内幕交易与股价崩盘风险--来自自然实验的国际证据

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Abstract: Using an international sample of 48 countries over the 25-year period of 1982-2006, this study investigates the impact of insider trading on stock price crash risk. In so doing, we exploit the initial enforcement of insider trading laws in a country as a natural experiment. Our results reveal that that initial insider trading law enforcement leads to a significant reduction in stock price crash risk. This mitigating effect is more pronounced in countries with poor quality of institutional infrastructures in terms of investor protection, financial disclosure environment, financial market liberalization, or product market competition. Our results support the view that insider trading restrictions discourage managers to engage in bad news hoarding.

Keywords: Insider Trading; Stock Price Crash Risk; Bad News Hoarding

JEL Classification: D82, G15, G38

1. Introduction

The accounting and finance literature has long debated over the costs and benefits of insider trading and whether insider trading should be regulated or prohibited by law. Recent business scandals indicate that large stock price crashes are preceded by insider trading.[1] These events provoke a considerable interest in whether and how insider trading affects the likelihood of asset price crashes. This is an important research question, because outside capital suppliers are concerned with the likelihood of extreme price declines or crash risk and this extreme negative tail risk cannot be diversified away (Sunder, 2010; Chang, Christoffersen, and Jacobs, 2013).

Our study addresses the aforementioned question from the perspective of managerial disclosure incentives. More specifically, we investigate a hitherto unexplored question of whether and how inside trading influences managerial incentive for bad news hoarding, which in turn affects stock price crash risk. In so doing, we exploit the legal regime shift associated with the first-time insider trading law enforcement in a country and consider this exogenous regulatory event as a natural experiment which allows us to make a causal inference on the impact of insider trading restrictions on stock price crash risk. We study this research question in a cross-country context because it is difficult to rule out a reverse causality between insider trading and stock price crash risk in a single country setting. For example, the firm-level relation between insider trading and crash risk is likely to be endogenous; firm-level insider trading data are also likely to suffer from a selection bias, since insiders do not voluntarily disclose their illegal trades and the publicly available data only reflect part of insiders' trading activities (Bhattacharya and Daouk, 2002).

Managers' disclosure behavior depends on the costs of disclosures and the associated benefits. On the one hand, career concerns and management compensation structure motivate managers to release good news but withhold bad news (Nagar, 1999; Nagar, Nanda, and Wysocki, 2003). On the other hand, litigation risk and reputation concerns incentivize the managers to quickly reveal bad news (Kasznik and Lev, 1995; Skinner, 1994, 1997; Baginski, Hassell, and Kimbrough, 2002). In general, as demonstrated in Kothari, Shu, and Wysocki (2009), managers have a tendency of quickly revealing good news to investors but withholding bad news up to a certain threshold, because the benefits of delaying disclosure of bad news usually outweigh the associated costs unless the total amount of hidden bad news accumulated within the firm does not exceed a certain threshold level.

Built on this finding, our study maintains that, for an *average* manager,[2] insider trading strengthens the manager's incentive to withhold bad news by providing her with an opportunity to exit through selling her stakes before realization of the bad news. Subsequently, as the manager hoards negative news and accumulate it within the firm, the firm's share price becomes severely overvalued (Jin and Myers, 2006; Hutton, Marcus, and Tehranian, 2009; Kim, Li, and Zhang 2011a,b; Kim and Zhang, 2015). However, she is only able to hide unfavorable information about firm performance and to accumulate this negative information over time up to a certain tipping point where the costs of withholding it do not exceed the associated benefits. When the accumulated negative information reaches the upper limit, the manager is forced to release the hidden bad news all at once, which in turn increases the occurrence of extreme negative outliers in stock return distributions in the future, i.e., stock price crash risk.[3] The enforcement of insider trading laws increases legal costs of the manager trading on her private or inside information with no compensating increase in the associated benefits. For example, the enforcement not only increases expected legal penalty associated with insider trading but also increases expected reputation losses associated therewith, and thus attenuates managerial incentive and ability to withhold bad news. We therefore hypothesize that the initial enforcement of insider trading laws in a country is associated with a significant reduction in stock price crash risk.[4]

Using an international sample of firms from 48 countries over the period 1982-2006, we test whether and how a firm's stock price crash risk changes from the pre-to the post-initial enforcement of a country's insider trading law. In so doing, we adopt a difference-in-differences approach by taking advantage of this natural experimental setting. Following prior studies (e.g., Chen, Hong, and Stein, 2001; Hutton, Marcus, and Tehranian, 2009; Kim, Li, and Zhang, 2011a,b; DeFond et al., 2015), we first measure firm-level crash risk by the skewness of firm-specific weekly returns and the asymmetric volatility of negative and positive stock returns. We then employ median values of firm-level crash risk measures in each country in each year to conduct a country-level analysis.[5] At the same time, we also control for several common variables that are known to have an impact on crash risk according to previous studies.[6]

Consistent with our prediction, the results of our baseline regressions reveal that median stock price crash risk declines significantly after the first-time enforcement of insider trading laws in a country. An event-study analysis using a reduced sample of 24 countries that initially enforced their insider trading laws during the sample period also reveals a substantial decline in stock price crash risk *around* the first-time enforcement year, which further confirms the findings in our baseline regressions. The economic impact of our findings is also significant. Specifically, our baseline regression results show that two measures of crash risk decline, on average, by more than a half of their standard deviations in countries where insider trading laws were enforced for the first time during the sample period, compared with those in countries where insider trading laws were not enforced yet during the same period. Similar inference is drawn based on the event study analysis.

To further identify the channel through which insider trading law enforcement reduces stock price crash risk, we explore whether and how the enforcement effect on reducing crash risk vary across countries, depending on a country's institutional characteristics. We find that this effect is more pronounced in countries where cost of bad news hoarding is lower, that is: (1) investor rights are poorly protected; (2) the disclosure environment is less transparent; (3) the financial market has not yet been liberalized; or (4) the product market is less competitive. These results lend further support to the notion that legal actions against insider trading do effectively lower the likelihood of stock price crash occurrences by discouraging managers to engage in bad news hoarding activities, and that such an effect is more pronounced in an environment conducive to higher managerial opportunism in bad news disclosures.

Our paper contributes to the existing literature in three different ways. First, to our knowledge, our study is the first to link insider trading and stock price crashes through the channel of managerial disclosure incentives. Second, we take advantage of the legal regime shift in insider trading as a natural experiment, which allows us to alleviate the endogeneity concern that insider trading and stock price crash risk are possibly determined simultaneously by common firm and/or market characteristics. Finally, our study adds to the debate about pros and cons of insider trading by highlighting the dark side of insider trading, i.e., the role of insider trading in incentivizing managers to withhold negative information within firms, which in turn increases the likelihood of extreme negative outliers in firm-specific return distribution or simply stock price crash risk.

The remainder of the paper is organized as follows. Section 2 reviews related literature. Section 3 describes the sample and variables. Section 4 presents the main empirical results. Section 5 presents the results of tests for potential endogeneity and additional results of sensitivity tests. The final section concludes.

2. Related Literature

By examining the impact of insider trading law enforcement on stock price crash risk, we bring together two strands of literature. First, our paper adds to the insider trading literature, particularly, that analyzes the economic consequences of insider trading. These studies can be categorized into two streams. On the one hand, the first stream of research 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 serve as a supplementary incentive for corporate insiders to engage more actively in value-enhancing activities. This implies that prohibiting insider trading is costly in that its prohibition may motivate insiders to seek alternative compensations for their opportunity losses associated with being unable to trade on the private information *ex ante*.

On the other hand, the other stream of research against insider trading is based on the fact that insider trading reduces gains available to outside investors by making their 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. Along this line of research, several studies provide supportive evidence for potential benefits of insider trading prohibitions. For instance, Bushman, Piotroski, and Smith (2005) 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).

Our study belongs to the second stream as we document the dark side of insider trading. However, our paper is distinct from the extant literature in one important aspect: we show a detrimental effect of insider trading on managerial disclosure of bad news, i.e., the role of insider trading in strengthening managerial incentive to withhold negative information.

Second, our study contributes to the literature on stock price crashes. Motivated by the agency-conflict model of Jin and Myers (2006), recent empirical studies focus on firm-specific internal factors, particularly, financial reporting opacity, which potentially drive stock price crashes.[7] For example, Hutton, Marcus, and Tehranian (2009) test the Jin-Myers prediction and find supportive evidence that information opaqueness increases the likelihood of stock price crash occurrences in the future. DeFond et al. (2015) document that the mandatory adoption of International Financial Reporting Standards (IFRS) 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 planning and strategies create managerial incentives to obfuscate financial reporting, which provides managers with a means to conceal negative information that increases crash risk. Furthermore, a recent work by Kim and Zhang (2015) 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.

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 trade on private information, 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, which in turn exacerbates future crash risk.

Finally, our work is also related to Marin and Olivier (2008), who theoretically and empirically demonstrate that the likelihood of stock price crashes is *negatively* associated with insiders' selling activities in the month before the crash, which yields an opposite prediction to our hypothesis. They offer an explanation to the *negative* relation: when insider selling, usually constrained by certain "floor" value, conveys to outside investors a credible signal that insiders are in possession of bad news, investors' uncertainty about how bad the news really is can lead to stock price crashes. Although Marin and Olivier (2008) in their analysis also find a positive relation between the likelihood of stock price crashes and insiders' selling activities in the year before the crash, they do not provide a detailed discussion on this relation. More importantly, their analysis is conducted in a single country, i.e., U.S., while our study employs the initial enforcement of insider trading laws cross countries as a natural experiment. Hence, our paper complements Marin and Olivier (2008) by furnishing a causal link between insider trading and stock price crashes from the perspective of managerial disclosure incentive.

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 listed companies from 48 countries over the period from 1982 to 2006.[8] We then exclude firm-years with negative sales (Fernandes and Ferreira, 2009), firms with less than 26 weeks of stock trading data in a given year (Kim, Li, and Zhang, 2011b), financial firms (Hutton, Marcus, and Tehranian, 2009), and American Depository Receipts (ADRs) and Global Depository Receipts (GDRs) (Jin and Myers, 2006). The years of enactment and enforcement of insider trading laws are obtained from Denis and Xu (2013).

Macroeconomic data for each country are extracted from World Bank World Development Indicators (WDI) database and International Monetary Fund (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 Porta, Lopez-De-Silanes, and Shleifer (2006), where we obtain the country-level financial liberalization year, anti-selfdealing 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 each 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 week *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 *ri,t* is the return on stock *i* in week *t* (in market *j*); *rm,j,t* is the Morgan Stanley Capital International (MSCI) country-specific market index return or the country index return compiled by Datastream in week *t*; *rUS,t* is the U.S. market index return (a proxy for the global market); *EXj,t* is the change in country *j*'s U.S. dollar exchange rate; and *εi,t* represents unspecified factors. The expression *rUS,t* + *EXj,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 *Wi,t*, is defined as the natural logarithm of one plus the residual return (*εi,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., 2015).[9] The first measure is negative conditional firm-specific weakly return skewness (*NCSKEW*).

Specifically, we calculate *NCSKEW* for a given firm in a year by taking the negative of the third moment of firm-specific weekly returns, *Wi,t*, during the same year and dividing it by the standard deviation of firm-specific weekly returns raised to the third power. For each firm *i* in year *t*, we obtain *NCSKEW* as follows:

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

The negative sign in the above equation creates a variable that increases as the return distribution becomes increasingly negatively skewed. Therefore, the higher the NCSKEW value, the higher the likelihood that extreme negative outliers in firm-specific return distribution are realized.

Our second measure of stock price crash risk is the down-to-up volatility of firm-specific weekly returns, denoted by DUVOL. Specifically, for each firm in a given year, we separate out all weeks with firm-specific weekly returns, Wi,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:

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

where *nd* and *nu* are the number of down and up weeks, respectively. The *DUVOL* variable captures asymmetric volatilities between negative and positive firm-specific weekly returns. *DUVOL* increases with downside risk (i.e., crash risk).

To conduct a country-level analysis, we use median values of the two crash risk measures across firms for each country in each year as the main dependent variable. To alleviate the time-trend effect on crash risk, we trend-adjust the two variables throughout the entire analysis following previous studies (Bushman, Piotroski, and Smith, 2005; Fernandes and Ferreira, 2009). Specifically, we use six countries, i.e., Brazil, Canada, France, Singapore, UK, and US that enforced their insider trading laws prior to 1982, i.e., the start of 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 restrictions on stock price crash risk from the effect of other market-wide factors, 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 macroeconomic conditions in a country, which can affect crash risk based on Povel, Singh, and Winton (2007) and Barro and Ursúa (2009). Specifically, we include the natural logarithm 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 behavior. 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 natural logarithm of the number of listed firms in each country in each year (*NSTOCK*) because stock price crash risk is likely to be correlated with stock market size. Finally, we include an indicator (*LIB*) to control for the potential effect of financial market liberalization in a country on stock price crash risk. Specifically, *LIB* takes 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 main variables used in this study by country. There are a total of 958 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 the start of 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.

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 a country's initial enactment of insider trading laws. ENFORCE year is the year of a country's first insider trading enforcement case. NCSKEW_raw is the median value of negative skewness of firms-specific-weekly return for each country in each year. DUVOL_raw is the median 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 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 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 a 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	0.062	0.013	0.086	0.080	8.89	6.48	0.17	0.13	3.43	1989
Australia	25	1982	1991	1996	0.018	-0.039	0.065	0.038	9.84	1.56	0.08	0.03	5.61	1969
Austria	25	1982	1993		-0.042	-0.068	0.005	0.009	9.93	1.23	0.14	0.09	3.76	1969
Belgium	25	1982	1990	1994	-0.088	-0.085	-0.041	-0.008	9.89	1.33	0.14	0.09	4.26	1969
Brazil	12	1995	1976	1978	-0.031	-0.065	-	-	8.23	1.89	0.22	0.17	3.59	1991
Canada	25	1982	1966	1976	-0.002	-0.050	-	-	9.93	1.98	0.07	0.02	6.03	1969
Chile	16	1991	1981	1996	-0.109	-0.094	-0.093	-0.030	8.49	2.52	0.10	0.04	4.63	1992
China	15	1992	1993		-0.022	-0.060	-0.008	0.003	6.79	2.01	0.27	0.18	4.84	
Colombia	15	1992	1990		-0.097	-0.084	-0.084	-0.021	7.85	2.22	0.19	0.09	3.16	1991
Czech Rep.	12	1995	1992	1993	0.114	0.036	0.109	0.089	8.69	2.52	0.20	0.11	3.52	
Denmark	25	1982	1991	1996	-0.071	-0.076	-0.024	0.002	10.17	1.54	0.12	0.06	4.55	1969
Finland	20	1987	1989	1993	-0.046	-0.070	-0.009	0.003	9.96	2.22	0.16	0.12	4.03	1969
France	25	1982	1967	1975	-0.073	-0.092	-	-	9.87	1.07	0.08	0.03	5.79	1969
Germany	25	1982	1994	1995	-0.061	-0.083	-0.014	-0.006	9.92	1.32	0.09	0.03	5.81	1969
Greece	19	1988	1988	1996	-0.061	-0.098	-0.029	-0.027	9.30	1.62	0.21	0.07	4.86	1987
Hong Kong	25	1982	1991	1994	-0.075	-0.100	-0.028	-0.022	9.99	3.47	0.13	0.07	5.32	1969
Hungary	15	1992	1994	1995	-0.019	-0.058	-0.005	0.005	8.41	2.24	0.27	0.22	2.99	1996
India	17	1990	1992	1998	-0.009	-0.071	0.006	-0.009	6.04	2.06	0.12	0.04	5.67	1992
Indonesia	17	1990	1991	1996	0.017	-0.051	0.032	0.011	6.66	3.15	0.11	0.05	4.99	1989
Ireland	25	1982	1990		-0.047	-0.076	-0.001	0.001	9.76	2.23	0.22	0.12	3.46	1969
Israel	14	1993	1981	1989	-0.023	-0.051	-0.012	0.012	9.83	2.22	0.13	0.07	4.17	1993
Italy	25	1982	1991	1996	-0.196	-0.151	-0.149	-0.074	9.75	1.22	0.15	0.08	4.77	1969
Japan	25	1982	1988	1990	-0.100	-0.111	-0.053	-0.034	10.43	1.46	0.05	0.01	7.57	1980
Korea	19	1988	1976	1988	-0.045	-0.089	-0.013	-0.018	9.19	3.05	0.09	0.03	5.90	1992
Luxembourg	15	1992	1991		-0.052	-0.071	-0.039	-0.008	10.67	2.45	0.36	0.21	2.68	1969
Malaysia	21	1986	1973	1996	-0.102	-0.107	-0.061	-0.032	8.12	3.25	0.06	0.02	5.70	1988
Mexico	19	1988	1975		-0.056	-0.077	-0.023	-0.006	8.58	2.85	0.11	0.05	4.00	1989
Netherlands	25	1982	1989	1994	-0.140	-0.126	-0.093	-0.049	9.92	1.27	0.15	0.13	4.56	1969
New Zealand	19	1988	1988		-0.023	-0.047	0.010	0.025	9.46	2.04	0.17	0.15	4.00	1984
Norway	25	1982	1985	1990	-0.028	-0.064	0.019	0.013	10.35	1.50	0.21	0.12	4.39	1969
Pakistan	16	1991	1995		-0.179	-0.159	-0.163	-0.095	6.24	1.72	0.13	0.08	4.16	1991
Peru	13	1994	1991	1994	-0.008	-0.054	-0.005	0.003	7.65	3.53	0.18	0.10	3.57	1992
Philippines	19	1988	1982		0.011	-0.037	0.043	0.035	6.94	2.46	0.27	0.20	3.99	1991
Poland	13	1994	1991	1993	-0.005	-0.060	-0.001	-0.003	8.37	2.16	0.16	0.08	4.21	
Portugal	19	1988	1986		-0.117	-0.107	-0.084	-0.035	9.22	1.98	0.19	0.07	3.96	1986
Russia	9	1998	1996		-0.144	-0.134	-0.167	-0.089	7.60	3.83	0.25	0.18	2.87	
Singapore	25	1982	1973	1978	-0.057	-0.076	-	-	9.80	3.46	0.09	0.06	4.83	1969
South Africa	25	1982	1989		0.023	-0.035	0.069	0.042	8.05	2.05	0.08	0.05	4.86	1996
Spain	21	1986	1994	1998	-0.140	-0.121	-0.100	-0.045	9.45	1.30	0.19	0.10	4.53	1978
Sri Lanka	15	1992	1987	1996	0.030	-0.017	0.044	0.046	6.70	1.53	0.17	0.08	3.37	1991
Sweden	25	1982	1971	1990	-0.024	-0.060	0.023	0.018	10.11	1.55	0.12	0.07	4.61	1969
Switzerland	25	1982	1988	1995	-0.137	-0.125	-0.091	-0.048	10.41	1.51	0.14	0.08	4.80	1969
Taiwan	19	1988	1988	1989	-0.102	-0.112	-0.069	-0.041	9.44	6.87	0.20	0.09	5.04	1991
Thailand	20	1987	1984	1993	-0.056	-0.087	-0.019	-0.014	7.49	3.37	0.22	0.07	5.02	1987
Turkey	19	1988	1981	1996	-0.051	-0.085	-0.019	-0.014	8.27	4.69	0.18	0.11	4.29	1989
UK	25	1982	1980	1981	-0.106	-0.109	-	-	9.98	1.33	0.06	0.02	6.97	1969
US	25	1982	1934	1961	0.021	-0.054	-	-	10.31	1.61	0.04	0.01	8.26	1969
Venezuela	17	1990	1998		0.081	0.008	0.095	0.070	8.50	6.26	0.25	0.19	2.57	1990
Total	958	-	-	-	-0.053	-0.077	-0.021	-0.006	9.13	2.34	0.15	0.08	4.74	-
Panel B: Summary statistics of the total sample														
Mean	-	-	-	-	-0.053	-0.077	-0.021	-0.006	9.13	2.34	0.15	0.08	4.74	-
SD	-	-	-	-	0.151	0.093	0.154	0.095	1.20	1.80	0.11	0.10	1.51	-
Q1	-	-	-	-	-0.136	-0.132	-0.103	-0.061	8.34	1.17	0.08	0.03	3.66	-
Median	-	-	-	-	-0.046	-0.078	-0.018	-0.005	9.60	1.78	0.12	0.06	4.68	-
Q3	-	-	-	-	0.043	-0.019	0.064	0.046	10.03	2.70	0.18	0.10	5.65	-
Sample size	-	-	-	-	958	958	821	821	958	958	958	958	958	-

The median (mean) values of the two country-level raw (unadjusted) crash risk measures, *NCSKEW_raw* and *DUVOL_raw*, are -0.046 (-0.053) and -0.078 (-0.077), respectively. Furthermore, the value of *NCSKEW_raw* (*DUVOL_raw*) varies widely across countries ranging from a minimum value of -0.196 (-0.159) to a maximum value of 0.114 (0.036), with a standard deviation of 0.151 (0.093). The similar inference is drawn upon the trend-adjusted crash risk variables. However, due to the exclusion of the 6 benchmark countries (that enforced their insider trading laws before the start of our sample period, i.e., 1982) used to trend-adjust the raw crash risk variables, the sample size for the trend-adjusted variables is reduced to 821 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. Empirical Results

4.1. Main findings

In our baseline regression, we employ a difference-in-differences approach to examine 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:

$$Crash_{i,t} = b_0 + b_1\, ENFORCE_{i,t} + \Sigma b_{1+k}\, Controls^k_{i,t} + Country\ FE_i + \mu_{i,t} \quad (4)$$

where $Crash_{i,t}$ refers to one of the two trend-adjusted 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. $Controls^k_{i,t}$ represents the k^{th} control variable as defined in Section 3.3. $Country\ FE_i$ denotes country fixed effects. Since we trend-adjust crash risk variables, we do not control for year fixed effects in the regressions.[10] The standard errors of the estimated coefficients allow for clustering of observations by country. Our conclusions are unaffected if we allow clustering by both country and year.

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. The dependent variable is NCSKEW or DUVOL. NCSKEW is the trend-adjusted median value of negative skewness of firms-specific-weekly return for each country in each year. DUVOL is the trend-adjusted median value of the log of the ratio of the standard deviations of down-week to up-week firm-specificweekly returns for each country in each year. ENFORCE is a dummy variable that takes the value of one in the year of a country's first insider trading enforcement case and thereafter, and zero otherwise. 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 a 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	-0.046***	-0.028***	-0.090***	-0.051***	-0.080***	-0.049***
	(-2.76)	(-2.83)	(-4.08)	(-3.68)	(-3.44)	(-3.37)
GDP					-0.120**	-0.052
					(-2.15)	(-1.52)
VGDP					0.008*	0.004*
					(1.81)	(1.82)
IHERF					-0.137	-0.057
					(-0.89)	(-0.55)
FHERF					0.411***	0.216**
					(3.03)	(2.42)
NSTOCK					0.024	0.014
					(1.59)	(1.48)
LIB					0.008	-0.006
					(0.26)	(-0.28)
Constant	-0.000	0.007	0.020*	0.017***	0.952*	0.407
	(-0.02)	(0.87)	(1.96)	(2.76)	(1.99)	(1.38)
Country fixed effects	No	No	Yes	Yes	Yes	Yes
Sample size	821	821	821	821	821	821
Number of countries	42	42	42	42	42	42
R-squared	0.02	0.02	0.06	0.05	0.09	0.07

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 -2.76 and -2.83, respectively. In columns (3) and (4), controlling for country fixed effects, we find that the coefficients on *ENFORCE* are 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 time-invariant country-specific 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 additional country-level dynamic variables, namely, GDP per capita in U.S. dollars (*GDP*), GDP growth variance (*VGDP*), the industry and firm Herfindahl indices (*IHERF* and *FHERF*, respectively), the number of listed firms (*NSTOCK*), and the financial liberalization indicator (*LIB*). The results confirm a significantly 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, the crash risk measures, *NCSKEW* and *DUVOL*, decline by 0.080 and 0.049, which are approximately 52% of their standard deviations, compared with those in countries that have never enforced their insider trading laws.[11] Hence the economic impact of legal restrictions on insider trading is material. Taken together, the baseline regression results in Table II are consistent with our prediction that a firm's stock price crash risk declines significantly after the initial enforcement of a country's 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 firm Herfindahl indices (*FHERF*) are associated with higher crash risk, suggesting that firms' stock prices are more likely to crash in countries where the product market is dominated by a few large companies. The coefficient on the number of stocks (*NSTOCK*) is positive but insignificant. Financial market liberalization also has an insignificant effect on crash risk.

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. We then 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. Our event study analysis is based on a reduced sample of 24 countries that initially enforced their insider trading laws during the sample period.[12]

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 three years before and three years after the enforcement of insider trading laws. ENFORCE year is the year of a country's first insider trading enforcement case. NCSKEW is the trend-adjusted median value of negative skewness of firms-specific weekly return for each country in each year. DUVOL is the trend-adjusted median 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 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	
		Before	*After*	*Before*	*After*	(3) - (2)	(5) - (4)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Argentina	1995	0.239	0.069	0.136	0.066	-0.170**	-0.070
Australia	1996	0.076	0.005	0.052	0.003	-0.072*	-0.050
Belgium	1994	-0.006	-0.184	0.016	-0.081	-0.178	-0.097
Chile	1996	-0.111	-0.013	-0.019	0.014	0.098	0.033
Denmark	1996	0.013	-0.042	0.026	0.015	-0.055	-0.011
Finland	1993	0.094	-0.088	0.057	-0.040	-0.183**	-0.097**
Germany	1995	0.044	-0.059	0.036	-0.039	-0.103	-0.074
Greece	1996	-0.002	-0.068	-0.017	-0.060	-0.066	-0.043
Hong Kong	1994	-0.161	-0.054	-0.103	-0.045	0.107	0.059
Hungary	1995	0.151	0.001	0.132	-0.022	-0.149***	-0.154***
India	1998	0.033	0.001	0.007	-0.019	-0.033	-0.026
Indonesia	1996	0.076	0.064	0.047	0.031	-0.012	-0.015
Italy	1996	0.020	-0.261	0.033	-0.143	-0.281**	-0.176**
Japan	1990	-0.063	0.011	-0.063	0.013	0.075	0.076
Malaysia	1996	0.007	0.081	0.008	0.055	0.073	0.047
Netherlands	1994	-0.033	-0.140	0.005	-0.059	-0.107**	-0.064*
Norway	1990	0.091	0.048	0.071	0.036	-0.043	-0.035
Spain	1998	-0.091	-0.183	-0.058	-0.096	-0.092	-0.038
Sri Lanka	1996	0.142	0.027	0.096	0.046	-0.115	-0.050
Sweden	1990	0.029	0.112	0.013	0.075	0.084	0.062
Switzerland	1995	-0.012	-0.192	-0.001	-0.099	-0.180*	-0.098*
Taiwan	1989	0.162	-0.075	0.161	-0.051	-0.237[a]	-0.212[a]
Thailand	1993	-0.041	0.076	-0.045	0.061	0.116	0.106
Turkey	1996	0.091	-0.022	0.047	-0.009	-0.112**	-0.056*
Total	-	0.027	-0.037	0.023	-0.014	-0.064***	-0.037***

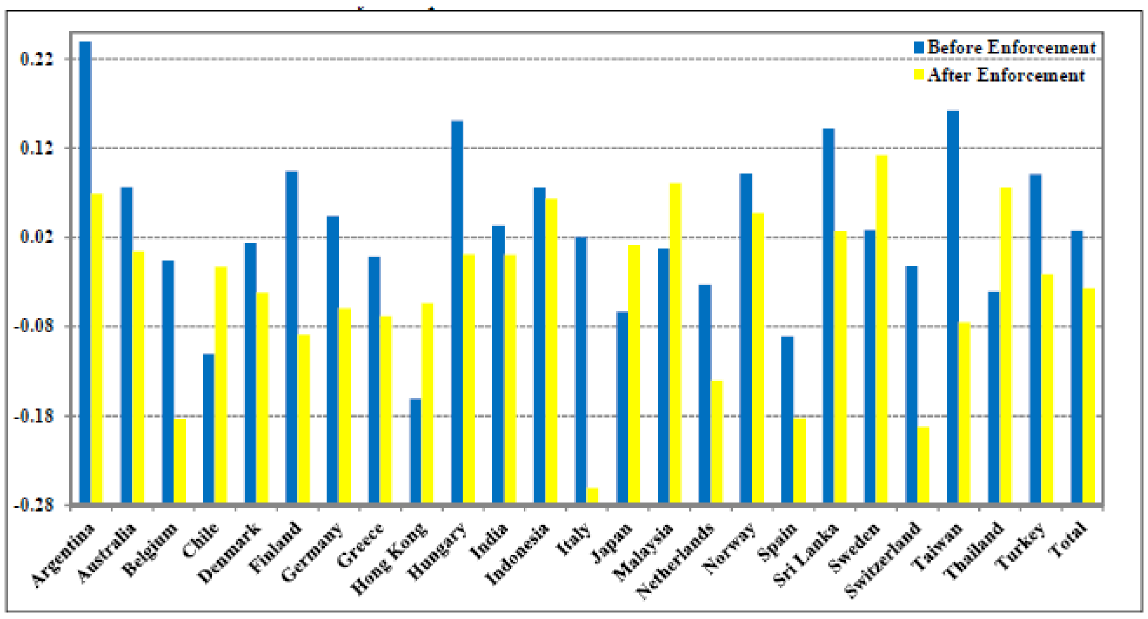
[a] The *t*-tests for Taiwan are infeasible because both countries have only one year of data before the enforcement year.

We first conduct a univariate test and report the results in Table III. We find that 18 out of 24 countries experience a decline in stock price crash risk from three years before to three years after the enforcement of insider trading laws.[13] 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 results indicate that *NCSKEW* and *DUVOL*, on average, become significantly lower after the insider trading law enforcement with *p*-values below 0.01. Figure 1 plots the data pattern in Table III.

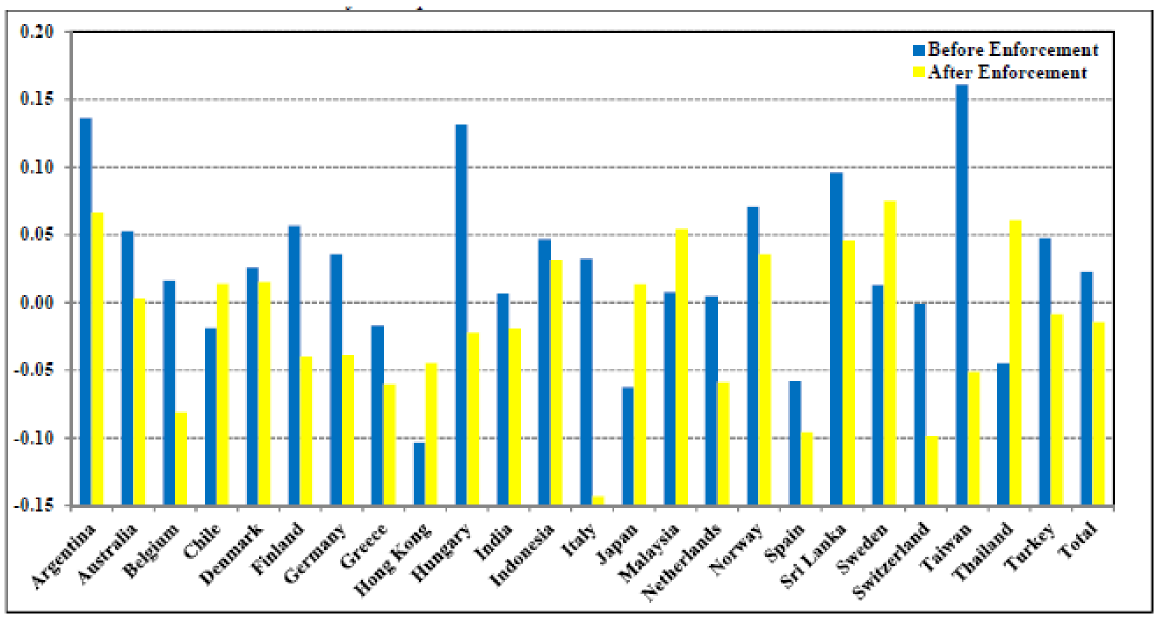
Figure 1: Stock price crash risk around the enforcement of insider trading laws

Panels A and B plot the average stock price crash risk three years before and three years after the enforcement of insider trading laws with trend-adjusted NCSKEW and DUVOL as main measures, respectively. NCSKEW is the trend-adjusted median value of negative skewness of firms-specific-weekly return for each country in each year. DUVOL is the trend-adjusted median 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 year.

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



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



We then perform a multivariate regression analysis for the sample of firms from 24 countries that experienced the insider trading law enforcement during the sample period.[14] 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 -2.23 to -2.60.

Table IV: Event study

This table shows the estimates of the event-study regressions. The event window consists of seven years centered on the enforcement year. The dependent variable is NCSKEW or DUVOL. NCSKEW is the trend adjusted median value of negative skewness of firms-specific-weekly return for each country in each year. DUVOL is the trend-adjusted median value of the log of the ratio of the standard deviations of down-week to up-week firm-specific-weekly returns for each country in each year. ENFORCE is a dummy variable that takes the value of one in the year of a country' s first insider trading enforcement case and thereafter, and zero otherwise. The definitions of other variables are in the legend of Table II. The 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	-0.056**	-0.033**	-0.055**	-0.032**	-0.066**	-0.045**
	(-2.55)	(-2.26)	(-2.50)	(-2.23)	(-2.51)	(-2.60)
GDP					0.228	0.183
					(0.98)	(1.12)
VGDP					0.013**	0.006*
					(2.30)	(1.76)
IHERF					-0.175	-0.167*
					(-1.12)	(-1.93)
FHERF					0.035	0.134
					(0.14)	(0.92)
NSTOCK					-0.017	-0.003
					(-0.32)	(-0.08)
LIB					-0.068	-0.088**
					(-1.15)	(-2.43)
Constant	0.027	0.023*	0.027**	0.022**	-1.891	-1.531
	(1.47)	(1.85)	(2.11)	(2.61)	(-0.95)	(-1.08)
Country fixed effects	No	No	Yes	Yes	Yes	Yes
Sample size	166	166	166	166	166	166
Number of countries	24	24	24	24	24	24
R-squared	0.05	0.04	0.06	0.05	0.09	0.09

Collectively, the event study provides strong evidence that lends further support to our prediction that firms in a country are less likely to experience stock price crashes after this country starts to enforce its insider trading laws.

4.2. Role of institutional characteristics

In this section, we further identify the channel through which insider trading restrictions reduce stock price crash risk by exploring how the results vary across countries, depending on different institutional characteristics. Specifically, our analyses focus on four dimensions of institutional characteristics that affect the cost of managers' bad news hoarding, that is: (1) investor protection; (2) financial disclosure environment; (3) financial market liberalization; and

(4) product market competition.[15] We report the results in Table V, where our test variable, *ENFORCE*, is interacted with each of the above four institutional characteristics. For brevity, in Table V, we present the estimated coefficients on *ENFORCE* and its interaction with each of four institutional characteristics, but do not tabulate the coefficients on all other variables. Panel A of Table V shows the heterogeneity in results across countries using the baseline regressions, while Panel B shows the results of tests conducted using the event study approach.

4.2.1. 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 deterring corporate insiders from withholding bad news can be relatively small.[16] We hence expect that the effect of insider trading restrictions on stock price crash risk is more evident in countries with poor investor protection than in countries with strong investor protection.

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

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. The dependent variable is NCSKEW or DUVOL. NCSKEW is the trend-adjusted median value of negative skewness of firms-specific-weekly return for each country in each year. DUVOL is the trend-adjusted median value of the log of the ratio of the standard deviations of down-week to up-week firm-specific-weekly returns for each country in each year. ENFORCE is a dummy variable that takes the value of one in the year of a country's first insider trading enforcement case and thereafter, and zero otherwise. 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 a 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	-0.198***	-0.118***	-0.262***	-0.154***	-0.182***	-0.133***	-0.044	-0.019
	(-4.28)	(-3.97)	(-3.48)	(-3.25)	(-4.40)	(-3.91)	(-1.42)	(-0.97)
ENFORCE×ASDI	0.264***	0.154***						
	(3.32)	(2.91)						
ENFORCE×DISC			0.335***	0.198**				
			(2.73)	(2.50)				
ENFORCE×LIB					0.107**	0.088**		
					(2.54)	(2.60)		
ENFORCE×FHERF							-0.465**	-0.392***
							(-2.15)	(-2.96)
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	821	821	742	742	821	821	821	821
Number of countries	42	42	36	36	42	42	42	42
R-squared	0.11	0.09	0.12	0.09	0.09	0.08	0.10	0.08

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	-0.170***	-0.105***	-0.214***	-0.126***	-0.199**	-0.217***	-0.022	0.000
	(-4.82)	(-4.71)	(-3.43)	(-3.78)	(-2.73)	(-4.27)	(-0.67)	(0.02)
ENFORCE×ASDI	0.261**	0.151**						
	(2.70)	(2.48)						
ENFORCE×DISC			0.261**	0.147**				
			(2.40)	(2.45)				
ENFORCE×LIB					0.138*	0.179***		
					(1.79)	(3.38)		
ENFORCE×FHERF							-0.519***	-0.536***
							(-3.92)	(-6.43)
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	166	166	159	159	166	166	166	166
Number of countries	24	24	23	23	24	24	24	24
R-squared	0.15	0.13	0.12	0.09	0.11	0.15	0.12	0.16

We reestimate the regressions in Tables II and IV by including the interaction term, *ASDI*×*ENFORCE*. Since the standalone effect of *ASDI* is absorbed by country fixed effects, we do not include *ASDI* itself as a separate control. The results are reported in columns (1) and (2) of Panels A and B in Table V with *NCSKEW* and *DUVOL*, respectively, as the dependent variable. We find that the coefficients on the interaction term are positive and significant with *t*-statistics ranging from 2.48 to 3.32.[17] The finding suggests that the effect of insider trading restrictions on reducing stock price crash risk is less pronounced in countries with stronger 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 against corporate insiders' expropriation and bad news hoarding associated therewith.

4.2.2. Financial disclosure

Based on Jin and Myers' (2006) theoretical analysis, Hutton, Marcus, and Tehranian (2009) empirically show that opaque firms are more prone to stock price crashes. To the extent that opaqueness incentivizes managers to hide unfavorable corporate information, rules and regulations that require more financial disclosure can serve as a tool to effectively mitigate this agency problem by increasing the cost of bad news hoarding. Given the lower chance of managerial misbehavior in a more transparent financial disclosure environment, we expect the negative impact of insider trading restrictions on reducing crash risk to be stronger in countries with less transparent disclosures. 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 legal requirements for disclosure of particular information 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 Eq. (4) by adding the interaction term, i.e., *DISC*×*ENFORCE*. Since country fixed effects take account of the independent effect of *DISC*, we do not include it as a separate control. The results reported in columns (3) and (4) 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 ranging from 2.40 to 2.73.[18] These results are consistent with the notion that more disclosure, e.g., information regarding key corporate insiders, mitigates insiders' incentives to trade on private information as well as the associated adverse effect on stock price crash risk. Stated another way, the crash risk-reducing effect of insider trading enforcement is further magnified in countries with lower disclosure requirements.

4.2.3. 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 tends to attract more analyst coverage, which improves transparency of a firm's information environment. As a consequence, the disciplinary effect of insider trading restrictions on managers' bad news hoarding behavior can be partially substituted by the reform on financial market liberalization. We thus expect the negative effect of insider trading restrictions on crash risk to be less pronounced in liberalized financial markets than in markets that are not yet liberalized.

To examine this prediction, 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 (5) and (6) of Panels A and B in Table V indicate that the coefficients on the interaction term, i.e., *LIB*×*ENFORCE*, are positive and significant at less than the 5% level, except for the regression in the event study with *NCSKEW* as the dependent variable, where the coefficient is significant at less than the 10% level. These findings suggest that the enforcement of insider trading laws plays a more important role in countries with financial markets not yet liberalized.

4.2.4. Product market competition

Prior research demonstrates that product market competition can work as a disciplinary and monitoring mechanism to curb agency problems (Hart, 1983; Shleifer and Vishny, 1997; Giroud and Miller 2010). In line with this notion, Balakrishnan and Cohen (2014) find that product market competition disciplines managers by constraining them from misreporting accounting information. Moreover, several studies also show that product market competition serves as a substitute to existing corporate governance. For example, 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. Therefore, we expect that insider trading restrictions, as an important governance mechanism which constrains managers' self-dealing behavior in our setting, 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 the regressions.[19] Following the tradition of previous literature (e.g., Low, 2009), we use *ex ante* measure of *FHERF* to examine the cross-sectional difference in our baseline regression results.[20] The regression results are presented in columns (7) and (8) of Panels A and B of Table V. We observe that the coefficients on *FHERF*×*ENFORCE* are negative and significant at less than the 5% level across all columns with *t*-statistics ranging from -2.15 to -6.43. These results suggest that the effect of initial enforcement of insider trading laws on reducing stock price crash risk is more evident 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).

5. Further Analysis

5.1. Test of reverse causality

Our main results show a negative association between insider trading restrictions and stock price crash risk. This finding can be viewed as supportive evidence for our argument that 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. There is also a possibility that the enforcement of insider trading laws is endogenous. For example, a country may start 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.

Table VI: Test of reverse causality

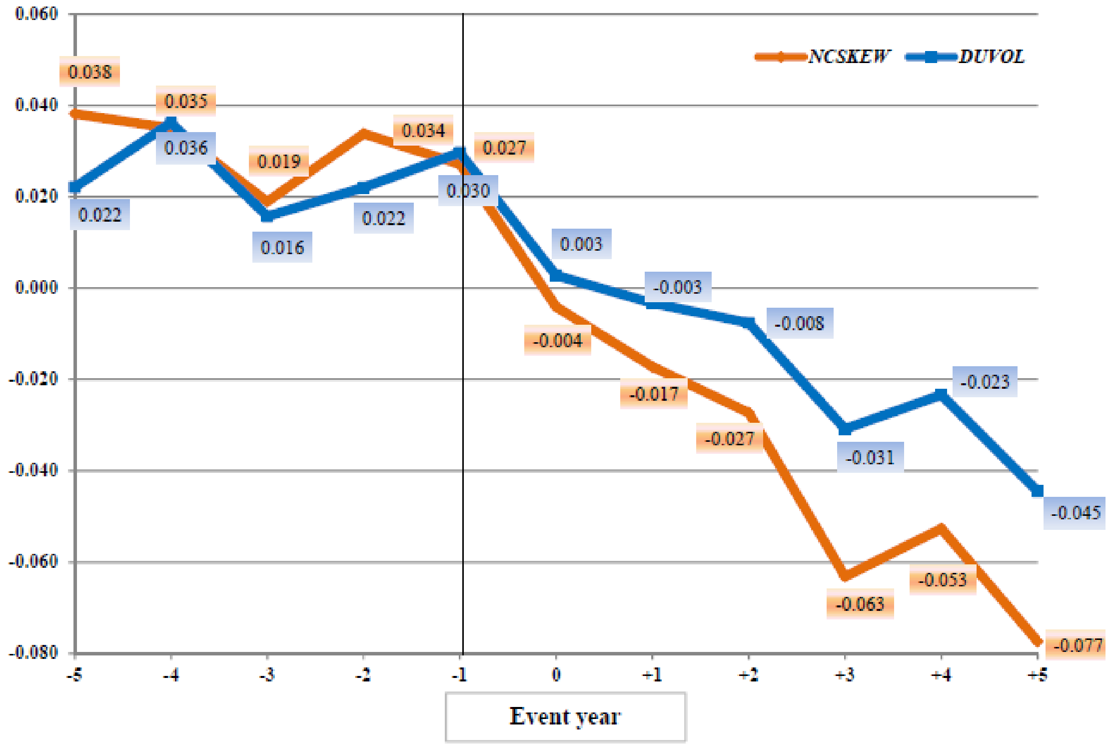
The sample consists of firms jointly covered in Datastream and Worldscope between 1982 and 2006. The dependent variable is NCSKEW or DUVOL. NCSKEW is the trend-adjusted median value of negative skewness of firms-specific-weekly return for each country in each year. DUVOL is the trend-adjusted median value of the log of the ratio of the standard deviations of down-week to up-week firm-specific weekly returns for each country in each year. YEAR-3 (YEAR-2 or YEAR-1) is a dummy variable that takes the value of one if a country enforces the insider trading laws in three (two or one) years, and zero otherwise. YEAR0 is a dummy variable that takes the value of one if a country enforces the insider trading laws this year, and zero otherwise. YEAR+1 (YEAR+2) is a dummy variable that takes the value of one if a country enforces the insider trading laws one (two) year(s) ago, and zero otherwise. YEAR+3 is a dummy variable that takes the value of one if a country enforces the insider trading laws three or more years ago, and zero otherwise. 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)
$YEAR_{-3}$	-0.023	-0.013
	(-1.06)	(-0.86)
$YEAR_{-2}$	-0.011	-0.008
	(-0.38)	(-0.41)
$YEAR_{-1}$	-0.021	-0.003
	(-0.63)	(-0.14)
$YEAR_{0}$	-0.056*	-0.034
	(-1.91)	(-1.47)
$YEAR_{+1}$	-0.074**	-0.040**
	(-2.77)	(-2.15)
$YEAR_{+2}$	-0.089***	-0.047**
	(-2.98)	(-2.59)
$YEAR_{+3}$	-0.154***	-0.087***
	(-5.30)	(-4.50)
GDP	0.066	0.063
	(0.64)	(0.85)
VGDP	0.006	0.003
	(1.32)	(0.87)
IHERF	-0.208	-0.098
	(-1.62)	(-1.19)
FHERF	0.451**	0.215*
	(2.69)	(1.94)
NSTOCK	0.022	0.008
	(1.13)	(0.63)
LIB	-0.018	-0.010
	(-0.61)	(-0.39)
Constant	-0.669	-0.588
	(-0.74)	(-0.90)
Country fixed effects	Yes	Yes
Sample size	512	512
Number of countries	24	24
R-squared	0.18	0.14

The above argument, however, is in conflict with our findings since it predicts that it is the increase in stock price crash risk that leads to the enforcement of insider trading laws, not *vice versa*. To provide further evidence that our results are not driven by the potential reverse causality, we conduct two additional analyses. First, we plot in Figure 2 the average values of the two country-level crash risk measures across our 24 sample countries over the 11-year period from five years before the initial enforcement event to five years after the event, i.e., (-5, +5), where year 0 is the event year when a particular country initially enforced its insider trading laws. As illustrated in Figure 2, stock price crash risk begins to decrease in the event year and continues to drop over the next five years despite a small reversal from year +3 to year +4. The overall declining trend in crash risk over the post-event years, but not in the pre-event years, is in line with the view that the negative impact of initial enforcement on crash risk is unlikely to be driven by potential reverse causality.

Figure 2: Stock price crash risk across the enforcement of country insider trading laws

This figure plots the time-series of countries' average trend-adjusted NCSKEW and DUVOL from five years before to five years after an insider trading enforcement. NCSKEW is the trend-adjusted median value of negative skewness of firms-specific-weekly return for each country in each year. DUVOL is the trend-adjusted median 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 year. Country-year observations are transformed to event time, with an enforcement event in a particular country being an event.



Second, following Bertrand and Mullainathan (2003), we examine the dynamics of stock price crash risk and its changes surrounding the enforcement event-years, using the sample of firms from 24 countries that enforced their insider trading laws during the period of 1982-2006. Specifically, we construct seven year indicators, namely, *YEAR*-3, *YEAR*-2, *YEAR*-1, *YEAR0*, *YEAR*+1, *YEAR*+2, and *YEAR*+3 to indicate the relative years around the initial enforcement of insider trading laws, where *0* denotes the enforcement year. We then reestimate the baseline regression by replacing *ENFORCE* with the seven year indicators.

The results are presented in Table VI. We find that the coefficients on *YEAR*-3, *YEAR*-2 and *YEAR*-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 *YEAR0*, *YEAR*+1, *YEAR*+2, and *YEAR*+3 are negative and significant except for *YEAR0* in the regression with *DUVOL* as the dependent variable, indicating that from the year of initial enforcement, firms' stock price crash risk starts to decline substantially. Interestingly, we find that the coefficients on *YEAR0*, *YEAR*+1, *YEAR*+2, and *YEAR*+3 are gradually and monotonically increasing, suggesting that the impact of insider trading enforcement on mitigating crash risk is long lasting.[21] Overall, our analysis suggests that it is the insider trading law enforcement that leads to a reduction in stock price crash risk, not *vice versa*.

5.2. Controlling for potential omitted variables

In the main analysis, we have controlled for a standard set of variables that can affect both stock price crash risk and insider trading regulations based on previous studies. However, the negative association observed between the initial enforcement of insider trading laws and crash risk can be driven by some other correlated omitted variables. To mitigate this concern, we explicitly describe and control for these potential omitted variables and tabulate the results in Table VII.

Table VII: Controlling for potential omitted variables

The sample consists of firms jointly covered in Datastream and Worldscope between 1982 and 2006. The dependent variable is NCSKEW or DUVOL. NCSKEW is the trend-adjusted median value of negative skewness of firms-specific-weekly return for each country in each year. DUVOL is the trend-adjusted median value of the log of the ratio of the standard deviations of down-week to up-week firm-specific weekly returns for each country in each year. ENFORCE is a dummy variable that takes the value of one in the year of a country's first insider trading enforcement case and thereafter, and zero otherwise. INFORM is the log of median value of relative firm-specific stock return variation for each country in each year. ANALYST is the log of mean number of analysts providing a forecast for each firm listed in IBES for each country in each year. IFRS is a dummy variable that takes the value of one after a country's mandatory adoption of IFRS, and zero otherwise. 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	-0.079***	-0.048***	-0.089***	-0.055***	-0.097***	-0.059***
	(-3.35)	(-3.26)	(-3.68)	(-3.89)	(-3.48)	(-3.64)
INFORM	-0.052**	-0.028*			-0.064**	-0.034*
	(-2.07)	(-1.96)			(-2.30)	(-2.00)
ANALYST	-0.003	-0.003			0.014	0.009
	(-0.23)	(-0.34)			(0.79)	(0.87)
IFRS	-0.071***	-0.025*			-0.042*	-0.005
	(-3.13)	(-1.80)			(-1.73)	(-0.33)
LIQ			-0.022	-0.022*	-0.025	-0.025**
			(-1.09)	(-1.92)	(-1.43)	(-2.36)
OPEN			-0.049	-0.028	-0.030	-0.023
			(-0.90)	(-0.84)	(-0.51)	(-0.66)
FDI			0.154***	0.072***	0.153***	0.073***
			(4.87)	(3.13)	(5.12)	(3.20)
SHORT			0.103***	0.066***	0.108***	0.069***
			(2.81)	(3.13)	(3.08)	(3.32)
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	821	821	643	643	643	643
Number of countries	42	42	41	41	41	41
R-squared	0.11	0.08	0.13	0.12	0.16	0.14

We first consider variables related to firms' information environment since managerial incentives that cause stock price to be crash-prone are likely to be affected by the information environment (Jin and Myers, 2006). 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 the regressions. Following Fernandes and Ferreira (2009), we define stock price informativeness (*INFORM*) as the natural logarithm 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. We then use median values of this measure across firms for each country in each year in the regressions. As in Bushman Piotroski, and Smith (2005), analyst coverage (*ANALYST*) is calculated as the natural logarithm of mean number of analysts providing a forecast for a firm included in the IBES for each country in each year.

Furthermore, prior literature documents that mandatory adoption of IFRS significantly improves firms' information environment, and thus reduces crash risk (DeFond et al., 2015). To control for the potential confounding effect of mandatory IFRS adoption on our results, we construct an indicator (*IFRS*) that takes the value of one after a country's mandatory adoption of IFRS, and zero otherwise, and include this indicator in the regressions.

The results are reported in columns (1) and (2) of Table VII. Consistent with Jin and Myers (2006), the coefficient on *INFORM* is negative and significant. The effect of *ANALYST* on crash risk is negative but statistically insignificant at conventional levels. Similar to DeFond et al. (2015), we find that mandatory IFRS adoption has a negative and significant impact on two crash risk measures. More importantly, after controlling for the three variables related to firms' information environment, *ENFORCE* is still negatively and significantly associated with crash risk at less than the 1% level, suggesting that our main findings regarding the effect of the insider law enforcement are robust.[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 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 correlated 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 take into account the possible effect of actual trade and financial flows on crash risk. 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 market for capital, goods and services 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) and that corporate governance is of higher quality in an economy with freer flows of capital and trade, one may argue that 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, i.e., a country's trade openness (*OPEN*) and foreign direct investment (*FDI*), to capture these effects. *OPEN* and *FDI* are defined, respectively, as the sum of imports and exports in a country scaled by the country's GDP and the amount of foreign direct investment in a country scaled by the country's GDP.[24]

Fourth, we consider the possible effect of short selling restrictions. Diamond and Verrecchia (1987), Chen, Hong, and Stein (2001), and Beber and Pagano (2013) provide evidence that when short selling is allowed and practiced, negative information is incorporated into stock prices faster, which lowers stock price crash risk. A number of countries lifted the bans on short selling during 1990s when several countries enforced their insider trading laws. In an attempt to control for the potential confounding effect of short selling restrictions on our results, we include an indicator variable that represents the feasibility of short-selling, denoted by *SHORT*. Specifically, *SHORT* takes the value of one if short selling is infeasible in a country, and zero otherwise, based on the information reported in Charoenrook and Daouk (2005).

The results are reported in columns (3) and (4) of Table VII. We find that the effect of insider trading restrictions on stock price crash risk is still negative and significant at less than the 1% level even after controlling for *LIQ, OPEN, FDI*, and *SHORT.* Moreover, we find that *LIQ* has a negative impact while *FDI* has a positive impact on *NCSKEW* and *DUVOL*, suggesting that firms are more likely to experience stock price crashes in markets with lower liquidity and countries with lower barrier for capital flows. We find that the coefficients on *SHORT* are all positive and highly significant. Consistent with Diamond and Verrecchia (1987) and Beber and Pagano (2013), the findings suggest that bans on short selling lead to an increase in stock price crash risk. We find that trade openness is negatively associated with crash risk, albeit insignificant.

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

Finally, prior studies show that managers' compensation, equity-based compensation in particular, tends to increase after insider trading becomes restricted so as to make up for their lost profit-taking opportunities from trading on inside information (Roulstone, 2003; Denis and Xu, 2013). Hence, it is possible that insider trading restrictions affect managers' incentives to hoard bad information (and thus crash risk) through the impact on their equity-based compensation. Due to the lack of detailed compensation data for international firms, we cannot empirically investigate this issue. However, this argument is likely to be biased against our findings, because crash risk is likely to rise in response to an increase in equity-based compensation (Kim, Li, and Zhang, 2011b) in the post-enforcement period of insider trading law.

5.3. Additional sensitivity tests

In this section, we conduct a variety of additional sensitivity tests to further verify the validity of our findings and report these results 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;[25] (2) using mean rather than median values of firm-level *NCSKEW* and *DUVOL* to construct country-level measures of stock price crash risk;[26] (3) removing the country-years prior to 1990, as Fernandes and Ferreira (2009) point out that the Datastream/Worldscope started to largely expand its country coverage since 1990; (4) removing country-years during the Asian financial crisis (1997-1998), a period that follows a significant number of enforcements that occurred in 1995-1996; (5) excluding Japan, as it experienced a market crash and enforced its insider trading laws in the same year; (6) using firms that survive over the entire event window, i.e., three years before and three years after the insider trading law enforcement; (7) excluding firms with total sales less than 10 million U.S. dollars when estimating country-level stock price crash risk variables to make a more meaningful comparison across countries; and (8) using country random effects instead of country fixed effects when estimating regressions. As shown in Panels A through H of Table VIII, we find that the estimated coefficients of *ENFORCE* are all qualitatively identical to those reported in Table II, suggesting that our main regression results are robust to these sensitivity checks.

Table VIII: Additional sensitivity tests

The sample consists of firms jointly covered in Datastream and Worldscope between 1982 and 2006. NCSKEW is the trend-adjusted median value of negative skewness of firms-specific-weekly return for each country in each year. DUVOL is the trend-adjusted median value of the log of the ratio of the standard deviations of down-week to up-week firm-specific-weekly returns for each country in each year. ENFORCE is a dummy variable that takes the value of one in the year of a country’s first insider trading enforcement case and thereafter, and zero otherwise. EXIST is a dummy variable that takes the value of one in the year of a country's initial enactment of insider trading laws and thereafter, and zero otherwise. 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 = 958		
ENFORCE	-0.063***	-0.038***
	(-2.79)	(-2.82)
Panel B: Using country-level mean NCSKEW and DUVOL as dependent variables: N = 821		
ENFORCE	-0.078***	-0.046***
	(-3.43)	(-3.37)
Panel C: Removing country-years prior to 1990: N = 678		
ENFORCE	-0.092***	-0.056***
	(-3.44)	(-3.52)
Panel D: Removing years during the Asian financial crisis (1997-1998): N = 738		
ENFORCE	-0.080***	-0.048***
	(-3.53)	(-3.39)
Panel E: Excluding Japan: N = 796		
ENFORCE	-0.088***	-0.055***
	(-3.95)	(-3.98)
Panel F: Using firms that survive during the entire event window: N = 159		
ENFORCE	-0.059**	-0.039**
	(-2.39)	(-2.37)
Panel G: Excluding small firms with total sales less than $10 mil: N = 821		
ENFORCE	-0.078***	-0.048***
	(-3.34)	(-3.28)
Panel H: Using country random effects instead of country fixed effects to estimate regressions: N = 821		
ENFORCE	-0.064***	-0.035***
	(-3.58)	(-3.08)
Panel I: Effect of enforcement vs. enactment of insider trading law: N = 821		
ENFORCE	-0.084***	-0.052***
	(-3.5)	(-3.5)
EXIST	0.025	0.019
	(0.9)	(1.1)

Furthermore, 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 defined as an indicator variable that equals one in the year of a country's first enactment of insider trading laws and thereafter, and zero otherwise. The results are reported in Panel I of Table VIII. With the inclusion of *EXIST*, the coefficient on *ENFORCE* remains negative and significant at less than the 1% level. However, the coefficient on *EXIST* is statistically insignificant, suggesting that the law 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 findings of Bhattacharya and Daouk (2002, 2009), who argue that what matters more is the enforcement rather than the enactment of insider trading laws.

5.4. Firm-level analyses

In this section, we conduct firm-level analyses to provide more evidence to supplement our main findings at the country level. Following previous studies, we include an array of firm-level control variables in the regressions. Specifically, we control for lagged detrended share turnover (*DTURN*) as a proxy for investor heterogeneity (Chen, Hong, and Stein, 2001) and lagged *NCSKEW* to capture the potential persistence of the third moment of stock returns. We control for standard deviation of firm-specific weekly stock returns (*SIGMA*) and the average of firm specific weekly stock returns (*RET*) over the year, since Chen, Hong, and Stein (2001) find that more volatile stocks and stocks with higher past returns are more likely to crash. Following Hutton, Marcus, and Tehranian (2009) and Kim, Li, and Zhang (2011a,b), we include firm size (*SIZE*), defined as the natural logarithm of market capitalization, market-to-book ratio (*MB*), financial leverage (*LEV*), defined as total long-term debt over book value of equity, operating performance measured as income before extraordinary items divided by lagged total assets (*ROA*), and financial reporting opacity (*OPAQUE*) as defined in Hutton, Marcus, and Tehranian (2009).

Table IX: Firm-level analyses

The sample consists of firms jointly covered in Datastream and Worldscope between 1982 and 2006. The dependent variable is NCSKEW or DUVOL. NCSKEW is the trend-adjusted negative skewness of firms specific-weekly return for each firm in each year. DUVOL is the trend-adjusted value of the log of the ratio of the standard deviations of down-week to up-week firm-specific-weekly returns for each firm in each year. ENFORCE is a dummy variable that takes the value of one in the year of a country's first insider trading enforcement case and thereafter, and zero otherwise. DTURN is the average monthly share turnover over the current year minus the average monthly share turnover over the previous 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 year. RET is the average firm-specific weekly returns over the 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. OPAQUE is the prior three years' moving sum of the absolutely 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*
	(1)	(2)	(3)	(4)
$ENFORCE_t$	-0.131***	-0.068***	-0.122***	-0.065***
	(-6.84)	(-5.82)	(-3.69)	(-3.33)
$DTURN_{t-1}$	-0.027	-0.004	-0.030	-0.007
	(-1.47)	(-0.35)	(-1.32)	(-0.47)
$NCSKEW_{t-1}$	0.047***	0.025***	-0.110***	-0.062***
	(9.81)	(9.47)	(-7.89)	(-8.46)
$SIGMA_{t-1}$	2.033**	0.603	1.759***	0.504
	(2.41)	(1.20)	(3.01)	(1.31)
RET_{t-1}	-3.262	-1.079	-1.358	-5.606
	(-0.99)	(-0.50)	(-0.28)	(-1.66)
$SIZE_{t-1}$	0.011**	0.007***	0.068***	0.042***
	(2.64)	(2.84)	(8.20)	(11.28)
MB_{t-1}	0.003***	0.002***	0.002	0.001
	(6.33)	(4.26)	(1.40)	(0.82)
LEV_{t-1}	0.056***	0.022***	0.225***	0.118***
	(6.65)	(4.43)	(4.70)	(5.19)
ROA_{t-1}	-0.069***	-0.036***	-0.054***	-0.031***
	(-4.47)	(-3.89)	(-4.62)	(-3.85)
$OPAQUE_{t-1}$	0.009	0.001	0.011	0.002
	(1.21)	(0.32)	(0.67)	(0.19)
Constant	-0.064	-0.009	-0.913***	-0.532***
	(-0.30)	(-0.08)	(-10.11)	(-12.69)
Industry fixed effects	Yes	Yes	No	No
Country fixed effects	Yes	Yes	No	No
Firm fixed effects	No	No	Yes	Yes
Sample size	69,018	69,018	69,018	69,018
Number of countries	42	42	42	42
R-squared	0.03	0.03	0.23	0.23

When estimating regressions with the inclusion of aforementioned firm-level variables, we adjust *t*-statistics using standard errors corrected for country-level clustering. We report the results that control for industry and country fixed effects in columns (1) and (2), while we report the results that control for firm fixed effect in columns (3) and (4) of Table IX. We find that the coefficients on *ENFORCE* remain negative and highly significant across all columns at less than the 1% level. The signs of the coefficients on control variables are, overall, consistent with those reported in previous literature. The firm-level analyses hence lend further support to the robustness of our findings at the country level.

6. Concluding Remarks

In this study, we attempt to establish a causal effect of insider trading on stock price crash risk from the perspective of managerial disclosure. In doing so, we examine cross-country and inter-temporal variation in stock price crash risk using a large sample of firms from 48 countries over the period 1982-2006, during which the majority of our sample countries started to enforce their insider trading laws. We find strong and robust evidence that stock price crash risk substantially declines after the initial enforcement of insider trading laws in a country. We interpret this finding as supportive evidence that insider trading restrictions weaken managers' incentives to withhold bad news from public disclosure by making insiders' trading activities costly and risky.

We then 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 in countries where managers' costs of delay in bad news release is stronger, e.g., where investors' rights are poorly protected, the financial disclosure environment is less transparent, the financial market has not yet been liberalized, or the product market is less competitive. Moreover, our findings are robust to a variety of sensitivity checks for potential problems associated with reverse causality, correlated omitted variables, and model misspecifications.

Collectively, our findings highlight insider trading as an important cause of occurrences of extremely negative prices. Furthermore, our study from the perspective of the third moment of stock returns (i.e., stock price crash risk) provides an alternative explanation for the negative relation between cost of capital and insider trading enforcement: insider trading restrictions reduce crash risk that cannot be diversified away, and thus contribute to lowering the cost of equity capital.

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[1] For example, according to Fortune (July 31, 2012), Zynga, a provider of social game services, was hit by a class action lawsuit alleging that its senior executives dumped shares before a stock price crash. Zynga's executives sold their stocks at $12 on April 3, 2012. On July 27, Zynga announced disastrous Q2 earnings and its shares fell to $3 one week later on July 27, 2012.

[2] In this paper, we assume that managers, on average, delay bad news disclosure and examine the incremental effect of insider trading on the consequences of such disclosure behaviors. How insider trading affects the disclosure behaviors of an average manager, however, is beyond the scope of this research.

[3] For the literature on stock price crashes caused by managerial disclosure incentives, see Kim, Li, and Zhang (2011a,b), DeFond et al. (2015), Hong, Kim, and Welker (2013), and Kim, Yeung, and Zhou (2013) among others.

[4] Our analysis focuses on the effect of insider trading on disclosure timing of bad news rather than that of good news. Arguably, insider trading could also motivate the manager to quickly release good news by giving her an opportunity to earn trading profits through purchasing her own company stocks ex ante. However, this incremental effect of insider trading on good new disclosure for an average manager is likely to be trivial given the huge benefits associated with timely disclosure of good news in terms of her reputation, job security, and personal wealth tied to firm performance (Kothari, Shu, and Wysocki, 2009). Hence it is unlikely that the manager chooses to forgo these benefits by delaying good news disclosure when insider trading is restricted by laws. Differently, delay in bad news disclosure is determined, according to Kothari, Shu, and Wysocki (2009), by the benefits regarding the manager's job security and personal wealth as well as the related costs, such as her reputation losses and litigation risk arising from the delay. As a result, the incremental effect of insider trading is crucial for the manager's decision on bad news release.

[5] We do the aggregation because: first, insider trading laws in a country affect all stocks in that country; second, firm-level analysis is tilted towards large countries with many firms, while country-level analysis treats each country equally. In Section 5.4, we conduct a firm-level analysis to supplement our main analysis at the country level.

[6] Time-invariant country-level governance variables are not controlled in the regressions since we explore the intertemporal variation in stock price crash risk before and after the insider trading law

enforcement in a country by including in the regression model country fixed effects. In untabulated tests, we include variables such as indices of investor protection, rule of law, corruption, good government, and shareholder rights in the random effect regressions and obtain qualitatively similar results.

[7] 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).

[8] 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).

[9] In our country-level analysis, we do not choose 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, as pointed out by DeFond et al. (2015), the aggregation of the firm level dichotomous measure to the country level has less power in detecting the hypothesized association between insider trading laws and crash risk compared to the other two continuous measures of crash risk. In addition, 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.

[10] Untabulated results indicate that our findings remain similar if year fixed effects are included in the regressions.

[11] 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 0.154 and 0.095, respectively. Since ENFORCE is a dummy variable, its coefficient captures the extent of crash risk decline before and after the enforcement.

[12] Among the 48 sample countries, 29 countries initially enforced their insider trading laws during the sample period. However, stock price data before the enforcement events are unavailable for Czech Republic, Israel, Korea, Peru, and Poland. Hence the sample size for the event study is reduced to 24 countries.

[13] 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 a higher crash risk in other five countries after initial enforcement of insider trading laws is that these countries are affected either by financial crisis or by banking crisis that occurred following the enforcement.

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

[15] Also, we follow Fernandes and Ferreira (2009) and Bushman, Piotroski, and Smith (2005) and examine the effect of the insider trading enforcement in both emerging and developed markets. Untabulated 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 difference in the effects between the two groups is statistically insignificant.

[16] Bushman, Piotroski, and Smith (2005) find that the enforcement of insider trading laws is associated with a smaller increase in analyst following in countries with stronger investor protection. In contrast, Fernandes and Ferreira (2009) show that the improvement of stock price informativeness

after initial enforcement of insider trading laws is restricted only in countries where investors' private property rights are well protected.

[17] We also 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 the negative effect of insider trading restrictions on stock price crash risk is less pronounced in common law countries and in countries with stronger shareholder rights, which are consistent with our findings on ASDI.

[18] We use the financial report opacity (OPAQUE) in Hutton, Marcus, and Tehranian (2009) as an alternative measure of disclosure transparency. As in Hutton, Marcus, and Tehranian (2009), we measure OPAQUE as the prior three years' moving sum of the absolutely value of discretionary accruals estimated according to Dechow, Sloan, and Sweeney (1995). We then use median values of firm-level OPAQUE for each country in each year for the country-level analysis. Untabulated results indicate that the negative effect of insider trading restrictions on stock price crash risk is stronger in countries that are more opaque in financial reporting, which is consistent with our findings that use DISC as a measure for disclosure transparency.

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

[20] Specifically, we use FHERF one year prior to the insider trading enforcement for countries that enforced their insider trading laws during the sample period and FHERF in the first available year for countries that haven't enforced their insider trading laws during the sample period, respectively.

[21] As shown in Table VI, the coefficient on YEAR+3 is almost twice as large as those on YEAR+1 and YEAR+2, suggesting that only approximately half of the effect is realized in years +1 and +2, and another half is realized in the following years after the enforcement.

[22] In an untabulated test, we consider two additional variables that measure firms' financial reporting environment i.e., financial reporting opacity and conditional accounting conservatism. Specifically, we include median values of Hutton, Marcus, and Tehranian's (2009) financial reporting opacity (OPAQUE) and median values of Khan and Watts' (2009) conditional conservatism (C_SCORE) across firms for each country in each year in the regressions. We find that our results after not affected by the inclusion of these two variables.

[23] In an untabulated test, we follow Chen, Hong, and Stein (2001) and Kim, Li, and Zhang (2011b) and replace LIQ with detrended monthly share turnover as a proxy for investor heterogeneity. The inclusion of this variable does not alter our results.

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

[25] To control for the time trend, we include year fixed effects in the regressions for this test. However, the results are unaffected if we exclude them.

[26] In an untabulated test, we replace equally-weighted average crash risk measures with value-weighted average crash risk measures as dependent variables, and find that the results remain similar.