Does Enterprise Risk Management Bolster Investor Confidence? Evidence from Options-Based Restatement Contagion, Investment, and Misstatements

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Abstract

Our study investigates the effectiveness of enterprise risk management (ERM) in mitigating downside risk and enhancing investor confidence, in the setting of an external negative shock—industry restatement contagion. We find that ERM curbs overinvestment and earnings misstatements among peers during periods of undisclosed misstatements, subsequently restated, in their industries. Moreover, peers with ERM experience smaller changes in implied volatility skew (IVS) following industry restatement announcements. These effects are driven by firms with young CEOs, complex segment structures, low prior earnings performance, and in competitive industries. Overall, our findings highlight ERM's role in bolstering investor confidence by effectively managing firms' underlying risk outcomes.

> "As we work to create an entirely new regime for the **derivatives markets**, I want to be sure we continue our efforts to improve the existing structure of our equities markets – and to further **bolster investor confidence**. […] Unfortunately, in the last few years, investors have appeared less confident. […O]ne reason may be a perception that the markets themselves contain risks that some investors are not willing to take. […S]ources of risk [can be] corporate cultures that under-emphasize compliance and risk management. […To address the risk, i]nvestors deserve – and we will be looking for – a commitment by boards and executives to make **enterprise risk management** part of a firm or corporation's culture."
>
> — Mary L. Schapiro (Chair of U.S. Securities and Exchange Commission) on March 23, 2011

1. INTRODUCTION

Enterprise risk management (ERM) is an integrated approach for firms to manage their entire portfolio of interconnected risks. ERM features board risk oversight and defines a firm's risk appetite consistent with its overall strategy, which drives firm performance and value (COSO 2004, 2017). Since the early 2000s ERM has gained popularity in the corporate world (Jemaa 2022). In response to market inefficiencies and evolving conditions, Chair Schapiro of the Securities and Exchange Commission (SEC) created an initiative in 2011 to promote ERM as an approach to address downside risk and bolster investor confidence in the financial system.[1] Using a novel setting, this study explores whether ERM effectively reduces firms' downside risk and assesses investor perceptions of ERM as a valuable safeguard against such risk.

We focus on restatement contagion—an external negative risk shock from another firm's restatement within the same industry—as a setting to measure downside risk. We present several reasons for using this setting. First, undisclosed misstatements (resulting in subsequent restatements) can distort industry peers' decision-making regarding investment, pricing, and

[1] Chair Schapiro delivered a speech following the 2010 market flash crash which revealed critical issues in the financial markets. She promoted ERM to address flaws and inefficiencies in financial systems and structures, navigate market complexities, foster a culture of risk management, and enhance market stability and investor confidence amidst evolving conditions.

advertising (Beatty et al. 2013; Li 2016). Second, disclosed restatements induce diverse and economically significant contagion effects, including peers' shareholder wealth destruction (Palmrose et al. 2004; Gleason et al. 2008), deterioration in perceived credit quality (Files and Gurun 2018), and initiation of misstatements (Kedia et al. 2015). Third, restatement announcements represent a single-day risk shock, enabling options-based event studies to precisely gauge changes in investor perception around the shock. Lastly, this setting offers identification advantages. With an average of 19 restatements per year each affecting a distinct set of industry peers, we can effectively control for refined time-varying unobservable, restatement-specific unobservable, and firm-level time-invariant unobservable characteristics using restatement cohort fixed effects and firm fixed effects.

We expect ERM to mitigate restatement contagion by constraining firms from behaviors linked to potential future restatements or operational inefficiencies. Thus, firms with ERM are less prone to being misled by industry peers' undisclosed behaviors which lead to their subsequent restatements. We also anticipate that investors understand this relationship, perceiving ERM programs as effective safeguards against restatement contagion risk. We focus on two mechanisms through which ERM may constrain firm behaviors. First, aggressive financial reporting (resulting in restatements) can lead to inflated estimates of industry profitability, inducing industry peers to overinvest (Durnev and Mangen 2009; Beatty et al. 2013). ERM promotes independent investment decision-making constrained by firms' unique risk appetites, reducing susceptibility to herding behavior. Further, ERM assesses the external information environment (Li et al. 2024; Liu and Xu 2024) and employs risk-adjusted capital allocation (Ai et al. 2012), enabling better evaluation of their own and industry investment profitability. Additionally, ERM incorporates governance and

monitoring into all decision-making processes (COSO 2004, 2017; Lundqvist 2015), enhancing scrutiny of potential investments.

Second, undisclosed earnings misstatement practices spread from restating firms to peers (Gleason et al. 2008; Kedia et al. 2015). However, ERM's long-term strategic focus nurtures a corporate culture that tolerates short-term setbacks (Lam 2017; Xu and Xie 2018), reducing managerial short-termism and incentives to inflate earnings (Cohen et al. 2017; Eastman et al. 2020). Moreover, ERM's enhanced risk identification permits firms to identify new income-producing opportunities and ERM's focus on stakeholder communication helps investors form realistic expectations for firm performance, which further curbs earnings inflation incentives (Baxter et al. 2013; Callahan and Soileau 2017).

Alternatively, firms may superficially implement ERM as window dressing rather than a substantive strategy. Such nominal adoption may allow self-interested managers to bypass capital market financing, directing resources towards personal "pet" projects (Tufano 1998), potentially resulting in overinvestment. Such adoption could also incentivize managers to prioritize their utility functions (Jin and Jorion 2006), fostering an environment where managers engage in earnings misstatement for personal interests (Graham et al. 2005; He and Tian 2013).

To capture investor perceptions of contagion risk, we use derivatives markets and examine changes in the implied volatility skewness (IVS) of peers' stock options surrounding restatement announcements in their industry. Using the spread between the implied volatility of put and call options, IVS captures lower-tail downside risk because it reflects investor beliefs of future stock price crashes (Bates 1991). Moreover, IVS allows us to disentangle risk information from cash flow information, a historically challenging task when using market measures such as stock returns (Hann et al. 2019; Smith and So 2021). Given our focus on contagion risk, IVS allows us to isolate

risk information in a clean way. Additionally, restatements may trigger the release of previously withheld bad news by peer firm management, a behavior effectively captured by IVS (e.g., Kim and Zhang 2014).

Our empirical analysis consists of three stages. First, we identify firm-years as exhibiting (undisclosed) earnings misstatements, characterized by cumulative overstatements, when at least one quarter is subject to a future Item 4.02 non-reliance restatement (SEC 2004). Focusing on peers of these misstating firms within the same eight-digit Global Industry Classification Standards (GICS) industry, we manually collect their ERM status from press media and SEC filings, restricting our sample to firms on the Standard and Poor's 500 index (S&P 500).[2] Given the voluntary nature of firms' ERM adoption and disclosure decisions, we employ a propensity score matched (PSM) sample and estimate a difference-in-differences (DiD) model with firm and year fixed effects. We find that ERM adoption is associated with a 0.8% reduction in overinvestment relative to assets and a 5.7 percentage point decrease in the probability of misstatement from the pre- to post-ERM period for adopters compared to non-adopters. Our results indicate that ERM affects the economics and decision-making of peer firms, reducing their underlying risks.

Further, we perform two sets of cross-sectional tests using four partitioning variables. We find that ERM's impact on overinvestment is concentrated among peers with younger CEOs and peers with complex segment structures, firm types that are prone to overinvestment problems (Li et al. 2017; Ozbas and Scharfstein 2010). We also find that ERM's impact on the probability of earnings misstatement is concentrated among peers with low prior earnings performance and in

[2] We follow prior studies (Gleason et al. 2008; Kravet and Shevlin 2010) and use eight-digit GICS code to identify industry peers in a misstatement/restatement setting. As Gleason et al. note "Bhojraj et al. (2003) show that GICS classifications are better at explaining stock return co-movements, valuation multiples, forecasted and realized earnings growth rates, R&D expenditures, and various financial ratios (e.g., ROE) than are Standard Industrial Classification (SIC) codes or Fama and French (1997) industry." The advantage of GICS is consistent over time and most pronounced among large firms (Bhojraj et al. 2003), suitable for our study on S&P 500 firms.

more competitive industries, where incentives for earnings inflation are stronger (Dechow et al. 2011; Shi et al. 2018).

In the second stage of our analysis, we examine peer firms' market consequences following restatement announcements by other firms in their industry. We again employ a DiD research design using a PSM sample, and control for firm and restatement (i.e., cohort) fixed effects and additional peer and industry characteristics. We find that ERM adoption is associated with a decrease of up to 0.64 standard deviations of Δ*IVS* (i.e., restatement contagion) from the pre- to post-ERM period for adopters compared to non-adopters. This result indicates that option traders perceive a reduced susceptibility of ERM firms to restatement contagion. Using cross-sectional tests with the same four partitioning variables discussed above, we find that ERM's effect on the IVS change is also concentrated among peers with young CEOs, complex segment structures, low prior earnings performance, and in competitive industries. Thus, option traders behave as if they anticipate and understand ERM's role in mitigating firms' risk outcomes.

In the final stage of our analysis, we conduct several tests to reinforce our primary findings and address alternative explanations. First, we conduct parallel trends analyses for our DiD design, revealing no pre-trends and observing temporal alignment between shifts in risk outcomes and the onset of treatment. Second, we find no empirical evidence in support of an alternative explanation that firms disclose ERM in response to industry restatements. Third, we provide empirical and anecdotal evidence to mitigate concerns regarding non-disclosure biases in ERM adoption. Fourth, we show that ERM peers exhibit higher abnormal returns and lower abnormal bid-ask spreads in equity markets after industry restatements, consistent with our derivatives markets' results. Fifth, we test the impact of firms' initial ERM announcement on risk perceptions, observing a decrease in IVS for ERM firms relative to matched control firms. Sixth, we use a stacked DiD research

design, which confirms our primary inferences. Lastly, we confirm the robustness of our IVS results across various horizons and measures.

Our study contributes to ERM literature by emphasizing its role in determining firms' optimal risk level based on risk appetite, rather than merely minimizing *all* risks, as certain risks have upsides which represent opportunities (Hayne and Free 2014; Arena et al. 2017). Unlike prior studies (e.g., Eckles et al. 2014; Berry-Stölzle and Xu 2018), we isolate downside risk from firms' risk portfolio and support the theory that the primary goal of risk management is to protect against lower-tail outcomes (Stulz 2008, 2022). Further, to our knowledge, we are the first to examine how options market investors view ERM, finding that they perceive ERM as a buffer against value destruction.[3] We also contribute to disentangling ERM adoption vs. disclosure decisions by providing empirical and anecdotal evidence.

While prior ERM archival research often focuses on the financial industry (e.g., Hoyt and Liebenberg 2011; Baxter et al. 2013; Liu and Xu 2024), our study extends to the broader economy. Additionally, our study extends recent research by Lawrence et al. (2018) and Johnston and Soileau (2020). While they focus on financial reporting deficiencies and accruals estimation, respectively, we exploit restatement contagion as a plausibly exogenous risk shock to firms' downside risk environments. Frequent restatements within our DiD design enable robust fixed effects controlling for various unobservables while estimating within-firm ERM effects. We also extend beyond financial reporting measures, investigating how ERM impacts real investment decisions and market perceptions of firm risk as integral parts of the restatement contagion study.

Our study also adds to the restatement contagion literature by examining contagion effects during both the misstatement period and the subsequent restatement. Moreover, while most studies

[3] Prior studies suggest that informed investors prefer the options markets to the stock markets (Jin et al. 2012) given the options markets' unique benefits (Amin and Lee 1997; Cao et al. 2005).

identify the circumstances in which contagion may occur and document its consequences, little research offers any resolution.[4] We identify an institutional framework, ERM, as effective in mitigating restatement contagion risk, with an economically significant impact. Additionally, our study explores mechanisms underlying how ERM affects firm risk outcomes and investor perceptions, highlighting that these effects are concentrated among firms with specific characteristics.

Further, we adapt an option-based measure of expected crash risk to an event study setting, allowing us to gauge investors' *changes* in beliefs and pinpoint their reactions to a specific event. Our study differs from recent literature examining long-window *levels* of firms' expected crash risk unrelated to specific events (e.g., Kim and Zhang 2014; Kim et al. 2016). Additionally, our study relates to recent studies on implied volatility (IV) changes around events (Hann et al. 2019). We argue our measure is not susceptible to criticism by Smith and So (2022).[5] Unlike IV that captures both upside and downside risks, IVS specifically gauges downside risk, aligning closely with our research focus.

Finally, our study carries policy implications. We find that investors buy in and view ERM favorably using evidence from derivatives markets, in addition to equity markets. Importantly, our results demonstrate that investor confidence is rooted in ERM's ability to reduce firms' underlying risk outcomes. Hence, our findings lend support to the SEC's initiative in promoting ERM and

[4] There are differences between reducing restatements *per se* and reducing the contagion effect induced by other firms' restatements. The latter presents external shocks to peer firms who *may* be free from any restatement. Resolutions to reduce restatement (e.g., auditing, board independence, internal control) may not apply to restatement contagion. One article (Kedia et al. 2015) documents that the contagion disappears if the restating firm receives SEC actions or class action lawsuits. However, this is not a resolution that industry peers can use.

[5] Smith and So (2021) posit that measuring risk information as the simple IV change around an event (e.g., earnings announcement) entails significant measurement error because the metric entangles the predictable and unpredictable components in the change in investor uncertainty surrounding the event. Thus, the risk metric needs adjustments. However, they note that, for information events that are unanticipated by investors, the IV change measure is still appropriate because "investors do not impound the effect of the event on volatility into option prices prior to its release (page 389)." Since we argue that restatements are unanticipated by investors of industry peers, our IVS change measure is not subject to Smith and So (2022)'s criticism.

calling for more coordinated actions among regulators, market participants, and professionals to advocate for ERM adoption.

2. BACKGROUND AND HYPOTHESIS DEVELOPMENT

2.1 Enterprise Risk Management

Traditionally, firms manage risks arising from business units (e.g., departments, divisions, plants, and stores) within each unit in isolation. ERM improves on this "silo"-based approach by managing all the firm's risks in a risk portfolio and coordinating various risk management activities throughout the enterprise (e.g., Meulbroek 2002; SEC 2007; Hoyt and Liebenberg 2011). Thus, ERM explores the interdependencies among diverse sources of risks (i.e., business units or risk categories) to facilitate natural hedges and explore business opportunities.

An ERM program requires the commitment of the board of directors to their risk oversight responsibilities. ERM prioritizes and manages risks based on firms' risk appetites and the changing nature of each risk. The ERM framework incorporates strategy setting, performance, governance, internal control, and communication with stakeholders, providing a clear path to creating, preserving, and realizing value (COSO 2004, 2017; Jemaa 2022).

2.2 Why Would Investors Expect ERM to Mitigate Restatement Contagion?

ERM represents a sophisticated risk-management regime, suggesting that firms adopting ERM are likely to adhere to their risk limits more effectively. As a result, these firms should be more likely to make informed, tailored decisions and be less susceptible to peer influence. Consequently, firms with ERM are expected to be less prone to blindly following industry peers and engaging in activities that could precipitate restatement contagion. As such, ERM should impact economics and decision-making of peer firms, mitigating their underlying risks and generating real effects for them. We anticipate investors recognizing this correlation, perceiving

industry peers' ERM programs as effective safeguards against intra-industry restatement contagion. ERM may enact these constraints through two mechanisms.

2.2.1 The Impact of ERM on Deterring Overinvestment

Investment behavior is the first mechanism through which ERM may constrain firms from activities related to restatement contagion. Peer firms may overestimate industry profitability when observing aggressive reporting in their industries. In turn, these peers can be misled into overinvestment. Beatty et al. (2013) find that peers react to undiscovered fraudulent reporting in their industry by increasing capital expenditure during the fraud period. Once a firm discloses the misstatement, Durnev and Mangen (2009) document that announcements of more severe restatements induce larger subsequent decreases in industry peer investment, suggesting that the restatements revealed greater initial overinvestment among those peers. Kedia et al. (2015) posit that a misstatement results in peer overinvestment that is eventually revealed by subsequent disclosure of the misstatement.

ERM defines a firm's risk appetite depending on its unique internal environment (e.g., corporate culture, distribution channels, customer base, and brand recognition) and external environment (e.g., socioeconomic, geopolitical, and demographic trends) (Li et al. 2024; Elliot 2018). Board risk oversight ensures that this unique risk appetite guides and constrains a firm's overall risk-taking (Braumann et al. 2020).[6] Thus, firms with an ERM program are more likely to make independent investment decisions tailored to their own needs and are less subject to herding behaviors following industry-peer firms. Further, ERM employs a risk-adjusted basis in developing capital budgeting and resource allocation (Ai et al. 2012), leading to greater accuracy

[6] Paul Cunha, Vice President, Enterprise Risk Management for TD Bank Group, illustrates that their ERM program is based on a key principle of "independent oversight." To manage the bank's risk profile, TD utilizes a "three lines of defense" model that specifies the independent challenge and independent assurance roles of governance, risk, business, and oversight groups (Cunha and Narvaez 2015).

in identifying positive versus negative net present value (NPV) projects. Additionally, ERM integrates strong governance (Baxter et al. 2013; Lundqvist 2015) to control and monitor risk in all decision-making processes. Thus, an increase in investment in response to artificially inflated industry profitability will face greater scrutiny among ERM firms.[7]

2.2.2 The Impact of ERM on Deterring Earnings Misstatement

Second, ERM mitigates managerial short-termism, reducing managers' incentives to inflate earnings when intra-industry restatement contagion occurs. Industry-peer firms are more likely to misstate the same accounts as restating firms (Scholz 2014) and initiate income-increasing earnings misstatement following restatements in their industry (Kedia et al. 2015).[8] Restatement contagion is more pronounced among industry peers with higher accruals (Gleason et al. 2008) and markets become more sensitive to the discretionary component of peers' information risk following an industry restatement (Kravet and Shevlin 2010), suggesting that industry peers may have managed earnings during the misstated (or restated) period of restating firms.

Short-term earnings targets can incentivize earnings misstatement because of managers' personal wealth, reputation and career concerns, and pressures from analysts and peer firms

[7] The consulting firm, McKinsey & Company (Brinded et al. 2022), stresses the importance of enterprise-wide initiatives for improving capital management efficiency. Despite the strong link between a firm's capital expenditure management and its performance, McKinsey observes that most companies continue to struggle with capital management, viewing it as non-core business, and "executives find it difficult to predict the performance of individual projects and the capital project portfolio as a whole." McKinsey notes that "the root causes of pool performance" include misaligned "incentives and mindsets." Firms may exceed optimal investment levels when influenced by inflated financial performance of industry peers (Beatty et al. 2013), driven by incentives to pursue projects primarily to match peer performance. To overcome such inefficiencies and enhance overall capital strategy, McKinsey underscores the value of "**enterprise-wide strategies**" to "identify internal and external opportunities to strengthen their portfolio based on affordability and strategic objectives (emphasis added)." Given that ERM represents an enterprise-wide risk management regime and integrates with strategy and performance, we expect ERM to discourage firms from overinvesting due to their peers' financial performance.

[8] For example, restatements in the banking/insurance segments involved accruals and/or estimates 36% of the time. Healthcare and pharmaceuticals segment restatements involved issues related to debt 32% of the time; while financing issues, generally, were involved in 29% of restatements in energy/mining/chemicals. Computer/software restatements involved revenue and/or stock compensation 20% and 18% of the time, respectively. Restatements in wholesale and retail involved expenses 59% of the time, with leases (20%) and depreciation (20%) being the most common issues (Scholz 2014).

(Graham et al. 2005; He and Tian 2013). ERM emphasizes a top-down risk management framework so that the board of directors makes ultimate decisions regarding enterprise-wide risk management (e.g., SEC 2007; Braumann et al. 2020). ERM assures that firms' overall risk-taking and risk management activities are consistent with their mission, vision, and core values. ERM promotes a long-term view that facilitates sustainable firm development rather than myopic operations that attain short-term targets at the cost of strategic goals (COSO 2004, 2017). ERM ensures that managers' compensation schemes and performance evaluations reflect firms' overall long-run strategies.[9] Thus, ERM cultivates a corporate culture that tolerates short-term setbacks and protects managers from reputation losses due to temporary failures (e.g., missing short-term earnings targets) (Lam 2017; Xu and Xie 2018; Shadaei and Xu 2023).[10] Consequently, we expect that ERM firms are less susceptible to managerial short-termism and related incentives to temporarily inflate earnings. Supporting this view, prior literature documents that ERM enhances firms' financial reporting quality (Cohen et al. 2017; Eastman et al. 2020) and ERM firms generate smaller absolute abnormal accruals (Johnston and Soileau 2020).

In addition, ERM facilitates business opportunity identification when managing risks (Hoyt and Liebenberg 2011; Callahan and Soileau 2017).[11] ERM has a strengthened risk

[9] During our search process, we find that many firms indicate in their SEC filings that, under their ERM program, the board oversees the compensation committee's decision-making in managerial compensations as an integral part of firms' enterprise-wide risk management. For example, Hologic Inc. states in its 2016 proxy statement that: "the **Compensation** Committee worked with Mr. MacMillan (Chairman, President and CEO) to align the executive leadership team's individual performance objectives with the top three risks identified in the annual Enterprise Risk Management process." Similarly, Fortune Brands Home & Security, Inc., in its 2018 proxy statement, describes its ERM program as consisting of "periodic management discussions analyzing and mitigating risks, an annual review of risks associated with each of the Company's operating businesses and an annual review of risks related to the Company's **compensation** programs and practices (emphasis added)."

[10] Despite corporate culture typically evolving gradually, we expect ERM adoption to lead to rapid shifts. ERM prompts a reassessment of culture by executives, introduces organizational restructuring and governance changes, and represents strategic initiatives, all contributing to rapid cultural change (Grennan 2019; Gorton et al. 2022). Moreover, ERM can swiftly influence culture via its impact on managerial compensation, as discussed in footnote 11 (Graham et al. 2022).

[11] ERM firms usually use the SWOT (i.e., strengths, weaknesses, opportunities, and threats) analysis and the PESTLE (political, economic, sociological, technological, legal, and environmental) approach to identify opportunities (Elliot

identification strategy that screens for upside risks outside standard risk silos, and a centralized view that enhances performance by linking firm objectives to risk. Thus, ERM programs should well position firms to identify *new* income-producing opportunities.[12] We expect this identification of emerging opportunities to be on a continuous rather than a one-time basis. Further, ERM focuses on stakeholder communications regarding firms' risk profiles, business operations, and strategic goals that are consistent with their risk appetite (Elliot 2018; Li et al. 2024).[13] This communication should help investors form realistic expectations of firm performance. Overall, we expect that ERM's opportunity identification and stakeholder communications will further reduce managers' incentives to inflate earnings.

2.2.3 Reasons that ERM May Not Deter Overinvestment or Earnings Misstatement

It is possible that firms employ ERM as a form of window dressing. Such cheap talking in the media and SEC filings, if present, would suggest that firms implement ERM nominally rather than substantively. If this is the case, firms would not fundamentally change their corporate risk infrastructure but instead continue with the traditional silo-based approach. Such nominal ERM adoption lacks a top-down framework in which the board makes ultimate risk-taking and management decisions. Nominal ERM adoption without board risk oversight can lead managers to pursue projects that contribute to overinvestment. While risk management reduces the likelihood

2018). In a case study of Kilgore Customer Milling, a manufacturer of power window assemblies, the SWOT analysis in their ERM program was critical in enabling them to seize business opportunities and grow substantially (Nason and McPhie 2015). SWOT is often employed for each of the six PESTLE categories. The opportunity analysis considers new technologies, new distribution channels, unmet customer needs, etc., while the threat analysis evaluates the emergence of competitors, new regulations, tax increases, etc.

[12] Consistent with this view, previous literature empirically documents that ERM is positively associated with return on assets (ROA) (Baxter et al. 2013; Callahan and Soileau 2017), Tobin's Q (Hoyt and Liebenberg 2011), and positive abnormal returns (Baxter et al. 2013; Eastman and Xu 2021).

[13] For example, in a case study of Mars, Inc., one of the largest food companies in the world that employs 125,000 people in over 80 countries, Larry Warner (who had global responsibility for Mars' ERM program) discussed how a series of facilitated workshops that engaged various stakeholders in their ERM program helped in communicating firm risk preference, as well as strategic and operations objectives (Warner 2015).

that firms will have to resort to costly external financing (Froot et al. 1993), it may also help entrenched managers circumvent the scrutiny of external financing and secure funding for their "pet" projects to exploit private benefits (Tufano 1998; Zou 2010), leading to overinvestment. In addition, prior studies note that traditional risk management activities (e.g., hedging with derivatives or insurance purchases in the absence of the ERM framework) may be motivated by managers' own utility functions (Jin and Jorion 2006; Zou 2010). As managerial utility maximization is an important motive for earnings misstatement (Graham et al. 2005; He and Tian 2013), nominal ERM adoption creates an environment that facilitates earnings misstatement practices.

Taken together the pro- and counter-arguments, we formulate our hypotheses in their alternative forms regarding whether ERM affects firms' underlying risk outcomes:

H1a: *ERM adoption is associated with a decrease in overinvestment, during periods of industry misstatements that result in restatements.*

H1b: *ERM adoption is associated with a decrease in the probability of misstatement, during periods of industry misstatements that result in restatements.*

We expect that investors understand ERM's effect on firms' underlying risk outcomes, perceiving industry peers' ERM programs as effective safeguards against intra-industry restatement contagion. Hence, we develop our hypothesis in its alternative form as follows:

H2: *ERM adoption is associated with a decrease in the change in options investors' perceived risk following industry restatements.*

3. FIRM RISK OUTCOMES

3.1 Main Variable Measurements

We draw our industry peer sample from the set of firms listed on the S&P 500 index for any fraction of time from January 1, 1990, to June 30, 2022. We hand collect the ERM status for each sample firm through a keyword search in two major sources: (1) newswire and press media,

including Factiva, complemented by Google and other searches, and (2) all SEC filings (e.g., 10-Ks, 8-Ks, proxy statements, etc.) from Edgar and Thomson One databases.[14] We then manually review the search hits of each firm from different sources.[15] Building on prior studies, we construct a binary variable (ERM_i) equal to one if a firm *i* has adopted an ERM program, and zero otherwise (e.g., Hoyt and Liebenberg 2011; Johnston and Soileau 2020). Table A1 in Appendix A provides the definitions of all variables used in this study.

We examine two underlying risk outcomes of firms. The first measures overinvestment (Biddle et al. 2009; Durnev and Mangen 2020). Specifically, we estimate annual regressions of asset-deflated capital expenditures on prior year sales growth and industry fixed effects. We retain the residuals as a continuous measure of overinvestment (*OVERINV*), with higher values implying greater overinvestments. Our second outcome variable (*MISSTATE*) is equal to one if Audit Analytics identifies a firm as engaging in a misstatement during the year that is subsequently restated in Item 4.02 on the 8-K and includes a negative income adjustment, and zero otherwise.[16]

3.2 Research Design and Sample

We employ a PSM sample and conduct a DiD analysis to estimate a within-firm impact of ERM on peer firms' risk outcomes in response to undisclosed earnings misstatements in their industry that are subsequently restated.

3.2.1 Propensity Score Matching

[14] These keywords and their abbreviations include "enterprise risk management," "chief risk officer," "chief risk," "risk committee," "strategic risk management," "consolidated risk management," "holistic risk management," and "integrated risk management."

[15] We review the search hits/articles because sometimes firms mention ERM in a different context that is unrelated to whether the firm itself uses the ERM approach (e.g., introduction of an officer with prior ERM experience from a previous job, sales of ERM products to clients, etc.).

[16] To identify a year as a misstatement year, we require that at least one quarter be included in the misstated period of a subsequent Item 4.02 restatement with a negative adjustment to net income.

Firms self-select into ERM implementation for economic reasons. Thus, firms that choose to adopt ERM (i.e., adopters or treatment group) and those that choose not to by the end of our sample period (i.e., non-adopters or control group) may differ substantially ex-ante. As shown in Table A2 of Appendix A, our treated and control groups exhibit significant univariate differences across most characteristics which we later use in the risk outcomes regressions. To minimize these differences, we employ a PSM procedure to compare the change in risk outcomes for ERM adopters with a comparable counterfactual.

Specifically, we model the likelihood that a firm implements an ERM program based on 14 one-year lagged determinants identified in prior literature (e.g., Hoyt and Liebenberg 2011; Berry-Stölzle and Xu 2018; Li et al. 2024), using all S&P 500 firm-years with available ERM status information.[17] Table 1, Panel A reports the results. This logistic model achieves an 83% concordance rate, indicating that the resulting propensity scores successfully categorize firm-years as ERM and non-ERM classification.

We employ one-to-one matching with replacement and only retain matches falling within a caliper of 25% of the standard deviation of the logit of the propensity score (Cochran and Rubin 1973; Rosenbaum and Rubin 1973; Lunt 2014; DeFond et al. 2016).[18] Then, we identify the best match for each treated firm-year observation from among all potential control firm-year observations within the same eight-digit GICS industry. Specifically, for each treated adopter that implements ERM in year *t*, we identify a control non-adopter in the same year *t* with the closest

[17] The 14 ERM determinants include: total assets (*LOGASSETS*); leverage (*LEV*); segment Herfindahl–Hirschman index (*DIVSEG*); identifier for international operations (*INT*); institutional ownership (*IOR*); cash slack (*SLACK*); earnings volatility (*EARNVOL*); stock return volatility (*RETVOL*); change in market value (*VALCHG*); book-to-market ratio (*BM*); revenue growth (*GROWTH*); intangible asset percent (*INTANG*); 5-year percent of loss quarters (*NEG*); and average absolute discretionary accruals (*OPAQUE*). See Table A1 of Appendix A for variable definitions.
[18] Table 2.3.1 of Cochran and Rubin (1973) suggests that a caliper of 20% (40%) [60%] of the standard deviation of the logit of the propensity score will remove 98% (94%) [86%] of the bias in a normally distributed covariate. Our caliper choice of 25% is consistent with DeFond et al. (2016) who re-examine the Big N audit quality effect.

propensity score in the same industry. These matched firms constitute a pair, and we assign a "pseudo" ERM adoption year *t* to the control firm, aligning with that of the treatment firm.[19]

As shown in Table 1, Panel B, we find no statistical differences between treatment and control firms for ERM determinants in the year preceding ERM adoption, as well as the regression covariates used in the firm risk outcomes tests. This indicates the effectiveness of our PSM procedure.

3.2.2 Difference-in-Differences Model

To test our hypothesis, we employ the following DiD model and estimate a within-firm estimator, examining the impact of the change in the ERM adoption status on the change in ERM adopters' underlying risk outcomes, relative to non-adopters:

$$OUTCOME_{i,t} = \beta_0 + \beta_1 POST_{i,t} + \beta_2 ERM_i \times POST_{i,t} + \text{i.firm} + \text{t.year} + \text{Controls}_{i,t} + \varepsilon_{i,t}. \quad (1)$$

where $OUTCOME_{i,t}$ is either *OVERINV* or *MISSTATE* (defined above). ERM_i equals one for treated ERM adopters, and zero for matched control non-adopters. $POST_{i,t}$ equals one for all years beginning with the first full fiscal year following firms' ERM adoption (pseudo adoption year for matched control firms), and zero otherwise. We include fixed effects for each peer firm (i.e., i.firm), year fixed effects (i.e., t.year), and firm-year control variables (i.e., Controls).[20] We cluster

[19] Thus, we are matching on firm characteristics just prior to the ERM firm's adoption. We use the same control firm (i.e., the best match) for all years that the treated firm needs a match. If the "best" match becomes unavailable (e.g., due to acquisition or delisting several years after the treated firm's ERM adoption year *t*), we identify a new control firm with the second nearest propensity score for the treated firm in year *t* (i.e., the best match in this case). This second control firm is then used for all *remaining* years requiring a match for the treatment firm. If no suitable second control firm is available (i.e., no other potential matches meet the caliper requirement), the treatment firm exits the sample at the same time as its matched control firm. The majority of treatment firms have a single matched control firm during the entire sample period. One treatment firm requires a series of three matched control firms. The remainder require two matches.

[20] These controls include the natural log of market value (*SIZE*); book-to-market ratio (*BM*); earnings-price ratio (*EP*); debt-to-equity ratio (*DEBTEQ*); operating cash flows (*CFO*), return on assets (*ROA*), identifier for loss years (*LOSS*); return volatility (*RETVOL*); and annual return (*RET*). See Table A1 of Appendix A for variable definitions.

standard errors by firm.[21] In this, and all analysis, we winsorize continuous variables at the 1% and 99% levels.

3.2.3 Sample

Our analysis for firm risk outcomes spans from 2001 to 2022.[22] Using eight-digit GICS codes, we identify 1,764 industry-years in which Audit Analytics labels at least one firm as engaging in an undisclosed earnings misstatement that requires a negative income adjustment through a 4.02 non-reliance restatement in a subsequent year (i.e., 'Big R' restatement). Of those, 1,610 industry-years have available eight-digit GICS industry peers.[23] These peers (which we restrict to S&P 500 firms) comprise 8,421 observations for ERM adopters during our sample period (i.e., treatment firms) and 3,621 observations for non-adopters (i.e., potential control firms). We require data to compute all regression variables and apply our PSM procedure, resulting in 1,649 pairs of treatment and matched control firm-years over 634 misstatement industry-years. Panel A of Table 2 reports details of the sample selection process. Panel B presents summary statistics for the variables used in Eq. (1).

Panel C of Table 2 presents a univariate DiD analysis of the two risk outcomes for the PSM sample. Consistent with our setting of industry restatements due to earnings inflation, we observe significant overinvestment in the pre-ERM-adoption period for both treatment firms (1.14% of total assets; $p < 0.01$) and control firms (0.62%; $p < 0.01$). While control firms' overinvestment remains virtually unchanged in the post-adoption period (0.59%; $p < 0.01$), it becomes statistically

[21] We do not cluster by year due to the relatively short time series in our sample (i.e. 22 years) (Angrist and Pischke 2009; Berry-Stölzle et al. 2019). However, all reported results in this section are robust to two-way clustering standard errors by firm and year.
[22] Audit Analytics Item 4.02 restatement data begin in 2004 and the average length of a misstated period is three years. Thus, we include any misstatement that occurred in or after 2001 and was disclosed in a restatement in 2004 or later.
[23] Thus, we keep peer firms in the industries where at least one **different** firm is misstating earnings.

indistinguishable from zero for treatment firms. Treatment firms also show a significant reduction in overinvestment from the pre- to post-adoption period (-1.27%; $p < 0.01$) and relative to control firms (-1.24%; $p < 0.01$). Similarly, misstatement likelihood decreases significantly for treatment firms from 8.09% to 0.94% following ERM adoption ($p < 0.01$), while control firms show a marginally significant decline from 4.55% to 2.81% ($p < 0.10$). The resulting DiD estimate (-5.41%) is also statistically significant ($p < 0.01$). These findings provide initial evidence that ERM adoption mitigates firms' susceptibility to contagion risk outcomes. Panel D reports the sample composition by year and by industry.[24,25] Panel E reports the number of sample firms with an ERM program by year.[26]

3.3 Main Regression Results

Table 3 displays our DiD regression (Eq. (1)) findings in the PSM sample. Panel A uses *OVERINV* as the dependent variable. Initially, we replace firm fixed effects in Eq. (1) with eight-digit GICS industry fixed effects, allowing us to identify a main effect term for *ERM*. This yields an insignificant coefficient on *ERM*, indicating no difference in overinvestment between treated and control firms pre-ERM adoption. However, the coefficient on *ERM*×*POST* (-1.033; $p < 0.01$) is significantly negative, consistent with hypothesis H1a. When we estimate the full model with firm fixed effects, the coefficient on *ERM*×*POST* (-0.768; $p < 0.05$) indicates that ERM adoption is associated with a 0.768% decrease in overinvestment relative to assets from the pre- to post-

[24] The number of firms decreases notably in the last two years of our sample period. Recall, however, that the sample only includes industry misstatements that were discovered (and restated) during our sample period.
[25] Our sample is based on eight-digit GICS code matching. But due to space limitations, we do not report the sample distribution by eight-digit GICS industry codes in the study and report the aggregated two-digit code distribution instead.
[26] The number of firms with an ERM program does not strictly increase monotonically within our sample, as it might in the overall population. This is due to our sample being restricted to firms in industries with severe earnings misstatements. For example, the number of industries with misstatements drops off during the 2008-2009 period. Also, the number of industries with misstatements drops off towards the end of our sample period due to the relatively short window available for the identification of those misstatement (i.e., subsequent restatement).

ERM period for adopters compared to non-adopters. Excluding all control variables yields consistent results (-0.976; $p < 0.05$). Restricting the sample to treatment firms that switch their ERM status during the sample period (and their matched control firms) also yields consistent results (-0.755; $p < 0.10$).[27]

In Panel B, we repeat these tests with *MISSTATE* as the dependent variable. We again use a model with industry fixed effects and find an insignificant coefficient on *ERM*, indicating that treatment and control firms do not differ in their probability of misstatement prior to the adoption year. However, the coefficient on *ERM×POST* (-0.056; $p < 0.05$) is significantly negative, consistent with hypothesis H1b. When estimating the full model with firm fixed effects, the coefficient on *ERM×POST* (-0.057; $p < 0.10$) indicates that ERM adoption is associated with a 5.7 percentage point decrease in the probability of misstatement from the pre- to post-ERM period for adopters compared to non-adopters. This result is also robust to excluding all control variables (-0.057; $p < 0.10$) and restricting the sample to treatment firms that switch their ERM status during the sample period (and their matched control firms) (-0.057; $p < 0.10$).

3.4 Cross-Sectional Results

In Section 2.2, we present theorical arguments on the underlying mechanisms through which ERM affects firms' risk outcomes. We offer empirical evidence in this section.

3.4.1 Overinvestment

[27] We only observe a switch in ERM status from a non-ERM to an ERM status. Theoretically, a firm could switch from an ERM to a non-ERM status by discontinuing its ERM program after adoption. To address this possibility, we conducted additional searches using our ERM-related keywords along with terms such as "stop," "abandon," "cancel," "cancellation," "suspend," "suspension," "terminate," and "termination" for each sampled firm. However, our search did not yield any instances of firms switching from ERM to non-ERM status. Given our DiD model's within-firm estimation, identification primarily relies on firms that switch ERM status during the sample period. Thus, it is important to conduct robustness tests focusing on the restricted sample of firms.

We posit that ERM curbs corporate overinvestment by improving the assessment of both own and industry investment profitability. ERM achieves this through systematic evaluation of external information, independent risk-adjusted capital allocation, and effective communication across the organization. Since cross-functional communication is particularly beneficial for firms with complex segment structures and such firms are prone to investment inefficiency (Ozbas and Scharfstein 2010), we expect ERM's impact to be stronger for these firms. Further, ERM integrates governance and monitoring across all decision-making processes, resulting in enhanced scrutiny of potential investments. This represents an additional mechanism through which ERM mitigates overinvestment tendencies. Since young CEOs, due to career concerns, tend to engage in more investment (Li et al. 2017), ERM's robust governance should better constrain their incentives. Thus, we expect a more pronounced ERM effect in firms with younger CEOs.

To test these expectations, we partition our sample based on the annual industry median values of either firm segment Herfindahl index or CEO age. We classify the subsample with a segment Herfindahl index below (above) its annual industry medians, indicating high (low) segment diversification, as more (less) complex firms. Similarly, we identify the subsample with CEO age below (above) its annual industry medians as firms with younger (older) CEOs. We estimate Eq. (1) in each subsample and present results in Table 4, Panel A. As expected, we find that the ERM effect is only significant among more complex firms (-0.798; $p < 0.10$) and firms led by young CEOs (-1.462; $p < 0.05$). We also find that the magnitude of the ERM effect is statistically larger among younger relative to older CEOs ($p < 0.01$), however the ERM effect does not statistically differ between more and less complex firms.

3.4.2 Earnings Misstatement

Restatements spread earnings misstatement practices from restating firms to peers (Gleason et al. 2008; Kedia et al. 2015). However, ERM's long-term strategic focus fosters a culture that tolerates short-term setbacks (Lam 2017; Xu and Xie 2018), reducing managerial short-termism, career concerns, and incentives to inflate earnings (Cohen et al. 2017; Eastman et al. 2020). Given that firms with low prior earnings performance are more prone to myopic behavior (Dechow et al. 2011) and firms facing intense industry competition are susceptible to earnings misstatement due to managerial short-termism, job security, reputational concerns (DeFond and Park 1999; Shi et al. 2018), we expect a stronger impact of ERM among these firms.

In addition, ERM's enhanced risk identification enables firms to identify new income opportunities, while its stakeholder communication reduces incentives for earnings inflation (Baxter et al. 2013; Callahan and Soileau 2017). Given the importance of identifying opportunities in competitive industries, we also anticipate ERM's effect to be more pronounced in firms facing intense competition.

We categorize firms based on their prior-year asset-deflated EBIT relative to the annual industry median, identifying them as low (high) performing firms. Similarly, we classify industries with a Herfindahl index below (above) the annual industry median as the subsample with high (low) industry competition. Table 4, Panel B reports that ERM's impact on earnings misstatement likelihood is only significant among firms with low recent earnings performance (-0.091; $p < 0.05$) and those in more competitive industries (-0.115; $p < 0.05$).[28] We also find that the magnitude of the ERM effect is statistically larger among low relative to high performers ($p < 0.05$) and among high relative to low competition industries ($p < 0.01$).

[28] Using income before extraordinary items to measure low prior earnings performance leads to identical inferences.

Overall, we provide evidence that is consistent with ERM deterring overinvestment through cross-functional communication and governance mechanisms, while mitigating earnings misstatement tendencies by fostering an anti-short-termism corporate culture and identifying new opportunities with stakeholder communication.

4. RESTATEMENT CONTAGION

4.1 Main Variable Measurement

Our primary measure of restatement contagion is based on the difference between the implied volatility of put and call options on a firms' stock. Implied volatility is an intuitive measure of stock options' relative expensiveness (Black and Scholes 1973) and implied volatility skewness measures investors' ex-ante perception of the likelihood of a large decline in stock price (i.e., stock price crash risk) (Bates 1991).[29] Our dependent variable to measure restatement contagion is the 11-day change in the implied volatility skewness (ΔIVS) of the restating firm's industry peers surrounding restatement announcements.[30] See Appendix B for a more in-depth discussion of our construction of ΔIVS.

4.2 Research Design and Sample

[29] IVS measures the difference in the implied volatility of put options and call options on the same underlying stock. In the context of lower-tail risk, our particular focus, volatility skewness increases when the implied volatility of out-of-the-money (OTM) put options increases relative to at-the-money (ATM) call options. This difference in volatilities is primarily driven by the expectations and behavior of market participants. If investors expect a significant decrease in stock price, these investors may be willing to pay more for put options that would profit from that price drop. That increase in option price reveals investors' beliefs about the size of the expected drop in stock price (i.e., implied volatility) and likelihood that the put option will finish in the money. Hence, a higher IVS, higher put option implied volatility, higher put option price, and higher perceived likelihood that the put option will finish in the money all reflect the same underlying construct: investors' perception of a higher likelihood of a large future drop in stock price (Bates 1991). Prior studies also validate this approach by showing that volatility skewness predicts future realized stock crashes (e.g., Bradshaw et al. 2010; Xing et al. 2010).

[30] We measure this IVS change using average IVS over days t-5 to t-3 prior to the restatement announcement and days t+3 to t+5 post-announcement (Neururer et al. 2016).

We again employ a PSM sample and conduct a DiD analysis to estimate the within-firm impact of ERM on peer firms' IVS change in response to restatements by other firms in their industry that disclose previously misstated earnings.

4.2.1 Propensity Score Matching

We utilize the same ERM determinant model described in Section 3.2.1 (results reported in Table 1, Panel A) to calculate propensity scores. Then we employ a matching process equivalent to that used in the risk outcomes analysis, with the distinction that matches are performed within restatements.[31] This ensures that firms are matched based on not only industry, but also time and restatement characteristics. In Table 1, Panel C, we find no significant differences between treatment and control firms regarding ERM determinants in the year prior to ERM adoption or covariates (except for one variable, debt-to-equity ratio) used in the IVS regressions. This, again, confirms the effectiveness of our PSM procedure.

4.2.2 Difference-in-Differences Model

We collect all treatment and control peer firms attached to any single restatement as a unique cohort. To estimate the average ERM effect over all restatements, we stack all available restatements and the respective cohorts of peer firms. We include fixed effects for each unique restatement (i.e., cohort fixed effects) to control for restatement-specific characteristics (e.g., severity, cause, type of restatement), differences in restatement nature across restating firms and the corresponding industries, and time fixed effects.[32] We also include fixed effects for each peer firm, resulting in the following model:

$$\Delta IVS_{i,j} = \beta_0 + \beta_1 POST_{i,j} + \beta_2 ERM_i \times POST_{i,j} + \text{i.firm} + \text{j.restatement} + \text{Controls}_{i,j} + \varepsilon_{i,j}, \quad (2)$$

[31] We match a given treatment firm to all available control firms attached to the same restatement event. By construction, this approach identifies matches within industry.

[32] Within our PSM sample, we observe an average of 19 restatements per year, approximately one every 2.7 weeks. Thus, our restatement fixed effects provide more granular time controls than year fixed effects.

where $\Delta IVS_{i,j}$ is the change in implied volatility skew for peer firm *i* in the cohort of restatement *j*. *ERM* and *POST* are defined in Section 3.2.2. We also include i.firm (fixed effects for each peer firm) and j.restatement (fixed effects for each unique restatement). The control variables account for overall changes in uncertainty around restatements (Dennis and Mayhew 2002), capital market pressures (Gleason et al. 2008), and IVS determinants (Kim and Zhang 2014).[33] We two-way cluster standard errors by firm and restatement.

4.2.3 Sample

We begin with all 4.02 restatement announcements available on Audit Analytics from 2004 to 2021 and retain those with a cumulative negative adjustment to net income.[34] We remove restatements if we cannot match the restating firm to Compustat and CRSP. To ensure that the restatement conveys negative news, we follow prior literature (Gleason et al. 2008; Kravet and Shevlin 2010) and only retain a restatement if the three-day (-1, 1) announcement abnormal return of the restating firm is less than -1 percent. Next, we delete any restatement preceded by another non-reliance restatement in the same eight-digit GICS industry within 10 trading days. Drawing from all S&P 500 firms with available ERM status information, described in Section 3.1, we match peer firms to each restatement based on the eight-digit GICS industry code.

We screen out any potential peer that announced a non-reliance restatement during the 24 months preceding the restatement file date (Gleason et al. 2008) or that issues an earnings

[33] These variables include earnings-to-price ratio (*EP*), natural log of market value of equity (*SIZE*), book-to-market ratio (*BM*), debt-to-equity ratio (*DEBTEQ*), concurrent change in the implied volatility of ATM call options (ΔIV^{ATMC}), call option volatility prior to the restatement (LAG_IV^{ATMC}), average stock turnover (*STOCKTURN*), cash flow volatility (*CFVOL*), earnings volatility (*EARNVOL*), sales volatility (*SALESVOL*), volatility of idiosyncratic stock returns (*IDOSYVOL*), volatility of stock returns (*TOTALVOL*), market beta (*BETA*), negative skewness of stock returns (*NEGSKEW*), industry concentration (*HHI*), prior annual stock returns (*RET*), 5-day announcement abnormal return centered on the filing date (*ANNRET*), and an indicator for whether the peer firm shares the same national level auditor as the restating firm (*SAMEAUD*). See Table A1 of Appendix A for the variable definitions.

[34] Our restatement sample period ends on Dec. 31, 2021 because we require peers' ERM status as of their most recent fiscal year end.

announcement during the 11-days surrounding the restatement filing date. Finally, we require that the firm have non-missing data on OptionMetrics to compute Δ*IVS* and non-missing data to compute all control variables. This results in a sample of 825 unique restatements with 3,282 treatment firms and 1,353 potential control firms. We next apply PSM to generate our test sample, resulting in 762 pairs of treatment and matched control firms over 340 unique restatements. We report sample selection in Table 5, Panel A. Panel B presents summary statistics for the variables used in Eq. (2). Notably, both mean and median Δ*IVS* are positive, indicating that industry peers tend to experience increased option volatility skewness (i.e., downside risk) surrounding restatement announcements. Thus, there appears to be evidence of a restatement contagion effect within our setting.

Panel C of Table 5 presents a univariate DiD analysis for the change in IVS around industry restatement announcements. During the pre-ERM-adoption period, consistent with restatement contagion, the average Δ*IVS* is significantly positive for both treatment firms (1.04; $p < 0.01$) and control firms (0.78; $p < 0.05$). In the post-adoption period, control firms maintain a positive and significant Δ*IVS* (2.01; $p < 0.01$), whereas treatment firms exhibit a Δ*IVS* that is statistically indistinguishable from zero. Furthermore, treatment firms experience a significant decrease in Δ*IVS* from the pre- to post-adoption period relative to control firms (-2.06; $p < 0.05$). These results provide initial evidence that ERM adoption reduces firms' susceptibility to restatement contagion.

4.3 Main Regression Results

Table 6 reports the DiD model (Eq. (2)) results in the PSM sample. Initially, excluding firm fixed effects and including a main effect for *ERM* yields an insignificant coefficient on *ERM*. Thus, we find no evidence of a difference in investors' perception of downside risk between treated and control firms prior to ERM adoption. However, the coefficient on *ERM*×*POST* (-2.370; $p <$

0.01) is significantly negative, consistent with hypothesis H2. When we estimate the full model with firm fixed effects, the coefficient on *ERM*×*POST* (-4.043; $p < 0.01$) indicates that ERM adoption is associated with a decrease of 0.64 standard deviations of Δ*IVS* (-4.043/6.28) from the pre- to post-ERM period for adopters compared to non-adopters.[35] This result is also robust to excluding the control variables (-3.314; $p < 0.05$), and restricting the analysis to treated firms that switch their ERM status during the sample period and their matched controls (-4.159; $p < 0.01$). Together, these findings indicate consistent investor beliefs that ERM mitigates downside risk shocks from industry peer restatements.[36]

4.4 Cross-Sectional Results

In Section 3.4, we explore cross-sectional variations of how ERM affects firms' risk outcomes. Here, we extend this analysis to the setting where industry misstatements are disclosed through subsequent restatements. Specifically, we divide the restatement contagion sample using the same four partitioning variables from the risk outcomes tests and estimate Eq. (2), reporting the results in Table 7. Consistent with the risk outcomes results, ERM's impact on restatement contagion is only significant among firms with complex segment structures (-7.306; $p < 0.10$), younger CEOs (-7.658; $p < 0.05$), low prior earnings performance (-6.078; $p < 0.05$), and in competitive industries (-7.085; $p < 0.01$). The magnitude of the ERM effect also differs statistically ($p < 0.05$) between the high and low groups in all partitions except segment complexity. This suggests that option traders behave as if they anticipate ERM's role in curbing behaviors among industry peers, such as overinvestment and earnings misstatement, which could lead to restatement

[35] This effect size estimate is based on the residual variation in Δ*IVS* (i.e., 6.28) after controlling for fixed effects. Using the raw variation (i.e., 9.20) results in an effect size of about 0.44 standard deviations.

[36] In untabulated analysis, we perform several regression diagnostics for influential observations, based on Cook's D, DFITS and DFBETA values. Together, these tests indicate that our results are robust to the exclusion of influential observations, and that a conservative lower bound for the average ERM treatment effect size is about 0.28 standard deviations of the residual variation in Δ*IVS*.

contagion. Our findings also suggest that investors seem to understand how ERM affects firm risk outcomes, shedding light on the mechanisms through which ERM mitigates restatement contagion.

5. ADDITIONAL ANALYSIS

5.1 Parallel Trends Analysis

To validate our DiD design, we conduct a test for the parallel trends assumption. The identification is achieved if the assumption is unviolated. We estimate the same specification as presented in Eq. (1), except that we interact firms' *ERM* with each year from *t*= -5 to *t*= 4 relative to the first full year (*t*= 0) of their ERM adoptions, using year *t*= -5 as the benchmark. Figures 1 and 2 (upper) present the coefficients of the interaction between *ERM* and each above-mentioned year (with 90% confidence intervals) for the overinvestment regression and misstatement likelihood regression, respectively. The coefficient estimates are statistically insignificant in the years leading up to the first full year of ERM adoption, suggesting that in the absence of treatment, risk outcomes of treated and control firms move in parallel. Thus, we find no pre-trends, supporting the parallel trends assumption and alleviating concerns regarding omitted variables and self-selection particularly.[37] Moreover, in both tests the shift in outcome differences between treated and control groups initiates at the onset of treatment (*t*= 0), with the differences remaining statistically significant and fairly persistent.[38]

5.2 Restatements and Peers' ERM Disclosure Decision

[37] The parallel trends assumption can be expressed as $cov\ (ERM_i, \varepsilon) = cov\ (POST_{i,t}, \varepsilon) = cov\ (ERM_i \times POST_{i,t}, \varepsilon) = 0$. Thus, the assumption is essentially a "no correlated omitted variable assumption" (Roberts and Whited 2013; Armstrong et al. 2022). By supporting this assumption, we mitigate omitted variable concerns. Moreover, self-selection is a form of omitted variable problem because decisions to adopt ERM are a result of self-selecting into favored choices based on private information that is unobservable (Li and Prabhala 2007; Che et al. 2022). Hence, the findings from our parallel trends analysis also alleviate the self-selection concern, in particular.

[38] This temporal alignment is crucial, as moving further away from the treatment onset increases the likelihood of other confounders influencing outcomes, thereby posing a threat to the internal validity of parallel trends (e.g., Roberts and Whited 2013). Hence, this timing provides assurance that endogeneity is improbable in driving our results.

We consider an alternative explanation for our result of a negative association between ERM and changes in IVS surrounding industry restatements: the possibility that peers' voluntary decisions to disclose ERM programs are driven by restatement announcements in their industries. Although the parallel trends analysis alleviates this self-selection concern to a certain extent, we present the following argument and design a model to address this issue directly.

First, we argue that our risk outcomes analysis serves as an effective laboratory to address this issue. It tests ERM's impact during periods of undisclosed misstatements that are subsequently restated. In essence, during these test periods, industry peer firms do not have the opportunity to observe restatements, which are only disclosed in the future. Therefore, decisions regarding ERM disclosure should be independent of industry restatements, and (future) restatements within the industry are unlikely to influence peers' (current) ERM decisions.

Further, we estimate regressions to examine whether industry restatements predict firms' ERM announcement decisions. We collect firm-quarters for treatment firms for whom we can identify the quarter of their initial ERM announcement, and all non-adopters. The dependent variable is an indicator (*ANNOUNCE*) that equals one if a peer announces ERM adoption in a quarter, and zero otherwise. We employ a hazard-type model by dropping all firm-quarters for adopters following their initial ERM announcement. Next, we create an indicator (*INDRES*) that equals one if a restatement is announced in the peer's industry during a quarter, and zero otherwise. We then estimate six regressions, controlling for the ERM determinants from Table 1 as well as firm and quarter fixed effects, to predict ERM announcement probability based on whether there is any industry restatement in each of the recent five quarters individually, as well as combined. The results in Table 8 show that the coefficients of current, one-, two-, three-, and four-quarter lagged *INDRES* are consistently insignificant. Estimating the model at the annual level indicates

that industry restatements over the current and prior four years also do not predict firms' ERM decisions. Together, our findings alleviate concerns regarding the alternative explanation.[39]

5.3 ERM Disclosure vs. ERM Adoption

We rely on the first ERM announcement obtained from SEC filings and news media releases to determine firms' initial ERM adoption year. It is possible that there are delays in firms' announcement following adoption, leading to a mismatch between ERM adoption and announcement and introducing non-disclosure biases. To mitigate this concern, we further exploit SEC Rule 33-9089, effective from February 2010, which mandated firms to discuss the board of directors' role in risk oversight, risk-based compensation policies, and communication between the board and management on risk management issues. Given the oversight features of ERM and the alignment of our ERM measure's keyword composition with the SEC Rule, we expect firms to make timely disclosures of their ERM programs in the post-2010 period.[40] Thus, we remove all observations for ERM adopters (and matched controls) that announce their programs prior to 2010 and re-estimate our main tests. We report the results in Table 9 and continue to find a smaller contagion effect for ERM adopters based on overinvestment (-0.819; $p < 0.10$), misstatement probability (-0.063; $p < 0.05$), and changes in *IVS* (-3.462; $p < 0.10$).

[39] In untabulated analysis, we also estimate an alternative model based on a Cox proportional hazard model. We model the duration from January 1, 2004 until the initial ERM announcement (or end of sample for non-announcement) conditional on the duration from January 1, 2004 until a firm's initial observation of an industry peer restatement announcement, controlling for ERM determinants (See Table 1) in the last fiscal year prior to January 1, 2004. Controlling for the 14 ERM determinants, we find no evidence that firms announce an ERM program sooner when they observe an industry peer restatement announcement. The start date of January 1, 2004 reflects the earliest availability of required restatement announcement dates (i.e., March 2004).

[40] Given that ERM emphasizes board-level commitment and oversight (Braumann et al. 2020; Eastman et al. 2024), compliance with this SEC Rule essentially necessitates firms to disclose their ERM program details, if any. Moreover, consistent with SEC Rule's requirements in board risk oversight, the establishment of a board-level risk committee, composed of board members, is a key characteristic we employ to identify ERM adoption. Additionally, consistent with board risk oversight and board-management communication requirements, the creation of a chief risk officer position, often resulting from a board decision, serves as another significant indicator of our ERM adoption measure. Therefore, we argue that the SEC Rule effectively ensures that firms disclose their ERM initiatives, if present.

Moreover, our parallel trends analyses in Section 5.1 indicate that improvements in risk outcomes coincide with firms' initial ERM adoption, mitigating non-disclosure concerns.[41] Further, due to the SEC Rule's mandate, firms tend to disclose their pre-2010 adoption in their post-2010 filings, which allows us to track back the first year of ERM adoption.[42] In addition, when disclosing their ERM programs, many firms use statements that clearly indicate the onset of implementation.[43]

While the parallel trends in the pre-ERM period are insignificant in Figures 1 and 2 (upper), they do exhibit some downward momentum leading up to the first full year of ERM adoption. This pattern suggests that there may be a disclosure delay among at least some ERM adopters. To further investigate this possibility, we repeat the parallel trends analysis on a sample of firms that were required to disclose their risk management practices under the SEC Rule. Figures 1 and 2 (lower) demonstrate that within this reduced sample, there are no apparent downward trends starting from t = -1 for both investment and misstatement outcomes, with the pre-trends coefficient estimates generally hovering closer to zero. This evidence indicates that after the SEC Rule, the self-selection issue related to ERM disclosure choice is mitigated, making post-Rule a cleaner sample period less subject to this issue. Overall, our empirical and anecdotal evidence above collectively suggests that the non-disclosure biases are unlikely to confound our findings.

[41] If our results were subject to non-disclosure biases, we would have observed a mismatch between the time when the outcomes of overinvestment and earnings misstatement manifest (driven by the actual adoption) and the timing of our identified ERM adoption (which may merely reflect the disclosure and deviate from actual adoption). Hence, our results suggest that our collection of ERM adoption years seems to appropriately capture the actual adoption.

[42] For example, Ball Corp. disclosed in its 2010 proxy statement that it started to implement ERM in 2007. Thus, we can correctly capture Ball Corp.'s initial ERM adoption year as 2007, rather than using the disclosure year (2010) as the adoption year.

[43] For instance, a news article states that "**Newly Created Role** Underscores Monster's Commitment to Global Risk Management and Compliance: Monster Worldwide, Inc., (Nasdaq: MNST) **today announced the appointment** of Timothy P. Spillane to the position of Chief Risk Officer." Also, in CSX Corp.'s 2005 Form 10-K, they disclosed: "In 2005, the Company **began** implementation of a formal Enterprise Risk Management ("ERM") program in order to identify, quantify, monitor, and potentially mitigate these risks (emphasis added)." The evidence suggests that these firms adopt and disclose the ERM program at the same time.

5.4 Stock Market-Based Tests

We expand our analysis to examine whether ERM is associated with peer firms' equity market reactions following an industry restatement announcement. We employ two measures: abnormal stock returns (*ABRET*) and abnormal bid-ask spreads (*ABSPRD*).[44] Since the stock market-based analysis does not require option data, it allows us to construct a new, less restrictive PSM sample following the procedure outlined in Section 4.2.1. From this sample, comprising 890 treatment firms with matched controls, we estimate Eq. (2) using *ABRET* and *ABSPRD*, reported in Table 10.[45] The findings are consistent with those of the option-based tests. ERM adoption is positively associated with abnormal return (0.414; $p < 0.10$) and negatively associated with abnormal bid-ask spread (-0.153; $p < 0.05$). Our results suggest that shareholders in the equity market exhibit reactions similar to option traders in the derivatives market, perceiving industry peers' ERM programs as effective in mitigating the risk of restatement contagion.

5.5 ERM Adoption Announcement and Change in Option IVS

To alternatively assess how investors perceive ERM as safeguarding firms' downside risk, we employ an event study independent of restatement contagion and examine option traders' response to firms' initial ERM program announcement. We identify ERM adopters with exact announcement dates and match each adopter to a non-adopter control firm. This results in a sample of 251 announcement firms with 251 matched controls that satisfy data requirements. We compute

[44] First, we examine peers' abnormal stock returns following a restatement announcement in their industry. We use a market model and estimate firm-specific regressions of daily stock return on the daily value-weighted market return over a (-126, -20) estimation window prior to the announcement. We use these estimated parameters to compute peers' abnormal return (*ABRET*) over the (0, 1) restatement announcement window. Second, we estimate each peer's average abnormal bid-ask spread during the same (0, 1) announcement window. Abnormal spreads are based on firm-specific regressions of daily natural log of bid-ask spread on daily volume, closing price, volatility (i.e., natural log of the daily squared return), and market volatility (i.e., natural log of the daily squared market return). We again use the (-126, -20) estimation window to obtain the parameters which we use to compute the abnormal spread (*ABSPRD*).

[45] In these tests, we exclude ΔIV^{ATMC} and LAG_IV^{ATMC} as control variables because they require option data, and exclude the stock market announcement return control, *ANNRET*.

IVS changes around ERM announcements for the treated and control firms and regress it on the *ERM* indicator and controls.[46] Table 11 reports the results. The coefficient on *ERM* is significantly negative both without (-1.687; $p < 0.01$) and with industry and year fixed effects (-1.592; $p < 0.05$). Thus, we find a decline in perceived riskiness associated with ERM adoption, itself, corroborating the inferences of our primary tests.

5.6 Stacked Difference-in-Differences

Given the staggered nature of treatments in our sample, it is essential to account for potential biases that may arise if treatment effects vary over time (Baker et al., 2022). To mitigate this concern, we re-estimate Eq. (1) using a stacked DiD design based on the PSM sample. Following the method in prior studies (Cengiz et al., 2019; Li et al., 2024), we construct 22 cohorts of panel datasets, corresponding to each ERM adoption year from 2001 to 2022. For each adoption year *t*, we form a cohort of treatment firms adopting ERM in *t* and their matched control firms, then stack all cohorts to produce a final sample of 13,237 firm-years. This approach incorporates separate firm fixed effects and year fixed effects for each cohort. The results, reported in Table 12, reaffirm our primary findings: the coefficient on *ERM*×*POST* remains significantly negative for both overinvestment (-0.858; $p < 0.05$) and misstatement likelihood (-0.088; $p < 0.01$).

We follow a similar process to generate a stacked sample of annual cohorts of restatement announcements to examine the change in IVS. This results in a sample of 5,378 restatement-firm observations. We continue to include cohort-firm fixed effects, but now include cohort-restatement

[46] We include the control variables in Eq. (2), industry fixed effects, and year fixed effects. We cannot include firm fixed effects because we have only one observation per firm. We exclude the *SAMEAUD* control because there is no restatement event in this analysis. We cluster standard errors by industry.

fixed effects, consistent with Eq. (2). The coefficient on *ERM*×*POST* also continues to be significantly negative with Δ*IVS* as the dependent variable (-3.349; $p < 0.05$).[47]

5.7 Other Robustness Tests

In Table 13, we confirm the robustness of our IVS results when extending post-restatement horizons of up to 60 days, employing market-adjusted measures of the change in IVS, controlling for prior audit issues, and utilizing two alternative approaches to measure IVS.[48]

6. CONCLUSION

ERM is an integrated approach that firms use to manage their entire portfolio of risks. Our research examines whether investors perceive ERM as safeguarding firms against downside risk. We focus on an external negative risk shock to firms where they face contagion effects arising from restatements of other firms in their industries. We find that ERM reduces peers' overinvestment and likelihood of earnings misstatements during periods of undisclosed misstatements in their industries which lead to subsequent restatements. Moreover, peers with ERM experience a smaller change in IVS surrounding industry restatement announcements. ERM's effect on peers' risk outcomes (overinvestment and misstatements) and investors' perception on these risks (IVS change) are concentrated among peers with young CEOs, complex segment structures, low prior earnings performance, and in competitive industries. Overall, we document that ERM bolsters investor confidence in the financial markets and the confidence is rooted in ERM's ability to reduce firms' underlying risk outcomes.

[47] The included cohort-year fixed effects subsume *POST* in Eq. (1) because pre- and post-adoption periods align by year within each cohort. However, the included cohort-restatement fixed effects in Eq. (2) do not subsume *POST* because of differences in adopting firms' fiscal-year-end dates in the adoption year. We include *POST* when estimating Eq. (2) on the stacked sample of cohorts but do not tabulate it.

[48] First, we use the single call with delta closest to 0.50 and single put with delta closest to -0.20. Second, we adopt an open interest-weighted IVS derived from all options that meet our criteria with maturities less than 180 days.

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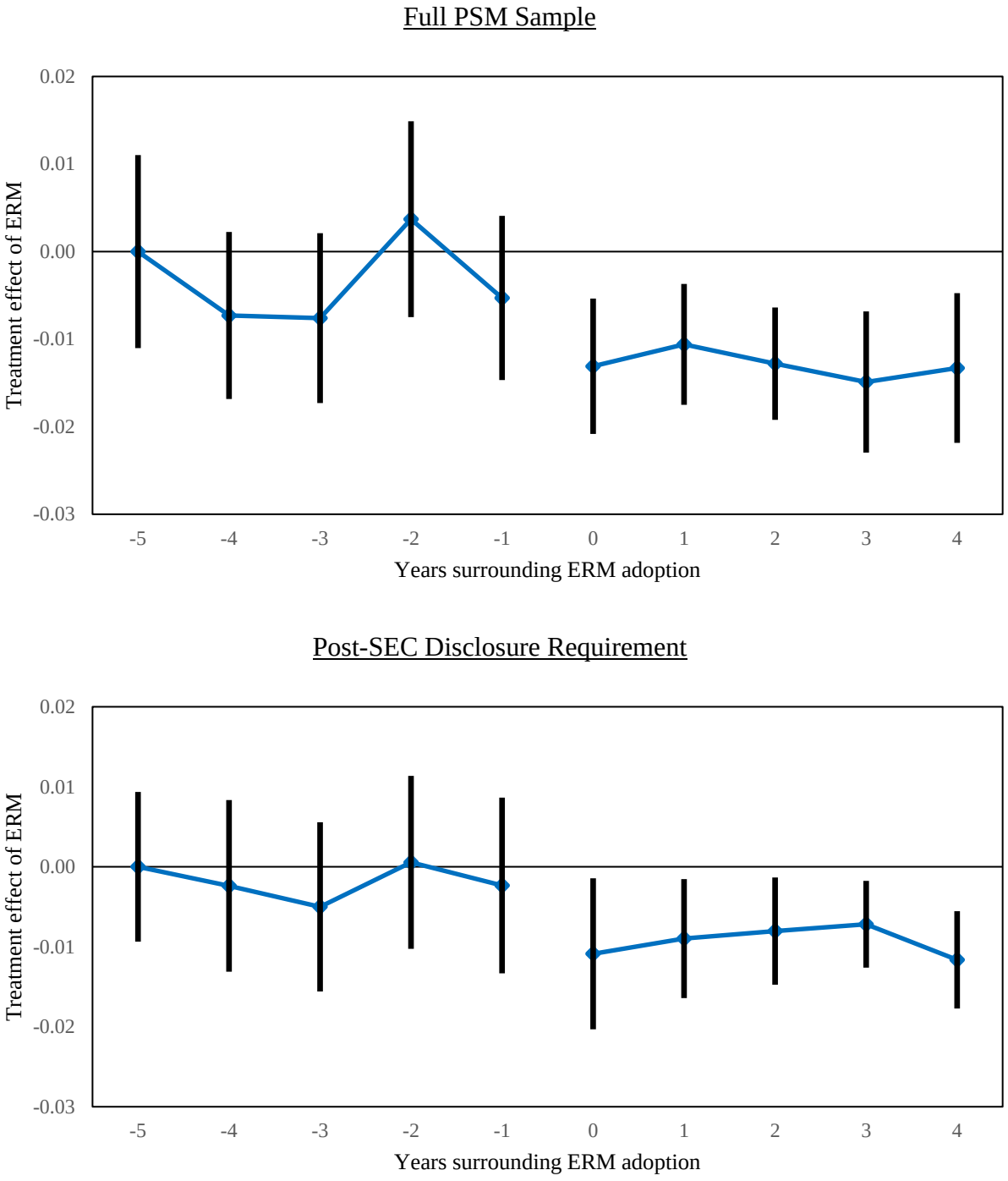
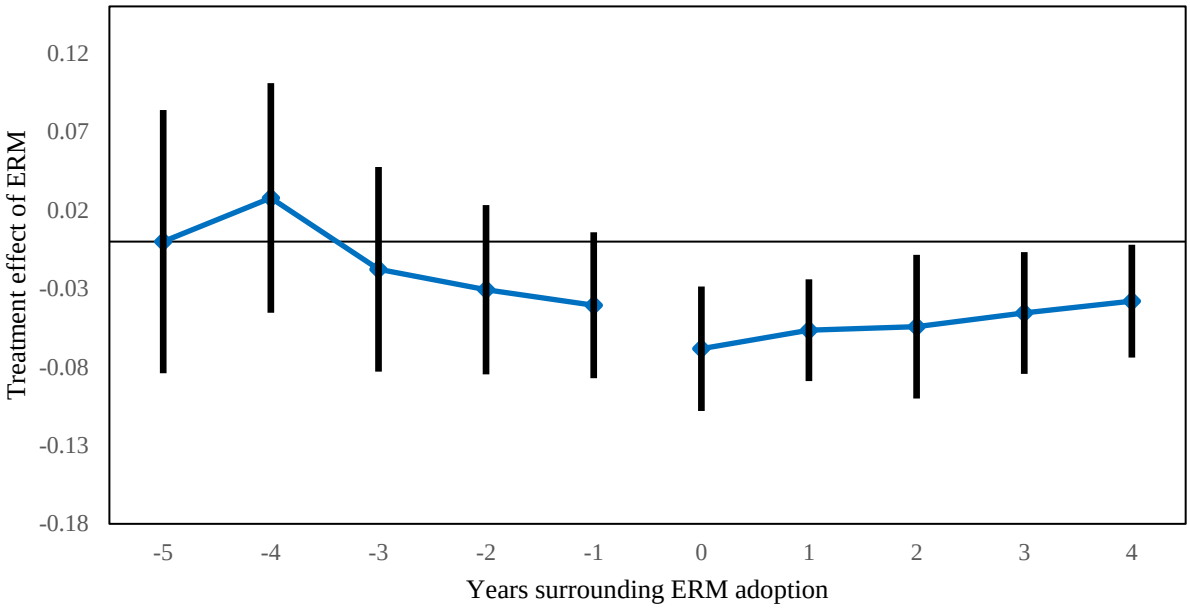
Figure 1. ERM effect on firms' overinvestment by years surrounding ERM adoption



This figure reports estimates of the ERM treatment effect on firms' overinvestment by years surrounding ERM adoption, for the full sample and post-SEC disclosure requirement sample. We estimate the specification as presented in Eq. (1), but interact firms' *ERM* with each year from *t*= -5 to *t*= 4 relative to the first full year (*t*= 0) of their ERM adoptions. Using year *t*= -5 as the baseline, we normalize all coefficients of the interactions between ERM and each year of the (-5, 4) window by subtracting the coefficient value of ERM from year = *t*-5. We include control variables and firm fixed effects. We include 90% confidence intervals based on standard errors clustered by firm. Variables definitions are in Table A1 of Appendix A.

Figure 2. ERM effect on firms' misstatement likelihood by years surrounding ERM adoption

Full PSM Sample



Post-SEC Disclosure Requirement

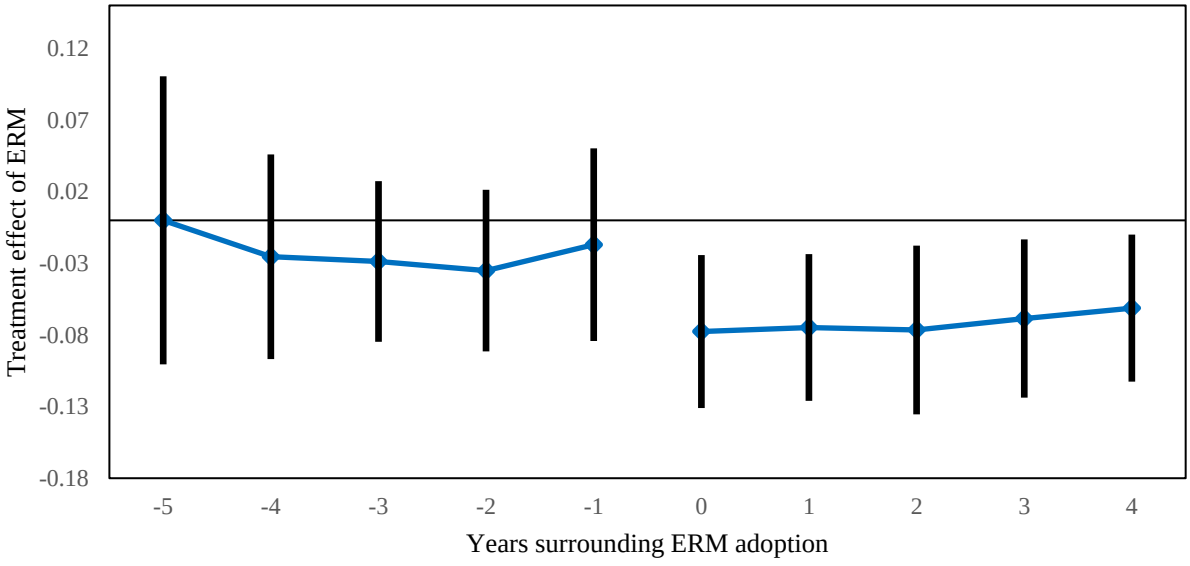


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Table 1: Propensity Score Model

Panel A: PSM estimation

	Coefficient	Std Error	*p*-value
Intercept	-8.836	0.160	<.0001
ASSETS	0.813	0.015	<.0001
LEV	-0.031	0.006	<.0001
DIVSEG	0.168	0.057	0.0029
INT	0.576	0.038	<.0001
IOR	0.027	0.030	0.3646
SLACK	1.651	0.143	<.0001
EARNVOL	2.980	1.183	0.0117
RETVOL	-0.301	0.401	0.4529
VALCHG	-0.068	0.038	0.0770
BM	0.068	0.052	0.1855
GROWTH	-3.160	0.138	<.0001
INTANG	1.649	0.090	<.0001
NEG	0.389	0.093	<.0001
OPAQUE	-0.190	0.161	0.2386
Percent Concordant	83.0%		
Percent Discordant	17.0%		
N	24,898		

Panel B: ERM determinants and covariate balance for firms' risk outcomes analysis

ERM determinants in year prior to ERM adoption				Firm-year covariates			
	Adopters	Controls	*p-value*		Adopters	Controls	*p-value*
ASSETS	8.933	8.816	*0.34*	*SIZE*	8.811	8.810	*0.99*
LEV	0.842	0.792	*0.71*	*BM*	0.408	0.437	*0.45*
DIVSEG	0.676	0.650	*0.60*	*EP*	0.016	0.023	*0.45*
INT	0.745	0.757	*0.84*	*DEBTEQ*	0.913	0.872	*0.79*
IOR	0.778	0.772	*0.83*	*CFO*	0.106	0.111	*0.57*
SLACK	0.133	0.136	*0.85*	*ROA*	0.048	0.052	*0.59*
EARNVOL	0.014	0.015	*0.80*	*LOSS*	0.136	0.136	*1.00*
RETVOL	0.093	0.092	*0.86*	*RETVOL*	0.048	0.047	*0.70*
VALCHG	0.251	0.336	*0.19*	*RET*	0.058	0.071	*0.32*
BM	0.397	0.429	*0.48*				
GROWTH	0.116	0.092	*0.30*				
INTANG	0.237	0.234	*0.92*				
NEG	0.131	0.145	*0.64*				
OPAQUE	0.134	0.126	*0.44*				

Panel C: ERM determinants and covariate balance for ΔIVS analysis

ERM determinants in year prior to ERM adoption				Firm-year covariates			
	Adopters	Controls	*p-value*		Adopters	Controls	*p-value*
ASSETS	8.946	8.747	*0.310*	*EP*	0.031	0.038	*0.548*
LEV	0.662	0.544	*0.247*	*SIZE*	9.081	8.935	*0.469*
DIVSEG	0.638	0.657	*0.771*	*BM*	0.325	0.382	*0.196*
INT	0.853	0.858	*0.929*	*DEBTEQ*	0.809	0.519	*0.056*
IOR	0.783	0.834	*0.146*	ΔIV^{ATMC}	3.476	3.831	*0.695*
SLACK	0.151	0.161	*0.756*	LAG_IV^{ATMC}	0.325	0.319	*0.738*
EARNVOL	0.013	0.018	*0.348*	*STOCKTURN*	0.204	0.201	*0.884*
RETVOL	0.087	0.088	*0.918*	*CFVOL*	0.044	0.046	*0.735*
VALCHG	0.201	0.330	*0.204*	*EARNVOL*	0.047	0.052	*0.649*
BM	0.354	0.392	*0.368*	*SALESVOL*	0.141	0.131	*0.497*
GROWTH	0.138	0.128	*0.838*	*TOTALVOL*	0.041	0.041	*0.769*
INTANG	0.255	0.295	*0.308*	*IDIOSYVOL*	0.036	0.034	*0.699*
NEG	0.107	0.128	*0.619*	*BETA*	1.093	1.114	*0.720*
OPAQUE	0.144	0.134	*0.524*	*NEGSKEW*	0.097	0.127	*0.702*
				HHI	0.159	0.171	*0.794*
				RET	0.173	0.206	*0.347*
				ANNRET	-0.001	-0.003	*0.381*
				SAMEAUD	0.118	0.112	*0.743*

This table reports the results for the ERM propensity score estimation based on a sample of 24,898 firms that are ever listed on the S&P 500 index. Panel A reports the results of the propensity score estimation. The dependent variable equals 1 beginning in the first full year that a firm uses an ERM program, and zero otherwise. Panel B compares (1) the ERM determinants used in Panel A and (2) regression covariates from Eq. (1) for the treatment and control firms in the risk outcomes analysis. Panel C compares (1) the ERM determinants used in Panel A and (2) regression covariates from Eq. (2) for the treatment and control firms in the analysis of change in IVS around industry restatements. *P*-values testing the equality of means are reported. Definitions for all variables are in Table A1 of Appendix A.

Table 2: Sample for ERM and peers' underlying risk outcomes

Panel A: Sample selection

	Ind-years	Adopters	Controls
Industry-years with 4.02 overstatement (2001-2022)	1,764		
Available industry peers	1,610	8,421	3,621
Data available for primary variables	1,379	5,894	3,115
Available industry-year PSM matches	895	4,241	2,555
PSM sample within caliper	634	1,649	1,649

Panel B: Summary statistics

			Percentiles		
Variable	Mean	Std.	25th	50th	75th
OVERINV	0.38	3.40	-1.29	-0.15	1.09
MISTATE	0.04	0.20	0.00	0.00	0.00
SIZE	8.81	1.24	7.99	8.80	9.67
BM	0.42	0.34	0.21	0.36	0.57
EP	0.02	0.14	0.02	0.05	0.06
DEBTEQ	0.89	2.10	0.23	0.55	1.09
CFO	0.11	0.07	0.06	0.10	0.15
ROA	0.05	0.08	0.02	0.06	0.09
LOSS	0.14	0.34	0.00	0.00	0.00
RETVOL	0.05	0.02	0.03	0.04	0.06
RET	0.06	0.33	-0.13	0.03	0.21

Panel C: Univariate difference in differences

	Treatment firms			Control Firms			
	Pre	Post	(i) Post-Pre	Pre	Post	(ii) Post-Pre	(i) – (ii)
OVERINV	1.14***	-0.13	-1.27***	0.62***	0.59***	-0.03	-1.24***
MISSTATE	8.09***	0.94***	-7.15***	4.55***	2.81***	-1.74*	-5.41***

(Continued)

Table 2: Continued

Panel D: Sample by year and industry

Sample by year				Sample by industry		
Year	Firms	Year	Firms	GICS Industry		Firms
2001	204	2012	154	10	Energy	416
2002	224	2013	130	15	Materials	136
2003	232	2014	112	20	Industrials	452
2004	260	2015	126	25	Consumer Discretionary	420
2005	252	2016	96	30	Consumer Staples	294
2006	224	2017	90	35	Health Care	688
2007	188	2018	98	40	Financials	88
2008	174	2019	98	45	Information Technology	456
2009	136	2020	106	50	Communication Services	76
2010	156	2021	50	55	Utilities	138
2011	164	2022	24	60	Real Estate	134
			3,298			3,298

Panel E: ERM programs by year

Year	N	Year	N	Year	N	Year	N
2001	6	2007	30	2013	42	2019	48
2002	10	2008	27	2014	43	2020	52
2003	13	2009	17	2015	54	2021	25
2004	22	2010	47	2016	41	2022	12
2005	34	2011	55	2017	38		**747**
2006	33	2012	54	2018	44		

This table presents sample selection and summary statistics related to Eq. (1) and tests of peer overinvestment and misstatement likelihood during years in which at least one industry peer engages in an undisclosed 4.02 misstatement. Panel A reports the selection process used to generate the propensity-matched sample. Panel B reports descriptive statistics for the PSM sample. Panel C reports a univariate difference in differences for *OVERINV* and *MISTATE*. Panel D reports the PSM sample by year and by aggregated two-digit GICS industry codes. Panel E reports the number of firms with an ERM program by year. Definitions for all variables are in Table A1 of Appendix A.

Table 3: ERM and peer risk outcomes during industry 4.02 misstatement years

Panel A: Overinvestment (*OVERINV*)

				Matched pairs with a switch in ERM status during sample period
ERM	0.439			
	(1.35)			
POST	-0.136	0.377*	0.412**	0.492**
	(-0.49)	(1.80)	(1.97)	(2.27)
ERM×POST	**-1.033*** **	**-0.768** **	**-0.976** **	**-0.755***
	(-2.71)	**(-1.98)**	**(-2.23)**	**(-1.95)**
SIZE	-0.067	-0.047		-0.015
	(-0.45)	(-0.26)		(-0.08)
BM	-1.169**	-1.349**		-1.379**
	(-2.31)	(-2.47)		(-2.46)
EP	1.598*	1.087		1.548*
	(1.96)	(1.27)		(1.75)
DEBTEQ	-0.025	-0.044		-0.046
	(-0.84)	(-1.36)		(-1.39)
CFO	11.078***	4.429*		4.781**
	(5.16)	(1.88)		(2.00)
ROA	0.103	4.244		3.600
	(0.04)	(1.35)		(1.13)
LOSS	-0.134	-0.136		-0.128
	(-0.34)	(-0.37)		(-0.35)
RETVOL	14.679***	1.754		1.885
	(2.66)	(0.34)		(0.35)
RET	-0.517**	-0.636***		-0.650***
	(-2.46)	(-3.08)		(-3.12)
Year FE	yes	yes	yes	yes
Industry FE	yes			
Firm FE		yes	yes	yes
N	3,298	3,298	3,298	3,104
R-squared	48.1%	66.9%	65.0%	67.0%

(Continued)

Table 3: Continued

Panel B: Misstatement likelihood (*MISSTATE*)

				Matched pairs with a switch in ERM status during sample period
ERM	0.036			
	(1.69)			
POST	0.031	0.003	0.002	0.005
	(1.36)	(0.14)	(0.08)	(0.24)
ERM×POST	**-0.056****	**-0.057***	**-0.057***	**-0.057***
	(-2.04)	**(-1.78)**	**(-1.84)**	**(-1.77)**
SIZE	0.011	0.021		0.021
	(1.45)	(1.33)		(1.33)
BM	0.013	0.064**		0.065**
	(0.45)	(2.17)		(2.15)
EP	-0.011	0.047		0.044
	(-0.17)	(0.84)		(0.70)
DEBTEQ	0.005**	0.004		0.004
	(2.13)	(1.61)		(1.62)
CFO	0.016	0.029		0.024
	(0.16)	(0.32)		(0.26)
ROA	-0.062	-0.091		-0.077
	(-0.48)	(-0.84)		(-0.66)
LOSS	0.030	0.027		0.030
	(0.93)	(1.08)		(1.12)
RETVOL	0.595	0.411		0.440
	(1.48)	(0.92)		(0.94)
RET	-0.003	0.014		0.014
	(-0.23)	(1.10)		(1.10)
Year FE	yes	yes	yes	yes
Industry FE	yes			
Firm FE		yes	yes	yes
N	3,298	3,298	3,298	3,104
R-squared	17.8%	48.6%	47.6%	48.5%

This table reports the results from estimating Eq. (1). The sample is restricted to S&P 500 firm-years in which at least one eight-digit GICS industry peer engages in an undisclosed 4.02 misstatement. *ERM* is a binary variable that equals one for treatment firms that use ERM during the sample period, and zero for non-treatment propensity score matched control firms. For treatment firms, *POST* is a binary variable that equals one beginning in the first full year that a firm uses an ERM program, and zero otherwise. For matched control firms, we code *POST* using the ERM adoption year of the relevant matched treatment firm. Panel A (B) reports results when using *OVERINV* (*MISSTATE*) as the dependent variable. The primary specification also includes year and firm fixed effects (untabulated). Industry fixed effects, when included rather than firm effects, are based on eight-digit GICS codes. Definitions for variables are in Table A1 of Appendix A. We cluster standard errors by firm and report *t*-statistics in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 4: Cross-sectional tests of ERM adoption and peer risk outcomes

Panel A: Overinvestment

	Segment Complexity		CEO Age	
	High (i)	Low (ii)	Low (i)	High (ii)
POST	0.248	0.370	0.393	0.214
	(1.06)	(1.18)	(1.53)	(0.69)
ERM×POST	**-0.798***	**-0.467**	**-1.462****	**0.099**
	(-1.88)	**(-0.72)**	**(-2.47)**	**(0.19)**
Tests of *ERM×POST* between groups				
(i) - (ii)	-0.331		-1.363	
p-value (i) < (ii)	*0.26*		*<0.01*	
Controls	Yes	Yes	Yes	Yes
Firm & Year FE	Yes	Yes	Yes	Yes
N	1,316	1,600	1,543	1,536
R-squared	73.7%	69.9%	68.2%	74.7%

Panel B: Misstatement likelihood

	Prior Performance		Competition (HHI)	
	Low (i)	High (ii)	High (i)	Low (ii)
POST	0.018	-0.003	0.028	-0.004
	(1.00)	(-0.10)	(0.87)	(-0.29)
ERM×POST	**-0.091****	**-0.042**	**-0.115****	**-0.037**
	(-2.55)	**(-0.81)**	**(-2.18)**	**(-1.05)**
Tests of *ERM×POST* between groups				
(i) - (ii)	-0.049		-0.078	
p-value (i) < (ii)	*0.04*		*<0.01*	
Controls	Yes	Yes	Yes	Yes
Firm & Year FE	Yes	Yes	Yes	Yes
N	1,625	1,673	1,628	1,670
R-squared	67.6%	45.2%	52.9%	61.2%

The table reports the results from estimating Eq. (1) on cross-sectional partitions. Panel A (B) reports results when using *OVERINV* (*MISSTATE*) as the dependent variable. Low (High) Segment Complexity includes firms with a segment Herfindahl index above (below) the annual 4-digit GICS industry median. Low (High) CEO age includes firms whose CEO is below (above) the annual 4-digit GICS industry median. Low (High) Prior Performance includes firms with asset-deflated prior year earnings before interest and taxes below (above) the annual 4-digit GICS industry median. Low (High) Competition includes industries with a Herfindahl index above (below) the annual median. Controls variables from Eq. (1) are included but not tabulated. Firm and year fixed effects are also included but not tabulated. Definitions for all variables are in Table A1 of Appendix A. We cluster standard errors by firm and report t-statistics in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. One-sided *p*-values are reported when testing equality of coefficients.

Table 5: Sample for ERM and change in IVS around industry 4.02 restatements

Panel A: PSM sample selection

	Restatements	Adopters	Controls
Audit Analytics 4.02 Non-Reliance Restatements (2004-2021)	6,106		
Restated earnings effect < 0	1,864		
Match on Compustat and CRSP	1,521		
Restatement BHAR (-1,1) less than -1%	1,134		
No Non-Reliance restatement in industry over prior 10 days	1,004		
Available industry peers	945	6,114	2,788
Peers without Non-Reliance restatement during prior 24 months	919	5,669	2,500
Peers without concurrent earnings announcement	900	4,471	1,998
Peers with available data to calculate Δ*IVS*	845	3,661	1,457
Peers with available controls data for primary tests	825	3,282	1,353
PSM sample	340	762	762

Panel B: Summary statistics

			Percentiles		
Variable	Mean	Std.	25th	50th	75th
Δ*IVS*	0.997	9.200	-2.215	0.372	2.803
EP	0.035	0.117	0.030	0.048	0.064
SIZE	9.008	1.050	8.192	8.962	9.726
BM	0.353	0.227	0.205	0.323	0.456
DEBTEQ	0.664	1.919	0.215	0.435	0.733
ΔIV^{ATMC}	3.653	16.298	-5.885	1.210	10.024
LAG_IV^{ATMC}	0.322	0.139	0.231	0.286	0.372
STOCKTURN	0.203	0.126	0.119	0.165	0.248
CFVOL	0.045	0.038	0.019	0.034	0.058
EARNVOL	0.050	0.058	0.016	0.030	0.054
SALESVOL	0.136	0.117	0.064	0.101	0.164
TOTALVOL	0.041	0.018	0.029	0.036	0.048
IDIOSYVOL	0.035	0.016	0.024	0.031	0.041
BETA	1.103	0.405	0.813	1.056	1.319
NEGSKEW	0.112	0.695	-0.256	0.145	0.482
HHI	0.165	0.180	0.053	0.081	0.212
RET	0.190	0.362	-0.015	0.162	0.352
ANNRET	-0.002	0.034	-0.020	-0.001	0.015
SAMEAUD	0.115	0.319	0.000	0.000	0.000

(Continued)

Table 5 Continued

Panel C: Univariate difference in differences

	Treatment firms			Control Firms			
	Pre	Post	(i) Post-Pre	Pre	Post	(ii) Post-Pre	(i) – (ii)
Δ*IVS*	1.04***	0.21	-0.83	0.78**	2.01***	1.23	-2.06**

This table presents sample selection and summary statistics related to Eq. (2) and tests of the change in implied volatility skewness (Δ*IVS*) surrounding an industry 4.02 restatement to tests of peer overinvestment and misstatement likelihood. Panel A reports the selection process used to generate the propensity-matched sample. Panel B reports descriptive statistics for the PSM sample. Panel C reports a univariate difference in differences for Δ*IVS*. Definitions for all variables are in Table A1 of Appendix A

Table 6: ERM and change in option IVS around industry 4.02 restatement announcements

				Matched pairs with a switch in ERM status during sample period
ERM	0.398			
	(0.66)			
POST	-0.084	-0.539	-0.753	-0.460
	(-0.10)	(-0.74)	(-1.03)	(-0.59)
ERM×POST	**-2.370*** **	**-4.043*** **	**-3.314** **	**-4.159*** **
	(-2.69)	**(-2.73)**	**(-2.57)**	**(-2.73)**
E/P	2.026	0.962		-2.476
	(1.10)	(0.45)		(-1.12)
SIZE	-0.153	-1.637		-1.597
	(-0.30)	(-1.11)		(-1.05)
BM	-0.165	0.213		5.049
	(-0.08)	(0.06)		(1.25)
DEBTEQ	0.042	-0.053		0.072
	(0.20)	(-0.24)		(0.30)
ΔIV^{ATMC}	-0.182***	-0.166***		-0.152***
	(-5.22)	(-5.10)		(-4.43)
LAG_IV^{ATMC}	-6.190	-9.503		-12.054*
	(-0.98)	(-1.52)		(-1.81)
STOCKTURN	-1.872	-7.559		-7.493
	(-0.58)	(-1.22)		(-1.06)
CFVOL	19.056**	4.904		3.520
	(2.03)	(0.38)		(0.26)
EARNVOL	-4.056	-2.803		-2.951
	(-0.71)	(-0.26)		(-0.26)
SALESVOL	-1.425	4.936		5.871
	(-0.74)	(1.45)		(1.46)
TOTALVOL	2.046**	0.408		-0.678
	(1.98)	-0.51		(-0.08)
IDOSYVOL	-1.881*	-0.426		0.072
	(-1.81)	(-0.53)		(0.08)
BETA	-1.683	-1.942		-0.752
	(-1.12)	(-1.10)		(-0.41)
NEGSKEW	0.118	0.651		0.960
	(0.23)	(1.11)		(1.48)
HHI	2.069	4.955		5.361
	(1.03)	(1.36)		(1.38)
RET	-0.650	1.207		2.833**
	(-0.85)	(0.98)		(2.15)
ANNRET	16.999	24.483*		26.537*
	(1.15)	(1.84)		(1.82)
SAMEAUD	-0.377	0.573		0.726
	(-0.57)	(0.82)		(1.03)
Restatement FE	yes	yes	yes	yes
Firm FE	no	yes	yes	yes
N	1,524	1,524	1,524	1,238
R-squared	40.3%	57.5%	54.1%	54.7%

This table presents the results from regressions of Eq. (2) and tests of ERM and the change in implied volatility skewness (Δ*IVS*) surrounding an industry 4.02 restatement. Δ*IVS* is the restatement peer average daily IVS during days *t*+3 to *t*+5 following the restatement minus average IVS during days *t*-5 to *t*-3 prior to the restatement. IVS is the daily difference in the implied volatility of an out-of-the-money put option and an at-the-money call option on peers' stock. *ERM* is a binary variable that equals one for treatment firms that use ERM during the sample period, and zero for non-treatment propensity score matched control firms. For treatment firms, *POST* is a binary variable that equals one beginning in the first full year that a firm uses an ERM program, and zero otherwise. For matched control firms, we code *POST* using the ERM adoption year of the relevant matched treatment firm. Restatement and firm fixed effects (where denoted) are included but not tabulated. Definitions for all variables are in Table A1 of Appendix A. We two-way cluster standard errors by firm and restatement and report t-statistics in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 7: Cross-sectional tests of ERM adoption and change in option IVS

	Segment Complexity		CEO Age		Prior Performance		Industry Competition	
	High (i)	Low (ii)	Low (i)	High (ii)	Low (i)	High (ii)	High (i)	Low (ii)
POST	0.660	-2.189**	0.632	-1.022	1.127	-1.066	-0.305	-0.772
	(0.71)	(-2.08)	(0.63)	(-1.28)	(1.13)	(-1.47)	(-0.28)	(-0.80)
ERM×POST	**-7.306***	**-2.855**	**-7.658****	**2.986**	**-6.078****	**-1.607**	**-7.085*****	**-1.144**
	(-1.66)	**(-1.36)**	**(-2.35)**	**(1.25)**	**(-1.96)**	**(-0.81)**	**(-3.82)**	**(-0.40)**
Tests of *ERM×POST* between groups								
(i) - (ii)	-4.451		-10.644		-4.471		-5.941	
p-value (i) < (ii)	*0.11*		*<0.01*		*0.04*		*0.01*	
Controls	yes	yes	yes	yes	yes	yes	yes	yes
Restatement FE	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes
N	607	804	730	749	728	796	791	733
R-squared	79.0%	70.0%	69.0%	81.3%	79.0%	76.6%	63.9%	56.5%

This table presents the results from regressions of Eq. (2) on cross-sectional partitions. The dependent variable, Δ*IVS*, is the restatement peer average daily IVS during days *t*+3 to *t*+5 following the restatement minus average IVS during days *t*-5 to *t*-3 prior to the restatement. IVS is the daily difference in the implied volatility of an out-of-the-money put option and an at-the-money call option on peers' stock. *ERM* is a binary variable that equals one for treatment firms that use ERM during the sample period, and zero for non-treatment propensity score matched control firms. For treatment firms, *POST* is a binary variable that equals one beginning in the first full year that a firm uses an ERM program, and zero otherwise. For matched control firms, we code *POST* using the ERM adoption year of the relevant matched treatment firm.

Low (High) CEO age includes firms whose CEO is below (above) the annual 4-digit GICS industry median. Low (High) Segment Complexity includes firms with a segment Herfindahl index above (below) the annual 4-digit GICS industry median. Low (High) Prior Performance includes firms with asset-deflated prior year earnings before interest and taxes below (above) the annual 4-digit GICS industry median. Low (High) Industry Competition includes industries with a Herfindahl index that is above (below) the annual industry median. Control variables from Eq. (2) are included but not tabulated. Restatement and firm fixed effects are also included but not tabulated. Definitions for all variables are in Table A1 of Appendix A. We two-way cluster standard errors by firm and restatement and report t-statistics in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. One-sided *p*-values are reported when testing equality of coefficients.

Table 8: Industry restatements and the likelihood of initial ERM announcement

	Quarterly						*Annual*					
$INDRES_{q}$	-0.07					-0.08						
	(-0.49)					(-0.50)						
$INDRES_{q-1}$		0.09				0.13						
		(0.66)				(0.86)						
$INDRES_{q-2}$			-0.10			-0.11						
			(-0.67)			(-0.68)						
$INDRES_{q-3}$				0.02		0.04						
				(0.11)		(0.27)						
$INDRES_{q-4}$					-0.08	-0.08						
					(-0.50)	(-0.50)						
$INDRES_{t}$							0.03					-0.03
							(0.26)					(-0.19)
$INDRES_{t-1}$								0.01				-0.04
								(0.10)				(-0.29)
$INDRES_{t-2}$									-0.01			-0.08
									(-0.10)			(-0.64)
$INDRES_{t-3}$										-0.03		0.03
										(-0.22)		(0.24)
$INDRES_{t-4}$											-0.02	-0.00
											(-0.20)	(-0.00)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes						
Year FE							Yes	Yes	Yes	Yes	Yes	Yes
N	23,914	23,914	23,914	23,914	23,914	23,914	7,196	6,480	5,788	5,140	4,526	4,526
Pseudo R^2	12.3%	12.3%	12.3%	12.3%	12.3%	12.3%	10.9%	9.9%	9.3%	9.1%	9.2%	9.2%

This table presents the results from logistic regressions that examine whether firms are more likely to announce an ERM program when they observe restatement announcements in their industry. The sample is drawn from firms that appear on the S&P 500 index at any time. The maximum sample period is from 2004 to 2022.

For the *Quarterly* regressions, the dependent variable (*ANNOUNCE*) is an indicator that equals one if the firm-quarter includes an initial announcement of an ERM program, and equals zero otherwise. The independent variables of interest (*INDRES*) are indicator variables that equal one if the firm-quarter (or prior one to four quarters) overlaps with the announcement by an 8-digit GICS industry peer of a 4.02 restatement with

a cumulative negative adjustment to net income. We estimate a hazard-type model by excluding all quarters for an announcing firm after the initial ERM announcement quarter.

For the *Annual* regressions, the dependent variable (*ANNOUNCE*) is an indicator that equals one if the firm-year includes an initial announcement of an ERM program, and equals zero otherwise. The independent variables of interest (*INDRES*) are indicator variables that equal one if the firm-year (or prior one to four years) overlaps with the announcement by an 8-digit GICS industry peer of a 4.02 restatement with a cumulative negative adjustment to net income. We estimate a hazard-type model by excluding all years for an announcing firm after the initial ERM announcement year. As the annual model requires additional lagged variables, we exclude additional years due to restatement announcements only being available beginning in 2004. For example, we exclude 2005 when we include $INDRES_{t-1}$, exclude 2005 and 2006 when we include $INDRES_{t-1}$ and $INDRES_{t-2}$, etc.

Control variables (untabulated) are from the ERM determinants model (See Table 1) and measured in the last fiscal year prior to adoption. Quarter and year fixed effects are included where denoted, but not tabulated. See Definitions for all variables are in Table A1 of Appendix A. We cluster standard errors by firm. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 9: Excluding pre-2010 adopters (and matched control firms)

	OVERINV	*OVERSTATE*	Δ*IVS*
POST	0.565	-0.009	-1.071
	(1.58)	(-0.55)	(-0.63)
ERM×POST	**-0.819***	**-0.063****	**-3.462***
	(-1.70)	**(-2.20)**	**(-1.79)**
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	
Restatement FE			Yes
N	2,370	2,370	1,000
R-squared	67.4%	40.4%	28.4%

This table reports results when we re-estimate Eq. (1) and Eq. (2) on reduced samples that exclude treatment firms that announce their ERM program prior to 2010 (and their matched controls). We draw the samples from our full PSM samples.

The first two columns re-estimate Eq. (1) with overinvestment (*OVERINV*) and a binary indicator that the firm is engaging in a 4.02 misstatement (*MISSTATE*). Control variables from Eq. (1) are included but not tabulated. Firm and year fixed effects are also included but not tabulated. We cluster standard errors by firm and report *t*-statistics in parentheses.

The third column re-estimates Eq. (2) with the change in IVS as the dependent variable. Control variables from Eq. (2) are included but not tabulated. Restatement and firm fixed effects are also included but not tabulated. We two-way cluster standard errors by firm and restatement and report *t*-statistics in parentheses. Definitions for all variables are in Table A1 of Appendix A. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 10: ERM and stock market reactions around industry 4.02 restatement announcements

	CAR	*ABSPRD*
POST	-0.280*	0.049
	(-1.83)	(1.02)
ERM×POST	**0.414***	**-0.153****
	(1.83)	**(-2.12)**
EP	-2.069*	-0.215
	(-1.76)	(-0.89)
SIZE	0.082	-0.112**
	(0.33)	(-2.23)
BM	0.675	0.070
	(1.23)	(0.52)
DEBTEQ	-0.044	0.015
	(-1.01)	(1.28)
STOCKTURN	0.846	0.255
	(0.70)	(0.78)
CFVOL	3.227	-0.508
	(0.99)	(-0.52)
EARNVOL	-1.633	-0.132
	(-0.93)	(-0.29)
SALESVOL	-0.145	0.272
	(-0.11)	(1.05)
TOTALVOL	19.355	-4.981
	(1.03)	(-0.84)
IDIOSYVOL	-29.268	2.215
	(-1.65)	(0.43)
BETA	0.420	-0.030
	(1.22)	(-0.28)
NEGSKEW	0.097	-0.022
	(0.81)	(-0.63)
HHI	-0.998	0.267
	(-1.04)	(0.77)
RET	0.217	-0.069
	(0.72)	(-0.97)
SAMEAUD	-0.277	0.031
	(-1.48)	(0.55)
Restatement FE	yes	yes
Firm FE	yes	yes
N	1,780	1,780
R-squared	56.6%	40.5%

This table presents the results from regressions of an adapted version of Eq. (2) substituting stock price-based measures of investor response as the dependent variables. Definitions for all variables are in Table A1 of Appendix A. We two-way cluster standard errors by firm and restatement and report *t*-statistics in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 11: ERM adoption and change in option IVS

ERM	**-1.687*** **	**-1.592** **
	(-2.87)	**(-2.47)**
E/P	-6.443***	-5.908***
	(-3.22)	(-3.03)
SIZE	0.365	0.279
	(1.35)	(0.93)
BM	0.626	0.535
	(0.32)	(0.26)
DEBTEQ	-0.009	-0.020
	(-0.03)	(-0.07)
ΔIV^{ATMC}	-0.028	-0.039
	(-0.60)	(-0.88)
LAG_IV^{ATMC}	0.075	0.054
	(1.29)	(1.48)
STOCKTURN	0.191	-2.609
	(0.13)	(-0.86)
CFVOL	-2.455	6.300
	(-0.22)	(0.48)
EARNVOL	-1.286	2.916
	(-0.16)	(0.31)
SALESVOL	-0.022	-1.441
	(-0.02)	(-0.76)
IDIOSYVOL	-30.402	-54.871
	(-0.60)	(-0.87)
TOTALVOL	-3.932	52.514
	(-0.06)	(0.69)
BETA	-1.177	-1.984
	(-1.67)	(-1.51)
NEGSKEW	-0.155	-0.013
	(-0.46)	(-0.04)
HHI	2.040	-1.165
	(0.85)	(-0.71)
RET	-0.189	-0.267
	(-0.25)	(-0.27)
ANNRET	5.775	9.421
	(0.63)	(0.83)
Industry FE		yes
Year FE		yes
N	502	502
R-squared	6.2%	16.1%

This table reports results from regressions of an adapted version of Eq. (2) using a sample of 251 matched pairs of ERM announcement firms and propensity score matched control firms that do not use ERM during our sample period. The dependent variable is the announcement period change in IVS for the treatment and matched control firms. *ERM* is a binary variable that equals one for the announcing treatment firms, and zero for matched control firms. Definitions for all variables are in Table A1 of Appendix A. We cluster standard errors by industry and report *t*-statistics in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 12: Stacked Difference-in-Differences

	OVERINV	*MISSTATE*	*ΔIVS*
ERM×POST	**-0.858****	**-0.088*****	**-3.349****
	(-2.04)	**(-2.68)**	**(-2.18)**
Controls	yes	yes	yes
Cohort-Firm Fixed Effects	yes	yes	yes
Cohort-Year Fixed Effects	yes	yes	
Cohort-Restatement Fixed Effects			yes
N	13,237	13,237	5,378
R-squared	84.1%	48.6%	69.6%

This table reports results when we re-estimate Eq. (1) and Eq. (2) using a stacked difference-in-differences regression design.

For estimations of Eq. (1), we draw from our primary PSM sample and form unique cohorts for each available ERM adoption year, totaling 22 cohorts. Each cohort includes all treatment firms that adopt ERM in year *t* and all available industry-matched control firms with data available in year *t*-1. Each cohort includes all available annual observations for these treatment and control firms. We stack all 22 cohorts together. We then estimate Eq. (1) using this stacked sample with the inclusion of separate firm fixed effects for each cohort and separate year fixed effects for each cohort. We also follow a similar process to generate a stacked sample of cohorts of restatement announcements based on ERM adoption years to test the change in implied volatility skew. We continue to include cohort-firm fixed effects, but now include cohort-restatement fixed effects, consistent with Eq. (2).

For tests of with *OVERINV* and *MISSTATE* as the dependent variable, we cluster standard errors by firm and report *t*-statistics in parentheses. For tests with Δ*IVS* as the dependent variable, we two-way cluster standard errors by firm and restatement and report *t*-statistics in parentheses. Definitions for all variables are in Table A1 of Appendix A. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 13: Additional robustness tests related to Δ*IVS*

Panel A: Longer post-restatement horizons

	(-5,5)	(-5,10)	(-5,20)	(-5,60)
POST	-0.539	-0.203	-0.083	0.122
	(-0.74)	(-0.35)	(-0.16)	(0.25)
ERM×POST	**-4.043*** **	**-4.302*** **	**-3.693*** **	**-3.234*** **
	(-2.73)	**(-3.45)**	**(-3.63)**	**(-3.42)**
Controls	yes	yes	yes	yes
Restatement FE	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
N	1,524	1,524	1,524	1,524
R-squared	57.5%	55.6%	53.3%	55.1%

Panel B: Market-adjusted Δ*IVS* as dependent variable

	Windows			
	(-5,5)	(-5,10)	(-5,20)	(-5,60)
POST	-0.483	-0.186	-0.073	0.154
	(-0.67)	(-0.32)	(-0.14)	(0.31)
ERM×POST	**-3.925*** **	**-4.439*** **	**-3.720*** **	**-3.365*** **
	(-2.73)	**(-3.47)**	**(-3.63)**	**(-3.43)**
Controls	yes	yes	yes	yes
Restatement FE	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
N	1,524	1,524	1,524	1,524
R-squared	63.3%	60.7%	57.7%	56.7%

Panel C: Additional controls for prior audit issues

	Windows			
	(-5,5)	(-5,10)	(-5,20)	(-5,60)
POST	-0.440	-0.242	-0.129	0.078
	(-0.60)	(-0.42)	(-0.24)	(0.16)
ERM×POST	**-4.060*** **	**-4.244*** **	**-3.667*** **	**-3.118*** **
	(-2.73)	**(-3.41)**	**(-3.64)**	**(-3.37)**
Weakness	0.554	0.776	-0.114	2.089
	(0.29)	(0.54)	(-0.08)	(1.41)
NoResBef	2.298	-1.587	-1.998	-1.600
	(1.14)	(-0.84)	(-1.28)	(-1.38)
Controls	yes	yes	yes	yes
Restatement FE	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
N	1,524	1,524	1,524	1,524
R-squared	57.3%	55.6%	53.3%	55.2%

(Continued)

Table 13: Continued

Panel D: Alternative Δ*IVS* measures

	Single call with delta closest to 0.50 and single put with delta closest to -0.20	Open interest-weighted IVS with maturities less than 180 days
POST	-0.705	-0.018
	(-0.93)	(-0.06)
ERM×POST	**-2.767****	**-0.965***
	(-2.35)	**(-1.76)**
Controls	yes	yes
Restatement FE	yes	yes
Firm FE	yes	yes
N	1,524	1,404
R-squared	56.9%	53.4%

This table reports miscellaneous robustness tests related to the change in IVS. In Panel A, we measure the change in IVS using windows that extend to 10, 20 and 60 days after restatement announcement. In Panel B, we adjust the change in IVS using an equal-weighted market index. In Panel C, we include additional controls for whether firms report a Section 302 or 404 weakness or previous restatement. In Panel D, we use two alternative approaches to measure IVS. First, we use the single call with delta closest to 0.50 and single put with delta closest to -0.20. Second, we use an open interest-weighted IVS from all options (as opposed to single options) that satisfy our criteria with maturities less than 180 days. Control variables from Eq. (2) as well as restatement and firm fixed effects are included. Definitions for all variables are in Table A1 of Appendix A. We two-way cluster standard errors by firm and restatement and report *t*-statistics in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Appendix A

Table A1: Description of variables

Variable Name	Definition
OVERINV	Overinvestment equal to the signed residuals from the following industry-year regressions: $CAPEX_{i,t} = \beta_0 + \beta_1 SALEGRWTH_{i,t-1} + \varepsilon_{i,t}$. *CAPEX* is capital expenditures scaled by prior year assets. *SALEGRWTH* is percent annual change in sales. We require 10 observations per industry-year.
MISSTATE	Indicator that equals one if the firm engages in a misstatement during the year that is subsequently reported in Item 4.02 on the 8-K and includes a cumulative negative income adjustment, and zero otherwise.
ΔIVS	Restatement peer change in IVS during the 11 days centered on the restatement filing date, where IVS is the difference between the implied volatility of an OTM put option and the implied volatility of an ATM call option. We multiply this difference by 100. See Section 4 and Appendix B.
ABRET	Abnormal stock return during the (0,1) window following an industry restatement, measured as the cumulative return minus the predicted return using a market model estimated over the (-126, -20) estimation window. See Section 5.
ABSPRD	Average abnormal bid-ask spread during the (0,1) window following an industry restatement. We predict "normal" spreads using parameters obtained from firm-specific regressions over the (-126, -20) window of daily log bid-ask spread on daily volume, closing price, volatility (i.e., natural log of the daily squared return), and market volatility (i.e., natural log of the daily squared market return). See Section 5.
ERM	Indicator that equals one if the firm has adopted an ERM program, and zero otherwise.
POST	Indicator that equals one beginning with the first full year following initial adoption of an ERM program (pseudo adoption year for control firms), and zero otherwise.
ANNOUNCE	Indicator that equals one if the firm-quarter (firm-year) includes an initial announcement of an ERM program, and equals zero otherwise.
ANNRET	Buy-and-hold return during five days centered on the restatement filing date, minus the CRSP value-weighted index return.
ASSETS	Natural log of total assets.
BETA	Slope coefficient from a regression of daily stock return on the CRSP value-weighted index return, measured over 12 months.
BM	Ratio of book value of equity to market value of equity.
CFO	Operating cash flow scaled by total assets.
CFVOL	Standard deviation of annual operating cash flow, scaled by FYB total assets, measured over 5 years.
DEBTEQ	Ratio of debt to book value of equity.
DIVSEG	Segment Herfindahl–Hirschman index.
EARNVOL	Standard deviation of annual income before extraordinary items, scaled by FYB total assets, measured over 5 years.
EP	Earnings per share scaled by price.

GROWTH	Average percent annual revenue growth, measured over 5 years.
HHI	Industry Herfindahl–Hirschman index.
IDIOSYVOL	Standard deviation of market-adjusted weekly returns, measured over 12 months.
INT	Indicator that equals one if the firm has international operations, and zero otherwise.
INTANG	Intangible assets scaled by total assets.
IOR	Institutional ownership percent.
LAG_IV^{ATMC}	Implied volatility of ATM call options prior to the restatement filing date. See section 4.
ΔIV^{ATMC}	Percent change in the implied volatility of ATM call options during the 11 days centered on the restatement filing date. See Section 4 and Appendix B.
LEV	Total liabilities scaled by market value of equity.
LOSS	Indicator variables that equals one if income before extraordinary items is negative, and zero otherwise.
NEG	Percentage of loss quarters, measured over 5 years.
NEGSKEW	Skewness of weekly returns over the last fiscal preceding the restatement filing date, multiplied by -1.
OPAQUE	Average absolute modified Jones model discretionary accruals, measured over 5 years.
INDRES	Indicator variables that equal one if the firm-quarter (or prior one to four quarters) overlaps with the announcement by an eight-digit GICS industry peer of a 4.02 restatement with a cumulative negative adjustment to net income. Also generated on an annual basis.
RET	Fiscal year stock return.
RETVOL	Standard deviation of monthly stock returns, measured over 12 months.
ROA	Income before extraordinary items scaled by total assets.
SALESVOL	Standard deviation of total revenue, scaled by FYB total assets, measured over 5 years.
SAMEAUD	Indicator equal to one if the peer firm and restatement firm are in the same eight-digit GIC industry, and zero otherwise.
SIZE	Natural log of market value of equity.
SLACK	Cash scaled by total assets.
STOCKTURN	Average ratio of daily trading volume to daily shares outstanding, measured over 12 months.
TOTALVOL	Standard deviation of weekly stock returns, measured over 12 months.
VALCHG	Percent annual growth in market value.

Table A2: Covariate balance for sample prior to PSM

Risk Outcomes				Change in *IVS*			
	Adopters	Controls	*p-value*		Adopters	Controls	*p-value*
SIZE	9.134	8.433	*0.00*	*EP*	0.034	0.026	*0.04*
BM	0.440	0.406	*0.00*	*SIZE*	9.354	8.756	*0.00*
EP	0.018	0.009	*0.00*	*BM*	0.416	0.354	*0.00*
DEBTEQ	1.207	0.787	*0.00*	*DEBTEQ*	0.939	0.458	*0.00*
CFO	0.096	0.108	*0.00*	ΔIV^{ATMC}	3.574	4.256	*0.19*
ROA	0.045	0.047	*0.29*	LAG_IV^{ATMC}	0.322	0.350	*0.00*
LOSS	0.131	0.168	*0.00*	*STOCKTURN*	0.216	0.246	*0.00*
RETVOL	0.047	0.053	*0.00*	*CFVOL*	0.041	0.057	*0.00*
RET	0.053	0.083	*0.00*	*EARNVOL*	0.045	0.071	*0.00*
				SALESVOL	0.135	0.152	*0.00*
				TOTALVOL	0.043	0.046	*0.00*
				IDIOSYVOL	0.036	0.040	*0.00*
				BETA	1.143	1.217	*0.00*
				NEGSKEW	0.107	0.093	*0.68*
				HHI	0.149	0.127	*0.00*
				RET	0.169	0.189	*0.23*
				ANNRET	-0.001	-0.003	*0.06*
				SAMEAUD	0.118	0.135	*0.09*

This table reports the covariate balance for the universe of treatment firms and non-adopting potential control firms prior to generating the PSM samples. The Risk Outcomes sample includes all potential sample firms for the analysis of overinvestment and misstatement likelihood and reports control variables from Eq. (1). The change in *IVS* sample includes all potential sample firms for the analysis of changes in IVS around industry restatements and reports control variables from Eq. (2). Definitions for all variables are in Table A1 of Appendix A. *P*-values testing the equality of means are reported.

Appendix B

Measuring the change in option implied volatility skewness (ΔIVS)

The implied volatility skewness ($IVS_{i,t}$) of stock i's options is the difference between the implied volatility of an out-of-the-money (OTM) put on day t ($IV_{i,t}^{OTMP}$) and the implied volatility of an at-the-money (ATM) call on day t ($IV_{i,t}^{ATMC}$):

$$IVS_{i,t} = IV_{i,t}^{OTMP} - IV_{i,t}^{ATMC} \quad \text{(A1)}$$

Using data from OptionMetrics, we follow previous studies (e.g., Kim and Zhang 2014) and apply the following restrictions to available options: (i) the implied volatility of the option is not missing and is between 0.03 and 2.00; (ii) the open interest of the option is not missing and is greater than zero; (iii) the total volume of option contracts is not missing; and (iv) the best offer price is equal to or greater than the best bid price and the best bid price is not zero. Consistent with prior studies (e.g., Kim and Zhang 2014), we define OTM puts as options with a delta value between -0.375 and -0.125 and define ATM calls as options with a delta value between 0.375 and 0.625. From the options that fall within each delta range, we retain the call and put options with the earliest maturity.

Retaining the earliest maturity option is consistent with prior studies that focus on options with shorter durations (e.g., Jin et al. 2012; Chan et al. 2015). When there are multiple call options with the same maturity, we keep the one whose delta is closest to 0.50 (Jin et al. 2012). When there are multiple put options with the same maturity, we keep the option whose delta is closest to -0.125. Thus, we use the put option that is further out of money (Bradshaw et al. 2010).

For a given restatement announcement j, we compute each industry peer i's $\Delta IVS_{i,j}$ as $IVS\_POST_{i,j}$ - $IVS\_PRE_{i,j}$. Consistent with Neururer et al. (2016), we define $IVS\_PRE_{i,j}$ as the peer's average daily IVS during days t-5 to t-3 prior to the restatement announcement date, and

$IVS\_POST_{i,j}$ is the peer's average daily *IVS* during days *t*+3 to *t*+5 following the restatement announcement date. We exclude options from the t-2 to t+2 window surrounding the restatement announcement date to permit the price of the underlying stock to adjust.