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Does Mandatory IFRS Adoption Affect Crash Risk?

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ABSTRACT: We test whether mandatory IFRS adoption affects firm-level "crash risk," defined as the frequency of extreme negative stock returns. We separately analyze nonfinancial firms and financial firms because IFRS is likely to affect their crash risk differently. We find that IFRS adoption decreases crash risk among nonfinancial firms, especially among firms in poor information environments and in countries where IFRS adoption results in larger and more credible changes to local GAAP. In contrast, IFRS adoption has no effect on crash risk for financial firms, on average, but decreases crash risk among firms less affected by IFRS's fair value provisions, and increases crash risk among banks in countries with weak banking regulations. Overall, our results are consistent with the increased transparency from IFRS adoption broadly reducing crash risk among nonfinancial firms, but more selectively among financial firms, and with financial regulations playing a complementary role in implementing IFRS among financial firms.

Keywords: *International Financial Reporting Standards; crash risk; nonfinancial firms; financial firms*.

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I. INTRODUCTION

Crash risk, defined as the frequency of extreme negative stock returns, is a significant concern for investors. Prior research suggests that the financial reporting environment is an important determinant of crash risk (Jin and Myers 2006; Bleck and Liu 2007; Hutton, Marcus, and Tehranian 2009). The financial reporting environment changed significantly in 2005 for thousands of public companies around the world when dozens of countries simultaneously mandated the adoption of International Financial Reporting Standards (IFRS). This event provides a natural setting for testing whether changes in the financial reporting environment affect firm-level crash risk across a variety of industries and institutional environments. Thus, the purpose of this study is to test whether mandatory IFRS adoption affects crash risk.

We separately analyze nonfinancial and financial firms because IFRS adoption is likely to affect crash risk for these two groups through different mechanisms. For nonfinancial firms, we expect IFRS to affect crash risk primarily through additional disclosure and improved comparability, which are expected to increase reporting transparency. This expectation is consistent with a large body of literature that finds IFRS results in a variety of capital market benefits (Daske, Hail, Leuz, and Verdi 2008; Li 2010; Byard, Li, and Yu 2011; DeFond, Hu, Hung, and Li 2011; Tan, Wang, and Welker 2011). Research suggests that increased transparency decreases crash risk by reducing managers' ability to withhold bad news (Jin and Myers 2006; Hutton et al. 2009). Thus, for nonfinancial firms, we predict that IFRS adoption decreases crash risk.

For financial firms, we expect IFRS adoption to affect crash risk through three channels, the net effect of which is difficult to predict. One channel is through the same additional disclosures that lead to increased reporting transparency for nonfinancial firms. However, most of these disclosures, such as the elimination of LIFO, apply primarily to nonfinancial firms. Thus, we expect these transparency effects to be relatively small for financial firms when compared to the effects for nonfinancial firms. A second channel is through the fair value consequences of implementing International Accounting Standard (IAS) 39. While we expect this to be the dominant channel for financial firms, we cannot predict whether it increases or decreases crash risk. On one hand, if fair value accounting better reflects firms' true underlying performance, then it should increase transparency, thereby reducing crash risk (Bleck and Liu 2007). On the other hand, if fair value accounting introduces measurement errors that reduce investors' ability to observe firms' true underlying performance, then it should increase opacity, thereby increasing crash risk (European Central Bank 2004). A third channel is through changes in management risk-taking, with crash risk effects that are also difficult to predict. Fair value accounting may encourage investment in risky assets by amplifying the upside potential of investment gains (Li 2009). If fair value estimates of riskier assets contain relatively more measurement errors, then they will increase opacity, thereby increasing crash risk. Alternatively, the increased volatility associated with fair value accounting may induce managers to reduce investments in risky assets, thereby reducing crash risk.

We test the effects of mandatory IFRS adoption on crash risk using a sample of 8,472 nonfinancial and 1,748 financial firm-year observations encompassing 27 countries that mandate IFRS in 2005. Our primary analysis focuses on 2005 IFRS adopters and uses two years before and after the IFRS mandate. We use this relatively short event window to reduce the effects of potential confounding events, such as the 2008 amendment to IAS 39 permitting reclassification of financial assets from market value to historical cost.

We employ a difference-in-differences research design that compares the change in crash risk for mandatory IFRS adopters with changes for three benchmark samples over the period 2003–2006. Our benchmark samples control for non-IFRS related changes that may affect crash risk (Li 2010; DeFond et al. 2011). The three benchmark samples consist of: (1) local GAAP users in non-IFRS adopting countries, termed "non-IFRS adopters," (2) non-IFRS adopters that are propensity-

score-matched (PSM) with mandatory adopters, termed "PSM non-IFRS adopters," and (3) firms that voluntarily adopt IFRS prior to 2005, termed "voluntary adopters."

Our analysis supports the hypothesis that crash risk decreases for nonfinancial firms after mandatory adoption. In addition, the decrease is greater among nonfinancial firms in poor information environments, and among nonfinancial firms in countries where IFRS results in larger and more credible changes to local GAAP.[1] These cross-sectional results support the notion that IFRS reduces crash risk for nonfinancial firms by increasing financial reporting transparency.

Our analysis of financial firms finds no significant change in crash risk, on average. We also find no evidence that crash risk increases among two subsamples of financial firms among which fair value reporting is likely to have a relatively larger impact: firms for which IAS 39 results in relatively more changes to local GAAP, and banks that have a larger proportion of their assets invested in trading and investment securities. We further find that crash risk decreases among financial firms for which IAS 39 results in relatively fewer changes to local GAAP, consistent with financial firms experiencing increased reporting transparency from IFRS adoption that is unrelated to IAS 39. Finally, we find that crash risk increases for banks in countries with less restrictive banking regulations, a setting that allows greater investment in risky assets, whose fair values are more difficult to estimate. This is consistent with IAS 39 encouraging greater risk-taking in countries with weak banking regulations, which in turn increases crash risk.

Finally, we find that the significant average post-IFRS decrease in crash risk for nonfinancial firms is robust to a variety of sensitivity tests, including the use of alternative sample countries, periods, crash risk measures, and control variables. However, the insignificant average post-IFRS change in crash risk for financial firms is sensitive to the choice of sample countries and crash risk measures, with some tests finding that crash risk declines for financial firms. This sensitivity is consistent with our overall conclusion that the effect of IFRS adoption is relatively more selective among financial firms.

Our study contributes to the literature in several ways. One contribution is investigating a consequence of IFRS adoption not previously examined. Crash risk captures negative return skewness (the third moment of stock returns), which is distinct from measures studied in prior research, such as the average return (the first moment), and the variance of returns (the second moment). The recent financial crisis has drawn increased attention to tail risk, a function of skewness, because extreme negative events can impose significant losses on investors. By examining crash risk, we contribute to research examining the impact of accounting standards on asset pricing, and to the recent research that examines tail events (Jin and Myers 2006; Hutton et al. 2009; Kim, Li, and Zhang 2011a, 2011b).[2]

We also contribute to the literature that links crash risk to financial reporting transparency. Using international data, Jin and Myers (2006) find that country-level crash risk is associated with several country-level measures of financial reporting transparency. Using U.S. data, Hutton et al. (2009) corroborate this association by finding that crash risk is associated with the average absolute value of discretionary accruals, where smaller discretionary accruals proxy for greater transparency. We differ from Hutton et al. (2009) in two ways. First, our proxy for increased transparency is IFRS adoption, which increases disclosure throughout the financial statements and, hence, is more comprehensive than discretionary accruals, which focuses narrowly on earnings quality. We also

[1] Examples of the differences in local GAAP include: (1) no or limited segment reporting, (2) no or limited capitalization of leases, (3) no disclosure of FIFO inventory cost when LIFO is used, (4) no or limited disclosure requirements for related-party transactions, and (5) no disclosure requirement of cash flow statement. Such changes are likely to reduce the ability of mandatory IFRS adopters to withhold or defer the disclosure of bad news.

[2] Left tail risk is also important if idiosyncratic return skewness is a priced component of stock returns, as suggested by prior research (Brunnermeier, Gollier, and Parker 2007; Boyer, Mitton, and Vorkink 2010; Conrad, Dittmar, and Ghysels 2013).

find that IFRS adoption is associated with decreased crash risk after controlling for discretionary accruals, indicating that IFRS adoption does not simply capture a decline in discretionary accruals.[3] Second, we employ a difference-in-differences research design, which, unlike cross-sectional association tests, controls for confounding concurrent factors and better identifies the causal effect of IFRS adoption.

Our study also adds to the literature by finding that IFRS adoption affects crash risk differently for nonfinancial and financial firms. While IFRS pervasively decreases crash risk for nonfinancial firms, IFRS has no effect on crash risk for financial firms, on average. We do, however, find that IFRS decreases crash risk for a subset of financial firms with small changes in fair value provisions, consistent with financial firms experiencing improved transparency through sources other than IFRS's fair value provisions. These findings complement prior international studies that examine the importance of institutional arrangements on the economic consequences of financial reporting regulations (Li 2010; Byard et al. 2011; DeFond et al. 2011).

Finally, our study adds to the research that examines the effects of fair value accounting on financial firms (Laux and Leuz 2010; Bhat, Frankel, and Martin 2011). While Laux and Leuz (2010) suggest that fair value accounting does not exacerbate financial crises, Bhat et al. (2011) find that fair value accounting increases systemic risk and feedback trading in the banking industry. Our study complements this research by examining whether a fair-value-based accounting regime affects firm-specific crash risk. Contrary to concerns expressed by regulators and the financial press (European Central Bank 2004; Hargreaves 2005), our evidence does not suggest that increased volatility from fair value accounting increases crash risk, on average. However, we do find that crash risk increases for banks in countries with weak banking regulations, consistent with IFRS encouraging increased risk-taking in the absence of strong financial regulations. However, an important caveat to our analysis of financial firms is that it is relatively exploratory in nature because little is known about the determinants of crash risk, or the effects of IFRS adoption, for financial firms. We provide some initial evidence in this area, which has been largely ignored in the literature.

II. HYPOTHESIS DEVELOPMENT

The Impact of IFRS Adoption on Crash Risk for Nonfinancial Firms

Proponents of IFRS adoption argue that it increases transparency, which improves financial performance comparisons across different jurisdictions (Tweedie 2006).[4] Consistent with this assertion, research finds that IFRS adoption, when credibly implemented, has favorable capital market consequences, including reducing the cost of capital, increasing liquidity, improving firms' information environments, and increasing financial reporting comparability (Daske et al. 2008; Li 2010; Byard et al. 2011; DeFond et al. 2011; Tan et al. 2011). However, most of these studies base their findings on samples composed entirely of nonfinancial firms, or on pooled samples that are dominated by nonfinancial firms.

For nonfinancial firms, we predict that the increased transparency associated with IFRS adoption reduces crash risk. The theoretical model in Jin and Myers (2006) suggests that increased opacity results in managers withholding firm-specific bad news from public disclosure. However,

[3] Recent research finds that IFRS adoption does not reduce discretionary accruals, further suggesting that our results are not explained by increased accruals quality (Ahmed, Neel, and Wang 2013).

[4] We use the term IFRS to refer to both IFRS issued by the International Accounting Standards Board (IASB) and the International Accounting Standards (IAS) issued by the IASB's predecessor, the International Accounting Standards Committee (IASC).

managers are only able or willing to suppress bad news up to a point. Once this threshold is met, the accumulated bad news is disclosed all at once, resulting in a stock price crash. Notably, this theory only requires that managers have the ability and incentives to control public access to at least some negative information about firm value. A classic example is Parmalat, whose opaque financial reporting enabled insiders to hide their tunneling activities for more than a decade (*Wall Street Journal* 2004). Supporting this information-based theory, Jin and Myers (2006) find that country-level differences in opacity are associated with cross-country differences in crash frequencies, and Hutton et al. (2009) find that discretionary accruals are associated with crash risk among nonfinancial U.S. firms. This suggests that an important channel through which IFRS adoption may reduce crash risk is increased disclosure and comparability that leads to increased transparency. Thus, our first hypothesis in alternative form is:

H1: Nonfinancial firms experience a decrease in crash risk subsequent to mandatory IFRS adoption.

We note that while increased opacity may also be associated with income smoothing, we expect that managers are more likely to hide bad news than good news (Kothari, Shu, and Wysocki 2009). In addition, we expect that when news is particularly bad, managers tend to under-report earnings in an attempt to reduce the precision of the bad news and shift discretionary income to future periods (Hutton et al. 2009).

We also expect IFRS's effect on crash risk to differ across firms. If IFRS decreases crash risk through increased financial reporting transparency, then we expect the effects to be more pronounced among nonfinancial firms in poor information environments. This is because firms in poor information environments are likely to be more opaque prior to IFRS adoption. Thus, they are likely to experience greater improvements in financial transparency, which in turn results in a larger decrease in crash risk after mandatory IFRS adoption. Consequently, we hypothesize the following:

H1A: Nonfinancial firms in poor information environments experience a greater decrease in crash risk than other nonfinancial firms subsequent to mandatory IFRS adoption.

Further, if IFRS decreases crash risk through improved transparency, then we also expect the effects to be more pronounced in countries where IFRS results in larger changes to local GAAP. However, we only expect this effect in countries with strong enforcement, where IFRS is credibly implemented. Consequently, we hypothesize the following:

H1B: Nonfinancial firms in countries with larger and more credible changes in accounting standards experience a greater decrease in crash risk than other nonfinancial firms subsequent to mandatory IFRS adoption.

The Impact of IFRS Adoption on Crash Risk for Financial Firms

Compared to its effect on nonfinancial firms, the impact of IFRS on crash risk for financial firms is likely to be more nuanced. This is because IFRS is likely to affect crash risk for financial firms through three channels, the net effect of which is ambiguous. One channel is through the increased transparency that affects nonfinancial firms, which is also expected to affect financial firms, although to a lesser degree. For example, while financial firms are not affected by changes such as the elimination of LIFO inventory accounting, they may be affected by requirements for disclosure of related-party transactions or a cash flow statement. To the extent that mandatory IFRS adoption increases transparency among financial firms, it should also decrease crash risk, although the effects are likely to be much smaller than the effects for nonfinancial firms.

A second channel is through the effects of fair value accounting that result from IAS 39, a provision in IFRS that specifically targets financial firms.[5] While we expect these fair value effects to dominate the transparency effects unrelated to IAS 39, it is not clear whether they increase or decrease crash risk. On one hand, if fair value accounting better reflects true underlying performance, then it reduces managers' ability to hide bad news. Theoretical work by Bleck and Liu (2007) suggests that mark-to-market accounting provides investors with an early warning system, while historical cost accounting offers management greater opportunities to mask firms' true economic performance. This is also consistent with recent empirical work that documents financial analysts frequently demanding fair value information in conference calls with global banks (Bischof, Daske, and Sextroh 2014).[6] Thus, if shifting from historical cost accounting to the fair value provisions under IFRS reduces opacity, then we expect it to decrease crash risk (Jin and Myers 2006).

On the other hand, if the fair value accounting associated with IAS 39 reduces transparency, then it should increase the ability of managers to hide bad news. Factors that may impair the ability of fair value accounting to better capture firms' underlying economics include noise in the fair value measures and managers' ability to bias those measures. Noise in the fair value measures can arise due to difficulties in measuring firms' underlying fundamentals, while bias can arise due to contracting incentives such as debt covenants and compensation contracts, capital markets effects via earnings expectations, and regulation.

The noise and bias in fair value estimates introduce measurement errors that potentially increase earnings volatility, which impairs the ability of investors to observe true firm performance and therefore increases reporting opacity (Ball 2006; Plantin, Sapra, and Shin 2008). The sources of such errors include "model noise" and opportunistic discretion in mark-to-market accounting, fair values that are unrepresentative of true exit prices in the event of a crisis, fair values that capture short-term fluctuations that are unreflective of long-term management decision making, large bid-ask spreads in illiquid markets, and trading by managers in illiquid markets to influence prices. Thus, if the increased volatility associated with IAS 39 increases opacity, then we expect IFRS adoption to increase crash risk.

The third channel for IAS 39 to affect crash risk is through changes in management's appetite for risky investments, with effects on crash risk that are difficult to predict. Critics of fair value accounting argue that it encourages excessive investment in risky assets because the recognition of unrealized gains amplifies the upside potential of risky investments (Li 2009). Because riskier assets include investments whose fair values are difficult to assess, such as real estate and thinly traded securities, their fair value estimates are more likely to contain measurement errors. If so, the risky investments are also more likely to reduce reporting transparency. Alternatively, it is possible that the higher expected volatility associated with fair value accounting instead reduces managers' incentives to make risky investments, because it exposes their firms to higher earnings volatility, increased cost of capital, and greater regulatory risk. If this is the case, then the reduction in risky assets makes it easier for fair values to capture true underlying performance, which increases reporting transparency. Thus, while fair value accounting may change management's investment in

[5] IAS 39 was originally issued in December 1998 and became effective January 1, 2001. The fair value accounting effects of IFRS adoption are likely to be negligible for nonfinancial firms because most of the IAS 39 fair value provisions are applicable only to financial instruments. While IFRS allows the voluntary use of fair values for nonfinancial assets such as property, plant, and equipment (PPE), Christensen and Nikolaev (2009) find that the majority of IFRS adopters choose to value these assets at historical costs.

[6] In addition to IAS 39, Muller, Riedl, and Sellhorn (2011) find that IAS 40 also results in substantial adoption of fair values in the real estate industry, which is included in our sample of financial firms.

risky assets, its effects on financial reporting transparency and crash risk are difficult to predict, leading to our second hypothesis, without a directional prediction:

H2: Financial firms may experience either an increase or a decrease in crash risk subsequent to mandatory IFRS adoption.

To shed further light on whether fair value accounting is a channel through which IFRS affects crash risk for financial firms, we also examine two subsamples for which fair value reporting is likely to have a larger impact. One is a subsample of firms whose local GAAP experiences a relatively larger increase in its fair value provisions under IAS 39.[7] The other is a subsample of banks that have a relatively larger proportion of assets invested in trading and investment securities, and thus have greater exposure to IFRS's fair value accounting provisions. These subsamples are chosen to increase the power of our tests to detect the fair value effects of IAS 39. Again, we do not have a directional prediction for these hypotheses:

H2A: Financial firms that experience relatively larger increases in fair value provisions experience either an increase or a decrease in crash risk subsequent to mandatory IFRS adoption.

H2B: Financial firms with relatively more exposure to fair value accounting experience either an increase or a decrease in crash risk subsequent to mandatory IFRS adoption.

Finally, we perform an analysis that attempts to isolate whether increased risk-taking is a channel through which IFRS affects crash risk for financial firms. This test examines a subsample of banks in countries with less restrictive banking regulations, because these countries tend to allow more aggressive risk-taking by management (Laeven and Levine 2009). We propose the following non-directional hypothesis:

H2C: Financial firms in countries with relatively weaker banking regulation experience either an increase or a decrease in crash risk subsequent to mandatory IFRS adoption.

III. SAMPLE, DATA, AND RESEARCH DESIGN

Sample

Our treatment sample includes mandatory IFRS adopters in countries that mandate IFRS adoption in 2005. We focus on the two years before and after the IFRS mandate, so that for a December year-end company, the pre-adoption period consists of 2003 and 2004, while the post-adoption period consists of 2005 and 2006. This relatively short event window reduces the effect of confounding events such as the Sarbanes-Oxley Act in 2002, and the financial crisis and IAS 39 amendment in 2008 (Bischof, Bruggemann, and Daske 2011).[8] To identify mandatory adopters, we select companies that report under local accounting standards in the two-year pre-adoption period and under IFRS in the two-year post-adoption period. We collect financial statement information from Compustat and Worldscope, stock return and volume data from Datastream, and information on analyst following from I/B/E/S. Our treatment sample consists of 10,220 firm-year observations

[7] Local GAAP for financial firms varies across countries. For example, domestic standards of some countries (e.g., Sweden and the U.K.) require trading securities to be valued at fair value. Some GAAP also requires lower-of-cost or market (LCM) for valuing financial assets, which might dampen the effects of fair value accounting on crash risk.

[8] We expect the effect of SOX to play a role in our setting because U.S. firms are included in the benchmark samples of non-IFRS adopters and PSM non-IFRS adopters, and international markets are often affected by the U.S. capital markets, as reflected in the inclusion of the U.S. market index return in our market model regression.

of mandatory IFRS adopters worldwide, including 8,472 nonfinancial and 1,748 financial observations, firms with the one-digit SIC code of 6.

We include three benchmark groups to control for the impact of potentially confounding concurrent events. The three benchmark groups are: (1) "non-IFRS adopters," which consist of local GAAP users in 19 non-IFRS adopting countries, including 26,228 nonfinancial and 4,152 financial observations; (2) "PSM non-IFRS adopters," which consist of local GAAP users that are propensity-score-matched based on characteristics that typify the treatment group of mandatory adopters, including 5,977 nonfinancial and 1,128 financial observations; and (3) "voluntary adopters," which consist of voluntary IFRS adopters, including 1,400 nonfinancial and 196 financial observations. We define the variables in our analysis in Appendix A and discuss the approach to develop our propensity-score-matched sample in Appendix B.

Each of the three benchmarks has its advantages and limitations. Voluntary adopters share economic and regulatory commonality with mandatory adopters, but they are often regarded as a non-random group subject to potential self-selection bias. Non-IFRS adopters or PSM non-IFRS adopters, on the other hand, control for contemporaneous effects that are unrelated to the introduction of IFRS, but are potentially influenced by unspecified cross-country differences. In addition, while PSM non-IFRS adopters reduce differences between treatment and control firms, the theoretical underpinning of our PSM model is limited because we should be using country-level factors to model the choice of mandatory IFRS adoption in order to derive our propensity scores. However, because we need to match at the firm level, we necessarily use firm-level determinants.

Because of the limitations of the benchmark groups, we draw our conclusions based on the results of all three benchmarks in our primary analysis. For parsimony, we present our subsequent partitioning and sensitivity analysis using just one of our benchmarks, the non-IFRS adopters. Besides having the largest sample size, this benchmark avoids the self-selection bias with the voluntary adopters, and the limited theoretical underpinning with the PSM non-IFRS adopters.

Table 1 reports the firm-year distribution of nonfinancial and financial firms across the treatment sample and the three benchmark samples. Panel A indicates that the number of firm-year observations of mandatory IFRS adopters vary considerably, ranging from zero in Estonia and Malta to 1,872 in Australia for nonfinancial firms, and similarly for financial firms. Voluntary adopters are dominated by observations from Germany and Switzerland, consistent with the cross-country variance in IFRS-related regulations. Panel B also shows a significant variation in sample distribution across non-IFRS adopters.

Variables

Measuring Crash Risk

Our crash risk measure is based on skewness, defined as the third moment scaled by the second moment.[9] This measure was initially proposed by Chen, Hong, and Stein (2001) to capture the asymmetry of the return distribution and has been frequently used in the literature (Kim et al. 2011a, 2011b). Negative (positive) values for the skewness indicate data that are skewed to the left (right). When a stock return distribution is left-skewed, the left tail is more pronounced and longer than the right tail, and the firm has a disproportionate likelihood of experiencing extreme negative stock returns. We multiply the skewness measure by −1 so that a higher value corresponds with greater downside tail risk.

To calculate our crash risk measure, we first estimate the following expanded market model regression based on prior studies (Morck, Yeung, and Yu 2000; Jin and Myers 2006):

[9] Bulmer (1979) suggests that for the absolute value of skewness greater than 1, between 1 and 0.5, and less than 0.5, the distribution is highly skewed, moderately skewed, and approximately symmetric, respectively.

TABLE 1

Sample Distribution

Panel A: Mandatory and Voluntary Adopters

	Mandatory IFRS Adopters		Voluntary IFRS Adopters	
Country	**Nonfinancial**	**Financial**	**Nonfinancial**	**Financial**
Australia	1,872	200	12	4
Austria	24	4	84	16
Belgium	140	48	36	8
Czech Rep	8	0	8	4
Denmark	204	76	48	0
Estonia	0	0	12	4
Finland	296	44	28	0
France	1,044	144	80	0
Germany	332	60	504	36
Greece	200	28	12	0
Hong Kong	364	240	4	24
Hungary	4	0	28	4
Ireland	76	20	0	0
Italy	408	136	0	0
Luxembourg	4	0	0	0
Malta	0	0	8	4
The Netherlands	304	60	8	0
Norway	220	76	8	4
Philippines	88	88	164	8
Poland	20	16	12	4
Portugal	100	12	4	4
Slovenia	12	0	4	0
South Africa	348	84	56	8
Spain	200	60	0	0
Sweden	608	96	20	0
Switzerland	84	12	252	64
U.K.	1,512	244	8	0
Total	8,472	1,748	1,400	196

Panel B: Non-IFRS Adopters and PSM Non-IFRS Adopters

	Non-IFRS Adopters		PSM Non-IFRS Adopters	
Country	**Nonfinancial**	**Financial**	**Nonfinancial**	**Financial**
Argentina	96	32	20	10
Brazil	288	32	66	6
Canada	1,536	164	498	59
Chile	248	64	76	23
China	1,132	0	271	0
Columbia	28	8	11	1
India	712	44	158	8
Indonesia	444	56	32	13
Israel	36	40	8	12

(continued on next page)

TABLE 1 (continued)

Country	Non-IFRS Adopters		PSM Non-IFRS Adopters	
	Nonfinancial	Financial	Nonfinancial	Financial
Japan	8,400	636	2,166	209
Korea	1,052	128	227	30
Malaysia	1,756	240	555	107
Mexico	168	12	46	4
Morocco	4	12	1	4
Pakistan	104	16	34	3
Sri Lanka	36	20	9	1
Taiwan	676	28	132	7
Thailand	680	164	207	44
U.S.	8,832	2,456	1,460	587
Total	26,228	4,152	5,977	1,128

Table 1 presents firm-year distributions of the treatment sample of mandatory IFRS adopters as well as the three benchmark groups. Our sample includes firm-year observations of these four groups two years before and after the IFRS mandate in 2005.

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

where $r_{i,t}$ is the return on stock i in week t in country j; $r_{m,j,t}$ is the return on the MSCI country-specific market index or the Datastream country index in week t; $r_{US,t}$ is the U.S. market index return in week t; and $EX_{j,t}$ is the change in country j's exchange rate versus the U.S. dollar.

Equation (1) includes weekly returns instead of daily returns for two reasons. First, some international securities are traded infrequently, which introduces measurement problems for daily returns. Second, the use of high-frequency daily returns can result in misleading residual-return distributions. For example, extreme negative returns on a particular day can reverse in the next few days, thereby introducing noise in measuring real crash events. In addition, Equation (1) includes lead and lag terms for the local and U.S. market index returns to allow for nonsynchronous trading, and includes a U.S. stock return index to proxy for global market returns because most international economies are exposed to foreign capital.

We then calculate firm-specific weekly return for firm i in week t, $W_{i,t}$, as the natural logarithm of 1 plus the residual return from Equation (1). We use residual returns because we are interested in firm-level crash risk caused by idiosyncratic factors. Finally, we compute our crash risk measure for each firm i in year t, $NCSKEW_{i,t}$, by taking the negative of the third moment of firm-specific weekly returns, $W_{i,t}$, for each sample year and dividing it by the standard deviation of firm-specific weekly returns raised to the third power:

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

Other Variables

Following prior studies such as Chen et al. (2001), Hutton et al. (2009), and Kim et al. (2011a, 2011b), we control for: (1) the change in average monthly stock turnover from year $t-1$

to year t, $DTURN_{t-1}$, to proxy for the change in differences of opinion among investors; (2) the lagged negative skewness of firm-specific weekly returns, $NCSKEW_{t-1}$, because firms with a high return skewness are likely to have a high crash risk in the following year; (3) the standard deviation of firm-specific weekly returns over the last year, $SIGMA_{t-1}$, because more volatile stocks are more likely to experience stock price crashes; (4) the average of firm-specific weekly returns over the last year, $\%RET_{t-1}$, as stocks with high past returns are more likely to crash; (5) the natural logarithm of the market value of equity in year $t-1$, $SIZE_{t-1}$, because prior studies find that firm size is positively associated with crash risk; (6) the market-to-book ratio in year $t-1$, MB_{t-1}, because growth stocks are more likely to experience future price crashes; (7) long-term debt divided by total assets in year $t-1$, LEV_{t-1}, as prior studies show that financial leverage is negatively related to crash risk;[10] (8) income before extraordinary items divided by lagged total assets, ROA_{t-1}, because operating performance is shown to be negatively related to crash risk; (9) the absolute value of discretionary accruals in year $t-1$, $ABACC_{t-1}$, as firms with more earnings management are more prone to stock price crashes;[11] (10) country and year indicators to control for country and year fixed effects; and (11) for analysis based on nonfinancial firms, industry indicators to control for industry fixed effects. To mitigate the influence of outliers, we winsorize all continuous variables in our analysis at the top and bottom 1 percent of their distributions.

Descriptive Statistics

Table 2, Panels A and B present descriptive statistics of the treatment and benchmark samples for nonfinancial and financial firms, respectively. Table 2, Panel A indicates that for nonfinancial firms, the average crash risk (*NCSKEW*) is −0.285 for mandatory adopters, −0.206 for non-IFRS adopters, −0.242 for PSM non-IFRS adopters, and −0.286 for voluntary adopters. The difference in mean *NCSKEW* is insignificant between mandatory adopters and voluntary adopters, likely because both groups come from countries that mandated IFRS adoption. The panel also indicates that mandatory adopters have a significantly lower *NCSKEW* than non-IFRS adopters and PSM non-IFRS adopters, likely because mandatory adopters are mostly in developed economies.

Table 2, Panel B for financial firms shows that the treatment sample of mandatory IFRS adopters has an average *NCSKEW* of −0.277, while the three benchmark groups have an average *NCSKEW* of −0.177 (non-IFRS adopters), −0.178 (PSM non-IFRS adopters), and −0.326 (voluntary adopters). Panel B also indicates that the average *NCSKEW* for mandatory adopters is similar to the average *NCSKEW* for voluntary adopters and is lower than the average *NCSKEW* for non-IFRS adopters and PSM non-IFRS adopters.

Table 2, Panel C presents Pearson correlation coefficients across the test variables for nonfinancial and financial firms. For nonfinancial firms, crash risk ($NCSKEW_t$) is positively correlated with the change in monthly share turnover from the previous year ($DTURN_{t-1}$), lagged negative return skewness ($NCSKEW_{t-1}$), average weekly return over last year ($\%RET_{t-1}$), lagged firm size ($SIZE_{t-1}$), lagged market-to-book ratio (MB_{t-1}), lagged leverage ratio (LEV_{t-1}), lagged return on assets (ROA_{t-1}), and negatively correlated with standard deviation of weekly returns over the previous year ($SIGMA_{t-1}$) and lagged absolute value of abnormal accruals ($ABACC_{t-1}$). For financial firms, the pattern is similar.

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[10] While the negative association between leverage and crash risk may seem counter-intuitive, one explanation is that more stable, less crash-prone firms are likely to have a greater ability to borrow (Hutton et al. 2009).

[11] Following the earnings management literature, we compute abnormal accruals for nonfinancial firms only.

TABLE 2

Descriptive Statistics

Panel A: Descriptive Statistics for Nonfinancial Firms

	$NCSKEW_t$	$DTURN_{t-1}$	$NCSKEW_{t-1}$	$SIGMA_{t-1}$	$\%RET_{t-1}$	$SIZE_{t-1}$	MB_{t-1}	LEV_{t-1}	ROA_{t-1}	$ABACC_{t-1}$
Treatment Group: Mandatory IFRS Adopters (n = 8,472)										
Mean	−0.285	0.003	−0.228	0.053	−0.214	4.836	2.438	0.128	−0.019	0.118
Median	−0.260	0.001	−0.221	0.041	−0.082	4.755	1.696	0.084	0.035	0.034
Std. Dev.	0.712	0.070	0.728	0.042	0.592	2.192	3.170	0.140	0.215	0.426
Benchmark Group #1: Non-IFRS Adopters (n = 26,228)										
Mean	−0.206	0.000	−0.205	0.052	−0.183	5.359	2.279	0.128	0.010	0.329
Median	−0.201	0.002	−0.194	0.043	−0.092	5.252	1.416	0.078	0.029	0.052
Std. Dev.	0.752	0.131	0.741	0.032	0.383	1.977	3.488	0.147	0.148	1.220
t-test of diff. in mean	***	**	**	***	***	***	***		***	***
Benchmark Group #2: PSM Non-IFRS Adopters (n = 5,977)										
Mean	−0.242	0.002	−0.220	0.050	−0.185	5.016	2.252	0.132	0.000	0.124
Median	−0.221	0.001	−0.199	0.040	−0.077	4.899	1.305	0.080	0.025	0.041
Std. Dev.	0.741	0.124	0.717	0.036	0.488	1.930	3.814	0.151	0.156	0.405
t-test of diff. in mean	***			***	***	***	***		***	
Benchmark Group #3: Voluntary IFRS Adopters (n = 1,400)										
Mean	−0.286	0.003	−0.236	0.049	−0.179	5.434	1.850	0.132	0.007	0.061
Median	−0.258	0.001	−0.223	0.038	−0.070	5.373	1.341	0.109	0.033	0.027
Std. Dev.	0.671	0.054	0.714	0.034	0.347	2.387	2.312	0.126	0.158	0.108
t-test of diff. in mean				***	**	***	***		***	***

Panel B: Descriptive Statistics for Financial Firms

	$NCSKEW_t$	$DTURN_{t-1}$	$NCSKEW_{t-1}$	$SIGMA_{t-1}$	$\%RET_{t-1}$	$SIZE_{t-1}$	MB_{t-1}	LEV_{t-1}	ROA_{t-1}
Treatment Group: Mandatory IFRS Adopters (n = 1,748)									
Mean	−0.277	0.002	−0.234	0.036	−0.118	6.135	1.499	0.180	0.033
Median	−0.224	0.001	−0.177	0.027	−0.035	6.145	1.195	0.128	0.017
Std. Dev.	0.664	0.054	0.709	0.039	0.529	2.215	1.174	0.184	0.068

(continued on next page)

TABLE 2 (continued)

	$NCSKEW_t$	$DTURN_{t-1}$	$NCSKEW_{t-1}$	$SIGMA_{t-1}$	$\%RET_{t-1}$	$SIZE_{t-1}$	MB_{t-1}	LEV_{t-1}	ROA_{t-1}
Benchmark Group #1: Non-IFRS Adopters (n = 4,152)									
Mean	−0.177	−0.003	−0.174	0.035	−0.090	6.056	1.757	0.148	0.021
Median	−0.154	0.001	−0.146	0.029	−0.041	5.964	1.531	0.079	0.013
Std. Dev.	0.615	0.099	0.641	0.024	0.217	1.928	1.221	0.176	0.047
t-test of diff. in mean	***	*	***		***		***	***	***
Benchmark Group #2: PSM Non-IFRS Adopters (n = 1,128)									
Mean	−0.178	0.002	−0.184	0.035	−0.090	6.257	1.590	0.165	0.026
Median	−0.159	0.001	−0.155	0.027	−0.036	6.264	1.391	0.091	0.014
Std. Dev.	0.617	0.091	0.637	0.025	0.191	2.019	1.152	0.185	0.049
t-test of diff. in mean	***		*		*		**	**	***
Benchmark Group #3: Voluntary IFRS Adopters (n = 196)									
Mean	−0.326	−0.004	−0.243	0.030	−0.062	6.670	1.579	0.152	0.027
Median	−0.270	0.000	−0.190	0.025	−0.030	6.786	1.202	0.073	0.016
Std. Dev.	0.614	0.055	0.626	0.019	0.088	2.188	1.191	0.185	0.096
t-test of diff. in mean		*		**		***		**	

Panel C: Pearson Correlations for the Full Sample (Mandatory Adopters, Non-Adopters, and Voluntary Adopters) with Nonfinancial Firms, in Bold, in the Lower Diagonal (n = 34,700) and Financial Firms in the Upper Diagonal (n = 5,900)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1. $NCSKEW_t$	1	0.012	0.085***	−0.102***	0.082***	0.178***	0.075***	0.028*	0.035**
2. $DTURN_{t-1}$	**0.013***	1	−0.027*	0.061***	−0.078***	0.051***	0.020	0.006	−0.023
3. $NCSKEW_{t-1}$	**0.075***	**−0.040***	1	−0.098***	0.081***	0.154***	0.024	0.036**	−0.040**
4. $SIGMA_{t-1}$	**−0.080***	**0.139***	**−0.090***	1	−0.942***	−0.434***	−0.044***	0.062***	−0.208***
5. $\%RET_{t-1}$	**0.078***	**−0.142***	**0.098***	**−0.955***	1	0.372***	0.055***	−0.086***	0.223***
6. $SIZE_{t-1}$	**0.167***	**0.037***	**0.105***	**−0.475***	**0.413***	1	0.279***	−0.012	0.121***
7. MB_{t-1}	**0.047***	**0.038***	**−0.01**	**0.076***	**−0.077***	**0.272***	1	0.043**	0.151***
8. LEV_{t-1}	**0.022***	**0.022***	**0.037***	**−0.049***	**0.035***	**0.200***	**0.021***	1	−0.007
9. ROA_{t-1}	**0.067***	**−0.012***	**0.011***	**−0.458***	**0.446***	**0.322***	**−0.070***	**0.004**	1
10. $ABACC_{t-1}$	**−0.018***	**−0.001**	**−0.011***	**0.135***	**−0.123***	**−0.076***	**0.014**	**0.021***	**−0.071***

(continued on next page)

TABLE 2 (continued)

*, **, *** Indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.
Table 2 presents descriptive statistics for stock price crash risk measure and control variables. Our sample includes firm-year observations of both treatment group and three benchmark groups two years before and after the IFRS mandate in 2005. Panels A and B present descriptive statistics for nonfinancial firms and financial firms, respectively and the t-test of differences in means between the treatment group and the three benchmark groups. Panel C reports Pearson correlations for the full sample.
See Appendix A for variable definitions.

Research Design

To test our hypotheses concerning the average effect of IFRS adoption on firm-specific crash risk, we regress our firm-specific crash risk measure on an indicator variable that captures the interaction between mandatory adoption (*Mandatory Adopters*) and the post-adoption period (*POST*), along with a set of control variables as listed in Appendix A:

$$Crash\ risk = \beta_0 + \beta_1(Mandatory\ Adopters \times POST) + \beta_j(Controls_j) + \varepsilon. \quad (3)$$

Our variable of interest is the coefficient on the interaction term, β_1, which captures the incremental change in crash risk for mandatory adopters after 2005 relative to the change for the benchmark group. A negative (positive) coefficient on β_1 is consistent with a decrease (increase) in crash risk. We suppress the coefficients on the variable indicating mandatory adopters and the variable indicating the post-adoption period because these variables are a linear combination of the country fixed effects and year fixed effects in our models. In this and all subsequent regression analyses, we adjust the standard errors by country clusters as mandatory IFRS adoption is a decision made at the country level. We present one-tailed p-values where we have directional predictions, and two-tailed otherwise.

IV. EMPIRICIAL ANALYSIS

Average Effect: Tests of H1 and H2

Table 3, Panels A and B report the regression results of the average effect of IFRS adoption on crash risk for nonfinancial and financial firms, respectively. For brevity, we suppress the reporting of the coefficients on country, industry, and year indicator variables.

In both panels, columns (1)–(3), (4)–(6), and (7)–(9) report the results where the benchmark groups are non-IFRS adopters, PSM non-IFRS adopters, and voluntary IFRS adopters, respectively. Columns (1), (4), and (7) include only the firm-level control variables, columns (2), (5), and (8) include variables of interest and country, industry, and year fixed effects, and columns (3), (6), and (9) report full regression models.

Table 3, Panel A reports that, for nonfinancial firms, the coefficient β_1 on the interaction term, *Mandatory Adopters* × *POST*, is negative and significant at $p \leq 0.10$ or better for all three benchmark groups. These results are consistent with H1 that nonfinancial firms experience a decrease in crash risk following mandatory IFRS adoption.

Table 3, Panel A also shows that the explanatory power of our full model ranges from 3 to 5 percent, which is comparable with prior studies such as Kim et al. (2011b). In addition, the firm-level control variables contribute 60 to 75 percent of the explanatory power. The decrease in crash risk associated with mandatory IFRS adoption among nonfinancial firms, depending on the benchmark group, is economically significant, which ranges from 20 to 44 percent.[12] These results are consistent with prior IFRS studies that suggest IFRS adoption improves a firm's information environment as reflected in increased analyst forecast accuracy and decreased forecast dispersion (Byard et al. 2011; Tan et al. 2011).

In contrast, the results in Table 3, Panel B find no significant change in crash risk among financial firms after IFRS adoption, based on the insignificant coefficient on *Mandatory Adopters* × *POST* in our full regression models (i.e., columns (3), (6), and (9). As for the

[12] 20 percent = −0.058/−0.285, where −0.058 is β_1 in columns (8) and (9) in Table 3, Panel A, and −0.285 is the mean crash risk for mandatory adopters reported in Table 2, Panel A. 44 percent = −0.126/−0.285, where −0.126 is β_1 in column (6) in Table 3, Panel A.

TABLE 3

The Average Effect of Mandatory IFRS Adoption on Firm-level Crash Risk

Panel A: Nonfinancial Firms

		Dep. Var. = *NCSKEW*								
Benchmark	**Pred. Sign**	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
		Non-IFRS Adopters			**PSM Non-IFRS Adopters**			**Voluntary Adopters**		
Mandatory Adopters β_1	−		−0.101***	−0.110***		−0.121***	−0.126***		−0.058*	−0.058*
× *POST*			(0.005)	(0.003)		(0.001)	(0.001)		(0.081)	(0.079)
$DTURN_{t-1}$	+	0.055		0.054*	−0.014		−0.023	−0.123		−0.140
		(0.127)		(0.069)	(0.589)		(0.647)	(0.714)		(0.759)
$NCSKEW_{t-1}$	+	0.058***		0.039***	0.064***		0.049***	0.051***		0.041***
		(0.000)		(0.000)	(0.000)		(0.000)	(0.000)		(0.000)
$SIGMA_{t-1}$	+	2.235		1.404	0.858		0.253	2.215**		1.700*
		(0.151)		(0.205)	(0.244)		(0.408)	(0.025)		(0.073)
$\%RET_{t-1}$	+	0.257		0.213	0.076		0.047	0.174*		0.152
		(0.143)		(0.133)	(0.285)		(0.341)	(0.075)		(0.108)
$SIZE_{t-1}$	+	0.060***		0.051***	0.051***		0.053***	0.057***		0.064***
		(0.000)		(0.000)	(0.000)		(0.000)	(0.000)		(0.000)
MB_{t-1}	+	−0.000		0.001	0.000		0.000	0.004*		0.002
		(0.505)		(0.353)	(0.389)		(0.388)	(0.059)		(0.287)
LEV_{t-1}	−	−0.058		−0.012	−0.037		0.019	−0.103**		−0.063*
		(0.151)		(0.280)	(0.174)		(0.768)	(0.016)		(0.051)
ROA_{t-1}	−	0.075		0.150	0.067		0.090	0.046		0.041
		(0.947)		(0.998)	(0.943)		(0.972)	(0.982)		(0.800)
$ABACC_{t-1}$	+	−0.003		0.006*	0.023*		0.005	0.092*		0.042
		(0.628)		(0.072)	(0.066)		(0.342)	(0.078)		(0.196)
Country fixed effects		No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Industry fixed effects		No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year fixed effects		No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations		34,700	34,700	34,700	14,429	14,429	14,429	9,872	9,872	9,872
Adj. R^2		0.032	0.032	0.054	0.025	0.018	0.041	0.024	0.009	0.033

(continued on next page)

TABLE 3 (continued)

Panel B: Financial Firms

Benchmark	Pred. sign	Dep. Var. = *NCSKEW* (1) Non-IFRS Adopters	(2) Non-IFRS Adopters	(3) Non-IFRS Adopters	(4) PSM Non-IFRS Adopters	(5) PSM Non-IFRS Adopters	(6) PSM Non-IFRS Adopters	(7) Voluntary Adopters	(8) Voluntary Adopters	(9) Voluntary Adopters
Mandatory Adopters × *POST* β_1	?		−0.009	−0.018		0.070*	0.057		0.042	0.041
			(0.817)	(0.644)		(0.077)	(0.151)		(0.552)	(0.565)
$DTURN_{t-1}$	+	0.042		−0.039	−0.067		−0.116	0.209		0.239
		(0.691)		(0.670)	(0.660)		(0.740)	(0.241)		(0.212)
$NCSKEW_{t-1}$	+	0.057***		0.022*	0.083***		0.038*	0.081***		0.035
		(0.001)		(0.084)	(0.001)		(0.068)	(0.004)		(0.134)
$SIGMA_{t-1}$	+	−2.319		−0.706	−0.038		0.022	1.295		1.813
		(0.911)		(0.687)	(0.506)		(0.497)	(0.332)		(0.252)
$\%RET_{t-1}$	+	−0.190		−0.088	−0.030		−0.050	0.039		0.126
		(0.765)		(0.668)	(0.540)		(0.568)	(0.459)		(0.365)
$SIZE_{t-1}$	+	0.042***		0.056***	0.056***		0.063***	0.061***		0.074***
		(0.000)		(0.000)	(0.000)		(0.000)	(0.001)		(0.000)
MB_{t-1}	+	0.032***		0.012**	0.003		−0.006	0.017		0.017
		(0.000)		(0.019)	(0.423)		(0.695)	(0.181)		(0.142)
LEV_{t-1}	−	0.098		0.093	0.037		−0.007	0.053		−0.002
		(0.927)		(0.875)	(0.697)		(0.464)	(0.715)		(0.491)
ROA_{t-1}	−	0.004		0.015	0.299		0.192	0.263		0.047
		(0.510)		(0.537)	(0.947)		(0.813)	(0.934)		(0.591)
Country fixed effects		No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Industry fixed effects		No	No	No	No	No	No	No	No	No
Year fixed effects		No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations		5,900	5,900	5,900	2,870	2,870	2,870	1,944	1,944	1,944
Adj. R^2		0.041	0.039	0.071	0.045	0.047	0.080	0.048	0.033	0.081

*, **, *** Indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

p-values in parentheses are based on standard errors clustered by country (one-tailed for coefficients with predicted signs and two-tailed otherwise).

Table 3 presents the regression results of the impact of mandatory IFRS adoption on firm-level stock price crash risk. Panel A reports the results for nonfinancial firms and Panel B reports those for financial firms.

See Appendix A for variable definitions.

control variables, both panels of Table 3 report that, except for column (9) in Panel B, lagged negative return skewness ($NCSKEW_{t-1}$) and firm size ($SIZE_{t-1}$) are positively related to crash risk across all columns for nonfinancial firms and for financial firms.[13] Although it is difficult to make direct comparisons with prior work due to differences in the sample and time period, these findings are generally consistent with Chen et al. (2001), Hutton et al. (2009), and Kim et al. (2011a, 2011b).

In summary, the results in Table 3 support our hypothesis that increased transparency under IFRS decreases crash risk among *nonfinancial* firms, but mandatory IFRS adoption has little overall impact on crash risk among financial firms.

Tests of Additional Hypotheses for Nonfinancial Firms

Test of H1A: Analysis Conditional on Firm-Level Information Environments

H1A predicts that if the decrease in crash risk for nonfinancial firms is due to improved financial reporting transparency, then this effect is likely to be stronger when the firm has a poor information environment. We test this hypothesis by partitioning our sample based on the quality of a firm's information environment prior to the adoption, and estimating Equation (3) for each of the partitions. We then compare the coefficient on *Mandatory Adopters* × *POST* across the two partitions to test whether crash risk differs between firms in poor information environments and firms in rich information environments. We compare across the two partitions, rather than using a three-way interaction on the pooled dataset, because this approach allows the coefficients on the control variables to vary across the partitions.

We classify firms into rich or poor information environments following prior studies, such as Armstrong, Barth, Jagolinzer, and Riedl (2010). Specifically, we create a variable labeled "*InfoEnviron,*" which equals the first principal component derived from the following six variables that capture the firm's information environment prior to mandatory IFRS adoption: (1) *ADR,* an indicator variable that equals 1 if a firm cross-lists its shares in the U.S. using American Depository Receipts (ADR) in the year before the adoption, and 0 otherwise; (2) *Index,* an indicator variable that equals 1 if the firm is included in any stock market index in the year before the adoption, and 0 otherwise; (3) *Exchanges,* the number of stock exchanges on which the firm is listed in the year before the adoption; (4) *Foreign sales,* the average foreign sales in the two years before the adoption; (5) *Analyst,* the number of analysts following in the year before the adoption; and (6) *Size*, the average natural logarithm of market value of equity in the two years before the adoption. We expect firms with higher values of each measure to have richer information environments.

We partition nonfinancial firms based on the median value of *InfoEnviron* in the full sample, i.e., treatment and non-IFRS benchmark firms, and label firms with the value of *InfoEnviron* above or equal to the median as those in rich information environments. We then estimate the regression model in Equation (3) for each subsample. Our hypothesis predicts that β_1, the coefficient on *Mandatory Adopters* × *POST,* will be significantly more negative among firms in poor information environments than among firms in rich information environments.

Table 4, Panels A and B report the results using *InfoEnviron* and each of its six components. To conserve space, we do not report the coefficients on the control variables. Panels A and B report that β_1 is significantly negative in each model except in columns (4) and (12), and is significantly more negative in

[13] Although the coefficients on $NCSKEW_{t-1}$ are statistically significant in Table 3, their magnitudes are relatively small, which implies little persistence in negative skewness. The low serial correlation in negative return skewness is consistent with prior research and our correlation analysis. Specifically, Table 2, Panel C finds that the Pearson correlation between $NCSKEW_t$ and $NCSKEW_{t-1}$ is 0.075 (0.085) for nonfinancial firms (financial firms), which are comparable to the autocorrelation of 0.05 documented in Chen et al. (2001).

TABLE 4

Subsample Analysis of The Effect of Mandatory IFRS Adoption on Crash Risk for Nonfinancial Firms

Panel A: Analysis Conditional on Firm-Level Information Environment, Using Non-IFRS Adopters as the Benchmark

			Dep. Var. = *NCSKEW*					
			(1)	(2)	(3)	(4)	(5)	(6)
			Aggregate Measure		*ADR*		*Index*	
Partition =		**Pred. Sign**	**Poor *InfoEnviron***	**Rich *InfoEnviron***	**No**	**Yes**	**= 0**	**≥ 1**
Mandatory Adopters × *POST*	β_1	−	−0.149***	−0.071*	−0.114***	0.174	−0.120***	−0.103**
			(0.001)	(0.060)	(0.003)	(0.807)	(0.007)	(0.017)
Prediction of difference in β_1			—		—		—	
Test of difference in β_1,			−0.078**		−0.288*		−0.017	
Poor-Rich *InfoEnviron*			(0.041)		(0.081)		(0.350)	
Firm-level controls			Yes	Yes	Yes	Yes	Yes	Yes
Country, Ind., Year fixed effects			Yes	Yes	Yes	Yes	Yes	Yes
Observations			17,348	17,352	34,320	380	19,628	15,072
Adj. R^2			0.047	0.052	0.054	0.045	0.053	0.065

Panel B: Continuation from Panel A

			Dep. Var. = *NCSKEW*							
			(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
			Exchanges		*Foreign sales*		*Analyst*		*SIZE*	
Partition =		**Pred. Sign**	**= 1**	**> 1**	**= 0**	**> 0**	**≤ 1**	**> 1**	**Small**	**Large**
Mandatory Adopters × *POST*	β_1	−	−0.115***	−0.104**	−0.145***	−0.065**	−0.140***	−0.056	−0.130***	−0.088**
			(0.006)	(0.049)	(0.001)	(0.048)	(0.001)	(0.105)	(0.003)	(0.045)
Prediction of difference in β_1			−		−		−		−	
Test of difference in β_1,			−0.011		−0.080***		−0.084**		−0.042	
Poor-Rich *InfoEnviron*			(0.437)		(0.007)		(0.031)		(0.224)	

(continued on next page)

TABLE 4 (continued)

		Dep. Var. = *NCSKEW*							
		(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
		Exchanges		*Foreign sales*		*Analyst*		*SIZE*	
Partition =	**Pred. Sign**	**= 1**	**> 1**	**= 0**	**> 0**	**≤ 1**	**> 1**	**Small**	**Large**
Firm-level controls		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country, Ind., Year fixed effects		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations		28,640	6,060	21,060	13,640	22,224	12,476	17,348	17,352
Adj. R^2		0.056	0.054	0.053	0.056	0.043	0.036	0.031	0.051

Panel C: Two-by-Two Table Conditional on Country-Level GAAP Change and Enforcement, Mandatory Adopters Only

	Strong Enforcement (Enforcement ≥ 9.14)	**Weak Enforcement (Enforcement < 9.14)**
Large change in GAAP (Change in GAAP ≥ 12)	(1) Austria, Belgium, Finland, Switzerland (n = 544)	(2) France, Greece, Italy, Portugal, Spain (n = 1,952)
Small change in GAAP (Change in GAAP < 12)	(3) Australia, Denmark, The Netherlands, Norway, Sweden, U.K. (n = 4,720)	(4) Germany, Hong Kong, Ireland, Philippines, South Africa (n = 1,208)

Panel D: Analysis Conditional on Country-Level GAAP Change and Enforcement, Mandatory Adopters Only

			Dep. Var. = *NCSKEW*			
			(1)	(2)	(3)	(4)
			Cell (1)	**Cell (2)**	**Cell (3)**	**Cell (4)**
Partition =		**Pred. Sign**	***Large GAAP change with strong enforcement***	***Large GAAP change with weak enforcement***	***Small GAAP change with strong enforcement***	***Small GAAP change with weak enforcement***
POST	β_1	−	−0.176***	−0.071**	−0.034	−0.016
			(0.008)	(0.017)	(0.251)	(0.401)
Column (1) − other columns				Cell (1) − (2)	Cell (1) − (3)	Cell (1) − (4)

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TABLE 4 (continued)

		Dep. Var. = *NCSKEW*			
		(1)	(2)	(3)	(4)
		Cell (1)	Cell (2)	Cell (3)	Cell (4)
Partition =	**Pred. Sign**	***Large GAAP change with strong enforcement***	***Large GAAP change with weak enforcement***	***Small GAAP change with strong enforcement***	***Small GAAP change with weak enforcement***
Prediction of difference in β_1,			—	—	—
Test of difference in β_1			−0.105***	−0.142***	−0.160***
			(0.003)	(0.005)	(0.007)
Firm-level control		Yes	Yes	Yes	Yes
Country fixed effects		Yes	Yes	Yes	Yes
Industry fixed effects		Yes	Yes	Yes	Yes
Year fixed effects		No	No	No	No
Observations		544	1,952	4,720	1,208
Adj. R^2		0.043	0.041	0.031	0.029

*, **, *** Indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.
p-values in parentheses are based on standard errors clustered by country (one-tailed for coefficients with predicted signs and two-tailed otherwise).
Table 4 presents the regression results of the impact of mandatory IFRS adoption on firm-level stock price crash risk for nonfinancial firms, conditional on firm-level information environment (Panels A and B), and country-level GAAP change and enforcement (Panels C and D). The sample firms are partitioned into subsamples based on the sample median values of the conditional variables.
See Appendix A for variable definitions

the poor information environment partition than in the rich information environment partition when we use the aggregate measure and three of its six components, i.e., ADR, foreign sales, and number of analysts following.[14] Overall, the evidence in Panels A and B are consistent with IFRS adoption widely reducing crash risk for nonfinancial firms, and with this reduction being relatively greater among firms in poor information environments. We also find that the decrease in crash risk among nonfinancial firms in poor information environments is economically significant. Mandatory IFRS adoption in poor information environments is associated with a decrease in crash risk of 52.2 percent when we use the aggregate information environment variable, relative to the overall average crash risk for nonfinancial firms.[15]

Test of H1B: Analysis Conditional on Country-Level GAAP Change and Legal Enforcement

H1B predicts that if IFRS adoption leads to increased disclosure that reduces crash risk, then this effect is likely to be more pronounced in countries with larger changes in accounting standards after IFRS adoption, but only when the standards are credibly implemented. We test this hypothesis by partitioning our sample on whether IFRS adoption results in large changes to local GAAP and whether the adopting country has a strong enforcement regime.

Our proxy for strong enforcement is the mean of the three enforcement measures from La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1998): (1) the efficiency of the judicial system, (2) the rule of law, and (3) corruption. Higher values of this enforcement index indicate relatively stronger legal enforcement.[16] We measure the number of changes in local GAAP after IFRS adoption using the *Gaapdiff1* variable in Bae, Tan, and Welker (2008). The *Gaapdiff1* variable is based on a comparison of 21 key accounting items between local GAAP with IAS. Examples of the differences include the following accounting treatments under local GAAP: (1) no or limited segment reporting, (2) no or limited capitalization of leases, (3) no disclosure of FIFO inventory cost when LIFO is used, (4) no or limited disclosure requirements for related-party transactions, and (5) no disclosure requirement of cash flow statement. Higher values of *Gaapdiff1* indicate larger changes in accounting standards after the IFRS mandate. When both enforcement and *Gaapdiff1* equal or exceed the sample country-level median, we include the firms in the partition (labeled *Large GAAP change with strong enforcement*). Appendix C reports descriptive statistics on the country-level index of GAAP change and enforcement.

We next classify mandatory adopters for nonfinancial firms into four partitions based on local GAAP changes and legal enforcement strength, and estimate the model in Equation (3). We include only mandatory adopters in this analysis because there are no GAAP changes in the non-IFRS adopting countries. We drop year fixed effects and test whether the coefficient on *POST* differs across the partitions. We expect this coefficient to be more negative among the firms in the *Large GAAP change with strong enforcement* partition.

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[14] In untabulated analysis, we find that the coefficients on the control variables exhibit a fair amount of variation across the partitions. For example, the coefficients on $\%RET_{t-1}$ and $SIGMA_{t-1}$ are significant and positive only for the rich information environment subsample, and the coefficient on $DTURN_{t-1}$ is significant and positive only for the poor information environment subsample. The significantly positive coefficients on $\%RET_{t-1}$ and $SIGMA_{t-1}$ for firms in rich information environments are likely due to negative information being incorporated into stock prices more promptly in these environments, which exacerbates the positive effects of past returns and volatility on crash risk. In addition, the significantly negative coefficient on $DTURN_{t-1}$ for firms in poor information environments is likely due to opacity contributing to a greater change in dispersion of investor opinions and, hence, a larger effect on crash risk.

[15] 52.2 percent = −0.149/−0.285, where −0.149 is β_1 in column (1) of Table 4, Panel A, and −0.285 is the mean crash risk in Table 2, Panel A.

[16] We also use alternative measures of enforcement in Kaufmann, Kraay, and Mastuzzi (2007): (1) the rule of law score in 2005, (2) the governance effectiveness score in 2005, or (3) the average of the six governance scores in 2005, and find consistent results as reported in Table 4, Panel C.

Table 4, Panel C reports the frequency of sample countries by changes in GAAP and enforcement. It shows that the largest subsample includes 4,720 mandatory adopters in countries with small changes in GAAP and strong enforcement, while the smallest subsample includes 544 mandatory adopters in countries with large changes in GAAP and strong enforcement.

Table 4, Panel D presents results of comparing companies in the *Large GAAP change with strong enforcement* partition (Cell 1 in Panel C) with those in the other partitions (Cells 2, 3, 4 in Panel C). Panel D reports that the coefficient on *POST* is significantly negative among the nonfinancial firms in the *Large GAAP change with strong enforcement* partition. In addition, consistent with H1B, the coefficient on *POST* is significantly more negative in the *Large GAAP change with strong enforcement* partition than in any other three subsamples. The decrease in crash risk among firms in the *Large GAAP change with strong enforcement* partition is economically significant. Mandatory IFRS adoption in the *Large GAAP change with strong enforcement* partition is associated with a decrease in crash risk of 62 percent, relative to the overall average crash risk for nonfinancial firms.[17] In summary, the findings in Table 4 suggest that the decrease in crash risk is larger among nonfinancial firms in poor information environments, and in countries where IFRS results in larger and more credible changes to local GAAP.

Tests of Additional Hypotheses for Financial Firms

Test of H2A: Analysis of Firms with Large Changes in Fair Value Provisions

The fair value provisions of IFRS are more likely to affect crash risk among financial firms for which IAS 39 has a relatively larger impact. H2A focuses on firms whose local GAAP experiences a larger increase in the number of fair value provisions as a result of IAS 39. We calculate the number of differences using data in Andersen et al. (2001). A higher value indicates a larger number of changes to local GAAP's fair value provisions under IFRS. We classify financial firms as having more changes to fair value provisions if the *change in fair value provisions* is greater than or equal to the treatment sample country-level median. We focus on mandatory adopters in this analysis and drop year fixed effects because none of the non-IFRS adopters experience changes related to IAS 39 around 2005. Appendix C presents descriptive statistics on the country-level index of changes in IAS 39 fair value provisions.

Table 5, Panel A presents the results of testing H2A and finds that the coefficient on *POST* is insignificant among financial firms with large changes in fair value provisions. Thus, we do not find that fair value accounting increases crash risk. To gain insights into the channels through which IFRS affects crash risk for financial firms, we also examine the subsample of firms with small changes in fair value provisions. If IFRS decreases crash risk for financial firms through increased transparency unrelated to IAS 39, then those effects should be strongest among the financial firms least affected by IAS 39. Table 5, Panel A finds that the coefficient on *POST* is significantly negative for the subsample of firms with small changes in fair value provisions. Thus, crash risk declines among the subset of financial firms that are less affected by IAS 39's fair value provisions, consistent with some firms benefiting from transparency effects that are unrelated to IAS 39. We further explore this by partitioning the subsamples in columns (1) and (2) on the firm's information environment, as reported in columns (3)–(6). Consistent with this explanation, the decline in crash risk among financial firms with small changes in fair value provisions is significantly greater among firms in poor information environments.

The results in Table 5, Panel A essentially separate the "fair value" effects from the non-fair-value-related "increased disclosure" effects. When the fair value effects are strong, crash risk is

[17] 62 percent = −0.176/−0.285, where −0.176 is β_1 in column (1) of Table 4, Panel C, and −0.285 is mean crash risk in Table 2, Panel A.

TABLE 5

Subsample Analysis of the Effect of Mandatory IFRS Adoption on Crash Risk for Financial Firms

Panel A: Analysis Conditional on Country-Level Change in IAS 39 Fair Value Provisions, Mandatory Adopters Only

			Dep. Var. = *NCSKEW*					
			(1)	(2)	(3)	(4)	(5)	(6)
			Change in fair value provisions		Large *Change in fair value provisions*		Small *Change in fair value provisions*	
Partition =		**Pred. Sign**	**Large Change**	**Small Change**	**Poor *InfoEnviron***	**Rich *InfoEnviron***	**Poor *InfoEnviron***	**Rich *InfoEnviron***
POST	β_1	?	0.017	−0.116*	0.030	−0.001	−0.144*	−0.005
			(0.749)	(0.051)	(0.714)	(0.990)	(0.062)	(0.934)
Prediction of difference in β_1,			?		?		?	
Test of difference in β_1			0.133*		0.031		−0.139*	
			(0.060)		(0.690)		(0.085)	
Firm-level controls			Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects			Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects			No	No	No	No	No	No
Year fixed effects			No	No	No	No	No	No
Observations			976	772	440	536	432	340
Adj. R^2			0.092	0.072	0.105	0.051	0.046	0.017

(continued on next page)

TABLE 5 (continued)

Panel B: Analysis Conditional on Firm-Level Fair Value Exposure, Using Non-IFRS Adopters as Benchmark

			Dep. Var. = *NCSKEW*		
Partition =		**Pred. Sign**	**(1) More Exposure to Fair Value**		**(2) Less Exposure to Fair Value**
Mandatory Adopters × *POST*	β_1	?	0.035		−0.090
			(0.663)		(0.111)
Prediction of difference in β_1				?	
Test of difference in β_1,				0.125	
More-less fair value exposure				(0.190)	
Firm-level controls			Yes		Yes
Country fixed effects			Yes		Yes
Industry fixed effects			No		No
Year fixed effects			Yes		Yes
Observations			1,684		1,684
Adj. R^2			0.080		0.044

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TABLE 5 (continued)

Panel C: Analysis Conditional on Country-Level Restrictiveness of Banking Regulations, Using Non-IFRS Adopters as Benchmark

			Dep. Var. = *NCSKEW*		
Partition =		**Pred. Sign**	**(1)** ***Less restrictive banking regulations***		**(2)** ***More restrictive banking regulations***
Mandatory Adopters × *POST*	β_1	?	0.375***		−0.060
			(0.003)		(0.328)
Prediction of difference in β_1				?	
Test of difference in β_1,				0.435***	
Less-more restrictive regulations				(0.000)	
Firm-level controls			Yes		Yes
Country fixed effects			Yes		Yes
Industry fixed effects			No		No
Year fixed effects			Yes		Yes
Observations			364		2,488
Adj. R^2			0.150		0.045

*, **, *** Indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.
p-values in parentheses are based on standard errors clustered by country (one-tailed for coefficients with predicted signs and two-tailed otherwise).
Table 5 presents the analysis on the impact of mandatory IFRS adoption on firm-level stock price crash risk for financial firms, conditional on the country-level change in fair value provisions (Panel A), the firm-level exposure to fair value (Panel B), and the country-level restrictiveness of banking regulations (Panel C).
See Appendix A for variable definitions.

little affected, suggesting that the increased volatility from IFRS's fair value provisions do not affect crash risk, on average. In contrast, when the fair value effects are weak, crash risk declines for companies in poor information environments, suggesting that increased transparency unrelated to IAS 39 decreases crash risk. Taken together, our results suggest that the fair value effects alone do not significantly affect crash risk for financial firms, but that the transparency effects alone decrease crash risk. Since the fair value effects of IFRS are expected to dominate the non-fair value related disclosure effects, the net effect of IFRS adoption on financial firms is statistically insignificant, as reported in our primary analysis in Table 3, Panel B. In other words, the "no effect on crash risk" due to the fair value provisions appears to swamp the "decrease in crash risk" due to increased disclosure, and the net effect is that IFRS does not significantly change crash risk for financial firms.

Test of H2B: Analysis of Firms with More Exposure to Fair Value Accounting

H2B predicts that the fair value effects of IFRS are more likely to affect crash risk among financial firms with assets that have more exposure to fair value accounting, although the direction of the change is difficult to predict. We test this hypothesis by examining the 175 commercial and investment banks in our treatment sample with two-digit SIC codes of 60–62. We restrict this analysis to banks because fair value accounting is particularly important in this industry, and measures of banks' exposure to fair value accounting are well defined in the literature.[18] We measure the extent of banks' fair value accounting exposure by the total of trading/dealing account securities and investment securities in the year before the adoption, scaled by total assets.[19] We classify banks as having more fair value exposure if their value of *fair value exposure* is greater than or equal to the sample firm-level median value of this variable.

Table 5, Panel B presents the results of testing H2B and indicates that β_1 is insignificant among banks with more fair value exposure. Thus, consistent with the results of testing H2A, we continue to find no evidence that the fair value effects of IFRS affect crash risk. For completeness, we also examine β_1 among banks with less fair value exposure and find it to be insignificant. Thus, unlike the results in Table 5, Panel A, the tests in Panel B find no evidence that crash risk decreases among banks that have relatively less exposure to fair value accounting. This difference may exist because banks, relative to the other financial institutions in our sample, tend to have a larger exposure to fair value, on average. Thus, even banks with low fair value exposure relative to other banks still have relatively high fair value exposure when compared to other financial institutions. We also note that the sample sizes of our partitions are relatively small, which may reduce the power of these tests.

Test of H2C: Analysis of Banks with Less Restrictive Regulations

H2C predicts IFRS may affect crash risk among financial firms in countries with relatively weaker banking regulation, although the direction is difficult to predict. We test this hypothesis on a treatment sample of 136 commercial banks in our treatment sample with two-digit SIC codes of 60 and 61.[20] To capture the extent of the country-level banking regulations, we use *Restrict,* an index of regulatory restrictions on the activities of banks from Barth, Caprio, and Levine (2006). *Restrict* measures regulatory impediments to banks engaging in securities market activities, insurance

[18] Banks are also relatively homogeneous in the composition of their assets subject to fair value accounting under IAS 39, compared to other financial institutions such as insurance companies and Real Estate Investment Trusts (REITs).

[19] We also perform a sensitivity test after excluding trading securities in our measure of fair value exposure, and our results remain qualitatively the same.

[20] We include only commercial banks in this analysis, rather than commercial and investment banks used in the analysis of fair value exposure, because the banking regulation survey in Barth et al. (2006) focuses on commercial banks.

activities, real estate activities, and the ownership of nonfinancial firms. We classify countries with less restrictive banking regulations as those with values of *Restrict* less than the sample median. If the emphasis on fair value accounting under IFRS increases (decreases) crash risk for financial firms with less restrictive banking regulations, then we expect β_1 to be significantly positive (negative) among the subsample of banks with less restrictive regulations. Appendix C presents descriptive statistics on the country-level index of regulatory restrictions.[21]

Table 5, Panel C reports the results of our regression analysis. We find that β_1 is significantly positive, consistent with IFRS adoption increasing crash risk among banks with less restrictive banking regulations. For completeness, we also examine banks in countries with relatively more restrictive banking regulations and find no significant change in crash risk. In addition, we find that β_1 is significantly larger in countries with less restrictive banking regulations than in countries with more restrictive regulations.[22] Thus, the results in Panel C are consistent with IFRS increasing crash risk in countries with weak banking regulations by encouraging greater investment in risky assets.

V. SENSITIVITY TESTS

Alternative Sample Countries

We repeat our analysis in Table 3 after (1) excluding the five EU countries with concurrent enforcement changes as identified in Christensen, Hail, and Leuz (2013): Finland, Germany, The Netherlands, Norway, and the U.K., (2) restricting the sample to EU countries, and (3) restricting the sample to non-EU countries. The results for nonfinancial firms are qualitatively identical to those reported in Table 3, Panel A.[23] For financial firms, however, crash risk increases significantly after restricting the treatment sample to the EU countries and decreases significantly after restricting the treatment sample to the non-EU countries.[24] We conjecture that the decreased crash risk among non-EU countries is because five of the six countries in this subsample experience small changes in IAS 39 provisions. As a result, the effect of increased transparency unrelated to IAS 39 is likely to dominate the fair value effects of IAS 39 among non-EU countries. While the increased crash risk among EU financial firms may be due to many of the EU countries having relatively weak banking regulations, we also find that this result is sensitive to the countries included in the analysis. Dropping one EU country at a time, we find no change in crash risk for eight of the 15 tests, and an increase in crash risk for seven. Thus, there is only weak evidence that IFRS increases crash risk among EU financial firms.

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[21] While not reported in Appendix C, the values of the *Restrict* index for our non-IFRS benchmark countries are: Argentina (7), Brazil (10), Canada (7), Chile (11), Columbia (missing), India (10), Indonesia (14), Israel (13), Japan (13), Korea (9), Malaysia (10), Mexico (12), Morocco (13), Pakistan (missing), Sri Lanka (7), Taiwan (12), Thailand (9), and the U.S. (12).

[22] We find our results (untabulated) are qualitatively the same using PSM non-IFRS adopters or voluntary adopters as the alternative benchmark in the partitioning analysis in which we include benchmark groups, i.e., Table 4, Panels A and B, and Table 5, Panels B and C, with three exceptions: (1) the difference in the coefficient β_1 across information environment partitions in Table 4, Panels A and B become insignificant when using PSM non-IFRS adopters as the benchmark, and (2) the coefficient β_1 in the subsample of banks with more fair value exposure partition in Table 5, Panel B becomes significantly positive and the difference in the coefficient β_1 across the more or less fair exposure partitions becomes significant when using voluntary adopters as the benchmark, and (3) the difference in the coefficient β_1 across the more or less restrictive banking regulation partitions in Table 5, Panel C becomes insignificant when using voluntary adopters as the benchmark.

[23] By "qualitatively identical results," we mean that the coefficient β_1 is negative and significant at $p \leq 0.10$ or better in column (3) of Table 3, Panel A and is insignificant at conventional level in column (3) of Table 3, Panel B.

[24] In untabulated analysis, we drop treatment countries one at a time and continue to find results consistent with those in Table 3, Panels A and B.

We also expand our sample period to 2001–2011 and our sample countries to post-2005 IFRS adopters. For nonfinancial firms, the post-2005 IFRS adopters and their IFRS adoption years are: Brazil (2010), Canada (2011), Israel (2008), New Zealand (2007), Peru (2011), and South Korea (2011). For financial firms, Israel is dropped because its IFRS mandate is not applicable to banks, but Morocco is added because it requires banks and financial institutions to adopt IFRS in 2008. The results are qualitatively identical to those reported in Table 3, Panel A. For the results in Table 3, Panel B, however, the coefficient β_1 becomes significantly negative, suggesting that the increased disclosure effect dominates the fair value effects, probably because the IAS 39 amendment in 2008 reduces the effect of fair value provisions.

Alternative Sample Periods

We repeat our analysis in Table 3 after (1) using the period 2001–2011 and (2) dropping the transition period 2005, and find qualitatively identical results.[25]

Alternative Measures of Crash Risk

We repeat our analysis in Table 3 using the down-to-up volatility (*DUVOL*) measure of crash likelihood (Chen et al. 2001; Kim et al. 2011b) and find qualitatively identical results. Following Hutton et al. (2009) and Kim et al. (2011 a, 2011b), we also measure crash risk using an indicator that equals 1 for a firm-year that experiences one or more firm-specific weekly returns falling 3.2 standard deviations below the yearly mean and find qualitatively identical results.[26] We also use an alternative *NESKEW* measure anchored on three months following fiscal year-end to ensure the market's incorporation of relevant financial disclosures, and find qualitatively identical results. Finally, we use alternative measures of *NCSKEW* by changing the following specifications of the return models: using the world market index return instead of U.S. market index return, using one lead and lag terms instead of two lead and lag terms, using no lead and lag terms, and including industry returns. We find qualitatively identical results to those reported in Table 3, Panel A. For the results reported in Table 3, Panel B, however, we find that the coefficient β_1 becomes significantly negative in three of the fours tests.[27] Thus, while the result for nonfinancial firms is robust to alternative specifications of the returns model used to calculate *NCSKEW*, the result for financial firms is sensitive to the model specifications.

Alternative Control Variables

Since Hung and Subramanyam (2007) find that IFRS affects the variability of accounting numbers, we remove the control variables based on accounting measures, i.e., LEV_{t-1}, ROA_{t-1}, and $ABACC_{t-1}$, and find qualitatively identical results. We also include the contemporaneous values of these accounting variables as additional controls and our conclusions are unaltered.

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[25] By "qualitatively identical results," we mean that the coefficient β_1 is negative and significant at $p \leq 0.10$ in column (3) of Table 3, Panel A and is insignificant at conventional level in column (3) of Table 3, Panel B.

[26] In untabulated analysis we examine stock price jumps, measured by an indicator variable that equals 1 for firm-specific weekly returns 3.2 standard deviations above the yearly mean, and find an insignificant change in jumps for both financial and nonfinancial firms ($p = 0.151$ for nonfinancial firms; $p = 0.538$ for financial firms), suggesting that IFRS reduces stock price crashes and does not simply reduce price variance.

[27] Some of these alternative return specifications, such as including industry returns, may be less suitable for capturing changes in crash risk for financial firms. Since these tests find that crash risk decreases for financial firms, on average, subsequent to mandatory IFRS adoption, we repeat our analysis in Table 5, Panel C after making the same changes of the return model specifications. We continue to find that, for all these alternative return specifications except the one including industry returns, the coefficient β_1 remains significantly positive for banks in countries with less restrictive banking regulations.

VI. CONCLUSION

We find a decrease in crash risk among nonfinancial firms after IFRS adoption, and that the effect is more pronounced among firms in poor information environments and in countries with large and credible changes to local GAAP. In contrast, for financial firms, we find no change in crash risk after IFRS adoption, on average. However, financial firms less affected by IFRS's fair value provisions experience a decrease in crash risk, and banks with less restrictive banking regulations experience an increase in crash risk.

We contribute to the literature by examining the effect of IFRS adoption on crash risk, a previously unexplored implication of IFRS adoption. Our findings complement prior studies that examine the consequences of financial reporting regulation and add to the debate on fair value accounting by examining how a shift from historical-based to fair-value-oriented standards affects crash risk. Our results suggest that IFRS increases transparency, thereby broadly reducing crash risk among nonfinancial firms. We find little evidence that fair value accounting from IFRS increases crash risk for financial firms, contrary to concerns expressed by regulators. We caution, however, that our analysis of financial firms is preliminary and exploratory in nature.

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APPENDIX A
Variable Definitions

Crash Risk Variable

NCSKEW = negative skewness of firm-specific weekly returns over the fiscal-year period. The firm-specific weekly return (W) is equal to ln(1 + residual), where the residual is from the following expanded market model regression based on Jin and Myers (2006):

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

where $r_{i,t}$ is the return on stock i in week t in country j; $r_{m,j,t}$ is the return on the MSCI country-specific market index or the country index compiled by Datastream in week t; $r_{US,t}$ is the U.S. market index return (a proxy for the global market); and $EX_{j,t}$ is the change in country j's exchange rate versus the U.S. dollar.

Variables of Interest

Mandatory Adopters = indicator variable equal to 1 if companies prepared their financial statements based on local accounting standards before 2005, and switched to IFRS in 2005, and 0 otherwise; and

POST = indicator variable equal to 1 if a firm-year falls in or after 2005, and 0 otherwise.

Firm-Level Controls

DTURN = average monthly share turnover over the current fiscal-year period minus the average monthly share turnover over the previous fiscal-year period, where monthly share turnover is calculated as the monthly trading volume divided by the total number of shares outstanding during the month;

SIGMA = standard deviation of firm-specific weekly returns over the fiscal-year period;

%RET = mean of firm-specific weekly returns over the fiscal-year period × 100;

SIZE = log of the market value of equity;

MB = market value of equity divided by the book value of equity;

LEV = total long-term debts divided by total assets;

ROA = income before extraordinary items divided by lagged total assets; and

ABACC = absolute value of discretionary accruals, where discretionary accruals are estimated from the modified Jones model.

Conditional Variables

InfoEnviron = first principal component derived from six variables capturing firm-level information environment before IFRS adoption. The six variables are: (1) *ADR,* an indicator variable that equals 1 if a firm cross-lists its shares in the U.S. using American Depository Receipts (ADRs) in the year before the adoption, and 0 otherwise; (2) *Index,* an indicator variable that equals 1 if the firm is included in any stock market index in the year before the adoption, and 0 otherwise; (3) *Exchanges,* the number of exchanges on which the firm is listed in the year before the adoption; (4) *Foreign sales,* the average foreign

sales in the two years before the adoption; (5) *Analyst,* the number of analysts following in the year before the adoption; and (6) *Size,* the average of natural logarithm of market value of equity in the two years before the adoption;

Change in GAAP = differences between national accounting standards and IFRS based on Bae et al. (2008, Table 1); higher values indicate greater changes in GAAP after IFRS adoption;

Enforcement = mean of the three enforcement measures from La Porta et al. (1998): (1) the efficiency of the judicial system, (2) the rule of law, and (3) the corruption index. Higher values indicate stronger enforcement;

Large GAAP change with strong enforcement = indicator variable equal to 1 for countries with values of *Change in GAAP* and *Enforcement* both above or equal to the sample country-level median for mandatory adopters, and 0 otherwise;

Change in IAS 39 fair value provisions = country-level index of the differences between national accounting standards and IFRS in IAS 39 fair value provisions based on the number of inconsistences between local GAAP and IAS 39 in Andersen et al. (2001). The inconsistencies that comprise the index consist of the following: (1) IAS 39.35/37/38, a financial asset should be derecognized when legal title is transferred even if the control is retained by the transferor, (2) IAS 39.69, trading, available-for-sale, and derivative financial assets are not recognized at fair value, (3) IAS 39.93, trading and derivative liabilities are not recognized at fair value, (4) IAS 39.103, gains and losses on the change in value of trading financial instruments are not required to be taken as income, and (5) IAS 39.142, hedge accounting is permitted more widely. Higher values indicate greater changes in fair value provisions after IFRS adoption;

Fair value exposure = firm-level variable capturing the use of fair value accounting for commercial and investment banks (two-digit SIC codes of 60–62), measured as the total of trading/dealing account securities and investment securities in the year before the adoption, scaled by total assets; and

Restrict = country-level index of regulatory restrictions on the activities of banks from Barth et al. (2006). This index measures regulatory impediments to banks engaging in securities market activities (e.g., underwriting, brokering, dealing, and all aspects of the mutual fund industry), insurance activities (e.g., insurance underwriting and selling), real estate activities (e.g., real estate investment, development, and management), and the ownership of nonfinancial firms. Higher values indicate more restrictive regulations.

Others

Country indicators = indicator variables for countries;

Industry indicators = variables indicating industry membership based on Campbell (1996); and

Year indicators = indicator variables for years.

APPENDIX B

Procedure to Develop Propensity-Score-Matched Sample

The propensity-score-matching approach involves pairing treatment and control firms based on similar observable characteristics (Dehejia and Wahba 2002). We implement this procedure by first estimating a logit regression to model the probability of being a mandatory IFRS adopter using the sample of treatment firms and the benchmark sample of local GAAP users in non-IFRS adopting countries. We use all of the firm-level control variables in Equation (3) as well as industry and year fixed effects as our predictors. Next, we estimate the propensity score for each firm using the

predicted probabilities from the logit model. We then match each treatment firm to the control firms using the caliper matching technique (with replacement), which uses all of the comparison observations within a pre-defined propensity score radius (or "caliper") of 0.01. The estimation result for our logit regression is as follows:

Variable	Dep. Var. = *Mandatory Adopters* Nonfinancial Firms	Dep. Var. = *Mandatory Adopters* Financial Firms
$DTURN_{t-1}$	0.689***	0.755***
	(0.000)	(0.007)
$NCSKEW_{t-1}$	−0.002	−0.152***
	(0.930)	(0.005)
$SIGMA_{t-1}$	−28.629***	−51.860***
	(0.000)	(0.000)
$\%RET_{t-1}$	−2.529***	−7.257***
	(0.000)	(0.000)
$SIZE_{t-1}$	−0.189***	0.103***
	(0.000)	(0.006)
MB_{t-1}	0.041***	−0.253***
	(0.000)	(0.000)
LEV_{t-1}	0.685***	−0.163
	(0.000)	(0.685)
ROA_{t-1}	−0.972***	1.923**
	(0.000)	(0.044)
$ABACC_{t-1}$	−1.680***	
	(0.000)	
Year fixed effects	Yes	Yes
Industry fixed effects	Yes	Yes
Observations	34,569	5,900
Pseudo R^2	0.105	0.107

*, **, *** Indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

APPENDIX C

Descriptive Statistics of Country-Level Conditional Variables

This table presents the descriptive statistics on the following country-level variables: change in GAAP, enforcement, change in IAS 39 fair value provisions, and banking restriction index.

Country	*Change in GAAP*	*Enforcement*	*Change in IAS 39 fair value provisions*	*Restrict*
Australia	4	9.51	4	8
Austria	12	9.36	2	5
Belgium	13	9.44	3	9
Czech Republic	14	—	—	—
Denmark	11	10.00	3	8
Finland	15	10.00	2	7
France	12	8.68	4	6
Germany	11	9.05	3	5
Greece	17	6.82	2	9
Hong Kong	3	8.91	1	—

Hungary	13	—	—	—
Ireland	1	8.36	3	8
Italy	12	7.07	2	10
Luxembourg	18	—	—	—
The Netherlands	4	10.00	2	6
Norway	7	10.00	2	—
Philippines	10	3.47	2	7
Poland	12	—	3	10
Portugal	13	7.19	3	9
Slovenia	9	—	—	—
South Africa	0	6.45	0	8
Spain	16	7.14	3	7
Sweden	10	10.00	3	9
Switzerland	12	10.00	2	5
U.K.	1	9.22	3	5
Median	12	9.14	3	8