Does extended auditor disclosure deter managerial bad-news hoarding? Evidence from crash risk

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Abstract

We examine how the mandatory adoption of extended auditor's reports (EARs) affects managerial bad-news hoarding through the lens of stock price crash risk. Relying on the UK's auditing standard change in 2013 as a quasi-natural experiment, we document a crash risk reduction for firms that were required to adopt EARs, relative to firms that were not so required. The crash risk reduction is related to EARs' disclosure of risks of material misstatement in revenue recognition. The negative effect of EARs adoption on crash risk is more pronounced for firms with scant public information and firms with non-Big-4 or non-industry-specialist auditors. EARs adoption induces firms to disclose more smaller pieces of negative information without changing firms' accruals management. Taken together, our results suggest that EARs adoption dampens bad-news hoarding by managers.

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1. Introduction

Statutory audits of financial statements have an important function in helping to ensure high standards of financial reporting and lubricating capital markets. The independent auditor's report, as the primary publicly available output from statutory audits, had long been criticized in terms of its form, content, and informational value to users (Mock et al., 2013). The extended auditor's report (EAR hereafter), first introduced by the Financial Reporting Council (FRC) in the UK and subsequently by regulators and standard-setters in other jurisdictions—the International Auditing and Assurance Standards Board (IAASB), the European Commission, and the Public Company Accounting Oversight Board (PCAOB) in the US—has been lauded as by far the most important attempt to date to overcome those challenges. EARs are required to communicate key audit matters[1] (KAMs) under the FRC and IAASB standards, or critical audit matters[2] (CAMs) under the PCAOB standards.[3]

Despite the worldwide regulatory intent to make the auditor's report more informative and relevant to users, empirical evidence from the UK (Gutierrez, Minutti-Meza, Tatum, & Vulcheva, 2018; Lennox, Schmidt, & Thompson, 2022) and the US (Gurbutt & Shih, 2020; Burke, Hoitash, Hoitash, & Xiao, 2022) generally suggests that EARs *per se* provide little incremental information to investors. In particular, Lennox et al. (2022) note that EARs lack incremental information because most risks of material misstatement (also known as KAMs) disclosed by EARs had already been disclosed by the firm's management in the prior year. However, to what extent these pre-emptive corporate disclosures are driven by management's response to the forthcoming disclosure of EARs remains unclear. Our paper sheds light on this question. Relying on the UK's mandatory adoption of EARs as an experimental

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[1] KAMs are "[t]hose matters that, in the auditor's professional judgment, were of most significance in the audit of the financial statements of the current period" (IAASB, 2015). KAMs are determined by considering the auditor's assessed risks of material misstatement or significant risks that require special audit consideration.

[2] CAMs are those matters arising from the audit of financial statements that "relate to accounts or disclosures that are material to the financial statements, and involved especially challenging, subjective, or complex auditor judgment" (PCAOB, 2017). In determining CAMs, auditors are required to take into account the assessed risks of material misstatement, including significant risks.

[3] KAMs and CAMs are similar in terms of intent and content. The PCAOB states, "Although the processes of identifying these matters vary across jurisdictions, there are commonalities in the underlying criteria regarding matters to be communicated and the communication requirements, such that expanded auditor reporting could result in the communication of many of the same matters under the various approaches" (PCAOB, 2017).

setting, we conjecture that EARs adoption alters the timeliness of negative relative to positive information disclosure by managers, which in turn affects firms' future stock price crash risk (hereafter crash risk).

Crash risk is a proxy measure of negative return skewness (Chen, Hong, & Stein, 2001) and a result of managers withholding bad news for prolonged periods but ultimately having to release stockpiled bad news once it exceeds a critical threshold (Jin & Myers, 2006; Hutton, Marcus, & Tehranian, 2009). In real-world business scenarios, managers use sophisticated and well-coordinated schemes to disguise bad news, such as accruals management (Hutton et al., 2009), earnings smoothing (Chen, Kim, & Yao, 2017), tax shelters (Kim, Li, & Zhang, 2011b), complex annual reports (Kim, Wang, & Zhang, 2019a), and off-balance sheet devices (Valukas, 2010). The return-based crash risk measure is a comprehensive metric that enables us to capture all manner of bad-news hoarding. More importantly, we are interested in determining whether managers respond by changing their disclosure of *value-relevant incremental* information, for which an ideal measurement should be market-based. Moreover, crash risk has devastating effects on investor wealth (Yan, 2011; Kelly & Jiang, 2014). Investors, however, have difficulties diversifying away their exposure to crash risk (Ibragimov & Walden, 2007). Regulators and standard setters have strong incentives to take actions aiming to reduce firms' crash risk.

We expect crash risk to decline after the mandatory adoption of EARs. In the current auditor reporting framework, auditors are not expected to use EARs to disclose *original* information regarding the risks of material misstatement present in audited firms' financial statements. If auditors consider any original information necessary for disclosure in EARs, they are likely to urge the firms' managers to disclose the information ahead of the auditor's report. Since risks of material misstatement tend to reflect more overstatement risks than understatement risks about firm performance, the pre-emptive corporate disclosures by managers generally constitute more negative information than positive information to the market. This reflects the alleviation of managers' bad-news hoarding, which in turn attenuates firms' future crash risk.

We test this hypothesis by employing the UK's auditing standard change as an identification due to its two appealing features. First, the UK was the first country to introduce the EAR through a substantive revision to the International Standard on Auditing (ISA) 700 (UK and Ireland). This revision was passed abruptly on June 4, 2013, only four months before EARs adoption came into force on September 30, 2013, for London Stock Exchange (LSE) premium-listed companies. For identification purposes, the UK's adoption serves as a more exogeneous shock compared with the adoption in other countries that latterly implemented the EAR. Second, there is a three-year gap between the first wave of adoption in 2013 for LSE premium-listed companies and the second wave of adoption in 2017 for all other public companies (i.e., LSE standard-listed companies and Alternative Investment Market [AIM] companies). The non-adopters during 2013–2016 can be used as a control group for a difference-in-differences (DiD) empirical design. In EU nations, all public companies adopted the report at the same time, which prevents a test for causality as to whether EARs adoption altered corporate disclosures or vice versa. In the US, large firms adopted the report first, while small firms adopted it one year later.[4] There is only a one-year gap. Any effect has to occur very quickly to be measured in a DiD empirical design. This works against finding any significant result or could favor economic outcomes that appear in the data quickly but reverse over the long term.

Using a DiD framework, we compare the change in future crash risk among mandatory adopters, relative to the contemporaneous change among non-adopters, over a seven-year period from 2010 to 2016 surrounding the mandatory adoption of EARs in 2013. We measure firm-specific crash risk using firm-specific weekly stock returns that are isolated from market-wide return variations. Our DiD regression results show that, relative to non-adopters, mandatory adopters experience a significant reduction in future crash risk following EARs adoption.

We then conduct a dynamic DiD analysis by assessing the timing with which the mandatory adoption of EARs affects future crash risk. We do not find that a significant treatment effect occurred before EARs were mandated, thereby strengthening our confidence in the parallel trends assumption. After EARs were adopted, a significant and sizeable treatment effect only appeared in the first post-

[4] On June 1, 2017, the PCAOB released a new auditing standard AS3101 "The Auditor's Report on an Audit of Financial Statements When the Auditor Expresses an Unqualified Opinion." The standard mandates the disclosure of CAMs for large accelerated filers from June 30, 2019, and for other filers from December 15, 2020.

adoption year. This finding reveals that the disciplining effect of EARs is concentrated in the first year post adoption.

Treatment firms that were selected to adopt EARs are different from non-adopters in many respects. We carry out a series of tests to address this concern. (1) We control for the unobserved factors related to a firm's choice of listing in the LSE premium market or AIM by using a Heckman selection model, where an instrumental variable (an indicator of whether the firm is incorporated before or after the launch of AIM) is included in the first-stage probit model to estimate the firm's probability of being premium-listed. (2) We set a placebo shock as two years before the actual treatment in 2013. We do not find the placebo shock to affect crash risk. (3) Since our primary crash risk measure reflects the third moment of stock returns, we conduct placebo tests to examine whether the mandatory adoption of EARs affects the first and second moments of stock returns. We find no such evidence. (4) The indexes of LSE premium-listed stocks and AIM stocks behave differently around the time of mandatory EARs adoption. To mitigate this concern, we match the two groups of firms on both pre-adoption and post-adoption mean stock returns. (5) AIM is intended to attract small- and medium-sized growth companies. We thus match treatment and control firms on firm size and growth potential. (6) As a further attempt to improve pretreatment covariate balance, we match the two groups of firms on pretreatment crash risk changes and other firm-level observables by using a propensity score matching technique. (7) We match UK mandatory adopters to comparable EU non-adopters in an effort to control for concurrent Europe-wide legal or regulatory regime shifts. (8) We use comparable US firms as an alternative control group to mitigate the distortion stemming from concurrent global changes beyond European markets. Our conclusion survives all the foregoing tests.

The conclusion also withstands a variety of robustness checks: (1) we control for additional drivers of crash risk, including earnings smoothing, accounting conservatism, financial statement comparability, tax avoidance, and annual report file size; (2) we include audit firm fixed effects and audit partner fixed effects; (3) we use FTSE All-Share index returns and AIM All-Share index returns as the UK market return proxies to estimate firm-specific stock returns; (4) we use daily data to estimate crash risk measures; (5) we adopt other univariate, bivariate, and multivariate crash risk measures; (6) we remove observations in the 2013 fiscal year to mitigate any temporary transitional effect; and (7) we drop firms that changed their auditors during the (pseudo) mandatory adoption year, because these auditor replacements may be driven by unobserved factors related to the requirement to adopt EARs.

Which risks of material misstatement disclosed in EARs are powerful enough to force managers to disclose bad news in a timelier manner? We argue that risks of improper revenue recognition[5] could spotlight the underlying bad-news hoarding and hence incentivize managers to pre-empt bad news in corporate disclosures. Consistent with this idea, we show that, among mandatory adopters, those with a higher number of improper revenue recognition risks identified by auditors are associated with less bad-news hoarding and consequently lower crash risk. Considering that managers may only need to modify their disclosures in response to their *unexpected* risks identified by auditors, we model the number of risks of improper revenue recognition as a function of firm-level characteristics. Consistent with our expectation, we find the significant results attributed to the unexpected, not the expected, component of the proxy.

EARs are not equally effective for all companies. In two recent accounting scandals, Tesco's (the largest grocery retailer in the UK) and Carillion's (the second-largest construction company in the UK) share prices fell drastically after huge overstatements of their profit figures were publicly discovered, despite the underlying revenue recognition risks having already been recognized as KAMs in the EARs issued earlier.[6] Both Tesco and Carillion were large, high-profile companies. Both of their auditors were Big-4 industry specialists. We find EARs to be less effective for such firms. As shown by our cross-sectional tests, the negative effect of EARs adoption on crash risk is less pronounced for firms with richer public information, as proxied by higher analyst coverage and higher media coverage. Information about these firms' risks of material misstatement may already be available from other

[5] Risks of improper revenue recognition are the most common KAM in EARs (ICAEW, 2017). The disclosure of risks of improper revenue recognition is particularly concerned about revenue overstatements. Stock investors are sensitive to revenue information. Changes in corporate disclosures regarding revenue recognition risks are most likely to have valuation implications.

[6] On September 22, 2014, Tesco plc revealed that it had overstated half-year profits by approximately £250 million, principally due to accelerated recognition of commercial income and delayed accrual of costs. Tesco's share price plunged 11.5% on a single day, although the auditor's report issued four months earlier by PwC had identified two KAMs relating to Tesco's revenue recognition risks. Before going into liquidation in 2018, Carillion had taken on many risky but unprofitable contracts and had been too aggressive in recognizing contractual revenues. On July 10, 2017, Carillion had to issue a profit warning following a write-off of £845 million from the value of contracts. Its share price crashed by 39%. Carillion's external auditor, KPMG, however, had acknowledged these revenue recognition risks as KAMs in the auditor's report issued four months earlier.

information sources. By contrast, in firms with scant public information, the pre-emptive corporate disclosures about these risks ahead of EARs are more incrementally informative to investors and more influential in alleviating bad-news hoarding. In addition, we show that the negative effect of EARs adoption on crash risk is weaker for firms with Big-4 or industry-specialist auditors. Firms employing these auditors are often associated with a richer information environment, in which EARs turn out to be less helpful in curtailing crash risk.

In the main tests, we argue that the mandatory adoption of EARs prompts timelier disclosures of negative information by managers, which in turn dampens bad-news hoarding and reduces crash risk. The hypothesized mechanism involves two implicit assumptions. First, prior to the adoption of EARs, managers disclose bad news in a lump-sum fashion. After the adoption, managers disclose bad news more frequently but in smaller doses. To provide more direct evidence on this assumption, we examine linguistic tone embedded in corporate disclosures. We measure the tone in a disclosure as the difference between the number of positive and negative words scaled by their sum. We show that the tone of corporate disclosures becomes less negatively skewed after the adoption of EARs, suggesting the unwinding of managers' bad-news hoarding. The second assumption posits that other determinants of crash risk do not change after EARs adoption. It is well established in crash risk literature that accruals management is a major device used by managers to disguise bad news. Two prior studies (Gutierrez et al., 2018; Reid et al., 2019) have examined the effect of EARs adoption on accruals management but found mixed evidence. We re-examine this relation using our sample and DiD set-up and find that EARs adoption does not significantly affect accruals management.

Mounting evidence has demonstrated the role of crash risk in asset pricing. Stocks with high crash risk tend to generate high future returns in order to entice investors to hold them (Ang, Chen, & Xing, 2006; Conrad, Dittmar, & Ghysels, 2013; Chabi-Yo, Ruenzi, & Weigert, 2018). Motivated by the idea that timelier bad-news disclosure following EARs adoption helps improve market efficiency, using a DiD approach, we show that the return premium of bearing crash risk diminishes after the adoption of EARs.

Our contribution is twofold. First, we contribute to the emerging literature on the economic consequences of EARs. The prior literature has examined *short-term* market reactions to the public disclosure of EARs (Gutierrez et al., 2018; Lennox et al., 2022) and the effects of the adoption of EARs on financial reporting quality and audit fees (Gutierrez et al., 2018; Reid et al., 2019). Although the relation between EARs adoption and financial reporting quality remains ambiguous (Gutierrez et al., 2018; Reid et al., 2019), previous studies generally suggest that EARs *per se* communicate little incremental information to investors upon public release (Gutierrez et al., 2018; Lennox et al., 2022) and that the adoption of EARs has little impact on audit fees (Gutierrez et al., 2018; Reid et al., 2019). No archival study has yet examined the effect of EARs adoption on corporate disclosures. While in the present audit and assurance framework auditors are not expected to provide *original* information about audited firms directly through EARs, auditors are not precluded from encouraging firms' managers to disclose original information regarding K/CAMs prior to issuing the auditor's report. This could affect managers' asymmetric disclosure of negative information relative to positive information. Using crash risk as a market-based proxy for managers' suppression and subsequent release of negative information, as well as a textual analysis to detect negative information reported in corporate disclosures, we find evidence suggesting that managers accelerate their disclosure of negative information following the mandatory adoption of EARs.

Second, our paper adds to the crash risk literature. It has been established that crash risk is exacerbated by accruals management, which is a major tool used by managers to obfuscate adverse information (Hutton et al., 2009; Zhu, 2016). Our paper finds evidence showing that the mandatory adoption of EARs does not reduce crash risk through accruals management. Our paper reveals that auditors, as an external monitor, have a strong motivation to screen and unravel managers' bad-news hoarding when facing the requirement to issue EARs. This broadens our prior knowledge of auditors' role in price crashes, which is primarily determined by auditor characteristics, such as auditor tenure (Callen & Fang, 2017). Our paper also enriches our understanding of the role played by mandatory financial reports in crash risk formation. Ertugrul, Lei, Qiu, and Wan (2017) and Kim et al. (2019a) show that managers use complex, ambiguous annual reports to camouflage bad news, thereby yielding higher crash risk. The annual reports examined in their samples do not contain the recently introduced EARs. Distinct from other parts of annual reports prepared by corporate insiders, the EAR is filed by an external monitor and reflects auditors' intention to scrutinize risks of material misstatement and restrain insiders' bad-news hoarding.

2. Regulatory background, related literature, and hypothesis development

2.1. UK auditor reporting standard changes

In the aftermath of the 2008 global financial crisis, auditors were criticized for negligence in drawing investors' and regulators' attention to the principal risks underlying audited financial statements.[7] To overcome this criticism, the FRC introduced the EAR, recognizing it as "a response to the post 2008 financial crisis and the need to enhance confidence in financial reporting and audit" (FRC, 2016). On June 4, 2013, the FRC issued a substantive revision to ISA 700 (UK and Ireland) "The Independent Auditor's Report on Financial Statements." Paragraph 19A of the revised standard required the independent auditor's report to (1) describe the auditor's assessed risks of material misstatement that had the greatest effect on the overall audit strategy, the allocation of resources in the audit, and directing the efforts of the engagement team; (2) specify the threshold used by the auditor as materiality for the financial statements as a whole; and (3) explain how the scope of the audit addressed the assessed risks of material misstatement. The revision applied to LSE premium-listed companies for fiscal years ending on or after September 30, 2013, while other listed companies did not have to conform to this requirement.

Regulation (EU) No 537/2014 published by the EU on April 16, 2014, created a number of new requirements for statutory audits of public interest entities (i.e., listed companies, credit institutions, and insurance companies). The regulation required the auditor's report to provide a description of the most significant assessed risks of material misstatement, a summary of the auditor's response to these risks, and the auditor's key observations arising from these risks. EU member nations had to incorporate the requirements into their national standards before June 17, 2016. On January 14, 2015, the IAASB released ISA 701 "Communicating Key Audit Matters in the Independent Auditor's Report," requiring the disclosure of key audit matters for audits of listed companies for fiscal years ending on or after

[7] For example, EY, the external auditor of Lehman Brothers, was accused of approving Lehman's use of off-balance sheet devices to hide its poor financial condition in the years leading up to Lehman's collapse in 2008.

December 15, 2016. On June 17, 2016, the FRC passed a final rule, ISA (UK) 701, which applied to all listed companies in the UK, updating its 2013 rule to implement the EU 2014 regulation and incorporate the IAASB requirements. The FRC adopted the IAASB's definition of key audit matters and stipulated that risks of material misstatement as determined under ISA (UK and Ireland) 700 (2013) and those of the EU were key audit matters under the IAASB's definition (FRC, 2016). ISA (UK) 701 became effective for fiscal years ending on or after June 16, 2017.

2.2. Literature on stock price crash risk

Prior research on equity markets employs the metrics of firm-specific crash risk to capture the hiding and subsequent release of value-relevant negative information by managers. There is a degree of discretion for managers in timing the disclosure of inside information (Verrecchia, 2001). Evidence suggests that managers, on average, strategically withhold or delay the release of bad news out of concern that bad news disclosures would damage their equity wealth and/or career prospects (Kothari, Shu, & Wysocki, 2009; Jung, Naughton, Tahoun, & Wang, 2018). Consequently, bad news will accumulate until it reaches a tipping point, at which point managers cannot absorb any more negative information and have to give up. The previously withheld bad news breaks out all at once, triggering a stock price crash.

The emergence of crash risk hinges on the extent to which managers are able or willing to withhold bad news. The existing literature documents higher crash risk (1) when managers have more opportunities to disguise negative information by taking advantage of accruals management (Hutton et al., 2009; Zhu, 2016), earnings smoothing (Chen et al., 2017), accounting aggressiveness (Kim & Zhang, 2016), incomparability of financial statements with industry peers (Kim, Li, Lu, & Yu, 2016a), tax avoidance (Kim et al., 2011b), and complex annual reports (Ertugrul et al., 2017; Kim et al., 2019a); (2) when managers have greater incentives to hide bad news due to increased equity-based compensation (Kim, Li, & Zhang, 2011a), clawback provisions in compensation contracts (Bao, Fung, & Su, 2018), and product market threats from rival firms (Li & Zhan, 2019); (3) when managers are affected by behavioral traits—younger age (Andreou, Louca, & Petrou, 2016), overconfidence (Kim, Wang, & Zhang, 2016b), and risk tolerance stemming from early-life disaster experience (Chen, Fan, Yang, &

Zolotoy, 2021); and (4) when managers' bad news hoarding is not effectively monitored due to distracted institutional investors (Ni, Peng, Yin, & Zhang, 2020), short-sale constraints (Deng, Gao, & Kim, 2020), lower analyst coverage (Kim, Lu, & Yu, 2019b), shorter auditor tenure (Callen & Fang, 2017), and poorly informed boards of directors via external social networks (Fang, Pittman, & Zhao, 2021).

2.3. Literature on EARs and our hypothesis

Research has thus far focused on how equity and debt market participants react to the public disclosure of EARs and the implications of EARs for audit fees and audit quality. Studies on equity markets generally find little evidence of EARs having *short-term* market impacts upon public release. The bulk of the empirical evidence comes from the UK market and relies on the UK standard change as an identification. Gutierrez et al. (2018) show that the mandatory adoption of EARs in UK firms does not affect the three-day cumulative absolute abnormal returns and abnormal trading volume around the report release date. They also show that audit fees and audit quality as measured by absolute discretionary accruals do not significantly change after the adoption of EARs. Lennox et al. (2022) show that the short-term abnormal returns around the public release of EARs are insignificant because most risks of material misstatement disclosed in EARs had already been disclosed by the firms in prior earning announcements, conference calls, or the prior year's annual report. By contrast, Reid et al. (2019) document an improvement in financial reporting quality, as evidenced by reduced absolute discretionary accruals, reduced propensity to just meet or beat analyst forecasts, and increased earning response coefficients after the adoption of EARs. However, they find no evidence that EARs adoption alters audit fees or audit report lags. They interpret the combined results as evidence indicating that managers improve disclosures in financial statements out of fear that their auditors would negatively comment on the financial statement areas that underlie risks of material misstatement. Porumb et al. (2021) study the consequences of EARs for the private debt market and document that borrowing firms encounter lower interest rate spreads and longer loan maturities in syndicated loan contracting after adopting EARs. This is consistent with the interpretation that public information presented in EARs provides a credible source of information that assists lenders in evaluating borrowers' risks.

Evidence from the US market generally mirrors the UK evidence. Burke et al. (2022) show that the disclosure of CAMs does not affect US firms' short-term abnormal stock returns, audit fees, or audit quality. However, they detect changes in financial statement footnotes referenced by CAMs and notice that these footnotes become longer and less sticky, and contain more uncertain words, in the post-adoption period. Similarly, Gurbutt and Shih (2020) find no evidence that the disclosure of CAMs changes short-term abnormal returns, audit fees, audit hours, or audit report lags. While the requirement for disclosing CAMs may not alter auditors' overall effort (as reflected by audit fees) or audit quality, it may draw auditors' and/or managers' focus to the areas identified as CAMs and lead to changes in the quality of the underlying accounts. Consistent with this argument, Drake, Goldman, Lusch, and Schmidt (2021) document that the disclosure of tax-related CAMs is associated with a lower likelihood that the audited firm uses tax expenses as an earnings management tool to meet analyst forecasts.

In the UK and US, auditors are not expected to provide original, nonpublic information through K/CAMs. According to the UK auditing standard, "Original information is any information about the entity that has not otherwise been made publicly available by the entity […]. Such information is the responsibility of the entity's management and those charged with governance. […] It is appropriate for the auditor to seek to avoid the description of a key audit matter inappropriately providing original information about the entity. […] When such information is determined to be necessary by the auditor, the auditor may encourage management or those charged with governance to disclose additional information, rather than the auditor providing original information in the auditor's report" (ISA UK 701, 2016). The US auditing standard discusses: "The communication of critical audit matters could also heighten management's attention to the relevant areas of financial statements and related disclosures. […] the communication of critical audit matters would give auditors leverage to encourage disclosure of information by management, and that management would likely modify its disclosure in response to the communication of critical audit matters in the auditor's report so the auditor would not be a source of original information" (PCAOB AS3101, 2017).

Practitioners share similar views. In the 2020 PD Leake Lecture of the Institute of Chartered Accountants in England and Wales, a senior assurance partner from EY said, "If I think there's a matter that's important enough that I want to comment on in a KAM, but it's not disclosed in the accounts, I will have a discussion with management and say, I think you should add this disclosure to your accounts because I'm minded to address it in a KAM. And, you know, depending on your relationship with management and your ability to be able to persuade management, that's what's happened. And it's been a number of cases. In my experience where I've ended up in that situation where I believe that something should be in a KAM and I persuaded management, they need to expand the disclosures that are then original information." In another talk, a managing director from Deloitte discussed that CAM disclosures in the US enhanced corporate disclosures, saying: "Sometimes our teams will write something, and then we look at the disclosures and they don't quite have all the information, and we don't want to be the provider of original information. Starting early helps the client get prepared, look at their disclosures, determine whether or not they need any enhancements, and have a back-and-forth with the auditor" (Smith, Zietsman, Mahoney, & Ray, 2020). In June 2020, the PCAOB surveyed US audit firms with at least 15 large accelerated filer clients, revealing that "more than one-third of engagement partners (39%) reported that the issuer made changes to financial statement disclosures or other corporate reporting as a result of CAMs communicated in the auditor's report" (PCAOB, 2020).

During the year, auditors are in frequent contact with audit committees and management teams to discuss the audited firm's interim or final financial results and the audit progress. In the course of audit engagement, auditors are required to communicate the risks of material misstatement they detected in the firm's financial statements. These risks already discussed during the year with the audited firm constitute a major source of KAMs subsequently presented in the EAR (Minutti-Meza, 2021).

Since EARs typically do not contain *original* information about risks of material misstatement, if there is any original information that is deemed necessary by the auditor for disclosure in the EAR, the auditor is likely to encourage the firm's managers to disclose the information ahead of the auditor's report.[8] Because managers tend to overstate rather than understate firm performance (Graham, Harvey,

[8] We argue that changes in corporate disclosures happen *before* the public disclosure of EARs. If managers provided original information in other parts of the annual report, the previous literature should detect a significant market reaction to the release of annual reports (which contain EARs). However, Gutierrez et al. (2018) find no such evidence. Importantly, Lennox et al. (2022) show that most risks of material misstatement had already been disclosed by managers; for example, in prior earning announcements or conference calls. However, Lennox et al. (2022) provide no clues as to the long-term valuation implications of these pre-emptive corporate disclosures.

& Rajgopal, 2005; Kothari et al., 2009), the risks of material misstatement identified by the auditor in the firm's financial statements generally reflect more overstatement risks than understatement risks about the firm's performance (see Appendix A "Other revenue judgements" as an example). Thus, managers' pre-emptive disclosures about these risks of material misstatement convey more negative information than positive information to the market. This reflects the unwinding of bad-news hoarding in corporate disclosures over the year for which the EAR is issued. It is thus less likely that bad news would accumulate up to a critical point and suddenly come out all at once within the following year. Therefore, we expect a lower level of one-year-ahead crash risk. Accordingly, we formulate our hypothesis as follows:

Hypothesis H1: The mandatory adoption of extended auditor's reports reduces one-year-ahead stock price crash risk.

3. Research design and sample selection

3.1. DiD identification

The UK FRC passed the revised ISA 700 (UK and Ireland) abruptly in June 2013, only four months before applying the extended reporting framework to financial statement audits. This setting constitutes a useful exogenous shock for a DiD empirical design. Following Gutierrez et al. (2018) and Porumb et al. (2021), we compare mandatory adopters (i.e., LSE premium-listed stocks) and non-adopters (i.e., LSE standard-listed stocks and AIM stocks) before and after the standard change. AIM is a segment of the LSE intended to assist small- and medium-sized growth-oriented companies in issuing public equity and building liquidity. AIM is designed to use customized regulation administered through the private sector to provide oversight comparable to traditional stock exchanges but with greater flexibility and lower costs than traditional exchanges. Gerakos, Lang, and Maffett (2013) were among the first to benchmark the long-term returns of AIM stocks against main market stock returns. They show that the AIM regulatory structure turns out to be less effective than traditionally regulated exchanges, although the objective of the AIM regulation is "not less oversight or weaker regulation, but less costly, customized, 'light touch' regulation overseen by the private sector" (Piotroski, 2013, p. 217).

Using a DiD empirical design creates two benefits for our study. First, the DiD design will difference out any permanent difference in the level of regulatory oversight and investor protection between the AIM and LSE main markets. The addition of time-varying covariates and firm fixed effects further alleviates the selection bias concern regarding firms' stock-listing choices. Second, the DiD design will remove biases associated with common time trends or concurrent events that affect the outcome of both the treatment and control groups. One concurrent confounding event is the Companies Act 2006 (Strategic Report and Directors' Report) Regulations 2013, which requires all listed companies to disclose a strategic report in their annual report for fiscal years ending on or after September 30, 2013. The strategic report describes the principal risks and uncertainties facing the company. The DiD design can control for the distortion introduced by the strategic report disclosure.

AIM is governed by the AIM Rules set out by the LSE and tailored to the needs of growth companies. We consider contemporaneous changes in the AIM regulatory environment by searching for any amendments made to the AIM Rules over our sample period. We identify one significant rule change relating to AIM stocks' information disclosure. The amended AIM Rule 26, effective as of August 11, 2014, requires an AIM company to publish on its website details of which corporate governance code it has decided to apply and information on how it complies with the code, or, if the company has decided not to adopt a code, this should be stated together with its current corporate governance arrangements. The increased disclosure about corporate governance compliance may attenuate crash risk, as existing evidence suggests that better corporate governance is associated with lower crash risk (Fang et al., 2021). If our DiD empirical results report a decrease (an increase) in mandatory adopters' crash risk from the pre- to post-adoption period relative to non-adopters, ignoring the tightening of the AIM rule will underestimate (overestimate) the magnitude of the treatment effect.

With these points in mind, we specify the DiD regression model as follows:

$$(1) \quad CrashRisk_{i,t+1} = \beta_0 + \beta_1 Mandatory\ Adopter_i \times Post_{i,t} + \beta_2 Post_{i,t} + \sum \beta_j Control\ Variables_{i,t} + Firm\ Fixed\ Effects + Year\ Fixed\ Effects + \varepsilon_{i,t+1}$$

where i indexes firm and t indexes fiscal year. *CrashRisk* denotes firm-specific crash risk. *Mandatory Adopter* is coded as one for mandatory adopters and zero for benchmark non-adopters. *Post* is set equal to one for fiscal years ending on or after September 30, 2013, and zero otherwise. Specifically, if a firm has a post-September fiscal year-end, the (pseudo) mandatory adoption year is 2013; if its fiscal year ends in or before September, the (pseudo) mandatory adoption year is 2014.[9] The DiD estimator (β_1) captures the incremental change in mandatory adopters' future crash risk surrounding the mandatory adoption of EARs relative to benchmark non-adopters. We include firm fixed effects to control for time-invariant firm-level unobservables. Accordingly, the main effect of *Mandatory Adopter* is subsumed by firm fixed effects. Year fixed effects control for common time-series variation in the dependent variable. We adjust standard errors for heteroscedasticity and clustering at the firm level to account for within-firm serial correlations in residuals (Petersen, 2009).

We use an array of time-varying firm-level control variables. In line with the prior literature on crash risk (Kim et al., 2011a, b), we control for the mean (*RETURN*) and standard deviation (*SIGMA*) of firm-specific weekly returns. We control for detrended turnover (*TURNOVER*) to capture liquidity effects and investor attention. *TURNOVER* is calculated as the change in the average monthly share turnover from the previous year to the current year, times 100, where monthly turnover is computed as the total number of shares traded over the month scaled by the number of shares outstanding at the end of the month. We control for earnings quality as proxied by the signed value of discretionary accruals (*ACCRUAL*) estimated using the modified Jones model (Dechow, Sloan, & Sweeney, 1995). We also control for financial leverage (*LEVERAGE*), market-to-book ratio (*MTB*), return on assets (*ROA*), firm size (*SIZE*), analyst coverage (*ANALYST*), and auditor characteristics, as proxied by the presence of a Big-4 auditor (*BIG4*) and auditor tenure (*TENURE*). All control variables are winsorized at the 0.5% and 99.5% levels. Variable definitions are summarized in Appendix B.

3.2. Measures of firm-specific stock price crash risk

[9] For example, Xaar plc's 2013 fiscal year ended on December 31, 2013. The firm had to include an EAR in its 2013 annual report. By comparison, Anglesey Mining plc's 2013 fiscal year ended on March 31, 2013. The firm was required to disclose an EAR in its 2014 annual report.

To isolate firm-specific crash risk from concurrent market-wide tail events, we calculate firm-specific stock returns—that is, returns driven by firm-specific idiosyncratic factors. Following Jin and Myers (2006) and DeFond, Hung, Li, and Li (2014), we specify an expanded market model that controls for UK market index returns, US market index returns (representing the global market return), and foreign exchange rates. For each firm-year, we estimate a time-series regression:

$$\text{(2)} \qquad r_{i,t} = \alpha_i + \beta_{1,i} r_{UK,t} + \beta_{2,i}\left[r_{US,t} + EX_t\right] + \beta_{3,i} r_{UK,t-1} + \beta_{4,i}\left[r_{US,t-1} + EX_{t-1}\right] + \beta_{5,i} r_{UK,t-2} + \beta_{6,i}\left[r_{US,t-2} + EX_{t-2}\right] + \beta_{7,i} r_{UK,t+1} + \beta_{8,i}\left[r_{US,t+1} + EX_{t+1}\right] + \beta_{9,i} r_{UK,t+2} + \beta_{10,i}\left[r_{US,t+2} + EX_{t+2}\right] + e_{i,t}$$

where $r_{i,t}$ is the return on stock *i* in week *t*, $r_{UK,t}$ is the return on the Datastream UK market return index in week *t*, $r_{US,t}$ is the return on the Datastream US market return index in week *t*, and EX_t is the change in the exchange rate of the pound sterling versus the US dollar in week *t*. We correct for non-synchronous trading by including two lead and two lag terms for both the UK and US market returns (Dimson, 1979). The residual return ($e_{i,t}$) is the return that is not explained by market return variations and, hence, is driven by changes in firm-specific idiosyncratic factors. Given that the residual returns are highly skewed (Hutton et al., 2009), we log-transform the residual returns and estimate the firm-specific weekly return ($W_{i,t}$) as the natural logarithm of one plus the residual return, i.e., $W_{i,t} = log\ (1 + e_{i,t})$.

The inclusion of firm fixed effects and the short time period in the DiD model lead us to choose two continuous crash risk measures that are able to exhibit sufficient time-series variation within a firm. The first variable, *NCSKEW*, is the negative skewness of firm-specific weekly returns in a year (Chen et al., 2001). *NCSKEW* quantifies the degree of asymmetry in the distribution of firm-specific stock returns. *NCSKEW* is calculated as the third moment of firm-specific weekly returns scaled by the cubed standard deviation of firm-specific weekly returns, times -1, as shown below:

$$\text{(3)} \qquad NCSKEW_{i,t} = -\frac{n(n-1)^{\frac{3}{2}} \sum_{\tau=1}^{n} (W_{i,t,\tau} - \bar{W}_{i,t})^3}{(n-1)(n-2)\left[\sum_{\tau=1}^{n} (W_{i,t,\tau} - \bar{W}_{i,t})^2\right]^{\frac{3}{2}}}$$

where $\bar{W}_{i,t}$ is the mean of firm-specific weekly returns on stock *i* in year *t*, and *n* is the number of trading weeks on stock *i* in year *t*.

The second crash risk variable, *DUVOL,* is the down-to-up volatility ratio of firm-specific weekly returns in a year (Chen et al., 2001). It measures the asymmetry in volatilities between negative and positive stock returns. Within each firm-year, we classify weeks with firm-specific weekly returns below (above) the annual mean as down (up) weeks. We calculate *DUVOL* as the natural logarithm of the standard deviation of firm-specific weekly returns in down weeks scaled by the standard deviation of firm-specific weekly returns in up weeks, as follows:

$$(4) \qquad DUVOL_{i,t} = log\left[\frac{(n_u - 1)\sum_{\tau=1}^{n_d}(W_{i_d,t,\tau} - \overline{W}_{i_d,t})^2}{(n_d - 1)\sum_{\tau=1}^{n_u}(W_{i_u,t,\tau} - \overline{W}_{i_u,t})^2}\right]$$

where $W_{i_d,t}$ ($W_{i_u,t}$) is the firm-specific weekly return in a down (an up) week on stock *i* in year *t*, and n_d (n_u) is the number of down (up) weeks on stock *i* in year *t*.

To summarize, a higher value of *NCSKEW* or *DUVOL* signifies a higher degree of firm-specific crash risk.

3.3. Sample construction and descriptive statistics

We collect a list of UK-incorporated firms with a primary share listing on the LSE from 2010 to 2016.[10] We remove utility firms (ICB code 7530–7570), financial firms (ICB code 8350–8990), and firms with missing financial account data in Worldscope. We gather each stock's LSE market affiliation (i.e., main market or AIM) and listing category (i.e., premium or standard listing) from the LSE website. We collect auditor's reports from Audit Analytics Europe and corporate websites. In a similar vein to IFRS-centric studies (e.g., Kim, Liu, & Zheng, 2012), we remove voluntary adopters (i.e., LSE standard-listed firms or AIM firms that adopted EARs on a voluntary basis), late mandatory adopters (i.e., LSE premium-listed firms that adopted EARs later than the mandatory adoption year), early mandatory adopters (i.e., LSE premium-listed firms that adopted EARs prior to the mandatory adoption year), and premium-listed non-adopters (i.e., firms that violated this requirement).[11]

[10] Our sample period ends in 2016, because all public companies were required to adopt EARs from 2017, precluding a DiD analysis since then.

[11] The intention of excluding these irregular firms is to avoid self-selection bias, but our finding still holds if these firms are added back to the sample.

To derive firm-specific crash risk, we collect from Datastream each firm's total return index (mnemonic: *RI*)—a stock price index adjusted for stock splits and dividend payout. Consistent with Kim et al. (2011a, b), we use weekly stock returns instead of daily stock returns (1) to overcome thin trading problems in AIM stocks; and (2) to appropriately capture real crash events, because daily stock returns are very noisy and a significant negative return on a particular day may reverse within the following few days. To mitigate the distortion associated with major corporate events, such as initial public offerings, prolonged trading suspension, and delisting, we require at least 26 non-missing weekly stock returns available in a firm-year to estimate annual crash risk.

After combining the data required for the baseline DiD regression, we obtain a sample of 1,090 firms (5,290 firm-years), which consists of 399 mandatory adopters (2,200 firm-years) and 691 non-adopters (3,090 firm-years). The non-adopter sample is predominated by AIM stocks, including 685 AIM stocks (3,062 firm-years) and 6 LSE standard-listed stocks (28 firm-years). Panel A in Table 1 reports the distribution of mandatory adopters and non-adopters in each sample year. Among mandatory adopters, 180 firms began issuing EARs in 2013, while 130 began issuing EARs in 2014.

<Insert Table 1 about here>

Panel B in Table 1 presents the summary statistics of crash risk and control variables. In the DiD sample, *NCSKEW* and *DUVOL* have mean values of -0.167 and -0.190, respectively, comparable to those reported for US public firms (e.g., Callen & Fang, 2017). We find that 59.5% of the sample firms are audited by Big-4 auditors. The average auditor tenure before log-transformation is approximately 6.57 years. We compare our descriptive statistics of mandatory adopters and non-adopters with the statistics reported by Gerakos et al. (2013). Both studies show that AIM stocks, composing the majority of our non-adopter sample, have lower stock returns (*RETURN*), lower leverage (*LEVERAGE*), lower market-to-book (*MTB*), and smaller market capitalization (*SIZE*) than stocks listed on traditional exchanges. Our study also shows that AIM stocks exhibit higher stock return volatility (*SIGMA*) than LSE premium-listed stocks, consistent with the argument that AIM attracts high-risk stocks (Piotroski, 2013). AIM stocks report lower *NCSKEW* and *DUVOL* than LSE premium-listed stocks. This is not perplexing, because AIM hosts smaller stocks and the returns of small stocks are more *positively* skewed than the returns of large stocks (Chen et al., 2001). In other words, AIM stocks experience many large negative returns and a few extreme positive returns.[12] The permanent difference between the stock return pattern of mandatory adopters and non-adopters will be differenced out in the DiD empirical design.

The success of a DiD strategy hinges critically on the validity of the parallel trends assumption. That is, although the treatment and control groups may have different levels of outcome before treatment onset, their trends in the pretreatment outcome should be the same. The expectation is that, absent treatment, the outcome of the two groups will change at the same rate. To assess the plausibility of this assumption, Panel C in Table 1 reports the yearly change in crash risk (*ΔNCSKEW* and *ΔDUVOL*) over a five-year period prior to the (pseudo) mandatory adoption (i.e., year 0) for mandatory adopters and non-adopters. The means of yearly crash risk changes are statistically indistinguishable between the two groups of firms from year -4 to year 0 (i.e., our pretreatment sample period),[13] lending credence to the parallel trends assumption. The difference in means turns to be statistically significant from year -5 (roughly calendar year 2008) to year -4 (roughly 2009), which is, though, out of our pretreatment sample period. As the financial crisis gradually subsided from 2008 to 2009, AIM stocks appear to have experienced a larger decline in crash risk than LSE premium-listed stocks. Further, to augment the parallel trends analysis, we calculate monthly crash risk using daily data and plot the means of monthly *NCSKEW* and *DUVOL* for mandatory adopters and non-adopters across event years in the figures presented in Online Appendix C. The monthly crash risk of AIM stocks grew higher than that of LSE premium-listed stocks during year -5 (roughly calendar year 2008). While the financial market turmoil in 2008 seems to jeopardize the parallel trends assumption, the assumption generally holds from year -4 to year 0.

4. Empirical results

4.1. Baseline regression results

[12] AIM accommodates a few growth-oriented stocks, offering the potential for extreme positive payoffs.

[13] The starting year of our pretreatment sample period is 2010, which is four years prior to the (pseudo) mandatory adoption in 2014 for firms with pre-September fiscal year-ends.

In Table 2, columns 1 and 2 examine mandatory adopters only and test the time-series difference in crash risk from the pre-adoption to post-adoption period for mandatory adopters.[14] The coefficient of the *Post* dummy is negative and statistically significant, reflecting a pre-post reduction in crash risk among mandatory adopters. The results from the single-difference model may be tainted by time trends or concurrent events other than the treatment. To dispel this concern, columns 3 and 4 present the DiD regression results using non-adopters as a control group. We report a negative and significant coefficient on the DiD estimator (*Mandatory Adopter*×*Post*). This result is based on firm fixed effects, indicating that, within any given firm, adopting an EAR is on average associated with a crash risk reduction relative to the contemporaneous change among non-adopters.[15] The coefficient of the *Post* dummy in columns 3 and 4 is positive and significant, suggesting an increase in non-adopters' crash risk in the post-period. One interpretation is that AIM companies believed that they were exempted from EARs adoption because of the weak regulatory environment of AIM, and thus engaged more in bad-news hoarding.

<Insert Table 2 about here>

The treatment effect is economically sizeable. In column 3 (4), the coefficient of the DiD estimator can be interpreted as mandatory adopters on average experiencing a decline of 0.190 (0.151) in *NCSKEW* (*DUVOL*) in the post-adoption period relative to the pre-adoption period compared with non-adopters, which represents a decrease of 18.13% (17.20%) of the one standard deviation of *NCSKEW* (*DUVOL*) from the pre- to post-adoption period.[16] Regarding the control variables, we document a positive and significant coefficient on the signed value of discretionary accruals (*ACCRUAL*), consistent with the implication in the accruals literature (Dechow et al., 1995; Xie, 2001)

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[14] The omission of year fixed effects in the time-series difference model in columns 1 and 2 in Table 2 is to avoid the problem of multicollinearity between the *Post* dummy and year dummies.

[15] Pooled OLS, or similar techniques such as univariate tests, do not distinguish between- and within-firm effects and may lead to misleading inferences. Between firms, there is higher crash risk among larger firms, which are more likely to be premium-listed on the LSE main market and consequently adopt EARs. This between-firm crash risk exacerbation may offset the within-firm crash risk reduction due to the treatment and obscure our inference.

[16] 18.13% = 0.190/1.048, where 0.190 is the absolute value of the coefficient on the DiD estimator in column 3 in Table 2, and 1.048 is the standard deviation of *NCSKEW* in the pre-adoption period. 17.20% = 0.151/0.878, where 0.151 is the absolute value of the coefficient on the DiD estimator in column 4, and 0.878 is the standard deviation of *DUVOL* in the pre-adoption period.

that firms with positive (i.e., income-increasing) discretionary accruals are associated with more hidden bad news than firms with negative (i.e., income-decreasing) discretionary accruals. Crash risk is positively related to firm size (*SIZE*), supporting the previous evidence that larger firms are more prone to stock price crashes (Kim et al., 2011b). The R^2 values are of similar magnitude to those reported by Kim et al. (2019a) for predicting crash risk for US firms. Crash risk regressions tend to produce low R^2 because crash risk measures are derived from firm-specific stock returns, which contain noisy, firm-specific information.

4.2. Sensitivity checks

4.2.1. Dynamic DiD regressions to validate the parallel trends assumption

To provide further assurance for the parallel trends assumption, we decompose the DiD estimator in the baseline regression model into the separate interaction terms between *Mandatory Adopter* and the different event year dummies $Before_{\leq\text{-}3}$, $Before_{\text{-}2}$, $Current_0$, $After_{+1}$, $After_{+2}$, and $After_{+3}$, representing year -4/-3,[17] year -2, year 0, year +1, year +2, and year +3, respectively, where year 0 is the (pseudo) mandatory adoption year. The omitted benchmark year is the year immediately preceding mandatory adoption (i.e., year -1). The dynamic DiD regression results are presented in Table 3. The treatment effects in the pre-adoption period, as proxied by the coefficients on $Mandatory\ Adopter \times Before_{\leq\text{-}3}$ and $Mandatory\ Adopter \times Before_{\text{-}2}$, are close to zero and statistically insignificant, suggesting that treatment and control firms follow similar pretreatment trends in crash risk, justifying the parallel trends assumption.

<Insert Table 3 about here>

The dynamic treatment effect analysis provides additional insights into the timing with which the mandatory adoption of EARs affects crash risk. We find that the coefficient of the interaction term $Mandatory\ Adopter \times Current_0$ is small and statistically insignificant. One explanation is that the final rule ISA 700 (UK and Ireland) appeared suddenly in June 2013, while auditors had to begin issuing EARs from September 2013. This left managers in audited firms little time in year 0 to pre-empt bad-

[17] We treat the starting year of the sample period for firms with different fiscal year-ends as a single dummy and code $Before_{\leq\text{-}3}$ as one for four or three years before treatment onset.

news disclosures. Consequently, crash risk had a delayed response to EARs adoption. Indeed, a significant treatment effect manifests one year after mandatory adoption, as reflected by the negative and significant coefficient on $Mandatory\ Adopter \times After_{+1}$. However, in the subsequent two years ($After_{+2}$ and $After_{+3}$), the significant treatment effect almost disappears. The diminished treatment impact could be due to many risks of material misstatement disclosed in year +3 and year +2 having already been disclosed in the prior year's audit report,[18] which causes EARs to play a lesser role in disciplining managers to pre-empt bad news.

4.2.2. Selection bias tests

We use the following tests to address the selection bias associated with differences in firm-level characteristics between the treatment and control groups.

(a) *Heckman selection model*. As EARs adoption was enforced for LSE premium-listed firms, the observed treatment effect might be endogenously determined by a firm's choice of listing in the premium market or AIM. We correct for this Heckman-type selection bias by using a treatment effect model with a maximum likelihood estimator (Heckman, 1978; Maddala, 1986). The first-stage probit regression models the probability of a firm being a mandatory adopter by employing an instrumental variable that equals one if a firm is incorporated before June 19, 1995 (i.e., the AIM launch date), and zero otherwise.[19] We expect that a firm incorporated before the establishment of AIM is less likely to be listed on AIM but more likely to be listed on the main market, and thus is more likely to enter our treatment sample. The second-stage regression incorporates *lambda*, which contains information from the first stage to control for unobserved factors relating to the treatment. In Panel A in Table 4, columns 1 and 3 show that firms incorporated before the launch of AIM are more likely to be premium-listed on the LSE main market. Columns 2 and 4 report negative coefficients on the DiD estimator, consistent

[18] In our sample, 80% of RMMs disclosed in year +3 had already been disclosed as RMMs in the auditor's report in year +2, 84% of the RMMs disclosed in year +2 had already been disclosed as RMMs in the auditor's report in year +1, and 74% of the RMMs disclosed in year +1 had already been disclosed as RMMs in the auditor's report in year 0 (i.e., the mandatory adoption year).

[19] The instrumental variable is time-invariant within each firm. This precludes the inclusion of firm fixed effects in the first-stage regression.

with our previous results. The coefficient of *lambda* is significant and negatively signed, suggesting that the unobserved factors that make premium listing more likely are associated with lower crash risk.

<Insert Table 4 about here>

(b) *Placebo shock in the pretreatment period*. In Panel B in Table 4, we apply a placebo treatment before the occurrence of the actual treatment in 2013. We set the placebo sample period to 2009–2012 to avoid the distortion of extreme market turbulence during the 2007–2008 global financial crisis. *Post* is coded as one for fiscal years ending on or after September 30, 2011 (i.e., two years before the actual treatment), and zero otherwise. If our previous results are driven by selection bias, then the bias is likely to pre-exist the actual treatment and yield a significant result on the fake DiD estimator. However, we do not find the fake DiD estimator to have a significant effect on crash risk.

(c) *Placebo tests on the first and second moments of stock returns*. Our primary crash risk measure *NCSKEW* reflects the third moment of firm-specific stock returns. If our conclusion was ascribed to our use of return-based crash risk as an indicator of bad-news hoarding, we might find that EARs adoption affected other moments of stock returns. In Panel C in Table 4, we conduct placebo tests to examine how the first and second moments of stock returns change around the mandatory adoption of EARs. The dependent variables *RETURN* and *VARIANCE* measure the mean (i.e., first moment) and variance (i.e., second moment) of firm-specific weekly returns in a year. Columns 1 and 2 report a significant reduction (increment) in *RETURN* (*VARIANCE*) from the pre- to post-adoption period for mandatory adopters. However, when AIM firms are used as a control group in columns 3 and 4, we find the coefficients of the DiD estimator statistically insignificant, suggesting that the treatment event does not change the first or second moment of stock returns for mandatory adopters relative to non-adopters.

(d) *Mandatory adopters and non-adopters matched on both pre-adoption and post-adoption mean stock returns*. Crash risk or return skewness is not determined only by managers' disclosure choices but also by the underlying information flow. To minimize the difference in underlying information flow, we use stock returns as a market-based proxy for the flow of information and match treatment and control firms on both pre-adoption and post-adoption mean stock returns. Specifically, we design a propensity score matching strategy that uses the means of firm-specific weekly returns in both the pre-adoption and post-adoption periods as predictors in the first-stage probit model to estimate each firm's propensity to be treated. Then we apply a one-to-one nearest neighbor matching algorithm without replacement and require matched pairs to have an identical (pseudo) mandatory adoption year (i.e., either 2013 or 2014). These steps yield 176 matched pairs, which constitute 57.70% of mandatory adopters with non-missing observations in the mandatory adoption year. After matching, we verify that the matched pairs do not exhibit significantly different mean stock returns in the pre- or post-adoption period. The results from this matched sample are reported in Panel D in Table 4 and are consistent with our main results.

(e) *Mandatory adopters and non-adopters matched on firm size and growth potential*. Since AIM is intended to attract small- and medium-sized growth companies, we match mandatory adopters and non-adopters based on firm size measured by stock market capitalization and firm growth opportunities proxied by market-to-book ratio. The matching is performed in the year immediately preceding mandatory adoption following the criteria: (1) matched pairs have an identical (pseudo) mandatory adoption year; (2) the percentage difference in stock market capitalization between matched pairs does not exceed 30%; and (3) for matched pairs meeting the above criteria, we choose the control firm that has the closest market-to-book ratio to the treatment firm. We match 206 mandatory adopters to non-adopters. Our conclusion continues to hold, as shown in Panel E in Table 4.

(f) *Mandatory adopters and non-adopters matched on propensity scores*. To further enhance the pretreatment covariate balance between the two groups of firms on *observed* firm-level characteristics, we estimate each firm's propensity to be treated using a probit model that controls for (1) the three-year pretreatment changes in crash risk variables (*ΔNCSKEW* and *ΔDUVOL*), to help satisfy the parallel trends assumption, and (2) all control variables in the DiD regression model measured in the year immediately preceding (pseudo) mandatory adoption. Based on the propensity scores, we perform one-to-one nearest neighbor matching without replacement and require matched pairs to have an identical (pseudo) mandatory adoption year (i.e., either 2013 or 2014). We match 161 mandatory adopters to comparable non-adopters. As reported in Panel F in Table 4, our conclusion survives this sample.

(g) *UK mandatory adopters matched to comparable EU firms*. During the sample period, EARs had not yet been implemented in other European nations. This allows us to match UK mandatory adopters to EU firms in an effort to control for Europe-wide shifts in regulation or legislation applied to stocks listed on traditionally regulated exchanges. We identify a sample of 1,550 EU firms without missing observations for our analysis to construct the matched sample. We apply the same one-to-one nearest neighbor propensity score matching procedures as specified in paragraph (f) and successfully match 173 UK mandatory adopters to comparable EU firms. As shown in Panel G in Table 4, our finding is unchanged.

(h) *UK mandatory adopters matched to comparable US firms*. During the sample period, US firms had been using the traditional boilerplate auditor's report. Using US firms as an alternative control group can alleviate the concern that contemporaneous global changes beyond European markets may confound our results. In addition, because UK firms' stock returns are likely to covary more with US firms (representing global dominant players) than with EU firms, the crash risk variation in UK firms could be more comparable to the variation in US firms, facilitating the satisfaction of the parallel trends assumption. Compared with AIM firms, we can gather a larger sample from US major stock exchanges as control firms for propensity score matching, and the US firms are more similar to LSE premium-listed firms in terms of firm size. Nonetheless, propensity score matching based on observed firm-level characteristics cannot eliminate all unobserved firm-level differences or cross-country differences between the UK and the US. With these caveats in mind, we identify a sample of 1,537 US firms with non-missing observations for our analysis to construct the matched sample. We repeat the one-to-one nearest neighbor propensity score matching procedures described in paragraph (f) and successfully match 301 UK mandatory adopters to comparable US firms. As shown in Panel H in Table 4, our main finding still holds.

4.2.3. Other robustness tests

(a) *Control for other determinants of crash risk*. Our baseline regression model controls for a standard set of variables commonly applied in crash risk literature (e.g., Kim et al., 2011a, b). To reduce omitted variable bias, we now include additional control variables that may be correlated with both the adoption of EARs and crash risk. Prior studies show that crash risk is affected by earnings smoothing (Chen et al., 2017), accounting conservatism (Kim & Zhang, 2016), financial statement comparability (Kim et al., 2016a), tax avoidance (Kim et al., 2011b), and 10-K report file size (Ertugrul et al., 2017). Since independent audits are a crucial disciplinary device for financial reporting, the adoption of EARs may have implications for the above financial reporting attributes. The absence of these control variables in our model may plague our inference. We calculate these variables as defined in Appendix B and add them as additional controls in Panel A of Table 5. Our main finding still holds. The coefficient on the DiD estimator in column 3 (4) reflects an average decrease of 20.77% (20.69%) of the one standard deviation of *NCSKEW* (*DUVOL*) among mandatory adopters from the pre- to post-adoption period, relative to the concurrent change among non-adopters. In addition, we report a negative effect of financial statement comparability on crash risk for mandatory adopters, consistent with the argument that improved financial statement comparability with industry peers discourages managers' bad-news hoarding (Kim et al., 2016a).

<Insert Table 5 about here>

(b) *Control for audit firm fixed effects and audit partner fixed effects*. To control for unobserved time-invariant audit firm characteristics and audit partner characteristics, we augment the baseline regression model with the addition of audit firm fixed effects and audit partner fixed effects (based on the name of the lead engagement partner). As shown in Panel B in Table 5, our inference is unaffected.

(c) *Use FTSE All-Share index returns and AIM All-Share index returns as proxies for UK market returns*. Despite the high correlation between the FTSE index and AIM index returns (correlation coefficient = 0.75), the subtle difference in time trends between the two submarkets may distort our analysis. Bearing this in mind, we replace the UK aggregate market return in equation (2) with the FTSE All-Share index return for main market-listed stocks and the AIM All-Share index return for AIM stocks. We re-estimate firm-specific weekly stock returns, which are then taken to recalculate *NCSKEW*, *DUVOL*, *RETURN*, and *SIGMA*. As reported in Panel C in Table 5, our results remain robust.

(d) *Use daily data to estimate crash risk measures*. We re-run the expanded market model specification in equation (2) using daily individual stock returns, daily market index returns, and daily pound sterling exchange rates. Using the estimated firm-specific daily stock returns, we recalculate the crash risk variables (*NCSKEW* and *DUVOL*) and the mean (*RETURN*) and standard deviation (*SIGMA*) of firm-specific daily stock returns in a year. The regression results based on these variables are presented in Panel D in Table 5 and are consistent with our main results.

(e) *Other univariate, bivariate, and multivariate crash risk measures*. In addition to the crash risk measures applied in our main model, we employ two univariate crash risk measures. *Count* is the number of actual crash weeks less the number of actual jump weeks over a year (Jin & Myers, 2006; Callen & Fang, 2015). Because actual crashes are rare events, *Count* has small time-series variation within a firm. To exploit more time-series variation, we use *COLLAR*, which gauges both the frequency and severity of actual crashes and jumps (Jin & Myers, 2006). Specifically, *COLLAR* is defined as the average payoff from a strategy of buying a put option and shorting a call option on firm-specific weekly returns over a year, times 1000, where the strike price of the put (call) is set to the mean of firm-specific weekly returns minus (plus) 3.09 standard deviations of the returns. The literature in asset pricing has developed measures of bivariate and multivariate crash risk. The bivariate measure, *LTD*, determines the crash sensitivity of a stock by its lower-tail dependence with the market (Chabi-Yo et al., 2018). Specifically, *LTD* assesses the extreme dependence between individual stock returns and market returns in the lower-left tail of their joint distribution. The multivariate crash risk measure, *MCRASH*, captures a stock's sensitivity to extreme downside realizations of multiple risk factors in an asset pricing model (Chabi-Yo, Huggenberger, & Weigert, 2021). Essentially, *MCRASH* is a stock's conditional probability to realize a left-tail event given that at least one of the risk factors realizes a left-tail event simultaneously. Appendix B describes the procedures in calculating each of the above crash risk measures. In short, higher values of *COUNT*, *COLLAR*, *LTD*, and *MCRASH* reflect higher levels of crash risk. As shown in Panel E in Table 5, all these crash risk measures are mitigated by the mandatory adoption of EARs.

(f) *Delete observations in fiscal year 2013*. Mandatory adopters with post-September fiscal year-ends began issuing EARs in fiscal year 2013, whereas those with pre-September fiscal year-ends had not adopted EARs until fiscal year 2014. To mitigate any temporary transitional effect, we drop observations in fiscal year 2013 from the sample. The regression results reported in Panel F in Table 5 support our conclusion.

(g) *Delete firms that changed their auditors during the (pseudo) mandatory adoption year*. In our sample, 44 firms replaced their auditors during the (pseudo) mandatory adoption year. The unobserved factors that influenced firms' decision to replace their auditors might be correlated with the new requirement to adopt EARs and contaminate our analysis. To circumvent this problem, we drop 44 firms that changed their auditors during the (pseudo) mandatory adoption year. The regression results reported in Panel G in Table 5 corroborate our conclusion.

4.3. Risks of material misstatement in revenue recognition

One of our key arguments posited in the paper is that the disclosure of risks of material misstatement (RMMs) in EARs forces managers to pre-empt bad news in corporate disclosures ahead of the auditor's report, which in turn reduces future crash risk. Admittedly, some RMMs (e.g., fair value determination) involve uncertain, subjective, or complex estimates or assumptions that are inherently difficult to audit. These RMMs may not be thought of as being related to positive or negative information. For this reason, we focus on a specific type of RMM—risks of improper revenue recognition. Revenue is the largest and most often manipulated component of earnings (Stubben, 2010; Dechow, Ge, Larson, & Sloan, 2011). Revenue is more informative for firm valuation than expenses (Ertimur, Livnat, & Martikainen, 2003; Chandra & Ro, 2008). The disclosure of risks of improper revenue recognition is particularly concerned about revenue overstatements (reflecting negative information) and, therefore, most relevant to the bad-news hoarding story of crash risk.

RMMs in revenue recognition cover the following topics as classified by Audit Analytics: (1) revenue and other income, (2) revenue recognition, (3) revenue from customer contracts, and (4) sales returns and allowances. In disclosing these RMMs, the disclosure often says that the timing and valuation of revenue recognized is subject to management bias that would lead to an overstatement, or that recognizing contract revenue in proportion to the stage of completion of the contract or estimating appropriate accruals for sales returns or allowances is at risk of management discretion or manipulation. These risks' forthcoming disclosure in EARs will spotlight the underlying bad-news hoarding by managers. To avoid any potential adverse consequences, managers would rather pre-empt the negative information in their own disclosures.

We restrict our sample to mandatory adopters and perform a pre-post test that takes into account the cross-sectional variation in the number of improper-revenue-recognition RMMs. We code $HIGH_{\#\ of\ revenue\ RMMs}$ as one if the average number of improper-revenue-recognition RMMs within a firm's EARs in the post-adoption period is above the sample median, and zero otherwise. In Panel A in

Table 6, we replace the DiD estimator in the main model with the interaction between the indicator created above and the *Post* dummy. The negative coefficient on the interaction term suggests that the adoption of EARs reduces crash risk to a greater extent for mandatory adopters with an above-median number of improper-revenue-recognition RMMs reported in their EARs. This finding reflects that EARs' greater revelation of value-relevant negative information mitigates managers' bad-news hoarding.

<*Insert Table 6 about here*>

Arguably, managers may only need to alter their disclosure pattern and release *incremental* information in response to their unexpected RMMs. We estimate the unexpected number of improper-revenue-recognition RMMs in each audit report by specifying a prediction model that includes the same set of firm-level control variables as in equation (1) with the addition of firm-level observables relating to revenue recognition or collection: *LOSS* (a dummy indicator equal to one if a firm reports a negative net income), *CATA* (current assets divided by total assets), *INTANGIBLE* (intangible assets divided by total assets), *CFO* (net operating cash flow divided by total assets), *DREVENUE* (deferred revenue divided by total assets), *NETCREDIT* (receivables minus payables, divided by total assets), and *SALEGROWTH* (compound growth rate in sales, measured as ln $(Sales_t/Sales_{t-1})$). The regression results from the prediction model are tabulated in Online Appendix D. We extract the predicted value and residual from the regression estimates to proxy for the expected and unexpected number of improper-revenue-recognition RMMs in each audit report. These proxies are then used to construct the dummy indicator $HIGH_{\#\ of\ revenue\ RMMs}$ in Panels B and C in Table 6. The DiD estimator in Panel B is statistically insignificant, indicating that managers' expected RMMs do not materially alter their bad-news disclosure. As suggested in Panel C, the significant reduction in crash risk is attributed to the unexpected RMMs, for which managers have to modify their disclosure.

4.4. Cross-sectional analyses

4.4.1. Information richness

The impact of EARs adoption on crash risk may depend on the richness of a firm's information environment. To the extent that managers are forced to accelerate their bad news disclosure in response to auditors' identified risks of material misstatement, bad news can move stock prices only when it is new to investors. In a firm with a rich information environment, managers' pre-emptive bad news disclosures are less likely to be incrementally informative to investors because investors may have already obtained the information from other information intermediaries.

We use analyst coverage and media coverage to proxy for a firm's information environment richness. *Analyst coverage* is the number of analysts following a firm (source: I/B/E/S). *Media coverage* is the number of media news articles reporting on a firm (source: RavenPack). Analysts process and disseminate financial information primarily to professional investors and companies. By contrast, the media investigates and publicizes information to the general public. Media reporters often visit or interview managers, directors, and regulators, among others, and produce editorials that can convey incremental information (Miller, 2006). We replace missing values of the two proxies with zero. The sample is then partitioned into high and low groups based on the median value of each proxy.

In Table 7, we show that the negative effect of EARs adoption on crash risk is more pronounced in firms with lower analyst coverage (Panel A) and lower media coverage (Panel B). That is, among firms with scant public information, managers' pre-emptive disclosures in response to EARs have greater informational value to investors. For firms falling into the high groups, the magnitude and statistical significance of the DiD estimator are much smaller, suggesting a negligible impact of EARs adoption on firms with abundant public information. These findings concur with the FRC's expectations: "The value added [by EARs] can be particularly important for those audited entities where there are fewer sources of other information" (FRC, 2016, p. 4).

<Insert Table 7 about here>

4.4.2. Auditor characteristics

It is challenging to predict how auditor characteristics, including Big-4 membership and auditor industry specialization, shape the effect of EARs adoption on crash risk. On the one hand, Big-4 or industry specialist auditors are believed to deliver high audit quality. Big-4 auditors have better professional competency and stronger reputational incentives to supply high-quality audits than non-Big-4 auditors (DeAngelo, 1981; Dopuch & Simunic, 1982; Francis & Wilson, 1988). Industry specialist auditors possess industry-specific knowledge that facilitates their detection of accounting irregularities in clients of the focal industry (Owhoso, Messier, & Lynch, 2002). To protect reputation and market share, Big-4 or industry specialist auditors may exploit their superior expertise to investigate and discover client firms' risks of material misstatement and propel client firms' managers to reveal adverse information about risks of material misstatement in a timely fashion. On the other hand, the perceived high audit quality of Big-4 or industry specialist auditors may be attributed to these auditors' intention to select high-quality clients (Lawrence, Minutti-Meza, & Zhang, 2011; Minutti-Meza, 2013). Previous literature shows that Big-N audited firms are associated with more accurate analyst earnings forecasts (Behn, Choi, & Kang, 2008) and greater stock price informativeness (Gul, Kim, & Qiu, 2010). Firms employing industry specialist auditors have better disclosure quality (Dunn & Mayhew, 2004). Put simply, a firm's choice of auditors may signal its information environment quality. For firms with a richer information environment, we have found that EARs adoption is less effective in reducing their crash risk.

In Table 8, we report regression results for the sample partitioned based on the presence of Big-4 auditors (Panel A) and industry specialist auditors (Panel B). An auditor is considered to be an industry specialist if it audits at least 20% of sales of the client's two-digit ICB industry (Dunn & Mayhew, 2004). We observe that the negative effect of EARs adoption on crash risk is more pronounced in firms with non-Big-4 or non-industry-specialist auditors. It is worth noting that the results in Panel A do not suggest that EARs adoption has no effect for all Big-4 audited firms, because this interpretation ignores the heterogeneity of information richness across Big-4 audited firms. In Online Appendix E, we focus on Big-4 audited firms and find that Big-4 audited firms with lower analyst coverage or lower media coverage experience a larger reduction in crash risk following the adoption of EARs. In fact, these firms, along with non-Big-4 or non-industry-specialist audited firms, tend to be associated with poorer information environments, where managers' timelier bad-news disclosures create more informational value to investors.

<Insert Table 8 about here>

4.5. Testable mechanisms

So far, we have interpreted the negative effect of EARs adoption on crash risk as evidence suggesting that RMM disclosures in EARs force managers to pre-empt bad news in corporate disclosures, which dampens bad-news hoarding and reduces future crash risk. The hypothesized mechanism rests on two implicit assumptions. First, prior to EARs adoption, managers disclose bad news in a lump-sum fashion. After adoption, managers disclose bad news more frequently but in smaller doses. Second, other determinants of stock price crash risk do not change after the adoption of EARs. This section offers direct evidence in support of these two assumptions.

4.5.1. EARs adoption and corporate disclosures of bad news

Our empirical strategy to test the first assumption starts with a textual analysis of corporate disclosures. Following the approach adopted by Edmans, Goncalves-Pinto, Groen-Xu, and Wang (2018), we collect firm-initiated news releases from the Capital IQ Key Developments database. This database provides structured summaries of corporate news stories from a variety of news sources, including corporate press releases, regulatory filings, company websites, investor presentations, call transcripts, and web mining. We exclude news items originated by the media and retain only news initiated by the firm. We count the number of positive and negative words in each news article using Loughran and McDonald's (2011) dictionary. We measure the *tone* of a news release as the difference between the number of positive and negative words, scaled by their sum. If our first assumption holds true, the distribution of the *tone* scores of firm-initiated news releases should become less negatively skewed after the adoption of EARs. To verify this prediction, we define the variable *Negative Skewness of Disclosure Tone* as the negative skewness of the *tone* scores of firm-initiated news releases in a firm-year. We anticipate a decline in *Negative Skewness of Disclosure Tone* from the pre- to post-adoption period for mandatory adopters relative to non-adopters.

In Table 9, we conduct a mediation analysis (see Lang, Lins, & Maffett, 2012) to determine whether *Negative Skewness of Disclosure Tone* is the channel through which EARs adoption affects future crash risk. In column 1, the DiD estimator (*Mandatory Adopter×Post*) significantly decreases *Negative Skewness of Disclosure Tone*, suggesting that the tone of corporate disclosures becomes less negatively skewed (reflecting the unwinding of bad-news hoarding) after the adoption of EARs. In columns 3 and 5, we use the mediator (i.e., *Negative Skewness of Disclosure Tone*) as an additional explanatory variable for future crash risk and compare the regression results with those presented in columns 2 and 4 without the inclusion of the mediator. We find the coefficients of *Negative Skewness of Disclosure Tone* in columns 3 and 5 statistically significant and positively signed, indicating that a decrease in *Negative Skewness of Disclosure Tone* (representing the unwinding of bad-news hoarding) reduces future crash risk. In columns 3 and 5, the mediation effect (i.e., the indirect effect of the DiD estimator via the mediator) accounts for 6.19~6.35% of the total effect of the DiD estimator on future crash risk.[20] The mediation effect is statistically significant at the 5% level according to the Sobel (1982) test.[21] The results taken together corroborate the argument that EARs adoption discourages bad-news hoarding in corporate disclosures, which in turn reduces future crash risk.

<Insert Table 9 about here>

4.5.2. EARs adoption and accruals management

The second assumption posits that other drivers of crash risk do not change after the mandatory adoption of EARs. The crash risk literature recognizes accruals management as an approach that managers exploit to disguise bad news (Hutton et al., 2009; Zhu, 2016). Two prior studies have examined how EARs adoption affects accruals management. Reid et al. (2019) document a decrease in accruals management following the adoption of EARs. Gutierrez et al. (2018) show that this finding disappears with a more appropriate research design. Given the mixed prior evidence, we examine which study's conclusion holds in our sample. The confirmation of Gutierrez et al.'s (2018) finding will provide some support for the second assumption.

The research designs of Gutierrez et al. (2018) and Reid et al. (2019) differ in terms of measurement of accruals management and construction of explanatory variables for accruals management. Gutierrez et al. (2018) use the absolute value of discretionary accruals (*DACCR*) estimated from the Jones model as a proxy for accruals management. Specifically, discretionary accruals are the residual obtained by fitting the following model:

$$TACCR_{i,t} = \beta_0 + \beta_1 ROA_{i,t} + \beta_2 \Delta Sales_{i,t} + \beta_3 PPE_{i,t} + \varepsilon_{i,t} \quad (5)$$

[20] In Table 9, the total effect of the DiD estimator on *NCSKEW* is the coefficient of -0.178 reported in column 2. The mediation effect is -0.011 (=-0.188*0.059), where -0.188 is the coefficient of the DiD estimator in column 1 and 0.059 is the coefficient of *Negative Skewness of Disclosure Tone* in column 3.

[21] The Sobel (1982) test assesses whether the indirect effect of the DiD estimator via the mediator (i.e., *Negative Skewness of Disclosure Tone*) is significantly different from zero.

where *i* denotes firm and *t* denotes fiscal year. Total accruals (*TACCR*) are calculated as net income before extraordinary items less cash flow from operations, scaled by average total assets. *ROA* is net income before extraordinary items, scaled by average total assets. Δ*Sales* is change in sales from the previous year to the current year, scaled by average total assets. *PPE* is gross property, plant, and equipment, scaled by average total assets. Equation (5) is then estimated annually for each two-digit SIC industry with at least 20 observations.

To study the effect of EARs adoption on *DACCR*, Gutierrez et al. (2018) adopt a DiD empirical design and include the following control variables in the regression model (see their Table 4). *SIZE* is the natural logarithm of total assets. *ROA* is net income before extraordinary items divided by total assets. *LOSS* equals one if ROA is negative, and zero otherwise. *MTB* is the ratio of equity market value to book value. *LEV* is long-term debt divided by total assets. *CFO* is cash flow from operations divided by total assets. *SALEVOL* is the standard deviation of the ratio of sales to total assets from year *t*-6 to year *t*. *BIG* equals one if the firm is audited by a Big-4 auditor, and zero otherwise. *LAGACCR* is the prior year's accruals (calculated as net income before extraordinary items plus depreciation and amortization less cash flow from operations, divided by total assets).

By comparison, Reid et al. (2019) employ the absolute value of performance-matched discretionary accruals (*ABS_ACC*) as a measure of accruals management. They estimate an annual cross-sectional regression for each two-digit ICB industry with at least 15 observations for the following model:

(6) $$TA_{i,t} = \beta_0 + \beta_1(1/Assets_{i,t-1}) + \beta_2(\Delta Sales_{i,t} - \Delta Receivables_{i,t}) + \beta_3 PPE_{i,t} + \varepsilon_{i,t}$$

where total accruals (*TA*) are measured as (change in current assets − change in cash − change in current liabilities + change in current debt − depreciation and amortization), scaled by lagged total assets. *Assets* denotes total assets. Δ*Sales* is change in sales, scaled by lagged total assets. Δ*Receivables* is change in receivables, scaled by lagged total assets. *PPE* is gross property, plant, and equipment, scaled by lagged total assets. They extract residuals from the regression estimates and match each firm-year observation with another firm from the same two-digit ICB industry and year with the closest ROA. *ABS_ACC* is defined as the absolute value of the given firm's residual less the matched firm's residual.

In the DiD regression model (see their Table 3 Panel A), Reid et al. (2019) apply the variable definitions of Gutierrez et al. (2018) to create their control variables for total assets (*SIZE*), earnings performance (*ROA* and *LOSS*), market-to-book ratio (*MB*), the prior year's accruals (*PRIOR_ACC*), cash flow from operations (*CFO*), and the presence of a Big-4 auditor (*BIG4*). Different from Gutierrez et al. (2018), Reid et al. (2019) define financial leverage (*LEVERAGE*) as the ratio of total debt to total assets and compute sales volatility (*VOLATILITY*) as the standard deviation of the ratio of sales to total assets over the prior three years.

In Table 10, we re-examine the effect of EARs adoption on accruals management by using our sample and DiD set-up. The regression specifications in Panel A and Panel B resemble those used by Gutierrez et al. (2018) and Reid et al. (2019) respectively. In columns 1 and 2 of Panel A, we do not find EARs adoption to significantly affect the absolute value of discretionary accruals (*DACCR*), consistent with the finding by Gutierrez et al. (2018). Using the same vector of control variables from Gutierrez et al. (2018), columns 3 and 4 show that EARs adoption does not change the signed value of discretionary accruals (*ACCRUAL*), which is our measure of accruals management that has been included as a control variable for crash risk in our main model. Moving to Panel B, which reproduces Reid et al.'s (2019) model specification, we observe a negative and significant coefficient on the *Post* dummy in column 1, where only mandatory adopters are in the sample. This is consistent with Reid et al.'s (2019) result (see their Table 3 Panel A, model 1) that mandatory adopters experience a significant reduction in the absolute value of performance-matched discretionary accruals (*ABS_ACC*) after the adoption of EARs. However, in column 2, where we use a DiD regression and AIM firms as a control group, we do not find the adoption of EARs to change *ABS_ACC* for mandatory adopters relative to non-adopters. In columns 3 and 4, there is also no evidence that EARs adoption affects the signed value of discretionary accruals (*ACCRUAL*) used in our paper. Overall, the results demonstrate that the mandatory adoption of EARs has little impact on accruals management in our sample. It is thus unlikely that the adoption of EARs reduces bad-news hoarding and consequently crash risk through accruals management.

<Insert Table 10 about here>

4.6. EARs adoption, crash risk, and stock returns

Investors are concerned about extreme adverse scenarios and are averse to suffering sharp price plunges. Investors, therefore, require compensation for bearing crash risk. Building on these ideas, the asset pricing literature documents a positive relation between crash risk and future stock returns (Ang et al., 2006; Conrad et al., 2013; Chabi-Yo et al., 2018). The theoretical and empirical work of Brunnermeier, Gollier, and Parker (2007) and Boyer, Mitton, and Vorkink (2010) provides additional evidence that stocks with positively skewed idiosyncratic returns earn low future returns, implying that negative idiosyncratic return skewness (i.e., crash risk) yields high future returns. To the extent that this relation may be driven by market mispricing, if, as we have argued, the adoption of EARs dampens bad-news hoarding and improves market efficiency, the premium of holding crash-prone stocks should shrink after the adoption of EARs.

To examine whether the adoption of EARs affects the crash risk premium, we employ the DiD empirical design introduced by Chu, Hirshleifer, and Ma (2020) for asset pricing studies. We calculate monthly crash risk for each stock and construct zero-cost arbitrage portfolios with treated stocks and control stocks separately. Specifically, at the end of each month, we sort all stocks of mandatory adopters into quintiles based on their previous month's crash risk and calculate the return of the zero-cost arbitrage portfolio $High\ crash\ risk - Low\ crash\ risk$ as the return difference between the fifth quintile and the first quintile. We do the same with all non-adopter stocks. Then we exploit the following DiD model to assess the change in monthly returns of the zero-cost arbitrage portfolios constructed using adopter stocks from before to after the adoption of EARs, relative to the contemporaneous change among non-adopter stocks.

$$(7)\quad Return_{i,t}(High\ crash\ risk - Low\ crash\ risk) = \beta_0 + \beta_1 Mandatory\ Adopter_i \times Post_t + \beta_2 Mandatory\ Adopter_i + Year\text{-}Month\ Fixed\ Effects + \varepsilon_{i,t}$$

where $Return(High\ crash\ risk - Low\ crash\ risk)$ represents the equal-weighted average return of the zero-cost arbitrage portfolio i of $High\ crash\ risk - Low\ crash\ risk$ in month t.[22] $Mandatory\ Adopter$ is a dummy variable equal to one if the zero-cost arbitrage portfolio i is formed

[22] The results remain similar if we use value-weighted returns.

on mandatory adopters, and zero if it is formed on non-adopters. $Post$ is a dummy variable equal to one if month t is in the post-adoption period, and zero otherwise.[23] Note that $Post$ itself is subsumed by the year-month fixed effects and dropped from the regression. The main coefficient of interest is the DiD coefficient β_1, which captures the effect of EARs adoption on the returns of the zero-cost arbitrage portfolios $High\ crash\ risk - Low\ crash\ risk$ for mandatory adopters relative to non-adopters.

In Table 11, we use each of the crash risk variables (*NCSKEW* and *DUVOL*) as the sorting variable to create the corresponding zero-cost arbitrage portfolios. We find the coefficients on the DiD estimator (*Mandatory Adopter*×*Post*) negatively signed and statistically significant. This result is consistent with the expectation that the premium of bearing crash risk declines following the adoption of EARs, as bad news is disclosed to the market in a timelier fashion after the adoption of EARs. This finding highlights the importance of predicting crash risk due to EARs in asset pricing.

<Insert Table 11 about here>

5. Conclusion

This paper shows that the mandatory adoption of EARs in the UK significantly reduces stock price crash risk. The crash risk reduction is associated with EARs' disclosure of risks of material misstatement on revenue recognition. Our cross-sectional results show that the mandatory adoption of EARs has a larger negative impact on crash risk for firms with scant public information and firms with non-Big-4 or non-industry-specialist auditors. Shedding light on the underlying mechanism, we find evidence suggesting that firms disclose more smaller pieces of negative information in corporate disclosures following the adoption of EARs. We do not find the adoption of EARs to affect crash risk through accruals management. Finally, we show that the adoption of EARs attenuates the return premium of holding crash-prone stocks. Taken together, our findings suggest that EARs play an important role in urging managers to make timelier bad-news revelations to investors.

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[23] ISA 700 (UK and Ireland) applied to mandatory adopters for fiscal years ending on or after September 30, 2013. As UK public companies do not have the same fiscal year-end, mandatory adopters in fact began issuing EARs in different calendar months. To clearly define the pre- and post-adoption periods for the zero-cost arbitrage portfolios $High\ crash\ risk - Low\ crash\ risk$ among adopter stocks and non-adopter stocks, we set *Post* to one if month t is after December 31, 2013, given that most UK public companies' fiscal year ends on December 31. Nevertheless, our finding is unaffected if we set *Post* to one for all months after September 30, 2013.

This paper has implications for regulation and academic research. While the public release of EARs does not directly communicate incremental information to investors (Gutierrez et al., 2018; Lennox et al., 2022), our evidence suggests that the report indirectly forces managers to accelerate their disclosure of negative information and consequently reduces future crash risk. The disciplining effect of EARs is mainly concentrated in the first year post adoption. This is something that policymakers and practitioners need to think about and try to address. Our evidence from the UK provides useful feedback to regulators in other developed markets. LSE premium-listed companies on average publish four KAMs per audit report (Gutierrez et al., 2018), whereas the majority of US large-accelerated filers issue only one CAM (Burke et al., 2022). The very few CAMs in US audit reports may limit the power of CAMs in forcing corporate disclosures. Further guidance from the PCAOB may help encourage CAM disclosures for US firms. Given the evidence documented in our paper, it is possible that EARs alter other managerial decisions or corporate policies.

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Table 1 Sample distribution and descriptive statistics

This table presents sample distribution and summary statistics. Panel A describes the distribution of sample firms by year. *Mandatory adopters* are firms that were required to prepare EARs from the 2013/2014 fiscal year onward, whereas *non-adopters* are firms that were not so required. Panel B reports the descriptive statistics of the DiD sample and the mandatory adopter and non-adopter subsamples. Panel C reports changes in crash risk during the pre-adoption period and compares the mean values of the changes between mandatory adopters and non-adopters. *ΔNCSKEW* is the one-year change in *NCSKEW*. *ΔDUVOL* is the one-year change in *DUVOL*. Year 0 denotes the (pseudo) mandatory adoption year, and year -1 to year -5 denote one to five years prior to (pseudo) mandatory adoption. Variables are defined in Appendix B.

Panel A: Sample distribution

Fiscal year	The independent auditor's report disclosed risks of material misstatement			Total
	Yes	No		
	Mandatory adopters	Mandatory adopters	Non-adopters	
2010	0	339	521	860
2011	0	326	495	821
2012	0	324	482	806
2013	180	130	438	748
2014	300	0	405	705
2015	302	0	389	691
2016	299	0	360	659
Total	1,081	1,119	3,090	5,290

Panel B: Summary statistics

Variable	DiD sample (N=5,290)					Mandatory adopters (N=2,200)		Non-adopters (N=3,090)	
	Mean	SD	P1	Median	P99	Mean	SD	Mean	SD
$NCSKEW_{t+1}$	-0.167	1.061	-2.750	-0.209	3.307	0.018	0.887	-0.298	1.151
$DUVOL_{t+1}$	-0.190	0.889	-2.126	-0.241	2.443	-0.016	0.777	-0.314	0.942
$RETURN_t$	-0.196	0.276	-1.526	-0.094	-0.009	-0.083	0.109	-0.276	0.326
$SIGMA_t$	0.053	0.033	0.014	0.044	0.172	0.037	0.018	0.065	0.036
$TURNOVER_t$	-0.139	4.867	-16.980	-0.017	16.240	-0.653	4.339	0.227	5.181
$ACCRUAL_t$	-0.019	0.141	-0.581	-0.008	0.374	-0.008	0.086	-0.027	0.170
$LEVERAGE_t$	0.158	0.200	0.000	0.104	0.900	0.199	0.171	0.129	0.213
MTB_t	2.889	5.885	-9.959	1.761	29.960	3.238	5.968	2.641	5.813
ROA_t	-0.047	0.307	-1.494	0.040	0.313	0.063	0.098	-0.124	0.374
$SIZE_t$	11.470	2.326	7.076	11.150	17.620	13.460	1.856	10.050	1.410
$ANALYST_t$	1.328	0.995	0.000	1.099	3.367	2.196	0.813	0.710	0.552
$BIG4_t$	0.595	0.491	0.000	1.000	1.000	0.934	0.248	0.353	0.478
$TENURE_t$	1.882	0.789	0.000	1.946	3.091	2.214	0.764	1.646	0.719

Panel C: Parallel trends diagnostic test

	Period	Mandatory adopters Mean	Non-adopters Mean	Difference in means	*t*-stat.	(*p*-value)
ΔNCSKEW	from year -1 to year 0	0.005	0.042	-0.037	-0.37	(0.71)
	from year -2 to year -1	-0.035	-0.112	0.077	0.72	(0.47)
	from year -3 to year -2	0.168	0.057	0.112	1.11	(0.27)
	from year -4 to year -3	0.064	0.129	-0.065	-0.70	(0.48)
	from year -5 to year -4	-0.293	-0.629	0.336	3.13	(0.00)
ΔDUVOL	from year -1 to year 0	0.029	0.021	0.008	0.10	(0.92)
	from year -2 to year -1	-0.041	-0.111	0.071	0.79	(0.43)
	from year -3 to year -2	0.166	0.083	0.083	0.99	(0.32)
	from year -4 to year -3	0.065	0.082	-0.018	-0.22	(0.82)
	from year -5 to year -4	-0.304	-0.604	0.300	3.32	(0.00)

Table 2 The effect of EARs adoption on crash risk

This table presents the effect of the mandatory adoption of EARs on crash risk. Columns 1 and 2 examine mandatory adopters only. Columns 3 and 4 use the full sample. Crash risk variables are one-year forward relative to the independent variables. Variables are defined in Appendix B. Intercepts are included but suppressed for brevity. The *t*-statistics based on robust standard errors clustered at the firm level are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Mandatory adopters only		DiD sample	
	(1)	(2)	(3)	(4)
	NCSKEW	*DUVOL*	*NCSKEW*	*DUVOL*
Mandatory Adopter × Post			-0.190***	-0.151***
			[-3.27]	[-3.02]
Post	-0.138***	-0.113***	0.131*	0.146**
	[-2.81]	[-2.66]	[1.70]	[2.22]
RETURN	0.237	0.052	0.331	0.184
	[0.29]	[0.08]	[1.44]	[0.94]
SIGMA	0.988	0.115	1.618	0.417
	[0.21]	[0.03]	[0.94]	[0.28]
TURNOVER	0.005	0.005	0.003	0.003
	[1.08]	[1.05]	[0.86]	[1.00]
ACCRUAL	0.028	0.046	0.008*	0.012***
	[0.81]	[1.62]	[1.76]	[3.24]
LEVERAGE	0.138	0.025	0.150	0.084
	[0.47]	[0.10]	[0.88]	[0.60]
MTB	-0.001	-0.001	-0.000	-0.001
	[-0.33]	[-0.17]	[-0.11]	[-0.27]
ROA	-0.246	-0.156	-0.098	-0.092
	[-0.61]	[-0.48]	[-0.93]	[-1.07]
SIZE	0.461***	0.416***	0.375***	0.332***
	[7.62]	[7.86]	[11.56]	[12.17]
ANALYST	0.047	0.037	0.020	0.021
	[0.53]	[0.48]	[0.37]	[0.47]
BIG4	-0.092	-0.019	-0.010	0.020
	[-0.53]	[-0.10]	[-0.10]	[0.22]
TENURE	0.053*	0.042	0.021	0.022
	[1.75]	[1.42]	[0.79]	[0.91]
Firm fixed effects	yes	yes	yes	yes
Year fixed effects	no	no	yes	yes
R^2	0.052	0.056	0.054	0.062
N	2200	2200	5290	5290

Table 3 The effect of EARs adoption on crash risk across event years

This table presents the dynamic DiD regression results for the effect of the mandatory adoption of EARs on crash risk across event years. *Mandatory adopter* is interacted with the four dummy indicators that represent the four or three years before (pseudo) mandatory adoption ($Before_{\leq -3}$), two years before ($Before_{-2}$), the mandatory adoption year ($Current_0$), one year after ($After_{+1}$), two years after ($After_{+2}$), and three years after ($After_{+3}$). Crash risk variables are one-year forward relative to the independent variables. Intercepts are included but suppressed for brevity. The *t*-statistics based on robust standard errors clustered at the firm level are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) $NCSKEW_{t+1}$	(2) $DUVOL_{t+1}$
Mandatory Adopter × $Before_{\leq -3}$	-0.018	-0.049
	[-0.21]	[-0.65]
Mandatory Adopter × $Before_{-2}$	0.076	0.031
	[0.86]	[0.41]
Mandatory Adopter × $Current_0$	-0.074	-0.070
	[-0.84]	[-0.91]
Mandatory Adopter × $After_{+1}$	-0.271***	-0.250***
	[-2.96]	[-3.12]
Mandatory Adopter × $After_{+2}$	-0.147	-0.135
	[-1.43]	[-1.52]
Mandatory Adopter × $After_{+3}$	-0.217	-0.224*
	[-1.64]	[-1.94]
$Before_{\leq -3}$	0.227	0.277*
	[1.27]	[1.82]
$Before_{-2}$	0.072	0.106
	[0.63]	[1.10]
$Current_0$	0.081	0.079
	[0.77]	[0.87]
$After_{+1}$	0.237	0.135
	[1.27]	[0.85]
$After_{+2}$	0.329	0.191
	[1.23]	[0.83]
$After_{+3}$	0.323	0.207
	[0.92]	[0.68]
Control variables	yes	yes
Firm fixed effects	yes	yes
Year fixed effects	yes	yes
R^2	0.062	0.068
N	5290	5290

Table 4 Selection bias tests for the effect of EARs adoption on crash risk

This table presents the results of a series of selection bias tests for the effect of the mandatory adoption of EARs on crash risk. In Panel A, we run a Heckman selection model with the inclusion of an instrumental variable (*Incorporated before the launch of AIM*) in the first-stage probit model. In Panel B, we study the pretreatment period of 2009–2012 and apply a placebo shock two years before the actual treatment. In Panel C, we conduct placebo tests to examine how the mandatory adoption of EARs affects the mean (*RETURN*) and variance (*VARIANCE*) of stock returns. In the subsequent few panels, we match mandatory adopters and non-adopters on both pre-adoption and post-adoption mean stock returns (Panel D), firm size and growth potential (Panel E), and propensity scores (Panel F). We then match UK mandatory adopters to comparable EU firms (Panel G) and US firms (Panel H) based on propensity scores. Crash risk variables are one-year forward relative to the independent variables. The *t*-statistics based on robust standard errors clustered at the firm level are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Heckman selection model

	(1)	(2)	(3)	(4)
	Mandatory Adopter	*NCSKEW*	*Mandatory Adopter*	*DUVOL*
Mandatory Adopter × Post		-0.196***		-0.158***
		[-3.41]		[-3.19]
Post		0.137*		0.152**
		[1.75]		[2.28]
Incorporated before the launch of AIM	1.167***		1.164***	
	[8.89]		[8.85]	
Lambda		-0.390***		-0.371***
		[-3.42]		[-4.74]
Control variables	yes	yes	yes	yes
Firm fixed effects	no	yes	no	yes
Year fixed effects	yes	yes	yes	yes
N	5290	5290	5290	5290

Panel B: Placebo shock in the pretreatment period

	Mandatory adopters only		DiD sample	
	(1)	(2)	(3)	(4)
	NCSKEW	*DUVOL*	*NCSKEW*	*DUVOL*
Mandatory Adopter × Post			-0.003	0.015
			[-0.04]	[0.25]
Post	-0.042	-0.018	-0.030	-0.042
	[-0.77]	[-0.35]	[-0.32]	[-0.55]
Control variables	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Year fixed effects	no	no	yes	yes
R^2	0.077	0.083	0.077	0.095
N	1347	1347	3521	3521

Panel C: Placebo tests on the first and second moments of stock returns				
	Mandatory adopters only		DiD sample	
	(1)	(2)	(3)	(4)
	RETURN	*VARIANCE*	*RETURN*	*VARIANCE*
Mandatory Adopter × Post			-0.009	0.017
			[-0.80]	[0.67]
Post	-0.018***	0.034***	0.022	-0.051
	[-2.92]	[2.83]	[1.30]	[-1.38]
Control variables	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Year fixed effects	no	no	yes	yes
R^2	0.124	0.116	0.108	0.098
N	2200	2200	5290	5290

Panel D: UK mandatory adopters and non-adopters matched on both pre-adoption and post-adoption mean stock returns				
	Mandatory adopters only		DiD sample	
	(1)	(2)	(3)	(4)
	NCSKEW	*DUVOL*	*NCSKEW*	*DUVOL*
Mandatory Adopter × Post			-0.229**	-0.196**
			[-2.46]	[-2.41]
Post	-0.167**	-0.134**	0.178	0.195*
	[-2.42]	[-2.24]	[1.32]	[1.69]
Control variables	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Year fixed effects	no	no	yes	yes
R^2	0.067	0.072	0.063	0.083
N	887	887	1774	1774

Panel E: UK mandatory adopters and non-adopters matched on firm size and growth potential				
	Mandatory adopters only		DiD sample	
	(1)	(2)	(3)	(4)
	NCSKEW	*DUVOL*	*NCSKEW*	*DUVOL*
Mandatory Adopter × Post			-0.358***	-0.378***
			[-2.80]	[-3.33]
Post	-0.229***	-0.203***	0.286	0.338*
	[-3.14]	[-3.25]	[1.39]	[1.76]
Control variables	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Year fixed effects	no	no	yes	yes
R^2	0.077	0.084	0.135	0.169
N	1106	1106	2212	2212

Panel F: UK mandatory adopters and non-adopters matched on propensity scores

	Mandatory adopters only		DiD sample	
	(1)	(2)	(3)	(4)
	NCSKEW	*DUVOL*	*NCSKEW*	*DUVOL*
Mandatory Adopter × *Post*			-0.292***	-0.290***
			[-3.32]	[-3.83]
Post	-0.272***	-0.234***	0.090	0.162
	[-3.41]	[-3.70]	[0.50]	[1.14]
Control variables	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Year fixed effects	no	no	yes	yes
R^2	0.060	0.061	0.067	0.079
N	949	949	1898	1898

Panel G: UK mandatory adopters matched to comparable EU firms

	UK mandatory adopters only		DiD sample	
	(1)	(2)	(3)	(4)
	NCSKEW	*DUVOL*	*NCSKEW*	*DUVOL*
Mandatory Adopter × *Post*			-0.190**	-0.158**
			[-2.20]	[-2.06]
Post	-0.313***	-0.260***	-0.052	-0.018
	[-4.03]	[-4.01]	[-0.45]	[-0.17]
Control variables	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Year fixed effects	no	no	yes	yes
R^2	0.068	0.064	0.060	0.059
N	921	921	1842	1842

Panel H: UK mandatory adopters matched to comparable US firms

	UK mandatory adopters only		DiD sample	
	(1)	(2)	(3)	(4)
	NCSKEW	*DUVOL*	*NCSKEW*	*DUVOL*
Mandatory Adopter × *Post*			-0.143**	-0.123**
			[-2.07]	[-2.09]
Post	-0.110**	-0.110**	0.166	0.125
	[-2.10]	[-2.41]	[1.63]	[1.44]
Control variables	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Year fixed effects	no	no	yes	yes
R^2	0.059	0.064	0.046	0.048
N	1623	1623	3246	3246

Table 5 Other robustness tests for the effect of EARs adoption on crash risk

This table reports the results of other robustness tests. We control for other determinants of crash risk (Panel A); control for audit firm fixed effects and audit partner fixed effects (Panel B); use the FTSE All-Share index returns and AIM All-Share index returns as proxies for UK market returns in the expanded market model to estimate firm-specific stock returns (Panel C); estimate *NCSKEW* and *DUVOL* using daily data (Panel D); use other univariate (*COUNT* and *COLLAR*), bivariate (*LTD*), and multivariate (*MCRASH*) crash risk measures (Panel E); remove observations in fiscal year 2013 from the sample (Panel F); and drop firms that changed their auditors during the (pseudo) mandatory adoption year (Panel G). Crash risk variables are one-year forward relative to the independent variables. Variables are defined in Appendix B. The *t*-statistics based on robust standard errors clustered at the firm level are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Control for other determinants of crash risk

	Mandatory adopters only		DiD sample	
	(1)	(2)	(3)	(4)
	NCSKEW	*DUVOL*	*NCSKEW*	*DUVOL*
Mandatory Adopter × Post			-0.216***	-0.181***
			[-3.23]	[-3.08]
Post	-0.150**	-0.136***	0.125	0.138*
	[-2.51]	[-2.64]	[1.49]	[1.91]
Earnings smoothing	0.075	0.048	0.020	0.022
	[1.02]	[0.79]	[0.42]	[0.55]
Accounting conservatism	-0.009	-0.013	-0.016	-0.013
	[-0.42]	[-0.64]	[-0.54]	[-0.49]
Financial statement comparability	-0.114**	-0.104**	-0.033	-0.009
	[-2.22]	[-2.26]	[-1.21]	[-0.38]
Tax avoidance	0.124	0.083	0.164	0.098
	[0.84]	[0.68]	[1.52]	[1.13]
Annual report file size	-0.019	-0.006	-0.032	-0.027
	[-0.50]	[-0.19]	[-1.15]	[-1.22]
Control variables	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Year fixed effects	no	no	yes	yes
R^2	0.052	0.054	0.058	0.066
N	2019	2019	4614	4614

Panel B: Control for audit firm fixed effects and audit partner fixed effects

	Mandatory adopters only		DiD sample	
	(1)	(2)	(3)	(4)
	NCSKEW	*DUVOL*	*NCSKEW*	*DUVOL*
Mandatory Adopter × Post			-0.240***	-0.206***
			[-3.09]	[-3.09]
Post	-0.124**	-0.088*	0.156	0.170**
	[-2.10]	[-1.67]	[1.63]	[2.15]
Control variables	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Year fixed effects	no	no	yes	yes
Audit firm fixed effects	yes	yes	yes	yes
Audit partner fixed effects	yes	yes	yes	yes
R^2	0.255	0.265	0.233	0.242
N	2200	2200	5290	5290

Panel C: Use FTSE All-Share index returns and AIM All-Share index returns as proxies for UK market returns

	Mandatory adopters only		DiD sample	
	(1)	(2)	(3)	(4)
	NCSKEW	*DUVOL*	*NCSKEW*	*DUVOL*
Mandatory Adopter × *Post*			-0.146**	-0.114**
			[-2.48]	[-2.24]
Post	-0.137***	-0.114***	0.069	0.110
	[-2.78]	[-2.69]	[0.86]	[1.62]
Control variables	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Year fixed effects	no	no	yes	yes
R^2	0.052	0.055	0.050	0.057
N	2200	2200	5290	5290

Panel D: Crash risk measures estimated using daily data

	Mandatory adopters only		DiD sample	
	(1)	(2)	(3)	(4)
	NCSKEW	*DUVOL*	*NCSKEW*	*DUVOL*
Mandatory Adopter × *Post*			-0.454***	-0.204***
			[-3.37]	[-3.62]
Post	-0.273***	-0.085**	0.448**	0.349***
	[-2.66]	[-2.09]	[2.53]	[4.63]
Control variables	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Year fixed effects	no	no	yes	yes
R^2	0.057	0.084	0.062	0.090
N	2161	2161	5223	5223

Panel E: Other univariate, bivariate, and multivariate crash risk measures

	Mandatory adopters only				DiD sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	COUNT	*COLLAR*	*LTD*	*MCRASH*	*COUNT*	*COLLAR*	*LTD*	*MCRASH*
Mandatory Adopter × *Post*					-0.091**	-0.191***	-0.027***	-0.009***
					[-2.15]	[-3.08]	[-6.84]	[-5.15]
Post	-0.072**	-0.094***	-0.040***	-0.009***	0.042	0.065	0.003	0.005**
	[-2.03]	[-2.69]	[-9.33]	[-5.47]	[0.73]	[0.78]	[0.95]	[2.09]
Control variables	yes	yes	yes	yes	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Year fixed effects	no	no	no	no	yes	yes	yes	yes
R^2	0.037	0.040	0.130	0.046	0.038	0.048	0.197	0.061
N	2200	2200	2161	2161	5290	5290	5223	5223

Panel F: Delete observations in fiscal year 2013

	Mandatory adopters only		DiD sample	
	(1)	(2)	(3)	(4)
	NCSKEW	*DUVOL*	*NCSKEW*	*DUVOL*
Mandatory Adopter × Post			-0.230***	-0.189***
			[-3.43]	[-3.30]
Post	-0.171***	-0.147***	0.057	0.034
	[-2.98]	[-2.97]	[0.79]	[0.56]
Control variables	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Year fixed effects	no	no	yes	yes
R^2	0.052	0.056	0.049	0.056
N	1890	1890	4542	4542

Panel G: Delete firms that changed their auditors during the (pseudo) mandatory adoption year

	Mandatory adopters only		DiD sample	
	(1)	(2)	(3)	(4)
	NCSKEW	*DUVOL*	*NCSKEW*	*DUVOL*
Mandatory Adopter × Post			-0.207***	-0.164***
			[-3.43]	[-3.15]
Post	-0.135***	-0.110**	0.150*	0.166**
	[-2.69]	[-2.56]	[1.87]	[2.42]
Control variables	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Year fixed effects	no	no	yes	yes
R^2	0.050	0.054	0.061	0.069
N	2140	2140	5047	5047

Table 6 How do risks of material misstatement in revenue recognition affect the relation between EARs adoption and crash risk?

This table examines how the effect of the mandatory adoption of EARs on crash risk varies depending on EARs' disclosure of risks of material misstatement in revenue recognition. The sample contains only mandatory adopters. $HIGH_{\#\ of\ revenue\ RMMs}$ equals one if the average number of revenue recognition RMMs within a firm's EARs in the post-adoption period is above the sample median, and zero otherwise. This indicator is constructed using the actual number of revenue recognition RMMs reported in EARs in Panel A, and the expected and unexpected number of revenue recognition RMMs in Panel B and Panel C, respectively. The expected and unexpected number of revenue recognition RMMs is estimated using the coefficients presented in Online Appendix D. Crash risk variables are one-year forward relative to the independent variables. Intercepts are included but suppressed for brevity. The *t*-statistics based on robust standard errors clustered at the firm level are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Dep. Var. =	*NCSKEW*	*DUVOL*
	(1)	(2)
Panel A: Actual number of revenue recognition RMMs		
$HIGH_{\#\ of\ revenue\ RMMs} \times Post$	-0.167**	-0.161**
	[-2.14]	[-2.30]
Post	0.134	0.161
	[1.14]	[1.58]
Control variables	yes	yes
Firm fixed effects	yes	yes
Year fixed effects	yes	yes
R^2	0.059	0.066
N	2096	2096
Panel B: Expected number of revenue recognition RMMs		
$HIGH_{\#\ of\ revenue\ RMMs} \times Post$	-0.085	-0.072
	[-1.07]	[-1.01]
Post	0.094	0.116
	[0.78]	[1.10]
Control variables	yes	yes
Firm fixed effects	yes	yes
Year fixed effects	yes	yes
R^2	0.057	0.063
N	2096	2096
Panel C: Unexpected number of revenue recognition RMMs		
$HIGH_{\#\ of\ revenue\ RMMs} \times Post$	-0.155**	-0.147**
	[-2.03]	[-2.10]
Post	0.126	0.151
	[1.08]	[1.51]
Control variables	yes	yes
Firm fixed effects	yes	yes
Year fixed effects	yes	yes
R^2	0.059	0.065
N	2096	2096

Table 7 The effect of EARs adoption on crash risk across firms with different levels of information richness

This table presents the effect of the mandatory adoption of EARs on crash risk, conditional on firms' information richness. The sample is partitioned into high and low groups based on the median of analyst coverage (Panel A) and media coverage (Panel B). Crash risk variables are one-year forward relative to the independent variables. Intercepts are included but suppressed for brevity. The *t*-statistics based on robust standard errors clustered at the firm level are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	NCSKEW		*DUVOL*	
	(1)	(2)	(3)	(4)
	High	Low	High	Low
Panel A: Analyst coverage				
Mandatory Adopter × Post	-0.152	-0.392***	-0.148	-0.329***
	[-1.23]	[-2.94]	[-1.40]	[-3.10]
Post	0.137	0.103	0.173	0.115
	[0.94]	[0.93]	[1.39]	[1.23]
Control variables	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes
R^2	0.052	0.052	0.061	0.059
N	2390	2900	2390	2900
Panel B: Media coverage				
Mandatory Adopter × Post	-0.126	-0.503***	-0.098	-0.413***
	[-1.25]	[-4.71]	[-1.12]	[-4.63]
Post	0.078	0.152	0.086	0.180*
	[0.63]	[1.24]	[0.78]	[1.78]
Control variables	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes
R^2	0.048	0.053	0.056	0.058
N	2564	2726	2564	2726

Table 8 The effect of EARs adoption on crash risk across firms with different auditor characteristics

This table presents the effect of the mandatory adoption of EARs on crash risk, conditional on different auditor characteristics. The sample is split based on the presence of Big-4 auditors (Panel A) and industry specialist auditors (Panel B). Crash risk variables are one-year forward relative to the independent variables. Intercepts are included but suppressed for brevity. The *t*-statistics based on robust standard errors clustered at the firm level are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	NCSKEW		*DUVOL*	
	(1)	(2)	(3)	(4)
Panel A: Big 4 auditors				
	Big 4	Non-Big 4	Big 4	Non-Big 4
Mandatory Adopter × Post	-0.090	-0.755***	-0.100	-0.655***
	[-1.14]	[-3.98]	[-1.42]	[-5.50]
Post	0.022	0.241*	0.099	0.196*
	[0.21]	[1.89]	[1.06]	[1.81]
Control variables	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes
R^2	0.057	0.062	0.067	0.068
N	3145	2145	3145	2145
Panel B: Auditor industry specialization				
	Specialist	Non-Specialist	Specialist	Non-Specialist
Mandatory Adopter × Post	-0.042	-0.306***	-0.049	-0.259***
	[-0.43]	[-3.46]	[-0.55]	[-3.34]
Post	0.053	0.161	0.131	0.155*
	[0.40]	[1.57]	[1.13]	[1.78]
Control variables	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes
R^2	0.046	0.063	0.057	0.069
N	1998	3292	1998	3292

Table 9 The effect of EARs adoption on corporate disclosures of bad news

This table examines a chain of relations, where the mandatory adoption of EARs affects corporate disclosures of bad news, which in turn affect future crash risk. *Negative Skewness of Disclosure Tone* is the negative skewness of the *tone* scores of firm-initiated news releases in a firm-year, where the *tone* of a news release is measured as the difference between the number of positive and negative words (classified using Loughran and McDonald's dictionary) scaled by their sum. Variables are defined in Appendix B. Intercepts are included but suppressed for brevity. The *t*-statistics based on robust standard errors clustered at the firm level are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Dep. Var. =	*Negative Skewness of Disclosure Tone$_t$*	*NCSKEW$_{t+1}$*		*DUVOL$_{t+1}$*	
	(1)	(2)	(3)	(4)	(5)
Mandatory Adopter × Post	-0.188***	-0.178***	-0.167***	-0.143***	-0.134**
	[-3.68]	[-2.97]	[-2.77]	[-2.75]	[-2.56]
Post	0.180***	0.118	0.107	0.135**	0.126*
	[2.83]	[1.47]	[1.34]	[1.97]	[1.84]
RETURN	0.024	0.168	0.167	0.057	0.056
	[0.13]	[0.67]	[0.66]	[0.27]	[0.26]
SIGMA	-0.862	0.295	0.345	-0.615	-0.574
	[-0.63]	[0.16]	[0.19]	[-0.38]	[-0.35]
TURNOVER	0.004*	0.003	0.002	0.003	0.003
	[1.65]	[0.74]	[0.68]	[0.96]	[0.90]
ACCRUAL	0.029	0.003	0.002	0.026	0.024
	[0.32]	[0.03]	[0.01]	[0.27]	[0.26]
LEVERAGE	-0.098	0.250	0.255	0.157	0.162
	[-0.79]	[1.29]	[1.32]	[0.99]	[1.02]
MTB	0.000	0.001	0.001	-0.000	-0.000
	[0.03]	[0.28]	[0.28]	[-0.02]	[-0.02]
ROA	0.011	-0.027	-0.028	-0.061	-0.061
	[0.15]	[-0.22]	[-0.23]	[-0.63]	[-0.64]
SIZE	0.121***	0.363***	0.356***	0.330***	0.324***
	[4.55]	[10.46]	[10.27]	[11.25]	[11.04]
ANALYST	0.012	0.020	0.019	0.019	0.019
	[0.28]	[0.35]	[0.34]	[0.40]	[0.39]
BIG4	-0.040	-0.000	0.002	0.013	0.015
	[-0.51]	[-0.00]	[0.02]	[0.13]	[0.15]
TENURE	0.016	0.015	0.015	0.013	0.012
	[0.70]	[0.57]	[0.53]	[0.53]	[0.50]
Negative Skewness of Disclosure Tone$_t$			0.059***		0.048**
			[2.64]		[2.48]
Firm fixed effects	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes
R^2	0.035	0.054	0.056	0.062	0.064
N	4713	4713	4713	4713	4713
Indirect effect/Total effect			6.19%		6.35%
Sobel test *p*-value			0.03		0.04

Table 10 The effect of EARs adoption on accruals management

This table presents the effect of the mandatory adoption of EARs on accruals management. In Panel A, we use the same set of control variables as in Gutierrez et al. (2018). Columns 1 and 2 use Gutierrez et al.'s (2018) proxy for accruals management *DACCR*, defined as the absolute value of discretionary accruals from the Jones model. Columns 3 and 4 use our accruals management measure *ACCRUAL*, calculated as the signed value of discretionary accruals from the modified Jones model. In Panel B, we use the same set of control variables as in Reid et al. (2019). Columns 1 and 2 adopt Reid et al.'s (2019) accruals management measure *ABS_ACC*, defined as the absolute value of performance-matched discretionary accruals. Columns 3 and 4 use our accruals management measure *ACCRUAL*. Intercepts are included but suppressed for brevity. The *t*-statistics based on robust standard errors clustered at the firm level are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Reproduce Gutierrez et al.'s (2018) model specification

Dep. Var. =	*DACCR* from Gutierrez et al. (2018)		*ACCRUAL* from our paper	
	(1)	(2)	(3)	(4)
	Mandatory adopters only	DiD sample	Mandatory adopters only	DiD sample
Mandatory Adopter × Post		0.004		-0.029
		[0.86]		[-0.90]
Post	0.000	-0.004	-0.011	0.048
	[0.07]	[-0.60]	[-1.11]	[0.80]
SIZE	-0.006	-0.015***	-0.021	-0.128
	[-0.79]	[-2.80]	[-1.25]	[-1.34]
ROA	-0.072***	-0.079***	0.188**	0.159***
	[-2.76]	[-4.26]	[2.09]	[4.16]
LOSS	0.008*	-0.005	0.001	-0.058***
	[1.81]	[-0.97]	[0.10]	[-3.04]
MTB	-0.000	0.000	-0.001	-0.018
	[-0.51]	[1.04]	[-0.94]	[-1.08]
LEV	-0.077***	-0.016	0.030	-0.074
	[-4.84]	[-0.70]	[0.88]	[-0.66]
CFO	0.118**	0.007	-0.384***	-0.343***
	[2.14]	[0.26]	[-3.30]	[-3.94]
SALEVOL	-0.015	0.006	-0.038*	-0.027
	[-0.72]	[0.53]	[-1.90]	[-0.89]
BIG	-0.026	0.004	-0.012	-0.058
	[-1.58]	[0.38]	[-0.37]	[-0.64]
LAGACCR	-0.004	-0.015	0.066	0.132
	[-0.16]	[-1.20]	[1.50]	[1.02]
Firm fixed effects	yes	yes	yes	yes
Year fixed effects	no	yes	no	yes
R^2	0.079	0.089	0.011	0.020
N	1410	4157	2376	5878

Panel B: Reproduce Reid et al.'s (2019) model specification

Dep. Var. =	*ABS_ACC* from Reid et al. (2019)		*ACCRUAL* from our paper	
	(1)	(2)	(3)	(4)
	Mandatory adopters only	DiD sample	Mandatory adopters only	DiD sample
Mandatory Adopter × Post		0.006		-0.028
		[0.92]		[-0.88]
Post	-0.008**	0.006	-0.010	0.045
	[-2.18]	[0.72]	[-1.03]	[0.78]
SIZE	0.013	0.001	-0.022	-0.139
	[1.15]	[0.06]	[-1.29]	[-1.34]
ROA	-0.132	-0.017	0.194**	0.221**
	[-1.28]	[-0.74]	[2.11]	[2.56]
LOSS	-0.002	0.010	0.001	-0.071***
	[-0.17]	[1.59]	[0.09]	[-2.60]
MB	0.000	0.000	-0.001	-0.017
	[0.33]	[0.54]	[-0.88]	[-1.08]
LEVERAGE	-0.050*	-0.032	0.057	0.548
	[-1.70]	[-1.27]	[1.25]	[1.08]
PRIOR_ACC	-0.044	-0.037*	0.068	0.128
	[-1.30]	[-1.74]	[1.54]	[1.04]
CFO	0.037	-0.009	-0.379***	-0.363***
	[0.62]	[-0.26]	[-3.28]	[-3.80]
VOLATILITY	-0.021	0.039**	-0.053*	-0.019
	[-0.79]	[2.35]	[-1.78]	[-0.25]
BIG4	-0.020	0.003	-0.010	-0.064
	[-0.76]	[0.19]	[-0.33]	[-0.73]
Firm fixed effects	yes	yes	yes	yes
Year fixed effects	no	yes	no	yes
R^2	0.015	0.011	0.011	0.025
N	2330	5523	2376	5881

Table 11 How does EARs adoption affect the relation between crash risk and subsequent stock returns?

This table examines how the mandatory adoption of EARs affects the return premium of bearing crash risk for mandatory adopters relative to non-adopters. At the end of each month, we sort all stocks of mandatory adopters into quintiles according to their previous month's crash risk (*NCSKEW* or *DUVOL*) and calculate *Return(High crash risk − Low crash risk)* as the equal-weighted average monthly return of the zero-cost arbitrage portfolio that goes long in the fifth quintile and short in the first quintile. We then do the same with all non-adopter stocks. *Mandatory Adopter* is equal to one if the zero-cost arbitrage portfolio is formed on mandatory adopters, and zero if it is formed on non-adopters. *Post* is equal to one for all months in the post-adoption period, and zero otherwise. The *t*-statistics based on robust standard errors are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Dep. Var. =	*Return (High crash risk - Low crash risk)*	
	(1)	(2)
Sorting variable:	*NCSKEW*	*DUVOL*
Mandatory Adopter × Post	-0.013**	-0.013**
	[-2.60]	[-2.28]
Mandatory Adopter	0.005	0.004
	[1.29]	[0.93]
Year-month fixed effects	yes	yes
R^2	0.595	0.517
N	168	168

Appendix A: Excerpts from Carillion plc 2016 independent auditor's report

Our assessment of risks of material misstatement

In arriving at our audit opinion above on the financial statements the risks of material misstatement that had the greatest effect on our audit, in decreasing order of audit significance, were as follows:

Recognition of contract revenue, margin, and related receivables and liabilities; Risk vs 2015:=

Refer to page 62 (Report of the Audit Committee), page 97 (note 1. Significant accounting policies – Revenue recognition) and page 132 (note 31. Accounting estimates and judgements – Revenue recognition).

The risk – The Group recognises revenue based on the stage of completion of construction contracts by reference to the proportion of costs incurred to the balance sheet date compared with the estimated final costs of the contract at completion and therefore relies on estimates in relation to the final out-turn of costs on each contract. Changes to these estimates could give rise to material variances in the amount of revenue and margin recognised. Contingencies may also be included in these estimates of cost to take account of specific risks, or claims against the Group, arising within each contract. These contingencies are reviewed by the Group on a regular basis throughout the contract life and adjusted where appropriate. Finally, variations and claims are recognised on a contract-by-contract basis, both on service and construction contracts, where the Group believes the rights and obligations exist given the progress of negotiations. There is therefore a high degree of judgement in: assessing the level of the cost contingencies to recognise; appropriately recognising variations and claims; and estimating the revenue recognised by the Group based on the projected final out-turn on contracts.

Our response – We evaluated the controls designed and implemented by the Group to monitor amounts owed on service and construction contracts, and in particular, the claims and variation elements across the Group. We attended a sample of, and inspected minutes from all, the Major Projects Committee meetings, which form a key part of the Group's risk process to fully challenge, at an executive level, both new tenders and contract bids and ongoing performance on existing contracts. We then selected a sample of contracts using a variety of quantitative and qualitative factors in order to assess and challenge the most significant and more complex contract positions. In this area our procedures, which varied by contract, included:

− considering the financial performance of the selected contracts against budget and historical trends to assess the historical accuracy of judgement in the recognition of claims and variations as well as the final out-turn on contracts;

− inspecting the contracts for key clauses, identifying relevant contractual mechanisms such as 'pain/gain' shares, liquidated damages and success fees and considered their impact on the completeness and existence of the amounts recognised in the financial statements;

− completing a number of site visits across the UK, Middle East and Canada, meeting local management, physically inspecting the stage of completion of individual projects and identifying areas of complexity through observation and discussion with site personnel;

− on the basis of detailed position papers obtained from the Group and conversations with senior operational, commercial and financial management, challenging the Group's estimates and judgements in respect of forecast construction contract out-turn, quantum and allocation of contingencies, settlements and the recoverability of contract balances with reference to our own assessments based on historical outcomes, third party evidence and industry norms;

− assessing the profile of aged work in progress on service contracts and challenging aged amounts for recoverability with a focus on claims and variations recognised on individual contracts;

− agreeing the above to correspondence and meeting minutes with customers around variations and claims, corroborating with assessments of these positions from the Group's legal or technical experts, if applicable; and

− we also considered the adequacy of the Group's disclosures in respect of these estimates and judgements.

Other revenue judgements (revenue £20.0 million (2015: Nil)) (New risk)

Refer to page 100 (note 2 Segments) and page 132 (note 31 accounting judgements and estimates).

The risk – The licensing agreement secured in 2016 was entered into at or around the same time and with the same counterparty to a series of contracts that extended the scope of the services provided by the Group's back-office outsourcing provider as described in the Strategic report on page 39. The income earned from this transaction was recognised immediately in revenue. The Group's rationale for these judgements is set out in the accounting estimates and judgements note 31. The risk is that:

1) the agreements are not independent, and that therefore the income should be spread over the term of the outsourcing contract;

2) there are ongoing obligations under the licencing agreement which indicate the income should be spread over some other period; and, or

3) the income earned should be recognised as other income, rather than revenue.

Our response – We first considered whether the licensing agreement could be deemed independent from extension to the scope of services provided by the Group's outsourcing provider by examining the terms of the respective contracts and meeting with the Group's legal advisors. Further, we sought to understand the cost of each of the respective elements and consider the fair value of each element, which included meeting with both the Group's third party expert and legal advisors to understand their valuation and benchmarking process for the outsourcing arrangements, the expert's historical experience and challenging the assumptions used.

We secondly considered whether there were any other ongoing obligations arising from the licence agreement which may indicate the income should be deferred. We assessed the nature and scope of the assets under licence, and examined the contractual agreements for ongoing arrangements or obligations.

We thirdly considered whether the licence income met the Group's accounting policy requirements to be deemed revenue (see accounting policies note 1) by comparing to similar historical transactions and the requirements of the accounting standards.

We finally considered the adequacy of the Group's disclosure of these transactions, both in the front end and the notes to these financial statements.

Carrying value of goodwill (£1,571.0 million (2015: £1,544.3 million)) Risk vs 2015: =

Refer to page 62 (Report of the Audit Committee), page 96 (note 1. Significant accounting policies – Goodwill and other intangible assets) and pages 109 and 110 (note 11. Intangible assets).

The risk – The Group's balance sheet includes goodwill, principally arising from historical acquisitions in the UK. The risk is that the goodwill allocated to cash generating units ('CGU') is not recoverable and should be impaired. Due to the inherent uncertainty involved in forecasting and discounting future cash flows, which are the basis of the assessment of recoverability, this is one of the key judgemental areas for our audit.

The Group annually carries out an impairment assessment of goodwill using a value-in-use model which is based on the net present value of the forecast earnings of the cash-generating unit (‘value-in-use’). This is calculated using certain assumptions around discount rates, growth rates and cash flow forecasts.

Given the relative size of the goodwill in the Group balance sheet, particularly in the UK Services CGU, relatively small changes in these assumptions could give rise to material changes in the assessment of the carrying value of goodwill.

Our response – Our procedures included critically assessing the key assumptions applied by the Group in determining the recoverable amounts of each CGU. In particular, we:

− considered the consistency and appropriateness of the allocation of businesses and related goodwill balances into CGUs;

− considered the underlying assumptions in determining the cash flows and growth assumptions applied with reference to historical forecasting accuracy and wider macro environment conditions;

− challenged the assumptions used in the calculation of the discount rates used by the Group, including comparisons with external data sources;

− performed our own sensitivity analysis, including a reasonably possible reduction in assumed growth rates and cash flows to identify areas to focus our procedures on and, sensitised the total discounted cash flows of the Group against the notional enterprise value of the group; and

− also assessed whether the Group’s disclosures about the sensitivity of the outcome of the impairment assessment to changes in key assumptions appropriately reflected the risks inherent in the valuation of goodwill.

Appendix B: Variable definitions

B.1. Measures of stock price crash risk

NCSKEW is negative one times the coefficient of skewness of firm-specific weekly returns estimated from equation (2) in a firm-year. Source: Datastream.

DUVOL is the natural logarithm of the standard deviation of down-week firm-specific weekly returns scaled by the standard deviation of up-week firm-specific weekly returns in a firm-year. Firm-weeks in which firm-specific weekly returns are below (above) the annual mean are classified as down-(up-)weeks. Source: Datastream.

COUNT is the number of crash weeks less the number of jump weeks in a firm-year. Firm-weeks in which firm-specific weekly returns are 3.09 standard deviations below (above) the annual mean are defined as crash (jump) weeks, where 3.09 is chosen to generate a weekly crash (jump) frequency of 0.1% in the normal distribution (Hutton et al., 2009). Source: Datastream.

COLLAR is the average payoff from a strategy of buying a put option and shorting a call option on firm-specific weekly returns in a firm-year, times 1000. The strike price is set to the average firm-specific weekly return minus (plus) 3.09 standard deviations for the put (call). Specifically, we use this equation:

$$COLLAR_{i,t} = \frac{1000}{n}\sum_{\tau=1}^{n}\left[max\left(0, \overline{W}_{i,t} - 3.09\sigma_{i,t} - W_{i,t,\tau}\right) - max\left(0, W_{i,t,\tau} - \overline{W}_{i,t} - 3.09\sigma_{i,t}\right)\right]$$

where $\overline{W}_{i,t}$ and $\sigma_{i,t}$ are the mean and standard deviation of firm-specific weekly returns on stock *i* in year *t*, respectively; $W_{i,t,\tau}$ is the firm-specific weekly return on stock *i* in week *τ* of year *t*; and *n* is the number of trading weeks on stock *i* in year *t*. This definition considers the frequency of actual crashes and jumps, as well as the average payoff in crash and jump weeks. Source: Datastream.

LTD measures the crash sensitivity of a stock by its lower-tail dependence with the market. Specifically, the *LTD* of stock *i* is given by

$$LTD = \lim_{q\to 0^+} Pr\left[r_i < F_{r_i}^{-1}(q) \middle| r_{UK} < F_{r_{UK}}^{-1}(q)\right]$$

where r_i is the daily return of an individual stock *i* and r_{UK} is the daily return of the Datastream UK market index, with corresponding marginal cumulative distributions F_{r_i} and $F_{r_{UK}}$. The marginal distributions of individual stock returns and market returns are estimated nonparametrically by their empirical distribution functions. We let $q = 10\%$ and compute daily *LTD* based on the combinations of selected copulas using a rolling window with one year of daily returns following Chabi-Yo et al. (2018). All the copula parameters are estimated via the canonical maximum-likelihood procedure. We take the average of daily *LTD* over a year to obtain annual *LTD* for each stock.

MCRASH captures a stock's sensitivity to extreme downside realizations of multiple risk factors in an asset pricing model. This measure is essentially the conditional probability that the stock realizes a left-tail event given that at least one of the risk factors realizes a left-tail event simultaneously. We follow Chabi-Yo et al. (2021) and define the multivariate crash risk (*MCRASH*) as follows:

$$MCRASH_i^{\boldsymbol{X}} = Pr\left[r_i \leq Q_q(r_i) \middle| \bigcup_{j=1}^{N} X_j \leq Q_q(X_j)\right]$$

where r_i is the daily return of an individual stock i, and $\boldsymbol{X} = (X_1, \ldots, X_N)$ denotes the daily returns of priced factors. $Q_q(r_i)$ and $Q_q(X_j)$ denote the upper q-quantile of r_i and X_j, respectively. Specifically, *MCRASH* represents the conditional probability that a stock return r_i does not exceed its q-quantile given that at least one of the factors X_j is also at or below its q-quantile. Following Chabi-Yo et al. (2021), we compute *MCRASH* in a seven-factor model that contains the market, size, value, profitability, and investment factors as in Fama and French (2015), the momentum factor as in Carhart (1997), and the betting-against-beta factor as in Frazzini and Pedersen (2014). We let $q = 10\%$ and apply the algorithm presented in Appendix B of Chabi-Yo et al. (2021) to estimate daily *MCRASH* for a stock using a rolling window with one year of daily returns. We then take the average of daily *MCRASH* over a year to obtain annual *LTD* for the stock.

B.2. Independent variables

Mandatory Adopter equals one for mandatory adopters and zero for non-adopters.

Post equals one for fiscal years ending on or after September 30, 2013, and zero otherwise.

RETURN is the mean value of firm-specific weekly returns in a firm-year, times 100. Source: Datastream.

SIGMA is the standard deviation of firm-specific weekly returns in a firm-year. Source: Datastream.

TURNOVER is change in average monthly stock turnover in the current year relative to the previous year, times 100, where monthly stock turnover is calculated as monthly trading volume scaled by the number of shares outstanding at the end of the month. Source: Datastream.

ACCRUAL is the signed value of discretionary accruals estimated from the modified Jones model (Dechow et al., 1995). Specifically, we estimate the following cross-sectional regression for each two-digit ICB industry with at least ten firms in a year:

$$TA_{i,t} = \beta_1(1/Assets_{i,t-1}) + \beta_2 \Delta Sales_{i,t} + \beta_3 PPE_{i,t} + \varepsilon_{i,t};$$

where *i* denotes firm and *t* denotes year;
TA = (change in current assets − change in cash − change in current liabilities + change in current debt − depreciation and amortization) / lagged total assets;
Assets = total assets;
Δ*Sales* = change in sales / lagged total assets;
PPE = gross property, plant, and equipment / lagged total assets.

The parameter estimates ($\hat{\beta}_1$, $\hat{\beta}_2$, and $\hat{\beta}_3$) from the above model are then used to compute discretionary accruals (*ACCRUAL*) for firm *i* in year *t*:

$$ACCRUAL_{i,t} = TA_{i,t} - \hat{\beta}_1(1/Assets_{i,t-1}) - \hat{\beta}_2(\Delta Sales_{i,t} - \Delta Receivables_{i,t}) - \hat{\beta}_3 PPE_{i,t}$$

where Δ*Receivables* = change in receivables / lagged total assets. Source: Worldscope.

LEVERAGE is the ratio of total debt to total assets. Source: Worldscope.

MTB is the ratio of market value to book value of equity. Source: Worldscope.

ROA is earnings before interest and taxes divided by total assets. Source: Worldscope.

SIZE is the natural logarithm of market capitalization (in pound sterling). Source: Worldscope.

ANALYST is the natural logarithm of one plus the number of analysts following the firm. Source: I/B/E/S.

BIG4 equals one if the firm is audited by a Big-4 auditor, and zero otherwise. Source: Audit Analytics.

TENURE is the natural logarithm of the number of consecutive years the auditor has served in the firm up to and including the current year. Source: Audit Analytics.

B.3. Additional control variables

Earnings smoothing is measured as the negative value of the correlation between changes in discretionary accruals and changes in pre-discretionary income over a rolling window of the prior five years. Discretionary accruals and pre-discretionary income are estimated following the model specified in Tucker and Zarowin (2006).

Accounting conservatism is measured using Khan and Watts's (2009) *C_Score*, which reflects the incremental timeliness of loss recognition relative to gain recognition in financial statements. A firm with a higher *C_Score* is considered more conservative.

Financial statement comparability is defined as the closeness with which two firms' accounting systems map economic events (as proxied by stock returns) onto financial statements (as evaluated by earnings). If two firms have comparable accounting systems, given similar economic events, they should produce similar financial statements. Following De Franco et al. (2011), we estimate each firm's accounting comparability with its peers in the same two-digit ICB industry as the average absolute difference between the predicted earnings of the two firms (estimated using the individual firm's accounting function but the same stock return) over past five years, then times -1. We calculate each firm's overall accounting comparability as the average of all of the firm's accounting comparability scores with its industry peers.

Tax avoidance is the negative value of the long-run cash effective tax rate, measured as the sum of income tax paid over the past five years scaled by the sum of pre-tax income less special items (Kim et al., 2011b). As a higher effective tax rate indicates a lower level of tax avoidance, we multiply the tax rate by -1 for ease of interpretation.

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