1991	1994	-0.100	-0.101	-0.032	-0.020	9.99	3.47	0.13	0.07	5.38	1969
Hungary	15	1992	1994	1995	-0.074	-0.068	-0.035	0.000	8.41	2.24	0.23	0.21	3.22	1996
India	17	1990	1992	1998	-0.029	-0.075	0.009	-0.009	6.04	2.06	0.12	0.04	5.67	1992
Indonesia	17	1990	1991	1996	0.046	-0.019	0.083	0.047	6.66	3.15	0.11	0.05	5.06	1989
Ireland	25	1982	1990		-0.044	-0.063	0.024	0.018	9.76	2.23	0.17	0.12	3.99	1969
Israel	14	1993	1981	1989	-0.022	-0.058	0.015	0.008	9.83	2.22	0.12	0.06	4.51	1993

Italy	25	1982	1991	1996	-0.190	-0.145	-0.123	-0.064	9.75	1.22	0.13	0.05	5.07	1969
Japan	25	1982	1988	1990	-0.107	-0.107	-0.040	-0.027	10.43	1.46	0.05	0.01	7.57	1980
Korea	20	1987	1976	1988	-0.059	-0.091	0.000	-0.015	9.16	2.99	0.09	0.03	5.80	1992
Luxembourg	15	1992	1991		-0.046	-0.064	-0.007	0.003	10.67	2.45	0.28	0.22	3.41	1969
Malaysia	21	1986	1973	1996	-0.121	-0.109	-0.059	-0.030	8.12	3.25	0.06	0.02	5.72	1988
Mexico	19	1988	1975		-0.074	-0.077	-0.020	-0.003	8.58	2.85	0.11	0.05	4.24	1989
Netherlands	25	1982	1989	1994	-0.135	-0.116	-0.068	-0.036	9.92	1.27	0.12	0.08	4.90	1969
New Zealand	19	1988	1988		-0.064	-0.075	-0.010	0.000	9.46	2.04	0.12	0.11	4.10	1984
Norway	25	1982	1985	1990	-0.037	-0.063	0.030	0.018	10.35	1.50	0.19	0.11	4.48	1969
Pakistan	16	1991	1995		-0.141	-0.123	-0.101	-0.056	6.24	1.72	0.14	0.08	4.13	1991
Peru	13	1994	1991	1994	-0.007	-0.051	0.019	0.008	7.65	3.53	0.17	0.07	4.00	1992
Philippines	19	1988	1982		-0.018	-0.058	0.036	0.017	6.94	2.46	0.22	0.14	4.23	1991
Poland	13	1994	1991	1993	-0.022	-0.065	0.004	-0.005	8.37	2.16	0.16	0.07	4.28	
Portugal	19	1988	1986		-0.140	-0.103	-0.086	-0.029	9.22	1.98	0.18	0.06	4.15	1986
Russia	11	1996	1996		-0.078	-0.093	-0.057	-0.035	7.56	4.07	0.25	0.15	3.28	
Singapore	25	1982	1973	1978	-0.087	-0.083	-	-	9.80	3.46	0.08	0.05	4.86	1969
South Africa	25	1982	1989		-0.008	-0.046	0.059	0.035	8.05	2.05	0.09	0.04	4.99	1996
Spain	21	1986	1994	1998	-0.182	-0.132	-0.120	-0.053	9.45	1.30	0.16	0.07	4.64	1978
Sri Lanka	15	1992	1987	1996	-0.041	-0.055	-0.002	0.013	6.70	1.53	0.17	0.08	3.41	1991
Sweden	25	1982	1971	1990	-0.041	-0.066	0.026	0.014	10.11	1.55	0.11	0.06	4.86	1969
Switzerland	24	1983	1988	1995	-0.158	-0.123	-0.090	-0.041	10.42	1.49	0.12	0.06	5.05	1969
Taiwan	19	1988	1988	1989	-0.121	-0.116	-0.066	-0.041	9.44	6.87	0.20	0.09	5.07	1991
Thailand	20	1987	1984	1993	-0.079	-0.092	-0.021	-0.015	7.49	3.37	0.22	0.07	5.04	1987
Turkey	19	1988	1981	1996	-0.055	-0.088	-0.001	-0.014	8.27	4.69	0.18	0.11	4.32	1989
UK	25	1982	1980	1981	-0.124	-0.106	-	-	9.98	1.33	0.06	0.02	7.13	1969
US	25	1982	1934	1961	-0.016	-0.061	-	-	10.31	1.61	0.05	0.00	8.34	1969
Venezuela	17	1990	1998		0.148	0.042	0.186	0.108	8.50	6.26	0.25	0.18	2.73	1990
Total	963	-	-	-	-0.064	-0.077	-0.009	-0.002	9.12	2.35	0.14	0.07	4.92	-
Panel B: Summary statistics of the total sample														
Mean	-	-	-	-	-0.064	-0.077	-0.009	-0.002	9.12	2.35	0.14	0.07	4.92	-
SD	-	-	-	-	0.142	0.083	0.139	0.083	1.20	1.80	0.09	0.08	1.45	-
Q1	-	-	-	-	-0.155	-0.128	-0.090	-0.049	8.31	1.18	0.08	0.03	3.93	-
Median	-	-	-	-	-0.066	-0.080	-0.011	-0.006	9.59	1.79	0.12	0.05	4.88	-
Q3	-	-	-	-	0.027	-0.027	0.067	0.041	10.03	2.72	0.16	0.08	5.72	-
Sample size	-	-	-	-	963	963	823	823	963	963	963	963	963	-

Table II: Effect of insider trading law enforcement on stock price crash risk

The sample consists of firms jointly covered in Datastream and Worldscope between 1982 and 2006. *ENFORCE* is a dummy variable that takes the value of one in the year of the country's first insider trading enforcement case and thereafter, and zero otherwise. *NCSKEW* is the trend-adjusted mean value of negative skewness of firms-specific-weekly return for each country in each fiscal year. *DUVOL* is the trend-adjusted mean value of log of the ratio of the standard deviations of down-week to up-week firm-specific-weekly returns for each country in each fiscal year. *GDP* is the log of the gross domestic product per capita in US dollars for each country in each year. *VGDP* is the sample standard deviation of the annual GDP per capita growth estimated using a five-year moving window for each country in each year. *IHERF* is the industry Herfindahl index calculated using two-digit SIC industry sales for each country in each year. *FHERF* is the firm Herfindahl index calculated using individual firm sales for each country in each year. *NSTOCK* is the log of the number of listed firms in each country in each year. *LIB* is a dummy variable that takes the value of one in the year of the country's official financial liberalization and thereafter, and zero otherwise. Dollar values are converted into 2000 constant US dollars using the GDP deflator. The *t*-statistics in parentheses are calculated from the Huber/White/Sandwich heteroskedastic consistent errors, which are also corrected for correlation across observations for a given country. The symbols ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	NCSKEW	*DUVOL*	*NCSKEW*	*DUVOL*	*NCSKEW*	*DUVOL*
	(1)	(2)	(3)	(4)	(5)	(6)
ENFORCE	-0.047***	-0.030***	-0.080***	-0.045***	-0.074***	-0.041***
	(-2.78)	(-3.24)	(-3.75)	(-3.57)	(-3.60)	(-3.46)
GDP					-0.098	-0.059*
					(-1.62)	(-1.86)
VGDP					0.009**	0.005**
					(2.53)	(2.09)
IHERF					-0.159	-0.057
					(-1.19)	(-0.78)
FHERF					0.463***	0.225**
					(2.75)	(2.43)
NSTOCK					0.022	0.014
					(1.41)	(1.45)
LIB					-0.008	-0.014
					(-0.24)	(-0.62)
Constant	0.012	0.012	0.027***	0.018***	0.773	0.475*
	(0.90)	(1.59)	(2.78)	(3.25)	(1.56)	(1.85)
Country fixed effects	No	No	Yes	Yes	Yes	Yes
Sample size	823	823	823	823	823	823
Number of countries	42	42	42	42	42	42
R-squared	0.03	0.03	0.06	0.05	0.09	0.08

Table III: Stock price crash risk before and after the insider trading law enforcement

This table shows the average trend-adjusted stock price crash risk in the period before and after the enforcement of insider trading laws. The event window includes the three-year period before and after the enforcement and the enforcement year. *ENFORCE* year is the year of the country's first insider trading enforcement case. *NCSKEW* is the trend-adjusted mean value of negative skewness of firms-specific-weekly return for each country in each fiscal year. *DUVOL* is the trend-adjusted mean value of log of the ratio of the standard deviations of down-week to up-week firm-specific-weekly returns for each country in each fiscal year. *T*-tests are conducted to test for differences in mean values between the *Before* and *After* subsamples. The symbols ***, **, and * indicate that subsample means are significantly different from each other at the 1%, 5%, and 10% levels, respectively.

Country	*ENFORCE* year	*NCSKEW*		*DUVOL*		Test of difference in mean	
		Before	*After*	*Before*	*After*		
	(1)	(2)	(3)	(4)	(5)	(3) - (2)	(5) - (4)
Argentina	1995	0.179	0.077	0.106	0.056	-0.102**	-0.050*
Australia	1996	0.092	0.025	0.060	0.019	-0.067**	-0.041**
Belgium	1994	-0.002	-0.132	0.022	-0.059	-0.130*	-0.081*
Chile	1996	-0.044	0.108	-0.030	0.078	0.151	0.109
Denmark	1996	0.007	-0.072	0.017	-0.023	-0.079**	-0.040*
Finland	1993	0.174	-0.004	0.125	-0.008	-0.179***	-0.133***
Germany	1995	0.056	-0.025	0.043	-0.008	-0.081	-0.051
Greece	1996	-0.006	-0.051	-0.016	-0.043	-0.045	-0.026
Hong Kong	1994	-0.160	-0.028	-0.088	-0.027	0.132**	0.060*
Hungary	1995	0.048	0.007	0.078	0.005	-0.041	-0.073
India	1998	0.055	-0.033	0.025	-0.032	-0.088	-0.057
Indonesia	1996	0.074	0.061	0.041	0.028	-0.013	-0.013
Italy	1996	0.040	-0.177	0.027	-0.098	-0.217**	-0.124**
Japan	1990	-0.063	-0.010	-0.059	-0.007	0.052	0.052
Korea	1988	0.089	-0.078	-0.003	-0.045	-0.167[a]	-0.041[a]
Malaysia	1996	-0.004	0.020	-0.017	0.015	0.023	0.032
Netherlands	1994	-0.050	-0.138	-0.009	-0.076	-0.089*	-0.067*
Norway	1990	0.091	0.083	0.055	0.044	-0.008	-0.012
Spain	1998	-0.087	-0.226	-0.043	-0.102	-0.139	-0.059
Sri Lanka	1996	0.136	0.026	0.066	0.030	-0.110*	-0.036
Sweden	1990	0.018	0.154	0.018	0.093	0.136*	0.075
Switzerland	1995	0.015	-0.151	0.019	-0.072	-0.166**	-0.091*
Taiwan	1989	0.149	-0.139	0.097	-0.102	-0.288[a]	-0.199[a]
Thailand	1993	-0.101	0.033	-0.072	0.019	0.134*	0.091*
Turkey	1996	0.056	0.010	0.000	-0.013	-0.046	-0.013
Total	-	0.025	-0.027	0.017	-0.013	-0.052***	-0.030***

[a] The *t*-tests for Korea and Taiwan are not infeasible because both countries have only one year of data before the enforcement year.

Table IV: Event study

This table shows the estimates of the event-study regressions. The event window includes the three-year period before and after the enforcement and the enforcement year. *ENFORCE* is a dummy variable that takes the value of one in the year of the country's first insider trading enforcement case and thereafter, and zero otherwise. *NCSKEW* is the trend-adjusted mean value of negative skewness of firms-specific-weekly return for each country in each fiscal year. *DUVOL* is the trend-adjusted mean value of log of the ratio of the standard deviations of down-week to up-week firm-specific-weekly returns for each country in each fiscal year. The definitions of other variables are in the legend of Table II. The *t*-statistics in parentheses are calculated from the Huber/White/Sandwich heteroskedastic consistent errors, which are also corrected for correlation across observations for a given country. The symbols ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	NCSKEW	*DUVOL*	*NCSKEW*	*DUVOL*	*NCSKEW*	*DUVOL*
	(1)	(2)	(3)	(4)	(5)	(6)
ENFORCE	-0.052**	-0.030**	-0.049**	-0.028*	-0.062**	-0.041**
	(-2.37)	(-2.14)	(-2.26)	(-2.02)	(-2.34)	(-2.58)
GDP					-0.037	0.075
					(-0.13)	(0.46)
VGDP					0.009	0.008**
					(1.68)	(2.59)
IHERF					-0.119	-0.144
					(-0.31)	(-0.85)
FHERF					0.086	0.031
					(0.15)	(0.13)
NSTOCK					0.038	0.013
					(0.64)	(0.38)
LIB					-0.055	-0.050
					(-0.77)	(-1.29)
Constant	0.025	0.017	0.024*	0.016*	0.225	-0.668
	(1.48)	(1.53)	(1.88)	(1.94)	(0.10)	(-0.49)
Country fixed effects	No	No	Yes	Yes	Yes	Yes
Sample size	171	171	171	171	171	171
Number of countries	25	25	25	25	25	25
R-squared	0.04	0.04	0.06	0.05	0.08	0.10

Table V: Role of institutional characteristics in the effect of insider trading law enforcement on stock price crash risk

The sample consists of firms jointly covered in Datastream and Worldscope between 1982 and 2006. *ENFORCE* is a dummy variable that takes the value of one in the year of the country's first insider trading enforcement case and thereafter, and zero otherwise. *NCSKEW* is the trend-adjusted mean value of negative skewness of firms-specific-weekly return for each country in each fiscal year. *DUVOL* is the trend-adjusted mean value of log of the ratio of the standard deviations of down-week to up-week firm-specific-weekly returns for each country in each fiscal year. *ASDI* is the aggregate anti-self-dealing index compiled by Djankov et al. (2008). *LIB* is a dummy variable that takes the value of one in the year of the country's official financial liberalization and thereafter, and zero otherwise. *FHERF* is the firm Herfindahl index calculated using individual firm sales for each country in each year. *DISC* is the disclosure requirements index compiled by La Porta, Lopez-de-Silanes, and Shleifer (2006). Control variables are the same as those used in Table II, but their coefficients are not tabulated. The definitions of these variables are in the legend of Table II. Dollar values are converted into 2000 constant US dollars using the GDP deflator. The *t*-statistics in parentheses are calculated from the Huber/White/Sandwich heteroskedastic consistent errors, which are also corrected for correlation across observations for a given country. The symbols ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Re-estimation of regressions using the main sample

	NCSKEW	*DUVOL*	*NCSKEW*	*DUVOL*	*NCSKEW*	*DUVOL*	*NCSKEW*	*DUVOL*
	Investor protection		*Financial disclosure*		*Financial liberalization*		*Product market competition*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ENFORCE	-0.166***	-0.095***	-0.275***	-0.156***	-0.220***	-0.147***	-0.047*	-0.019
	(-3.96)	(-3.91)	(-3.83)	(-3.60)	(-6.53)	(-4.08)	(-1.90)	(-1.27)
ENFORCE×ASDI	0.204**	0.121**						
	(2.64)	(2.61)						
ENFORCE×DISC			0.329***	0.188**				
			(2.78)	(2.60)				
ENFORCE×LIB					0.152***	0.110***		
					(4.64)	(3.12)		
ENFORCE×FHERF							-0.431**	-0.357***
							(-2.67)	(-3.25)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	823	823	742	742	823	823	823	823
Number of countries	42	42	36	36	42	42	42	42
R-squared	0.11	0.09	0.13	0.10	0.10	0.09	0.10	0.09

Table V: Role of institutional characteristics in the effect of insider trading law enforcement on stock price crash risk (Cont'd)

Panel B: Re-estimation of regressions using the sample for the event study

	NCSKEW	*DUVOL*	*NCSKEW*	*DUVOL*	*NCSKEW*	*DUVOL*	*NCSKEW*	*DUVOL*
	Investor protection		*Financial disclosure*		*Financial liberalization*		*Product market competition*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ENFORCE	-0.157***	-0.092***	-0.197***	-0.118***	-0.200***	-0.142**	-0.047	-0.022
	(-3.97)	(-4.58)	(-3.66)	(-3.69)	(-4.35)	(-2.63)	(-1.45)	(-1.14)
ENFORCE×ASDI	0.233***	0.126***						
	(2.91)	(3.01)						
ENFORCE×DISC			0.233**	0.133**				
			(2.70)	(2.68)				
ENFORCE×LIB					0.136***	0.100*		
					(3.10)	(1.95)		
ENFORCE×FHERF							-0.254	-0.324***
							(-1.27)	(-2.87)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	171	171	164	164	171	171	171	171
Number of countries	25	25	24	24	25	25	25	25
R-squared	0.12	0.13	0.17	0.13	0.09	0.12	0.08	0.13

Table VI: Controlling for potential omitted variables

The sample consists of firms jointly covered in Datastream and Worldscope between 1982 and 2006. *ENFORCE* is a dummy variable that takes the value of one in the year of the country's first insider trading enforcement case and thereafter, and zero otherwise. *NCSKEW* is the trend-adjusted mean value of negative skewness of firms-specific-weekly return for each country in each fiscal year. *DUVOL* is the trend-adjusted mean value of log of the ratio of the standard deviations of down-week to up-week firm-specific-weekly returns for each country in each fiscal year. *INFORM* is the log of mean value of relative firm-specific stock return variation for each country in each fiscal year. *ANALYST* is the log of mean number of analysts providing a forecast for each firm listed on IBES for each country in each fiscal year. *LIQ* is the ratio of trading volume to market capitalization for each country in each calendar year. *OPEN* is the ratio of the sum of a country's total imports and exports scaled by the country's GDP in each calendar year. *FDI* is the ratio of the net inflow of foreign direct investment in a country scaled by the country's GDP in each calendar year. *SHORT* is a dummy variable that takes a value of one if short selling is infeasible in a country, and zero otherwise based on Charoenrook and Daouk (2005). Control variables are the same as those used in Table II, but their coefficients are not tabulated. The definitions of these variables are in the legend of Table II. Dollar values are converted into 2000 constant US dollars using the GDP deflator. The *t*-statistics in parentheses are calculated from the Huber/White/Sandwich heteroskedastic consistent errors, which are also corrected for correlation across observations for a given country. The symbols ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	NCSKEW	*DUVOL*	*NCSKEW*	*DUVOL*	*NCSKEW*	*DUVOL*
	(1)	(2)	(3)	(4)	(5)	(6)
ENFORCE	-0.076***	-0.043***	-0.073***	-0.040***	-0.081***	-0.045***
	(-3.24)	(-3.14)	(-3.62)	(-3.27)	(-3.34)	(-3.18)
INFORM	-0.034	-0.011			-0.045**	-0.015
	(-1.63)	(-0.93)			(-2.02)	(-1.25)
ANALYST	0.006	0.004			0.023	0.014
	(0.56)	(0.57)			(1.38)	(1.56)
LIQ			-0.024*	-0.017**	-0.027*	-0.020**
			(-1.81)	(-2.42)	(-2.02)	(-2.58)
OPEN			-0.017	-0.009	-0.014	-0.008
			(-0.39)	(-0.32)	(-0.31)	(-0.28)
FDI			0.029	0.022	0.031	0.020
			(0.73)	(0.88)	(0.75)	(0.76)
SHORT			0.094***	0.055**	0.101***	0.060**
			(2.71)	(2.42)	(2.87)	(2.59)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	823	823	645	645	645	645
Number of countries	42	42	41	41	41	41
R-squared	0.10	0.08	0.13	0.12	0.15	0.13

Table VII: Test of reverse causality

The sample consists of firms jointly covered in Datastream and Worldscope between 1982 and 2006. $YEAR_{t-2}$ ($YEAR_{t-1}$) is a dummy variable that takes the value of one if a country enforces the insider trading laws in two (one) years, and zero otherwise. $YEAR_t$ is a dummy variable that takes the value of one if a country enforces the insider trading laws this year, and zero otherwise. $YEAR_{t+1}$ is a dummy variable that takes the value of one if a country enforces the insider trading law one year ago, and zero otherwise. $YEAR_{t+2}$ is a dummy variable that takes the value of one if a country enforces the insider trading law two years ago or more, and zero otherwise. *NCSKEW* is the trend-adjusted mean value of negative skewness of firms-specific-weekly return for each country in each fiscal year. *DUVOL* is the trend-adjusted mean value of log of the ratio of the standard deviations of down-week to up-week firm-specific-weekly returns for each country in each fiscal year. The definitions of other variables are in the legend of Table II. Dollar values are converted into 2000 constant US dollars using the GDP deflator. The *t*-statistics in parentheses are calculated from the Huber/White/Sandwich heteroskedastic consistent errors, which are also corrected for correlation across observations for a given country. The symbols ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	NCSKEW	*DUVOL*
	(1)	(2)
$YEAR_{t-2}$	0.001	-0.008
	(0.05)	(-0.54)
$YEAR_{t-1}$	-0.027	-0.022
	(-0.92)	(-1.33)
$YEAR_t$	-0.055*	-0.039**
	(-1.92)	(-2.17)
$YEAR_{t+1}$	-0.067**	-0.044**
	(-2.29)	(-2.66)
$YEAR_{t+2}$	-0.125***	-0.075***
	(-4.28)	(-4.75)
GDP	0.047	0.045
	(0.58)	(0.86)
VGDP	0.005	0.003
	(1.30)	(1.32)
IHERF	-0.114	-0.084
	(-0.95)	(-1.22)
FHERF	0.275	0.140
	(1.37)	(1.12)
NSTOCK	0.019	0.009
	(1.23)	(0.94)
LIB	-0.032	-0.033
	(-0.63)	(-0.97)
Constant	-0.467	-0.400
	(-0.68)	(-0.91)
Country fixed effects	Yes	Yes
Sample size	531	531
Number of countries	25	25
R-squared	0.14	0.12

Table VIII: Additional sensitivity tests

The sample consists of firms jointly covered in Datastream and Worldscope between 1982 and 2006. *ENFORCE* is a dummy variable that takes the value of one in the year of the country's first insider trading enforcement case and thereafter, and zero otherwise. *NCSKEW* is the trend-adjusted mean value of negative skewness of firms-specific-weekly return for each country in each fiscal year. *DUVOL* is the trend-adjusted mean value of log of the ratio of the standard deviations of down-week to up-week firm-specific-weekly returns for each country in each fiscal year. Control variables are the same as those used in Table II, but their coefficients are not tabulated. The definitions of these variables are in the legend of Table II. Dollar values are converted into 2000 constant US dollars using the GDP deflator. The *t*-statistics in parentheses are calculated from the Huber/White/Sandwich heteroskedastic consistent errors, which are also corrected for correlation across observations for a given country. The symbols ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	NCSKEW	*DUVOL*
	(1)	(2)
Panel A: *Using NCSKEW_raw and DUVOL_raw as dependent variables*: N = 963		
ENFORCE	-0.059***	-0.033***
	(-2.9)	(-2.8)
Panel B: *Using median values of NCSKEW and DUVOL as dependent variables*: N = 823		
ENFORCE	-0.060***	-0.033***
	(-3.43)	(-3.09)
Panel C: *Removing the country-years prior to 1990*: N = 680		
ENFORCE	-0.076***	-0.043***
	(-3.47)	(-3.39)
Panel D: *Removing years during the Asian financial crisis* (*1997-1998*): N = 739		
ENFORCE	-0.069***	-0.038***
	(-3.78)	(-3.60)
Panel E: *Excluding Japan*: N = 798		
ENFORCE	-0.071***	-0.040***
	(-4.13)	(-4.21)
Panel F: *Excluding firms in financial industries* (*SIC*:*6000-6999*): N = 822		
ENFORCE	-0.058***	-0.032***
	(-3.57)	(-3.38)
Panel G: *Excluding small firms with total sales less than $10 mil*: N = 823		
ENFORCE	-0.062***	-0.034***
	(-3.45)	(-3.32)
Panel H: *Excluding ADRs and GDRs*: N = 823		
ENFORCE	-0.063***	-0.035***
	(-3.54)	(-3.39)
Panel I: *Using country random effects instead of country fixed effects to estimate regressions*: N = 823		
ENFORCE	-0.064***	-0.035***
	(-3.58)	(-3.43)

Table IX: Firm-level analysis

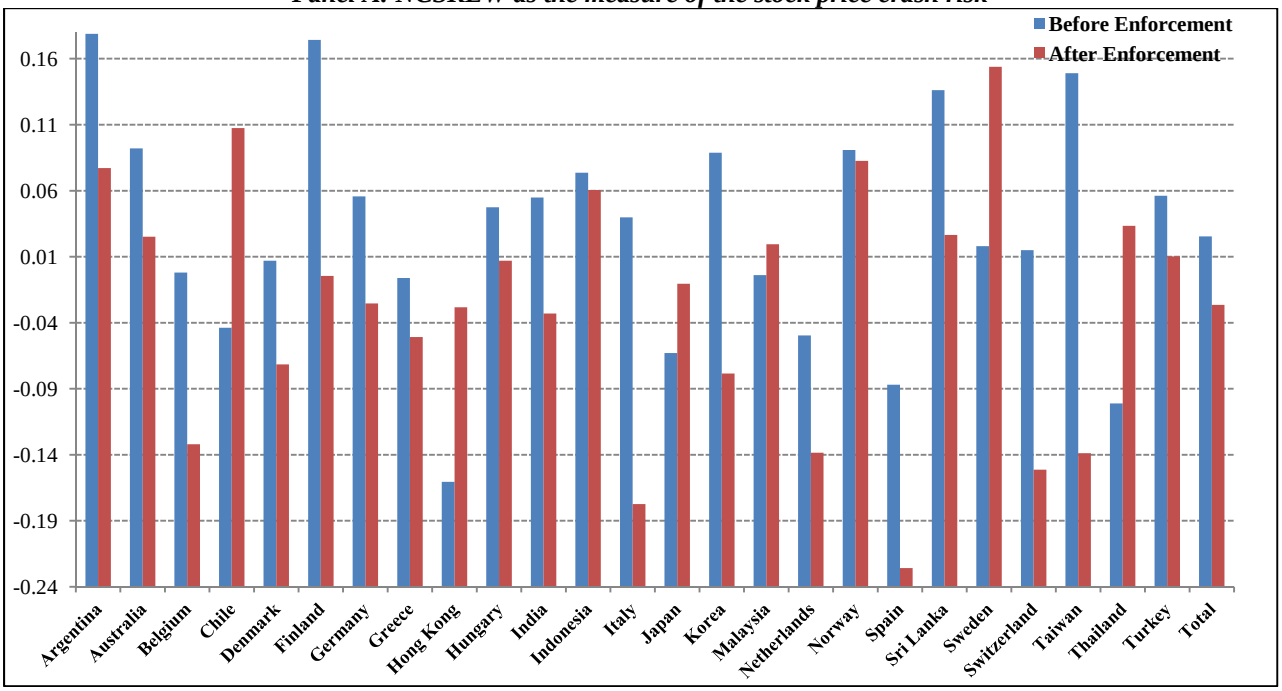
The sample consists of firms jointly covered in Datastream and Worldscope between 1982 and 2006. *ENFORCE* is a dummy variable that takes the value of one in the year of the country's first insider trading enforcement case and thereafter, and zero otherwise. *NCSKEW* is the trend-adjusted negative skewness of firms-specific-weekly return for each firm in each fiscal year. *DUVOL* is the trend-adjusted log of the ratio of the standard deviations of down-week to up-week firm-specific-weekly returns for each firm in each fiscal year. *DTURN* is the average monthly share turnover over the current fiscal year minus the average monthly share turnover over the previous fiscal year, where monthly share turnover is calculated as the monthly trading volume divided by the total number of shares outstanding during the month. *SIGMA* is the standard deviation of firm-specific weekly returns over the fiscal year. *RET* is the mean of firm-specific weekly returns over the fiscal year, times 100. *SIZE* is the log of the market value of equity. *MB* is the ratio of market value of equity over book value of equity. *LEV* is the total long-term debt divided by total assets. *ROA* is operating income before depreciation and amortization over total assets. *DISACC* is the absolute value of discretionary accruals, where discretionary accruals are estimated from the modified Jones model (Dechow, Sloan, and Sweeney, 1995). Dollar values are converted into 2000 constant US dollars using the GDP deflator. The *t*-statistics in parentheses are calculated from the Huber/White/Sandwich heteroskedastic consistent errors, which are also corrected for correlation across observations for a given country. The symbols ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	NCSKEW	*DUVOL*	*NCSKEW*	*DUVOL*
	Industrial firms		*Financial firms*	
	(1)	(2)	(3)	(4)
ENFORCE	-0.092***	-0.048***	-0.094**	-0.045**
	(-4.44)	(-3.66)	(-2.59)	(-2.11)
$DTURN_{t-1}$	-0.016	-0.013	-0.011	-0.017
	(-0.84)	(-1.16)	(-0.33)	(-0.83)
$NCSKEW_{t-1}$	0.047***	0.029***	0.051***	0.028***
	(8.70)	(8.09)	(5.71)	(5.00)
$SIGMA_{t-1}$	1.490**	0.392	1.785***	0.411
	(2.37)	(1.06)	(2.79)	(1.12)
RET_{t-1}	-0.029	-0.011	-0.056	-0.035
	(-1.52)	(-1.00)	(-0.73)	(-0.72)
$SIZE_{t-1}$	0.003	0.003	0.006	0.004*
	(0.77)	(1.23)	(1.35)	(1.80)
MB_{t-1}	0.004***	0.002***	0.007***	0.003***
	(4.53)	(2.71)	(4.72)	(3.79)
LEV_{t-1}	0.046***	0.014	-0.003	-0.005
	(3.13)	(1.57)	(-0.16)	(-0.44)
ROA_{t-1}	-0.074***	-0.038***	-0.121***	-0.069***
	(-6.45)	(-6.73)	(-6.07)	(-5.25)
$DISACC_{t-1}$	0.014	0.001	NA	NA
	(1.35)	(0.20)		
Constant	-0.161	-0.090	-0.128*	-0.058
	(-1.65)	(-1.53)	(-1.78)	(-1.43)
Industry fixed effects	Yes	Yes	No	No
Country fixed effects	Yes	Yes	Yes	Yes
Sample size	111,318	111,318	17,790	17,790
Number of countries	42	42	42	42
R-squared	0.03	0.02	0.04	0.03

Figure 1: Stock price crash risk around the enforcement of insider trading law

Panels A and B plot the average stock price crash risk in the period before and after the enforcement of insider trading laws with trend-adjusted *NCSKEW* and *DUVOL* as main measures, respectively. The event window includes the three-year period before and after the enforcement and the enforcement year. *NCSKEW* is the trend-adjusted mean value of negative skewness of firms-specific-weekly return for each country in each fiscal year. *DUVOL* is the trend-adjusted mean value of log of the ratio of the standard deviations of down-week to up-week firm-specific-weekly returns for each country in each fiscal year.

Panel A: NCSKEW as the measure of the stock price crash risk



Panel B: DUVOL as the measure of the stock price crash risk