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 non-financial firms and financial firms because IFRS is likely to affect their crash risk differently. We find that crash risk decreases among non-financial firms after IFRS adoption, especially among firms in poor information environments and in countries that experience larger and more credible GAAP changes. In contrast, crash risk does not change among financial firms after IFRS adoption, on average, but decreases among financial firms that are less affected by IFRS's fair value provisions, and increases among banks in countries with weak banking regulations. Taken together, our results are consistent with increased transparency from IFRS adoption broadly reducing crash risk among non-financial firms, but more selectively among financial firms, and with financial regulations playing a complementary role in implementing IFRS among financial firms.

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I. INTRODUCTION

Crash risk, defined as the frequency of extreme negative stock returns, is an important 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 et al., 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 settings. Thus, the purpose of this study is to test whether mandatory IFRS adoption affects crash risk.

We separately analyze non-financial firms and financial firms because IFRS adoption is likely to affect crash risk for these firms through different mechanisms. For non-financial firms, we expect IFRS adoption to affect crash risk primarily through additional disclosure and improved comparability, which in turn is expected to increase reporting transparency. This expectation is consistent with a large body of prior literature that finds IFRS adoption results in a variety of capital market benefits (e.g., Daske et al., 2008; Li, 2010; Byard et al., 2011; DeFond et al., 2011; Tan et al., 2011). Prior 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 non-financial 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 non-financial firms. However, while some of these disclosures apply to financial firms (e.g., disclosure of related party transactions), many apply only to non-financial firms (e.g., the elimination of LIFO). Thus, we expect these transparency effects to be relatively small for financial firms when compared to non-financial firms. A second channel is through the fair value consequences of implementing 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, it should increase transparency and thereby reduce crash risk (IASB, 2003; 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, it should increase opacity, thereby increasing crash risk (European Central Bank, 2004). A third channel is through changes in management risk taking, with effects on crash risk that are also difficult to predict. Fair value accounting may encourage investment in risky assets by amplifying the upside potential of investment gains (Burkhardt and Strausz, 2006; Li, 2009). If fair value estimates of riskier assets contain relatively more measurement errors, they will increase opacity, thereby increasing crash risk. However, this effect is likely to be limited to jurisdictions that do not restrict managements' ability to invest in risky assets (Laeven and Levine, 2009). Alternatively, the increased volatility associated with fair value accounting may induce managers to reduce investments in risky assets (Gigler et al., 2013), thereby reducing crash risk.

We test the effects of mandatory IFRS adoption on crash risk using a sample of 8,472 non-financial 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 potentially 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 among mandatory IFRS adopters with three benchmark samples over the period 2003–2006. Our benchmark samples control for changes in the economic environment that impact crash risk but are unrelated to IFRS adoption (Li, 2010; DeFond et al., 2011). The three benchmark samples consist of: (1) local GAAP users in non-IFRS adopting countries, which we term "non-IFRS adopters," (2) local GAAP users in non-IFRS adopting countries that are propensity-score-matched (PSM) with mandatory adopters, which we term "PSM non-IFRS adopters," and (3) firms that voluntarily adopt IFRS prior to 2005, which we term "voluntary adopters."

Our analysis supports our hypothesis that crash risk decreases for non-financial firms after mandatory adoption. In addition, as predicted, the decrease is larger among non-financial firms in poor information environments, and among non-financial firms in countries where IFRS results in larger and more credible changes to local GAAP. These cross-sectional results support the notion that IFRS reduces crash risk for non-financial 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 for

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.[1] We further find that crash risk decreases among financial firms for which IAS 39 results in relatively fewer changes to local GAAP, which is 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 managers to increase their investment in riskier assets (Laeven and Levine, 2009), which are more difficult to fair value. 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 non-financial firms is robust to a variety of sensitivity tests, including the use of alternative sample countries, sample periods, crash risk measures, and control variables. However, the insignificant average post-IFRS change in crash risk for financial firms is somewhat sensitive to our choice of sample countries and crash risk measures, with some tests finding evidence that average crash risk declines for financial firms. This sensitivity is consistent with our overall conclusion that the effect of IFRS adoption is more selective among financial firms than among non-financial firms.

Our study contributes to the literature in several ways. One contribution is that our study investigates a consequence of IFRS adoption not previously examined in this literature. Crash risk captures negative return skewness (the third moment of stock returns),

[1] We restrict this analysis to banks because fair value accounting is particularly important in this industry, and because measures of banks' exposure to fair value accounting are well defined in the literature.

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).[2] The recent financial crisis has drawn increased attention to tail risk (which is a function of skewness), because extreme negative events can impose significant losses on investors. By examining crash risk, we contribute to the research that examines the impact of accounting standards on asset pricing, and to the relatively recent research that examines tail events (Jin and Myers, 2006; Hutton et al., 2009; Kim et al., 2011a and b).[3]

Second, we make a significant contribution to the literature that attempts to link 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 that capture financial reporting transparency. Hutton et al. (2009) use US data to further 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. Our analysis differs from Hutton et al. (2009) in several important ways. One is that our proxy for increased transparency is the adoption of accounting standards that increases firm-level disclosure and comparability, which is a more comprehensive measure of financial reporting transparency than discretionary accruals. Importantly, we also find that IFRS adoption is associated with decreased crash risk even after controlling for discretionary accruals, which means that IFRS adoption does not simply capture a decline

[2] Examples of IFRS studies examining the first and second moments of stock returns include Armstrong et al. (2010), which documents market reactions to IFRS-related events, and Beuselinck et al. (2009), which investigates the effect of IFRS on stock return synchronicity. We note that implications of IFRS adoption for the third moment cannot be mechanically inferred from findings related to the first or second moments. For example, Chen et al. (2001) find that the correlation between negative return skewness and stock return volatility is less than 0.01, suggesting that these are distinct constructs. Nevertheless, we control for the first and second moments in our analysis to ensure that our results capture effects beyond these measures.

[3] Left tail risk is also important if idiosyncratic return skewness is a priced component of stock returns, as suggested by some theoretical and empirical research (Brunnermeier et al., 2007; Mitton and Vorkink, 2007; Barberis and Huang, 2008; Boyer et al., 2010; Conrad et al., 2013).

in discretionary accruals.[4] Another important difference is that we employ a difference-in-differences research design that examines whether changes in accounting standards are related to changes in crash risk. An advantage of this design over cross-sectional association tests is that it controls for time-invariant omitted correlated variables.

Our study also adds to the literature by finding that IFRS adoption affects crash risk differently for non-financial firms and financial firms. While we find that IFRS pervasively decreases crash risk for non-financial firms, we find that 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; Landsman et al., 2012).

Finally, our study adds to the research that examines the effects of fair value accounting on financial firms (Khan, 2010; Laux and Leuz, 2010; Bhat et al., 2011). While Laux and Leuz (2010) suggest that fair value accounting does not exacerbate financial crises, Khan (2010) and 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

[4] Critically, recent research finds that IFRS adoption does not reduce discretionary accruals, further suggesting that our results are not explained by increased accruals quality (Ahmed et al., 2012).

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. An important caveat to our analysis of financial firms, however, 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. However, 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 Non-financial Firms

Proponents of IFRS argue that it increases reporting transparency, which enables investors to more easily compare financial performance across different jurisdictions (Tweedie, 2006).[5] Consistent with these assertions, 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 (e.g., Daske et al., 2008; Li, 2010; Byard et al., 2011; DeFond et al., 2011; Tan et al., 2011). Notably, however, most of these studies base their findings on samples composed entirely of non-financial firms, or on pooled samples that are dominated by non-financial firms. Thus, while the literature is consistent with mandatory IFRS adoption increasing financial reporting transparency among non-financial firms, its effects on financial firms are less clear.

[5] 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).

For non-financial firms, we predict that the increased transparency associated with IFRS adoption is likely to reduce 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, managers are only able or willing to withhold the bad news up to a point. Once this threshold is met, the accumulated negative information is disclosed all at once, resulting in a stock price crash. Importantly, this theory only requires that managers have the ability and incentives to control public access to at least some negative information about firm value. When managers lose either their ability or incentives to withhold this information, accumulated negative information is abruptly released, causing a stock price crash (Hutton et al., 2009). A classic example is the downfall of Parmalat, whose opaque financial reporting enabled insiders to hide their tunneling activities for over a decade (Coffee, 2005).[6] Supporting this information-based theory, Jin and Myers (2006) find that country-level differences in opacity are associated with cross-country differences in stock return crash frequencies, and Hutton et al. (2009) find that discretionary accruals are associated with crash risk among non-financial firms in the U.S. 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 is (in alternative form):

Hypothesis 1: Non-financial 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. This is

[6] Parmalat's accounting scandal involved a fictitious €3.9 billion account with Bank of America and misappropriation of assets by corporate insiders through related party transactions with companies affiliated with or controlled by its founder (*Wall Street Journal*, 'How Parmalat Spent and Spent,' July 23, 2004).

consistent with Kothari et al. (2009), who find that managers are more likely to delay the release of bad news than good news. 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 incrdeased financial reporting transparency, we expect the effects to be more pronounced among non-financial 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 (in alternative form):

Hypothesis 1A: Non-financial firms in poor information environments experience a greater decrease in crash risk than other non-financial firms subsequent to mandatory IFRS adoption.

Further, if IFRS decreases crash risk through improved transparency, 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 (in alternative form):

Hypothesis 1B: Non-financial firms in countries with larger and more credible changes in accounting standards experience a greater decrease in crash risk than other non-financial firms subsequent to mandatory IFRS adoption.

The Impact of IFRS Adoption on Crash Risk for Financial Firms

Compared to its effect on non-financial 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 effects of which are ambiguous. One channel is through the increased transparency that affects non-financial 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 such as the disclosure of related party transactions or the requirement to disclose 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 for non-financial 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.[7] 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, it reduces managers' ability to hide bad news. This is consistent with the IASB press release accompanying the standards on financial instruments:

> *'The Standards require companies to disclose their exposure to financial instruments and to account for their effects – in most cases as they happen, rather than allowing problems to be hidden away. In particular, IAS 39 requires derivatives to be reported at their 'fair' or market value, rather than at cost. This overcomes the problem that the*

[7] IAS 39 was originally issued in December 1998 and became effective January 1, 2001 (Deloitte, 2012). The fair value accounting effects of IFRS adoption are likely to have a negligible effect on non-financial firms because most of the fair value provisions (contained in IAS 39) are applicable only to financial instruments. While IFRS allows the voluntary use of fair values for non-financial 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.

cost of a derivative is often nil or immaterial and hence if, derivatives are measured at cost, they are often not included in the balance sheet at all and their success (or otherwise) in reducing risk is not visible. In contrast, measuring derivatives at fair value ensures that their leveraged nature and their success (or otherwise) in reducing risk are reported.' (IASB, 2003, p.1).

Consistent with the view that fair value accounting results in more informative financial statements, 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 of global banks (Bischof et al., 2012).[8] Thus, if shifting from historical cost accounting to the fair value provisions under IFRS reduces opacity, 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, it should increase the ability of managers to hide bad news. The benefits from fair value accounting derive from its ability to better capture firms' underlying economics. Factors that may impair this ability include noise in the fair value measures and the manager's 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 (e.g., debt covenants and compensation contracts), capital markets (e.g., earnings expectations), and regulation.

The presence of noise and bias in fair value estimates introduces measurement errors that potentially increase earnings volatility, which impairs the ability of investors to

[8] In addition to IAS 39, Muller et al. (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.

observe true firm performance and therefore increases reporting opacity (Ball, 2006; Plantin et al., 2008).[9] The source of such errors includes "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.

Concerns that fair value estimates introduce volatility that increases opacity are consistent with apprehensions expressed prior to the adoption of IFRS in the EU.[10] French president Jacques Chirac, in a letter to the president of the European Commission in 2003, states that IAS 39 would lead to increased volatility among banks and have 'nefarious consequences for financial stability' (PriceWaterhouseCoopers, 2006).[11] The European Central Bank indicates a similar concern in their 2004 staff report (European Central Bank, 2004, p.2).[12] Thus, if the increased volatility associated with IAS 39 increases opacity, we expect IFRS adoption to increase crash risk.

[9] Volatility introduced by measurement error and bias is sometimes referred to as "artificial volatility" (e.g., Plantin et al., 2008; European Central Bank, 2004).

[10] Consistent with increased reporting volatility, we find (in untabulated analysis) that the standard deviation of ROA among financial firms significantly increases (at the 1% level) subsequent to IFRS adoption. In contrast, we find no significant change in the standard deviation of ROA among non-financial firms. It is unclear, however, whether this increased volatility is due to IAS 39's ability to better capturing underlying performance, or from bias and noise in the fair value measures.

[11] The European Union (EU) initially endorsed IAS 39 with two "carve-out" provisions on fair value options and portfolio hedging of demand deposits (Armstrong et al., 2010). The carve-out on fair value option was eventually eliminated after the IASB amended IAS 39 with a new fair value option in 2005. The effect of IAS 39 on reporting volatility was also tempered during the financial crisis in 2008, when the IASB issued amendments allowing companies to reclassify financial assets in order to avoid recognition in the income statements.

[12] In addition, to the extent that fair values increase investor uncertainty about the true volatility of firms' underlying cash flows, they may increase the required risk premium. As the risk premium increases, stock prices fall, which reinforces the effects of bad news but offsets the effect of good news, thus generating negative skewness (i.e., crash risk). This is also consistent with the empirical observation that volatility is related to negative returns (French et al., 1987) and with volatility feedback effects (Campbell and Hentschel, 1992). Greater volatility also increases the probability of violating accounting-based debt covenants (such as net worth covenants), thereby triggering costly renegotiation with the lenders and in turn increasing

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 (Burkhardt and Strausz, 2006; Li, 2009). Because riskier assets include investments that are more difficult to fair value (such as real estate and thinly traded securities) their fair value estimates are more likely to contain measurement errors. If so, they are also more likely to reduce reporting transparency. However, the ability of managers to respond to incentives to engage in excessive risk taking is limited by financial regulations that restrict managers' abilities to invest in risky assets (Laeven and Levine, 2009).[13] 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 the firm to higher earnings volatility, increased cost of capital, and greater regulatory risk (Gigler et al., 2013). If this is the case, 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 risky assets, its effects on financial reporting transparency, and hence crash risk, are difficult to predict.

borrowing costs. Since contract renegotiation tends to happen when bad news arrives, this may also increase crash risk.

[13] Fair value accounting may also lead to increased crash risk through its effect on investor behavior during financial crises, such as feedback trading (Allen and Carletti, 2008; Plantin et al., 2008; Bhat et al., 2011). However, given that our primary analysis does not encompass the financial crisis, such behavior is unlikely to explain our results.

In summary, given these multi-dimensional effects of IFRS adoption on crash risk for financial firms, we are unable to provide a directional prediction in our second hypothesis, which is:

Hypothesis 2: Financial firms may experience either a decrease or an increase in crash risk subsequent to mandatory IFRS adoption.

To further shed 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.[14] 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. However, because increases in fair value accounting can either increase or decrease crash risk, we do not have a directional prediction for these hypotheses, which are as follows:

Hypothesis 2A: 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.

[14] Local GAAP for financial firms varies across countries. For example, some domestic standards (e.g., Sweden and the U.K.) require some financial instruments (e.g., 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.

Hypothesis 2B: Financial firms with relatively high exposure to fair value accounting experience either an increase or a decrease in crash risk subsequent to mandatory IFRS adoption.

Finally, we also perform analysis that attempts to isolate whether increased risk taking is a channel through which IFRS affects crash risk due 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). As discussed above, it is difficult to predict whether IFRS will increase or decrease risk taking. Thus, we have the following non-directional hypothesis:

Hypothesis 2C: Financial firms in countries with relatively weak 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 last two years before the IFRS mandate (i.e., the pre-adoption period), and the first two years after the IFRS mandate (i.e., the post-adoption period). For example, 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. We use this relatively short event window to reduce the effect of confounding events such as the Sarbanes Oxley Act in 2002, the financial crisis, and the IAS 39 amendment in 2008

(Bischof et al., 2011).[15] To identify mandatory adopters, we select companies that report under local accounting standards ("DS" code in Compustat) in the two-year pre-adoption period and under IFRS ("DI" code in Compustat) 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 of mandatory IFRS adopters worldwide, including 8,472 non-financial and 1,748 financial observations (i.e., firms with 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 30,380 firm-year observations of local GAAP users in 19 non-IFRS adopting countries, including 26,228 non-financial and 4,152 financial observations; (2) "PSM non-IFRS adopters," which consist of 7,105 firm-year observations of local GAAP users that are propensity-score-matched based on characteristics that typify the treatment group of mandatory adopters, including 5,977 non-financial and 1,128 financial observations; and (3) "voluntary adopters," which consist of 1,596 firm-year observations of voluntary IFRS adopters, including 1,400 non-financial 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. [16]

[15] We expect the effect of SOX to play a role in our setting because US firms are included in the benchmark samples of non-IFRS adopters and PSM, and international markets are often affected by the US capital markets, as reflected in our inclusion of US market index return in equation (1).

[16] We evaluate the effectiveness of the propensity score matching by examining (1) the differences of firm characteristics across the treatment and control groups subsequent to the match, and (2) the pre- and post-match changes in pesudo-R^2 and the model fitness of the logit regression. In untabulated analysis, we find that for non-financial firms only the differences in lagged sigma and lagged leverage ratio are significant (at the 10% level) after the match; and that for financial firms only the difference in lagged ROA is significant. In addition, the pesudo-R^2 for non-financial firms (financial firms) drops dramatically from 10.5%

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. This benchmark has the advantage of not suffering from a self-selection bias (as with the voluntary adopters) and limited theoretical underpinning (as with the PSM non-IFRS adopters). Importantly, the non-IFRS adopters also have the largest number of observations, which is important in our partitioning analyses (which requires adequate sample sizes).

Table 1 reports the firm-year distribution of non-financial and financial firms across the treatment sample and the three benchmark samples. Panel A reports the sample distribution for mandatory and voluntary adopters. The panel indicates that the number of firm-year

(10.7%) before the match to 1.3% (2.0%) after the match; and a Chi-squared test for the overall model fitness after the match shows that we cannot reject the null hypothesis that all coefficient estimates of the independent variables are zero (with a p-value of 0.167 for non-financial firms and 0.170 for financial firms). Thus, the propensity score matching seems to be effective in removing meaningful differences in the matched variables across the treatment and control groups.

observations of mandatory IFRS adopters vary considerably, for example, ranging from zero in Estonia and Malta to 1,872 in Australia for non-financial firms. A similar pattern is evident for the distribution of financial firms. Voluntary adopters, on the other hand, are dominated by observations from Germany and Switzerland, consistent with the cross-country variance in IFRS-related regulations. Panel B reports the sample distribution for non-IFRS adopters and PSM non-IFRS adopters. The panel also shows a significant variation in sample distribution across non-IFRS adopters. For example, the U.S. has the largest number of observations for non-financial (8,832) and financial firms (2,456), while Morocco has the smallest number of observations for non-financial firms (four) and China has the smallest number of observations for financial firms (zero).

Variables

Measuring Crash Risk

Our crash risk measure is based on skewness, defined as the third moment scaled by the second moment.[17] This measure was initially proposed by Chen et al. (2001) to capture the asymmetry of the return distribution and has been frequently used in the literature (Kim et al., 2011a, b). 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. That is, the firm has a disproportionate likelihood of experiencing extreme negative stock returns. We multiply the skewness measure by -1 so that a higher number corresponds with greater downside tail risk.

We use the following procedures to calculate our crash risk measure. We begin with estimating firm-specific weekly returns for each firm-year. The firm-specific weekly return,

[17] Bulmer (1979) suggests the following general rule of thumb for judging the extent of skewness: If the absolute value of skewness greater than 1, between 1 and ½, and less than ½, the distribution is highly skewed, moderately skewed, and approximately symmetric, respectively.

denoted by W, is defined as the natural logarithm of one plus the residual return from the following expanded market model regression based on prior studies (Morck et al., 2000; Jin and Myers, 2006):

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

where r_{it} 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 US market index return (a proxy for the global market) in week t, and $EX_{j,t}$ is the change in country j's exchange rate versus the US 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 lagged terms for the local and US market index returns to allow for nonsynchronous trading (Dimson, 1979), and includes a US 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 Ln $(1+ \varepsilon_{i,t})$. We use residual returns from equation (1) instead of raw or actual returns to calculate firm-specific weekly return because we are interested in firm-level crash risk caused by idiosyncratic factors. Actual and raw returns can overstate crash events because they reflect

overall market declines.

Finally, we compute our crash risk measure for each firm *i* in year *t*, denoted by $NCSKEW_{i,t}$, 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. The equation is as follows:

$$NCSKEW_{it} = -\left[n(n-1)^{3/2}\sum W^{3}{}_{it}\right] \Big/ \left[(n-1)(n-2)\left(\sum W^{2}{}_{it}\right)^{3/2}\right] \qquad (2)$$

Other Variables

Following prior studies such as Chen et al. (2001), Hutton et al. (2009), and Kim et al. (2011a, b), our analysis includes the following control variables: (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 one-year 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 in the future; (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 prior studies document that 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;[18] (8) income

[18] 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).

before extraordinary items divided by lagged total assets in year t-1, 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; [19] (10) country and year indicators to control for country and year fixed effects; and (11) for analysis based on non-financial firms, industry indicators to control for industry fixed effects. To mitigate the influence of outliers, we winsorize all continuous variables in our multivariate regression analysis at the top and bottom 1% of their distributions.

Descriptive Statistics

Panels A and B of Table 2 present descriptive statistics of the treatment and benchmark samples for non-financial and financial firms, respectively, and Appendix A provides detailed variable definitions. Panel A of Table 2 indicates that, for non-financial 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.

Panel B of Table 2 presents the descriptive statistics for financial firms. It 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

[19] Following the earnings management literature, we compute abnormal accruals for non-financial firms only.

adopters), −0.178 (PSM non-IFRS adopters), and −0.326 (voluntary adopters). Similar to Panel A, 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. The panel also indicates a reasonably high degree of variation in many of the control variables for the treatment sample as well as the three benchmark groups.

Panel C of Table 2 presents Pearson correlation coefficients across the test variables for non-financial and financial firms separately. For non-financial firms, the crash risk measure ($NCSKEW_t$) 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 - the crash risk measure is positively related to 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}), and lagged return on assets (ROA_{t-1}), and negatively correlated with standard deviation of weekly returns over the previous year ($SIGMA_{t-1}$).

Research Design

To test our hypotheses of the average effect of mandatory 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. Our regression model follows:

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

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 indicator variable indicating mandatory adopters and the indicator variable indicating the post-adoption period because these variables are a linear combination of the country fixed effects and year fixed effects included in our models.

In this and all subsequent regression analysis we adjust the standard errors by country cluster as the 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 Hypotheses 1 and 2

Panels A and B in Table 3 report the regression results of the average effect of IFRS adoption on crash risk for non-financial 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) report the results where the benchmark group is non-IFRS adopters, columns (4)-(6) report the results where the benchmark is PSM non-IFRS adopters, and columns (7)-(9) report the results where the benchmark is voluntary IFRS adopters. Columns (1), (4), and (7) include only the firm-level control variables, columns

(2), (5), and (8) include our variables of interest and country, industry, and year fixed effects, and columns (3), (6), and (9) report our full regression models.

Panel A of Table 3 reports that for non-financial firms, the coefficient β_1 on the interaction term, *Mandatory Adopters×POST*, is negative and significant at the 10% level for all three benchmark groups. These results are consistent with our first hypothesis that non-financial firms experience a decrease in crash risk following mandatory IFRS adoption.

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. We also find that the decrease in crash risk among non-financial firms is economically significant. Specifically, depending on the benchmark group, mandatory IFRS adoption among non-financial firms is associated with a decrease in crash risk ranging from 20 to 44 percent, compared with the overall average crash risk for non-financial firms.[20] 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 (e.g., Byard et al., 2011; Tan et al., 2011).

In contrast, Panel B of Table 3 indicates that for financial firms, the coefficient on *Mandatory Adopters×POST* is insignificant in all of our three tests that include control variable. Thus, we find no significant change in crash risk among financial firms after mandatory IFRS adoption, on average. As for the control variables, both panels of Table 3

[20] 20% = -0.058/-0.285, where -0.058 is β_1 in columns (8) and (9) in Panel A of Table 3, and -0.285 is the mean crash risk for mandatory adopters reported in Panel A of Table 2. 44% = -0.126/-0.285, where -0.126 is β_1 in column (6) in Panel A of Table 3. The numbers are qualitatively similar if we use mean crash risk in the pre-adoption period to assess economic significance.

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 non-financial firms and for financial firms.[21] 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, b).

In summary, the results in Table 3 support our hypothesis that increased transparency under IFRS decreases crash risk among *non-financial* firms. In contrast, the results find that mandatory IFRS adoption has little overall impact on crash risk among financial firms. In the next section, we test several hypotheses that attempt to find evidence that corroborates our hypothesis that increased reporting transparency explains why IFRS adoption reduces crash risk among non-financial firms. We also test additional hypotheses that provide more powerful tests of the various channels through which IFRS adoption affects crash risk for financial firms.

Tests of Additional Hypotheses for Non-financial Firms

Test of Hypothesis 1A: Analysis Conditional on Firm-level Information Environments

Hypothesis 1A predicts that if the decrease in crash risk for non-financial firms is due to improved financial reporting transparency, 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 Adoption*POST*, across the two partitions to test whether crash risk differs

[21] 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, Panel C of Table 2 finds that the Pearson correlation between $NCSKEW_t$ and $NCSKEW_{t-1}$ is 0.075 (0.085) for non-financial firms (financial firms), which are comparable to the autocorrelation of 0.05 documented in Chen et al. (2001).

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 (Covrig et al., 2007, footnote 18).

We classify firms into rich or poor information environments following prior studies (e.g., Armstrong et al., 2010). Specifically, we create a variable labeled "*InfoEnviron*," which equals the first principal component derived from six variables that capture the firm's information environment prior to mandatory IFRS adoption. The six variables are: (1) *ADR*, an indicator variable that equals one if a firm cross-lists its shares in the U.S. using American Depository Receipts (ADR) in the year before the adoption, and zero otherwise; (2) *Index*, an indicator variable that equals one if the firm is included in any stock market index in the year before the adoption, and zero 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 analyst 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 non-financial firms based on the median value of "*InfoEnviron*" in the full sample (i.e., treatment and non-IFRS benchmark firms) and label firms 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 the coefficient on *Mandatory adoption*POST*, which is captured by β_1, to be significantly more negative

among firms in poor information environments than among firms in rich information environments.

Panel A of Table 4 presents the results of this analysis. We report the results using our *InfoEnviron* measure, as well as those using each of its six components. To conserve space, we do not report the coefficients on the control variables. Panel A reports that β_1 is significantly negative in each model except in columns (4) and (12). In addition, consistent with our hypothesis, we find that β_1 is significantly more negative in the poor information environment partition than in the rich information environment partition (significant at the 10% level or less) when we use the aggregate measure and three of its six components (i.e., ADR, foreign sales, and number of analyst following).[22] Overall, the evidence in Panel A is consistent with IFRS adoption widely reducing crash risk for non-financial firms, and with this reduction being relatively greater among firms in poor information environments. We also find that the decrease in crash risk among non-financial firms in poor information environments is economically significant. Mandatory IFRS adoption in poor information environments is associated with a decrease in crash risk of 53 percent when we use the aggregate information environment variable, relative to the overall average crash risk for non-financial firms.[23]

[22] While not reported in the table, we also 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.

[23] 53% = -0.149/-0.285, where -0.149 is β_1 in column (1) of Panel A of Table 4 and -0.285 is mean crash risk in Panel A of Table 2.

Test of Hypothesis 1B: Analysis Conditional on Country-level GAAP Change and Legal Enforcement

Hypothesis 1B predicts that if the switch to IFRS leads to increased disclosure that reduces crash risk, 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 based 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 et al. (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.[24] We measure the number of changes in local GAAP after IFRS adoption using the *Gaapdiff1* variable in Bae et al. (2008).[25] 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 changes and strong enforcement*).

We next classify mandatory adopters for non-financial firms into four partitions based on local GAAP changes and legal enforcement strength, and estimate the regression 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

[24] We also use alternative measures of enforcement in Kaufmann et al. (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 Panel B of Table 4.

[25] The *gaapdiff1* variable in Bae et al. (2008) is based on a comparison of local GAAP with 21 IAS items (such as those related to segment disclosure, accounting for employee benefit obligations, impairment testing of intangibles, and capitalization of research and development costs).

be more negative among the firms in the *Large GAAP change with strong enforcement* partition.

Appendix C reports descriptive statistics on the components of country-level large GAAP changes with strong enforcement. For example, Luxembourg has the largest change in accounting standards following the IFRS mandate (with a value of 18), while South Africa has the smallest (with a value of zero). Denmark, Finland, the Netherlands, Norway, Sweden and Switzerland have the strongest legal enforcement environment (with a value of 10), while Philippines has the weakest legal enforcement environment (with a value of 3.47).

Panel B1 of Table 4 reports the two-by-two frequency table of sample countries by changes in GAAP and enforcement. It shows that the largest subsample (4,720 mandatory adopters) falls in countries with small changes in GAAP and strong enforcement, while the smallest subsample (544 mandatory adopters) falls in countries with the largest changes in GAAP and strong enforcement.

Panel B2 of Table 4 presents results of comparing companies in countries with *Large GAAP change with strong enforcement* (Cell 1 in Panel B1) with the other cells (Cells 2, 3, 4 in Panel B1). Panel B2 reports that the coefficient on *POST* is significantly negative among the non-financial firms in the *Large GAAP change with strong enforcement* partition. In addition, consistent with our hypothesis, the coefficient on *POST* is significantly more negative in the *Large GAAP change with strong enforcement* partition than in any other three subsamples (significant at the 1% level). In untabulated analysis, we also find that the coefficient on *POST* is significantly more negative in the subsample with large GAAP change and strong enforcement (Cell 1) than in the subsample with only small

changes in GAAP or only weak enforcement (Cells 2 and 3 combined), or in the subsample with either small changes in GAAP or weak enforcement (Cells 2, 3, and 4 combined) (significant at the 1% level). We find that the decrease in crash risk among non-financial 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%, relative to the overall average crash risk for non-financial firms.[26]

In summary, the findings in Panel A of Table 4 suggest that non-financial firms in poor information environments experience a more significant reduction in crash risk than those in rich information environments following the IFRS mandate. The findings in Panel B of Table 4 indicate that the effect of mandatory IFRS adoption in reducing non-financial firms' crash risk is more pronounced when the adoption results in large credible changes in accounting standards. These results corroborate our main conclusions that increased reporting transparency explains why IFRS adoption reduces crash risk among non-financial firms.

Tests of Additional Hypotheses for Financial Firms

Test of Hypothesis 2A: 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. *Hypothesis 2A* 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 GAAP (2001). A higher value indicates a larger number of changes to local GAAP's fair value provisions

[26] 62% = -0.176/-0.285, where -0.76 is β_1 in column (1) of Panel B2 of Table 4 and -0.285 is mean crash risk in Panel A of Table 2.

under IFRS. For example, Germany and the U.K. have a value of three because their local GAAP differs from IAS 39 in three fair value provisions: (1) trading, available-for-sale and derivative financial assets are not recognized at fair value (IAS 39.69), (2) trading and derivative liabilities are not recognized at fair value (IAS 39.93), and (3) hedge accounting is permitted more widely (IAS 39.142).[27] 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, as 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. It shows that South Africa has the least change in our sample (zero), while Australia and France have the most changes (four). Panel A of Table 5 presents the results of our hypothesis testing and finds that the coefficient on *POST* is insignificant among financial firms with large changes in fair value provisions. Thus, our analysis does not find that fair value accounting increases crash risk.

In an attempt to gain additional 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 that is unrelated to IAS 39, those effects are likely to be strongest among the financial firms least affected by IAS 39. Thus, Panel A of Table 5 also examines the change in crash risk among firms with small changes in fair value provisions. This analysis finds that the coefficient on *POST* is significantly negative in this subsample (significant at the

[27] As shown in Appendix C, while countries such as Germany and Greece have larger overall changes in GAAP than Anglo-Saxon countries such as Australia and the U.K., they experience a similar level of accounting changes when it comes to specific IAS 39 fair value provisions.

10% level). Thus, crash risk declines among the subset of financial firms that are less affected by fair value provisions under IAS 39, consistent with some financial firms benefiting from the transparency effects of IFRS that are unrelated to IAS 39. We further explore this explanation by partitioning the subsamples in columns (1) and (2) based on the firm's information environment and report those results in columns (3)-(6). As with our analysis of non-financial firms, this analysis finds that the decline in crash risk among financial firms with small changes in fair value provisions is significantly greater among firms in poor information environments. Thus, the results in Table 5 are consistent with IFRS reducing crash risk for financial firms through increased transparency that is unrelated to IAS 39.

The results in Panel A of Table 5 paint a nuanced picture of how IFRS affects financial firms by essentially separating the "fair value" effects from the (non-fair value related) "increased disclosure" effects. When the fair value effects are strong (in the large change in fair value provisions partition) or the disclosure effects are weak (in the rich information environment partition), crash risk is little affected. This suggests 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 (in the small change in fair value provisions partition) and the increased disclosure effects are strong (in the poor information environment partition), crash risk declines. This indicates that increased transparency from IFRS that is 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 Panel B of Table 3. 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 Hypothesis 2B: Analysis of Firms with More Exposure to Fair Value

Hypothesis 2B 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 (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. [28]

We measure the extent of banks' fair value accounting exposure by the total of trading securities, dealing accounting securities, and investment securities in the year before the adoption, scaled by total assets.[29] In untabulated analysis, we find that the mean and median values of this measure for mandatory adopters are 0.141 and 0.120, respectively. We classify banks as having more fair value exposure if their value of *fair value exposure* is greater or equal to the sample firm-level median value of this variable.

Panel B of Table 5 presents the results of testing *Hypothesis 2B* and indicates that β_1 is insignificant among banks with more fair value exposure. Thus, consistent with the results of testing *Hypothesis 2A*, we continue to find no evidence that the fair value effects of IFRS

[28] Banks are also relatively homogeneous in the composition of their assets subject to fair value accounting under IAS 39, compared with other financial institutions such as insurance companies and REITs.

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

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 Panel A of Table 5, 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.[30]

Test of Hypothesis 2C: Analysis of Banks with Less Restrictive Regulations

Hypothesis 2C predicts IFRS may affect crash risk among financial firms in countries with relatively weak banking regulation, although the direction is difficult to predict. This test is designed to gather evidence on whether IFRS's effect on management's appetite for risk is one channel through which IFRS affects crash risk. We test this hypothesis on a treatment sample of 136 unique commercial banks (with two-digit SIC codes of 60 and 61).[31] 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 et al. (2006). *Restrict* 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.

[30] Jayaraman and Kothari (2012) find evidence that improved transparency in non-financial firms after IFRS reduces the incidence of bank failures by increasing banks' ability to monitor loans.

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

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, 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. It shows that for our treatment sample, Italy and Poland have the most restrictive banking regulations in our sample (10), while Austria, Germany, Switzerland, and the U.K. have the least restrictive regulations (five).[32]

Panel C of Table 5 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.[33] Thus, the results in Panel C are consistent with IFRS increasing crash risk in countries with weak banking regulations by encouraging greater investment in riskier assets.

[32] 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), Indonesia (14), India (10), Israel (13), Japan (13), Korea (9), Sri Lanka (7), Morocco (13), Mexico (12), Malaysia (10), Pakistan (missing), Thailand (9), Taiwan (12), and US (12).

[33] 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 where we include benchmark groups (Panel A of Table 4 and Panels B and C in Table 5), with three exceptions: (1) the difference in the coefficient β_1 across information environment partitions in Panel A of Table 4 becomes 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 Panel B of Table 5 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 Panel C of Table 5 becomes insignificant when using voluntary adopters as the benchmark.

V. SENSITIVITY TESTS

We conduct several robustness checks for alternative sample countries, sample periods, crash risk measures, and control variables for our primary results in Table 3. All of these robustness checks continue to find a significant post-IFRS decrease in crash risk for non-financial firms, as in Panel A of Table 3. The insignificant results for financial firms documented in Panel B of Table 3, however, are somewhat sensitive to the choice of sample countries and crash risk measures. This is not surprising because the effect of IFRS adoption is more selective among financial firms and depends on the number of fair value provision changes and strength of banking regulations. Panel A of Table 6 reports the tests for non-financial firms and Panel B of Table 6 reports the tests for financial firms. We summarize the results of these robustness checks below.

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 et al. (2012): 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 first three rows of Panel A, Table 6 report that our results for non-financial firms are qualitatively identical to those reported in Panel A of Table 3. For financial firms, however, Panel B, Table 6 shows that β_1 becomes significantly positive after restricting the treatment sample to the EU countries and significantly negative after restricting the treatment sample to the non-EU countries.[34] We conjecture that the decreased crash risk among non-EU countries is because five of the six countries in our non-EU sample experience small changes in IAS 39 provisions. As a

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

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. We conjecture that the increased crash risk among EU financial firms may be due to many of the EU countries have relatively weak banking regulations. However, we find that the results for EU financial firms are sensitive to dropping one country at a time. Specifically, if we drop one EU country at a time, eight of the 15 tests experience no change in crash risk, while seven experience an increase in crash risk.

We also perform a test that expands both our sample period to 2001–2011 and our sample countries to those that adopt IFRS after 2005.[35] For non-financial firms, the additional recent IFRS adoption countries and their adoption years are: Brazil (2010), Canada (2011), Israel (2008), New Zealand (2007), Peru (2011), and South Korea (2011). For financial firms, Israel is dropped from the above country list (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). This analysis finds results that are qualitatively identical to those reported in Panel A of Table 3. For the results in Panel B of Table 3, however, this analysis finds the coefficient β_1 becomes significantly negative. This suggests that for many financial firms adopting IFRS after 2008 the increased disclosure effect dominates the fair value effects, which is likely 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 alternative sample period 2001-2011

[35] For the test including recent IFRS adoption countries, we use the PSM non-IFRS adopters as the benchmark so we can code the post adoption period for the benchmark firm based on the matched treatment firm. This is because using non-2005 mandatory adopters will make it difficult to code the post-adoption years for the "non-IFRS adopters" benchmark, our primary benchmark group.

(reported in column (4) in Panels A and B of Table 6), and (2) dropping the transition period 2005. We find results that are qualitatively identical to those reported in Panels A and B of Table 3.[36]

Alternative Measures of Crash Risk

To examine the sensitivity of our results to alternative measures of crash risk, we repeat our analysis in Table 3 by using the down-to-up volatility (*DUVOL*) measure of crash likelihood as in Chen et al. (2001) and Kim et al. (2011b). Specifically, for each firm *i* over a fiscal year period *t*, we separate all the weeks with firm-specific weekly returns, $W_{i,t}$, below the annual mean ("down" weeks) from those with firm-specific returns above the annual mean ("up" weeks) and calculate the standard deviation for each of these subsamples separately. We then measure *DUVOL* as the natural logarithm of the ratio of the standard deviation in the down weeks relative to the standard deviation in the up weeks. In untabulated analysis, we find that *DUVOL* is highly correlated with our crash risk measure, *NCSKEW*, with a correlation coefficient of 0.96. Not surprisingly, Panels A and B of Table 6, we find results that are qualitatively identical to those reported in Panels A and B of Table 3.

We also perform a robustness test after using an alternative *NESKEW* measure anchored on three months following fiscal year end to ensure the market's incorporation of relevant financial disclosures. This analysis also finds results that are qualitatively identical to those reported in Panels A and B of Table 3.

Finally, we perform several robustness checks for alternative measures of *NCSKEW* by changing the following specifications of the return models: using the world market index

[36] By "qualitatively identical to those reported in Panel A of Table 3," we mean that the coefficient β_1 in column (3) is negative and significant at $p \leq 10\%$. By "qualitatively identical to those reported in Panel B of Table 3," we mean that the coefficient β_1 in column (3) is insignificant at conventional level.

return (instead of US market index return), using one lead and lagged terms (instead of two lead and lagged terms), using no lead and lagged terms, and including industry returns. These tests find results that are qualitatively identical to those reported in Panel A of Table 3. For the results reported in Panel B of Table 3, however, we find that the coefficient β_3 becomes significantly negative in three of these fours tests.[37] Thus, while the result for non-financial 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. However, the evidence continues to suggest mandatory IFRS adoption, on average, does not increase crash risk for financial firms.

Alternative Control Variables

Hung and Subramanyam (2007) find that the value and variability of accounting numbers are different between IFRS and German GAAP. To ensure that differences between IFRS and local GAAP accounting numbers do not drive our results, we remove the control variables constructed based on accounting measures (i.e., LEV_{t-1}, ROA_{t-1}, and $ABACC_{t-1}$) and find results that are qualitatively identical to those reported in Panels A and B of Table 3. We also include the contemporaneous values of these three accounting variables as additional controls and our conclusions are unaltered. Thus, our primary conclusion is not sensitive to alternative specifications regarding accounting-based control variables.

[37] It is possible that some of these alternative return specifications (such as including industry returns) are less suitable for capturing overall 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 also repeat our analysis in Panel C of Table 5 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. Thus, our finding that crash risk increases for banks in countries with less restrictive banking is generally not sensitive to these robustness checks.

VI. CONCLUSION

We examine whether mandatory IFRS adoption affects firm-specific stock price crash risk. It is important to understand crash risk in the context of accounting standards because prior research suggests that financial reporting quality is a critical factor in explaining crash risk. The mandatory adoption of IFRS by thousands of companies in 2005 is an ideal setting for testing how changes in financial reporting standards affect crash risk.

For non-financial firms, we find a decrease in crash risk after IFRS adoption, on average. We also find that the effect is more pronounced among firms in poor information environments and in countries with large GAAP changes. These results suggest that increased transparency from IFRS adoption widely reduces crash risk among non-financial firms. In contrast, for financial firms, we find no change in crash risk after IFRS adoption, on average. We do, however, find evidence that financial firms less affected by IFRS's fair value provisions experience a decrease in crash risk, and that banks with less restrictive banking regulations experience an increase in crash risk after 2005.

Our study contributes to the literature by examining the effect of IFRS adoption on crash risk, a previously unexplored implication of IFRS adoption. Crash risk is particularly important for investors because it may not be diversifiable (Brunnermeier et al., 2007; Mitton and Vorkink, 2007; Barberis and Huang, 2008; Boyer et al., 2010; Conrad et al., 2013; Sunder, 2010). Our findings complement prior international studies on the economic consequences of financial reporting regulations. Our study also adds to the research on fair value accounting by examining how a shift from historical-based accounting standards to fair-value-oriented accounting standards affects firm-specific crash risk. Our overall

evidence suggests that the fair value accounting associated with IFRS adoption does not increase crash risk for financial firms, contrary to concerns expressed by regulators and the financial press (European Central Bank, 2004; Hargreaves, 2005),. We caution, however, that our analysis on financial firms is preliminary and exploratory in nature, and that there are many factors to consider in deciding fair value provisions under IFRS.

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APPENDIX A
Variable Definition

Crash risk variable

NCSKEW: The 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):

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

where r_{it} 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 US market index return (a proxy for the global market), and $EX_{j,t}$ is the change in country j's exchange rate versus the US dollar.

Variables of interest

Mandatory Adopters: An indicator variable equal to one if companies prepared their financial statements based on local accounting standards before 2005, and switched to IFRS in 2005.

POST: An indicator variable equal to one if a firm-year falls in or after 2005.

Firm-level controls

DTURN: The 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: The standard deviation of firm-specific weekly returns over the fiscal year period.

RET: The mean of firm-specific weekly returns over the fiscal year period, times 100.

SIZE: The log of the market value of equity.

MB: The 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.

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

Conditional variables

InfoEnviron: The 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 one if a firm cross-lists its shares in the US using American Depository Receipts (ADR) in the year before the adoption, and zero otherwise; (2) *Index*, an indicator variable that equals one if the firm is included in any stock market index in the year before the adoption, and zero 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 analyst 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: The 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: The 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: An indicator variable equal to one for countries with values of *Change in GAAP* and *Enforcement* both above or equal to the sample country-level median for mandatory adopters, and zero otherwise.

Change in IAS 39 Fair value provisions: A 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 GAAP (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 to income, (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: A 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 or dealing accounting securities and investment securities in the year before the adoption, scaled by total assets.

Restrict: A 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).

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, non-financial 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-squared	0.105	0.107

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 bank 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 Rep	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	.	.	.
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
UK	1	9.22	3	5
Median	12	9.14	3	8

TABLE 1
Sample Distribution

Table 1 presents the firm-year distribution 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.

Panel A: Mandatory and Voluntary Adopters

	Mandatory IFRS adopters		Voluntary IFRS adopters	
Country	**Non-financial**	**Financial**	**Non-financial**	**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
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
UK	1,512	244	8	0
Total	8,472	1,748	1,400	196

TABLE 1, CONTINUED

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

	Non-IFRS adopters		PSM non-IFRS adopters	
Country	**Non-financial**	**Financial**	**Non-financial**	**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
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
US	8,832	2,456	1,460	587
Total	26,228	4,152	5,977	1,128

TABLE 2
Descriptive Statistics

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 non-financial 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 correlation for the full sample. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. See Appendix A for variable definitions.

Panel A: Descriptive Statistics for Non-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}	$ABACC_{t-1}$
Treatment group: mandatory 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 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				***	**	***	***		***	***

TABLE 2, CONTINUED

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 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
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 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		*		**		***		**	

TABLE 2, CONTINUED

Panel C: Pearson Correlations for the Full sample (Mandatory Adopters, Non-adopters, and Voluntary Adopters) with Non-financial Firms 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***

TABLE 3
The Average Effect of Mandatory IFRS Adoption on Firm-level Crash Risk

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 non-financial firms and Panel B reports those for financial firms. *p*-values in parentheses are based on standard errors clustered by country (one-tailed for coefficients with predicted signs and two-tailed otherwise). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. See Appendix A for variable definitions.

Panel A: Non-financial Firms

Dep. var. = NCSKEW	**Pred.**	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Benchmark =	**sign**	**Non-IFRS adopters**			**PSM non-IFRS adopters**			**Voluntary adopters**		
Mandatory Adopters β_1	-									
×POST			**-0.101*** **	**-0.110*** **		**-0.121*** **	**-0.126*** **		**-0.058***	**-0.058***
			(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-squared		0.032	0.032	0.054	0.025	0.018	0.041	0.024	0.009	0.033

TABLE 3, CONTINUED

Panel B: Financial Firms

Dep. var. =NCSKEW		**Pred.**	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Benchmark =		**sign**	**Non-IFRS adopters**			**PSM non-IFRS adopters**			**Voluntary adopters**		
Mandatory Adopters	β_1	**?**									
×POST				**-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-squared			0.041	0.039	0.071	0.045	0.047	0.080	0.048	0.033	0.081

TABLE 4
Subsample Analysis of The Effect of Mandatory IFRS Adoption on Crash Risk for Non-financial Firms

Table 4 presents the regression results of the impact of mandatory IFRS adoption on firm-level stock price crash risk for non-financial firms, conditional on firm-level information environment (Panel A), and country-level GAAP change and enforcement (Panel B). The sample firms are partitioned into sub-samples based on the sample median values of the conditional variables. *p*-values in parentheses are based on standard errors clustered by country (one-tailed for coefficients with predicted signs and two-tailed otherwise). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. See Appendix A for variable definitions.

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

Dep. var. = NCSKEW		**Pred. sign**	(1)	(2)	(3)	(4)	(5)	(6)
			Aggregate measure		**ADR**		**Index**	
Partition =			***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_3				-		-		-
Test of difference in β_3, Poor-rich InfoEnviron				***-0.078** ** ***(0.041)***		***-0.288** *** ***(0.081)***		***-0.017*** ***(0.350)***
Firm-level controls			yes	yes	yes	yes	yes	yes
Country fixed effects			yes	yes	yes	yes	yes	yes
Industry fixed effects			yes	yes	yes	yes	yes	yes
Year fixed effects			yes	yes	yes	yes	yes	yes
Observations			17,348	17,352	34,320	380	19,628	15,072
Adj. R-squared			0.047	0.052	0.054	0.045	0.053	0.065

TABLE 4, CONTINUED

Dep. var. = NCSKEW		**Pred. sign**	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
			Exchange		**Foreign sale**		**Analyst**		**Size**	
Partition =			***=1***	***>1***	***=0***	***>0***	***<=1***	***>1***	***Small***	***Large***
Mandatory Adopters ×POST	β_1	**-**	**-0.115*** (0.006)**	**-0.104** (0.049)**	**-0.145*** (0.001)**	**-0.065** (0.048)**	**-0.140*** (0.001)**	**-0.056 (0.105)**	**-0.130*** (0.003)**	**-0.088** (0.045)**
Prediction of difference in β_3				-		-		-		-
Test of difference in β_3, Poor-rich InfoEnviron				***-0.011 (0.437)***		***-0.080*** (0.007)***		***-0.084** (0.031)***		***-0.042 (0.224)***
Firm-level controls			yes	yes	yes	yes	yes	yes	yes	yes
Country fixed effects			yes	yes	yes	yes	yes	yes	yes	yes
Industry fixed effects			yes	yes	yes	yes	yes	yes	yes	yes
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-squared			0.056	0.054	0.053	0.056	0.043	0.036	0.031	0.051

TABLE 4, CONTINUED

Panel B1: Two-by-two Table Conditional on Country-level GAAP Change and Enforcement, Mandatory Adopters Only

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

Panel B2: Analysis Conditional on Country-level GAAP Change and Enforcement, Mandatory Adopters Only

Dep. var. = NCSKEW			(1)	(2)	(3)	(4)
		Pred. sign	**Cell (1)**	**Cell (2)**	**Cell (3)**	**Cell (4)**
Partition =			***Large GAAP change and strong enforcement***	***Large GAAP change and weak enforcement***	***Small GAAP change and strong enforcement***	***Small GAAP change and 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)**
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-squared			0.043	0.041	0.031	0.029

TABLE 5
Subsample Analysis of The Effect of Mandatory IFRS Adoption on Crash Risk for Financial Firms

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). *p*-values in parentheses are based on standard errors clustered by country (one-tailed for coefficients with predicted signs and two-tailed otherwise). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. See Appendix A for variable definitions.

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

Dep. var. = NCSKEW		**Pred. sign**	(1)	(2)	(3)	(4)	(5)	(6)
			Change in fair value provisions		***Large change* in fair value provisions**		***Small change* in fair value provisions**	
Partition =			***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-squared			0.092	0.072	0.105	0.051	0.046	0.017

TABLE 5, CONTINUED

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

Dep. var. = NCSKEW			(1)	(2)
Partition =		**Pred. sign**	***More exposure to fair value***	***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-squared			0.080	0.044

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

Dep. var. = NCSKEW			(1)	(2)
Partition =		**Pred. sign**	***Less restrictive banking regulations***	***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-squared			0.150	0.045

TABLE 6
Sensitivity Tests

Table 6 presents sensitivity analysis on the impact of mandatory IFRS adoption on firm-level stock price crash risk with non-IFRS adopters as the benchmark. Panel A reports the tests for non-financial firms and Panel B for financial firms. *p*-values in parentheses are based on standard errors clustered by country (one-tailed for coefficients with predicted signs and two-tailed otherwise). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. See Appendix A for variable definitions.

Panel A: Sensitivity Tests for Panel A of Table 3 (Non-Financial Firms)

	Coefficient on Mandatory Adopters × POST Pred. sign ='-'	***p*-value**	**Obs.**	**Adj. R-squared**
Alternative sample countries				
Excluding five EU countries with concurrent enforcement changes in Christensen et al. (2012)	-0.135***	(0.000)	32,036	0.056
EU firms only	-0.097**	(0.030)	31,724	0.056
Non-EU firms only	-0.132***	(0.007)	29,204	0.054
Including recent IFRS adopters in six countries	-0.050**	(0.014)	60,512	0.060
Alternative sample period				
Extended sample period 2001-2011	-0.066***	(0.009)	89,651	0.063
Dropping 2005	-0.140***	(0.006)	26,025	0.056
Alternative dependent variable				
DUVOL	-0.052***	(0.006)	34,700	0.055
NCSKEW estimated with 15-month return	-0.081***	(0.004)	34,406	0.055
NCSKEW estimated with world index return	-0.117***	(0.003)	34,407	0.063
NCSKEW estimated with one lead/lag term	-0.129***	(0.007)	34,407	0.072
NCSKEW estimated with no lead/lag term	-0.118**	(0.017)	34,407	0.076
NCSKEW estimated with industry return	-0.072**	(0.016)	31,682	0.042
Alternative control variables				
Dropping accounting controls	-0.108***	(0.003)	34,700	0.053
Adding concurrent accounting controls	-0.108***	(0.003)	34,700	0.056
Firm-level control variables	Yes			
Country fixed effects	Yes			
Industry fixed effects	Yes			
Year fixed effects	Yes			

TABLE 6, CONTINUED

Panel B: Sensitivity Tests for Panel B of Table 3 (Financial Firms)

	Coefficient on Mandatory Adopters × POST Pred. sign ='?'	*p*-value	Obs.	Adj. R-squared
Alternative sample countries				
Excluding five EU countries with concurrent enforcement changes in Christensen et al. (2012)	-0.052	(0.168)	5,416	0.074
EU firms only	0.058*	(0.098)	5,200	0.070
Non-EU firms only	-0.130***	(0.009)	4,852	0.066
Including recent IFRS adopters in six countries	-0.080*	(0.052)	10,390	0.059
Alternative sample period				
Extended sample period 2001-2011	-0.060	(0.128)	16,477	0.060
Dropping 2005	-0.038	(0.489)	4,425	0.071
Alternative dependent variable				
DUVOL	-0.011	(0.538)	5,900	0.071
NCSKEW estimated with 15-month return	-0.077	(0.159)	5,720	0.086
NCSKEW estimated with world index return	-0.115**	(0.012)	5,721	0.097
NCSKEW estimated with one lead/lag term	-0.077**	(0.039)	5,721	0.101
NCSKEW estimated with no lead/lag term	-0.056	(0.182)	5,721	0.109
NCSKEW estimated with industry return	-0.087**	(0.010)	5,462	0.035
Alternative control variables				
Dropping accounting controls	-0.017	(0.660)	5,900	0.070
Adding concurrent accounting controls	-0.009	(0.823)	5,900	0.073
Firm-level control variables	Yes			
Country fixed effects	Yes			
Industry fixed effects	Yes			
Year fixed effects	Yes			