Do Mandated Risk Disclosures Affect Corporate Risk-Taking?

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ABSTRACT

I examine whether mandated risk disclosures affect corporate risk-taking. I hypothesize that mandated risk disclosures influence corporate risk-taking by mitigating risk-related agency conflicts and informing managers about their firms' risks. Utilizing the 2005 risk factor disclosure mandate, I find that firms susceptible to shareholder-debtholder conflicts reduce their risk-taking following the mandate. I also find that firms whose managers systematically underestimate the volatility of future outcomes reduce their risks after the mandate. To further investigate the mechanisms through which firms change risks, I exploit granular operational data from a sample of U.S. power plants. After the mandate, power plants with a high propensity for shareholder-debtholder conflicts or managerial learning reduce their exposure to risks by making several operational changes—geographically diversifying their operations, holding more fuel stock, expanding their supplier bases, etc. Collectively, my findings suggest that mandated risk disclosures influence corporate risk-taking behavior by reducing shareholder-debtholder conflicts and prompting managerial learning.

Keywords: Real Effects of Disclosures; Risk Factor Disclosure; Item 1A; Corporate Risk-Taking; Power Plants

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1. Introduction

I examine whether mandated risk disclosures affect corporate risk-taking behavior. Risk disclosures reveal information about the riskiness of a firm's cash flows and reduce the uncertainty about their variance (Heinle and Smith 2017). Prior studies provide evidence that these disclosures convey risk-relevant information and influence financial statement *users'* perceptions of the riskiness of a firm's cash flows (e.g., Schrand 1997; Kravet and Muslu 2013; Campbell, Chen, Dhaliwal, Lu, and Steele 2014; Hope, Hu, and Lu 2016). However, it is unclear whether, when, and how *firms* respond to risk disclosure requirements and change their behavior. In this paper, I examine whether risk disclosure requirements lead to changes in firms' operational and investment decisions by informing outside investors and managers, thereby influencing corporate risk-taking.

There are (at least) three non-mutually exclusive reasons why mandated risk disclosures may affect corporate risk-taking: they may (1) mitigate shareholder-debtholder conflicts, (2) mitigate manager-shareholder conflicts, and (3) induce managerial learning. First, theory suggests that shareholders prefer more risk than debtholders, and thus firms have incentives to take more risk than their debtholders would like (Jensen and Meckling 1976; Myers 1977; Smith and Warner 1979).[1] To the extent that mandated risk disclosures incrementally inform debtholders of the risks their portfolio firms face, these disclosures can lead to a decrease in corporate risk-taking.[2] Thus, I predict that firms susceptible to shareholder-debtholder conflicts reduce risk-taking after a risk disclosure mandate.

Second, mandated risk disclosures can affect corporate risk-taking by mitigating conflicts between managers and shareholders. Because managers have significant equity and human capital tied up in their firms, their exposure to the firms' idiosyncratic cash flow risk is significantly

[1] See Stein (2003) for a review of the literature on agency conflicts in general. See Roychowdhury, Shroff, and Verdi (2019) for a review in the context of financial reporting and disclosures.
[2] For this mechanism to hold, debtholders must learn from these risk disclosures and be able to exert influence on corporate decisions. I discuss both in detail later.

greater than that of shareholders who are typically diversified. Thus, managers have incentives to take less risk than their shareholders prefer (Amihud and Lev 1981; Smith and Stulz 1985). Mandated risk disclosures can incrementally inform shareholders about firm risks, allowing shareholders to better monitor managers and discourage them from taking too little risk. Consequently, mandated risk disclosures can lead to an increase in firms' risk-taking. I predict that firms susceptible to manager-shareholder conflicts (i.e., managerial risk aversion) increase risk-taking after a risk disclosure mandate.

Third, mandated risk disclosures can influence corporate risk-taking by broadening managers' information sets about their firms' risks (i.e., the managerial learning channel; McNichols and Stubben 2008). Recent studies show that, even absent agency conflicts, financial reporting requirements affect corporate decisions by prompting managers to collect and process additional information (e.g., Shroff 2017).[3] I predict that corporate risk-taking changes after a risk disclosure mandate as managers become better informed of their firms' risks. The sign of the relation is *ex-ante* unclear and depends on whether managers have been under- or over-estimating their firms' volatility of future outcomes.

My paper exploits the 2005 risk factor disclosure mandate (henceforth, "the mandate"), which requires firms to disclose "the most significant factors that make an investment of the company speculative and risky" in their 10-K filings (item 1A). The mandate was intended to provide investors with "a clear and concise summary of the material risks to an investment in the issuer's securities" (SEC 2005). Indeed, the discussion of risk factor topics in 10-K filings increased significantly after the mandate (Dyer, Lang, and Stice-Lawrence 2017). Studies document that risk factor disclosures meaningfully reflect firm risks, and influence risk perceptions

[3] While another managerial learning channel in the literature examines managers learning from stock market reactions to disclosures (e.g., Bond, Edmans, and Goldstein 2012; Jayaraman and Wu 2019), the managerial learning channel explored in my paper relates to managers learning from preparing their *own* disclosures (e.g., Shroff 2017).

of different financial statement users, such as shareholders, debtholders, and analysts (Campbell et al. 2014; Hope et al. 2016; Chiu, Guan, and Kim 2018).

I test my hypotheses using two samples: (1) a broad sample of firms using Compustat data (i.e., "the full sample") and (2) a sample of U.S. power plants, where I observe detailed operational decisions closely tied to their material risks. To identify firms that are more or less *treated* by the mandate, I exploit variation in firms' voluntary disclosure of their risk factors in the *pre*-mandate period (e.g., Huang, Shen, and Zang 2021).[4,5] Considering the costs and benefits of the disclosure, each firm would have chosen whether, and to what extent, to voluntarily disclose risk information in the pre-period. Treated firms are those that did not voluntarily disclose much risk information prior to the mandate.[6] Following prior research (e.g., Ljungqvist, Zhang, Zuo 2018; Yost 2018), I measure corporate risk-taking by using earnings volatility in the full sample and then apply more refined measures in the power plant sample (described below).[7]

To test my predictions, I identify firms with a high likelihood of the two risk-related conflicts or with a high chance of managerial learning, and examine their risk-taking behavior around the mandate employing a triple-difference research design. With respect to shareholder-debtholder conflicts, I exploit variation in firms' bankruptcy risk. Theory suggests that firms closer to bankruptcy are likely to take more risk than is desired by their debtholders because their financial difficulties exacerbate the conflicting appetite for risks between shareholders and debtholders (Jensen and Meckling 1976; Eisdorfer 2008). With regard to manager-shareholder

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[4] Exploiting *ex-ante* variation before the implementation of a regulation and defining voluntarily disclosing/adopting firms as control firms are common in financial reporting and International Financial Reporting Standards (IFRS) literature (e.g., Daske, Hail, Leuz, and Verdi 2008; Li and Yang 2016; Kim, Wang, and Wu 2022). I also validate the research design in Appendix B.

[5] As an alternative approach, I also exploit *changes* in risk factor disclosures around the mandate (see Appendix C).

[6] Note that the identifying assumption of my research design does not require the random assignment of treated vs. control firms. Instead, I assume and provide supporting evidence that, in the absence of the mandate, the risk-taking behavior of treated and control firms would have followed a similar trend.

[7] In Appendix D, I show that my main findings with the full sample are generally robust to alternative risk-taking proxies, such as return on invested capital (ROIC) volatility, cash flow volatility, and R&D expenses.

conflicts, I exploit variation in the timing of managers' labor market entry. Prior literature suggests that the early work environment of managers shapes their management styles and that managers who started their careers during recessions (henceforth, "recession managers") are more likely to have conservative management styles (Schoar and Zuo 2017). Consequently, firms led by recession managers are inclined to take less risks from the shareholders' standpoint. Finally, regarding managerial learning, I exploit variation in managerial miscalibration—managers' tendency to systematically *underestimate* the volatility of their firms' future outcomes (Ben-David, Graham, and Harvey 2013).[8] Insofar as complying with the disclosure mandate forces managers to collect and process additional risk information, previously miscalibrated managers are more likely to be affected and reduce their firms' risks.

I begin my analyses by examining a large cross-section of firms, encompassing 16,116 observations from 2003 to 2008. With this full sample, I find support for the shareholder-debtholder conflict and managerial learning channels. Specifically, treated firms close to bankruptcy respond to the mandate by reducing their risks. Economically, for a firm with high bankruptcy risk, a one-standard-deviation increase in the exposure to the mandate leads to a 9.4% relative decrease in earnings volatility after the mandate. With respect to managerial learning, I find that treated firms with miscalibrated managers reduce their risks after the mandate. In terms of economic magnitude, for a firm with a miscalibrated manager, a one-standard-deviation increase in the exposure to the mandate leads to a 31.8% relative decrease in earnings volatility after the mandate. However, I do not find any evidence showing that the disclosure mandate leads to a significant change in the risk-taking of firms with recession managers. Collectively, the results

[8] Ben-David et al. (2013) construct the managerial miscalibration measure based on a survey of CFOs, who are asked to predict market-wide stock returns and provide an 80% confidence interval around their predictions. The variable captures the extent to which CFOs systematically underestimate the volatility of future stock market returns. Corporate managers, like many individuals, are generally miscalibrated because they either underestimate the volatility of future outcomes or overestimate their predictive ability. Miscalibrated managers tend to adopt more aggressive corporate policies (Ben-David et al. 2013).

suggest that mandated risk disclosures influence corporate risk-taking by mitigating shareholder-debtholder conflicts during periods of financial distress and by inducing managerial learning for managers who systematically underestimate risk. The dynamic analyses indicate a smooth trend in risk-taking in the pre-period and a clear divergence in the relative trend after the mandate, providing comfort that the parallel trends assumption is unlikely to be violated in my sample.

Next, I conduct several analyses to corroborate my main findings. Research suggests that banks are better informed about borrowing firms than bondholders are, mainly because banks privately communicate with firms (e.g., Fama 1985; Bharath, Sunder, and Sunder 2008). Accordingly, I partition my sample based on firms' proportion of bank vs. public debt and find that the decrease in risk-taking of high bankruptcy-risk firms is entirely driven by firms with a high proportion of public debt relative to bank debt (i.e., firms whose debtholders are likely to learn new information from the disclosures). Second, considering that debtholders have limited control rights outside of default (Shleifer and Vishny 1997), my main findings do not clearly address how debtholders can induce firms to reduce risks. To identify cases where debtholders likely have greater influence, I exploit cross-sectional variation in firms' long-term debt maturity (Almeida, Campello, Laranjeira, and Weisbenner 2012). As predicted, the results show that the decrease in risk-taking of high bankruptcy-risk firms is driven by firms with a large fraction of debt maturing in the first year of the mandate, supporting the refinancing mechanism (Shleifer and Vishny 1997).[9]

With regard to the managerial learning channel, I provide more direct evidence of the learning process. Specifically, I find that the managerial miscalibration of treated firms decreases after the mandate, which suggests that managers can better estimate future volatility once they provide risk disclosures. Additionally, while the managerial miscalibration measure is well-suited

[9] In addition to the refinancing mechanism, litigation concerns of directors and managers can lead them to consider the debtholders' interests when a firm faces a high bankruptcy risk. I discuss this further in Section 3.

for testing the managerial learning channel, the measure is available only for a limited sample. Thus, I re-examine the channel using an alternative proxy—management forecast quality—which is known to reflect managers' forecasting ability and firms' internal information quality (e.g., Goodman, Neamtiu, Shroff, and White 2013; Gallemore and Labro 2015).[10] I show that treated firms with low management forecast quality (i.e., those whose managers are more likely to learn from complying with the mandate) reduce their risks after the mandate, confirming the managerial learning channel with the alternative proxy covering a broader sample of firms.[11]

I then turn to my second sample, which consists of power plants where I can observe their operational decisions at a granular level. The primary goal is to shed light on the mechanism driving the results in the full sample and to examine whether and how firms alter their operations and investments to change risks. Owners of power plants are required to publicly disclose monthly operational data, including electricity generation, fuel stock, and fuel suppliers at the plant level. The detailed data allows me to observe changes in various operational decisions closely related to the risks that power plants are exposed to. In addition, the granularity of the data allows me to include a variety of fixed effects so that I can identify the effect of the mandate on corporate risk-taking while holding other effects constant (e.g., the difference in electricity supply and demand across state-years; the difference in the supply of and demand for fuels across fuel-years).

I focus on the following three operational decisions of power plants, all of which are directly tied to their material risk factors: (1) geographic diversification, (2) inventory management, and (3) supplier base. Owners of power plants frequently mention, in their risk factor disclosures, geographic risk (related to weather, regulation, customer base, etc.), fossil fuel shortages, and

[10] The quality of management forecasts is known to reflect managers' understanding of the firm's economic environment and their ability to predict future business prospects (Goodman et al. 2013).
[11] Similar to my approach with managerial miscalibration, I also provide more direct evidence of the learning process by showing that the management forecast quality of treated firms improves after the mandate, particularly for those that previously had low forecast quality.

relationships with suppliers, lending credence to my assumption that these are important sources of risks for power plants (see Appendix E for examples of power plants' risk factor disclosures).[12]

Examining 784 unique gas- and coal-fired power plants owned by 79 firms from 2003 to 2008, I find that, in response to the mandate, owners of power plants with a high propensity for shareholder-debtholder conflicts or with a high chance of managerial learning change their operations in a way that attenuates their risk exposure. First, those owners become more geographically diversified and reduce their reliance on certain plants, counties, or states. Second, these power plants hold more fuel stock, thereby lowering the risk of a stockout after the mandate. Finally, these plants expand their supplier bases in response to the mandate. Overall, the collective evidence from the power plant data shows that firms susceptible to shareholder-debtholder conflicts or with a high chance of managerial learning respond to the mandate by changing their operations and investments, illuminating how mandated risk disclosures alter corporate risk-taking.

This paper contributes to the literature on the effect of financial reporting on investment decisions by documenting its impact on corporate risk-taking. While there is a large stream of literature on the relation between financial reporting and investment *levels* or *efficiency*, "direct evidence of financial reporting on managerial *risk-taking* and investment *riskiness* is limited" (Roychowdhury et al. 2019). As every investment decision involves balancing its risks and returns, understanding the role of financial reporting in both aspects is crucial to getting a complete view of the impact of financial reporting on corporate decisions and outcomes. Additionally, taking advantage of the power plant data, I tackle the challenge of observing firms' risk-related behaviors and investigate whether and how financial reporting alters firms' operations and investments that are closely tied to their material risks, answering Roychowdhury et al.'s (2019) call for research.[13]

[12] Admittedly, these proxies do not capture all major sources of risk faced by power plants. While other types of risks, such as plant outages, are also frequently discussed in the disclosures, I carefully choose these proxies by considering both the significance of the topics covered in the disclosures and the availability of relevant data.
[13] While I explicitly focus on *risk* disclosures and their impact on corporate risk-taking, other general disclosures can also influence corporate risk-taking to the extent that they convey information about the variance of a firm's cash

This paper also contributes to the literature on the economic consequences of risk disclosures. Prior studies often investigate whether risk disclosures reflect the risks firms face (e.g., Rajgopal 1999; Kravet and Muslu 2013; Campbell et al. 2014; Lobo, Siqueira, Tam, and Zhou 2019; Badia, Barth, Duro, and Ormazabal 2020; Filzen, McBrayer, and Shannon 2023) and whether they affect financial statement *users'* perceptions and behavior (e.g., Hodder, Koonce, and McAnally 2001; Hope et al. 2016; Hail, Muhn, and Oesch 2021). Two papers examine the real effects of risk disclosures. Examining the mandatory 2010 climate change risk (CCR) disclosures, Kim, Wang, and Wu (2022) find that CCR disclosures induce more pro-environmental activities. While both Kim et al. (2022) and my paper examine the real effect of risk disclosures, their study focuses specifically on climate-related disclosures and outcomes. Au and Tan (2025) document that the risk factor disclosure mandate reduces innovations. While Au and Tan (2025) point to the unintended negative consequences of the mandate, my findings suggest that the reduced risk-taking could be beneficial, as it results from mitigated agency conflicts and enhanced managerial learning. Overall, I extend the literature by providing evidence that mandated risk disclosures reduce agency conflicts and inform managers about their firms' risks, thereby influencing corporate risk-taking.[14]

2. Institutional Details: The 2005 Risk Factor Disclosure Mandate

In 2005, the SEC started to require firms to include a section for risk factors (i.e., item 1A) in their 10-K filings that discusses the most significant factors that make the company speculative

flows. I examine risk disclosures mainly because they provide the most salient information about the uncertain variance of cash flows.

[14] The banking literature has examined whether bank transparency enhances market discipline over banks' risk-taking (e.g., Nier and Baumann 2006; Bushman and Williams 2012). In the context of derivative disclosures, Zhang (2009) shows that derivative accounting rules influence financial hedging. I complement these streams of literature by exploring a broader disclosure requirement applicable to all firms and also by focusing on operational hedging.

and risky (SEC 2005).[15,16] The main objective of the mandate was to provide "investors with a clear and concise summary of the material risks to an investment in the issuer's securities" (SEC 2005). While some firms voluntarily disclosed information about their risk factors even before the mandate, the length of the disclosure increased significantly after its implementation (Dyer et al. 2017).

The mandate is well-suited for testing my research question. First, risk factor disclosures provide comprehensive, risk-relevant information that covers a wide variety of topics, including operational, financial, tax, and legal risks (Campbell et al. 2014). Other risk disclosures, such as market risk or hedging disclosures, focus on more specific types of risks and are limited in the range of information they convey (Ryan 2012). Second, because some firms voluntarily disclosed their risk factors prior to the mandate, I can exploit variation in the extent to which firms are affected by the mandate. Lastly, examining the real effects of risk factor disclosures is timely, as the SEC adopted rules in March 2024 to enhance and standardize climate risk disclosures (SEC 2024). The SEC is also considering a proposal regarding human capital risk disclosures, with the risk factor section being considered one of the potential locations (Kingsley, Solomon, and Jaconi 2022). My findings offer insights into whether mandating such disclosures can influence firm behavior.

Prior studies examine the informativeness of risk factor disclosures, supporting their usefulness to various financial statement users. For instance, Campbell et al. (2014) document that the length of risk factor disclosures is positively associated with firm risks, indicating that these disclosures meaningfully reflect the risks firms face. Hope et al. (2016) examine the specificity of

[15] Although risk factor disclosures are not directly audited, they are reviewed by auditors to ensure consistency with the information provided in financial statements (Auditing Standards 2710). Similarly, the SEC reviews qualitative disclosures, with the risk disclosure section being one of the primary focuses (Brown, Tian, and Tucker 2018).

[16] In 2008, the SEC implemented a size-based threshold and relieved disclosure and reporting requirements for smaller reporting companies (SRCs; SEC 2007). I do not exploit this exemption mainly because SRCs were exempted not only from risk factors disclosures but also from other various types of disclosures (e.g., MD&A, executive compensation), which impedes drawing clean inferences.

risk factor disclosures and find that equity market participants and analysts can better evaluate fundamental risks when the disclosures are more specific. Lyle, Riedl, and Siano (2023) document that the addition or removal of risk factors is negatively associated with the variance risk premium (i.e., the market's pricing of uncertainty about firm risk). Risk factor disclosures are also useful to debt market participants. Using credit default swap (CDS) spreads, Chiu et al. (2018) find that risk factor disclosures help creditors better evaluate firms' credit risk. Dai, Landsman, and Peng (2024) further find that enhanced risk disclosures reduce the cost of accessing secondary credit markets.

From the perspective of disclosing firms, providing risk disclosures involves both benefits and costs. On the one hand, risk disclosures can mitigate litigation risk by providing "meaningful cautionary language" as defined in the Private Securities Litigation Reform Act (PSLRA; Huang et al. 2021). Also, since market participants find these disclosures helpful—as discussed earlier—firms may provide them to meet investor demand. On the other hand, revealing specific risks a firm faces can be useful to competitors, leading to proprietary costs (Verrecchia 1983). Hope et al. (2016) find that firms with greater proprietary concerns tend to provide less specific risk disclosures. Mandated disclosure can also reduce innovation (Au and Tan 2025). Additionally, providing additional risk factors can result in negative capital market outcomes (e.g., Kravet and Muslu 2013; Campbell et al. 2014). Considering the trade-offs, each firm would have made its decision in the pre-period about whether, and to what extent, to voluntarily disclose risk information.

The main mechanisms in my paper rely on the assumption that the mandate provides additional risk-relevant information. Specifically, my hypotheses are a joint test of whether the disclosures convey useful information and whether firms adjust their risk-taking behavior in response. However, researchers and regulators have raised concerns that risk factor disclosures are becoming longer and more generic, potentially losing their informational value (SEC 2016; Dyer et al. 2017).[17] Relatedly, Beatty, Cheng, and Zhang (2019) find that the relevance of risk factor disclosures has declined, especially following the financial crisis. In response to these concerns, the SEC has made ongoing efforts to modernize and improve the quality of risk factor disclosures (SEC 2016, 2020, 2024).

Despite these concerns, prior studies provide evidence that risk factor disclosures have a meaningful impact on various outcomes, including equity pricing, debt pricing, analysts' forecasting ability, legal protections, environmental activities, and innovation, even in recent years. Firms frequently update these disclosures by reordering, adding, or removing risk factors, and investors respond to such changes (Lyle et al. 2023). I further validate my assumption using the risk information measure developed by Smith and So (2022), which estimates changes in investors' perception of a firm's riskiness following an information event (see Appendix B). Results from this option-based measure indicate that the length of risk factor disclosures is positively associated with the amount of risk information conveyed in 10-K filings. Additionally, the disclosure mandate leads to an increase in the amount of risk information provided in 10-Ks, particularly for treated firms in my analysis.

3. Hypotheses Development

Risk disclosures reveal information about the riskiness of a firm's cash flows and mitigate uncertainty about their variance. For instance, theory suggests that risk disclosures reduce firms' cost of capital by decreasing the variance uncertainty premium that outside investors demand (Heinle and Smith 2017; Heinle, Smith, and Verrecchia 2018). Empirical evidence also shows that risk disclosures convey risk-relevant information and change financial statement users' perception of a firm's cash flow risks (e.g., Schrand 1997; Kravet and Muslu 2013; Hope et al. 2016). I argue

[17] Legal protection from risk factor disclosures is one of the primary reasons these disclosures have become lengthier (SEC 2016). Risk factor disclosures provide legal protections to firms that make forward-looking statements under the PSLRA (Cazier, McMullin, and Treu 2021). Relatedly, practitioner articles suggest that lawyers often assist managers in preparing risk factor disclosures, or even draft them (Gelfond, Wechsler, and Cohen 2018).

that risk disclosure requirements influence corporate risk-taking by informing both outside investors and managers about the riskiness of a firm's cash flows.

First, I hypothesize that mandated risk disclosures affect corporate risk-taking by resolving risk-related agency problems between shareholders and debtholders. Theory suggests that firms often take more risk than is desired by debtholders, and this conflict arises mainly because shareholders and debtholders have different risk preferences derived from their heterogeneous payoff structures (Jensen and Meckling 1976; Myers 1977; Smith and Warner 1979). Shareholders, who are residual claimants, have incentives to demand risky negative NPV projects at the expense of debtholders, which is referred to as a risk-shifting or asset substitution problem. Insofar as mandated risk disclosures provide incremental risk-relevant information to debtholders, the disclosure mandate can lead to a decrease in corporate risk-taking by allowing debtholders to better monitor and discipline corporate decisions (Stein 2003).

Although debtholders have limited control rights outside of firms' default, they can potentially influence corporate risk-related decisions. First, debtholders can exert a stronger influence when a firm's debt matures and needs to be refinanced (Shleifer and Vishny 1997). In the later section, I examine whether the decrease in risk-taking is concentrated in cases where a large fraction of long-term debt matures in the first year of the mandate. Second, litigation concerns of managers and directors can alter corporate risk-taking. While corporate managers and directors owe fiduciary duties only to shareholders when a firm is solvent, these duties are extended to other stakeholders, such as debtholders, when the firm becomes insolvent or is close to insolvency (e.g., Scheler, Kaplan, and Rodburg 2020). Consequently, debtholders can sue managers and directors of financially distressed firms for breach of duty if firms disregard debtholders' interests when making decisions. Given that debtholders are better informed of firm risks after the mandate, in the post-mandate period, firms are less likely to take actions that favor shareholders at the expense of debtholders (i.e., making risky investments) so that they can avoid any potential lawsuits from debtholders (Becker and Strömberg 2012).[18] Building on these arguments, I offer my first hypothesis.

H1*: Firms that are susceptible to shareholder-debtholder conflicts decrease risk-taking after the mandate.*

Another risk-related agency conflict stems from the disparate risk preferences between managers and shareholders. Managers are often more exposed to their firms' cash flow risks than their diversified shareholders because managers have both their equity and human capital tied to their firms. Therefore, managers tend to have incentives to take on *less* risk than their diversified shareholders prefer (Amihud and Lev 1981; Smith and Stulz 1985; Panousi and Papanikolaou 2012). To the extent that mandated risk disclosures incrementally inform shareholders about firm risks, the disclosure mandate can lead to an increase in corporate risk-taking by allowing shareholders to better monitor and prevent managers from taking too little risk. Thus, I offer my second hypothesis.

H2*: Firms that are susceptible to manager-shareholder conflicts increase risk-taking after the mandate.*

Even without any agency conflicts, mandated risk disclosures can influence corporate risk-taking through managerial learning (McNichols and Stubben 2008). While managers are regarded as having good (or the best among market participants) knowledge of their firm, their information set is incomplete. Indeed, managers are known to gain information and learn from various sources, including investors, analysts, and peer firms (e.g., Jayaraman and Wu 2019; Seo 2021; Guo and Zhong 2023). Specifically related to risk information, for instance, Dai et al. (2024) find that private loan issuance enables managers to learn new information about their firms' risk exposures.

[18] Indeed, several lawsuits have been filed by creditors claiming breaches of fiduciary duties by managing a firm in a way that benefited shareholders at the expense of creditors (e.g., by undertaking risky investments; North American Catholic Educational Programming v. Gheewalla in 2008; Quadrant v. Vertin in 2015; see Scheler et al. (2020) for more details). Relatedly, Becker and Strömberg (2012) find that the extended fiduciary duties to debtholders mitigate risk-shifting problems of firms close to financial distress.

Recent studies suggest that financial reporting requirements prompt managers to gather and process additional information, leading to changes in corporate decisions (e.g., Cho 2015; Shroff 2017; see Roychowdhury et al. 2019 for a review). Speficially, the information collected for external reporting can broaden managers' information set (Dichev, Graham, Harvey, and Rajgopal 2013), and the improved internal information can lead to more effective operational decisions, such as inventory management and acquisitions (e.g., Feng, Li, McVay, and Skaife 2015; Harp and Barnes 2018). By helping managers to be better informed of the risks their firms face, mandated risk disclosures can facilitate firms' risk-related decisions. Also, to the extent that the mandate heightens managers' attention to risk and related firm decisions, it can lead to improved information processing of *existing* risk-relevant information (Blankespoor, deHaan, and Marinovic 2020).

The sign of the relation is *ex-ante* unclear. For instance, if managers have been overestimating their firms' risks compared to the actual metrics, the disclosure mandate will allow them to adjust their estimated risks downward, potentially leading to an increase in corporate risk-taking (and vice versa). To identify cases where managers likely learn new information during risk disclosure preparation, I focus on managers' tendency to systematically *underestimate* future volatility and risks. Corporate managers, like many individuals, typically underestimate the range of potential future outcomes, which is referred to as miscalibration. This is either because they underestimate the volatility of future outcomes (e.g., Deaves, Luders, and Luo 2009; Bar-Yosef and Venezia 2014) or overestimate their predictive ability (e.g., Alpert and Faiffa 1982). Firms with miscalibrated managers tend to adopt more aggressive corporate policies in investing and financing (Ben-David et al. 2013). Therefore, I predict that the disclosure mandate prompts managerial learning, particularly among those who have systematically underestimated their firms' risks. This learning will lead miscalibrated managers to revise their expectations of future volatility upward, resulting in reduced corporate risk-taking. Motivated by these arguments, I offer my third hypothesis.

H3*: Firms whose managers systematically underestimate the volatility of their firms' future outcomes decrease risk-taking after the mandate.*

4. Sample and Data

I test my hypotheses with two different samples: (1) the full sample and (2) the power plant sample. This section primarily focuses on the full sample, while the power plant sample is detailed in Section 6. To avoid the impact of the global financial crisis, the sample period spans from 2003 to 2008, encompassing three years before and after the mandate. This sample period aligns with the time when Beatty et al. (2008) document that risk factor disclosures were informative.[19]

To construct the full sample, I begin with 55,582 firm-year observations in Compustat from 2001 to 2010 with positive assets, non-missing historical SIC codes, and non-missing permno variables. I drop firms in the financial sector (SIC codes: 6000–6999) and public sector (SIC codes: 9000–9999). Retaining observations from the period between 2003 and 2008, and requiring firm-year observations to have all the main variables used in the regressions (e.g., earnings volatility, the *TREAT* variable) leaves me with 16,806 observations. I then require firm-year observations to have at least one of the cross-sectional variables used in the analyses available, leaving me with 16,388 observations. After removing observations with any missing control variables, the final sample size is 16,116 observations. Table 1 presents the sample selection procedure in detail.

To construct the treatment variable, I employ Dyer et al. (2017)'s measure of the length of risk factor discussions from 10-K filings. The Business Cycle Dating database of the National Bureau of Economic Research (NBER) is used to define recession years. The managerial

[19] As I examine the period when risk factor disclosures are relatively more informative, the findings I document may not apply to later years when the relevance of the disclosures declines. However, this does not weaken the importance of my findings. Rather, my findings highlight the additional adverse outcomes of the recently reduced relevance of disclosures, supporting the SEC's initiative to increase the informativeness of the disclosures (e.g., SEC 2020, 2024).

miscalibration variable, which estimates the extent to which CFOs systematically underestimate future volatility, is from Ben-David et al. (2013).[20] Variables needed to define management forecast quality are from I/B/E/S. For the debt maturity test, I obtain the maturity dates of bonds and bank loans from FISD and Dealscan, respectively. To match the bank loan data to the main sample, I use the Dealscan-Compustat linking table from Chava and Roberts (2008). OptionMetrics and I/B/E/S databases are used to construct Smith and So's (2022) risk information measure. All other variables are from Compustat and CRSP.

5. Empirical Analyses: Full Sample

5.1. Research Design and Descriptive Statistics

To test my hypotheses, I exploit two sources of variation around the mandate: (1) the extent to which firms are affected by the mandate and (2) whether firms have a high propensity for risk-related agency conflicts (***H1*** and ***H2***) or managerial learning (***H3***). Because some firms voluntarily provided information about their risk factors even before the mandate, I exploit this *ex-ante* variation and capture the exposure to the mandate using Dyer et al.'s (2017) measure of the length of risk factor discussions in 10-K filings.[21] Treated firms are those that did not voluntarily disclose much risk information right before the mandate. Defining treated and control groups based on voluntary disclosures or adoption in the pre-regulation period is common in the literature on financial reporting and IFRS (e.g., Daske et al. 2008; Li and Yang 2016; Kim et al. 2022). In the context of the risk factor disclosure mandate, Huang et al. (2021) employ a similar approach to examine its impact on firms' litigation risk (see Section 4.1 for their validation of the *ex-ante* approach).

[20] I thank the authors of Dyer et al. (2017) and Ben-David et al. (2013) for sharing their data on the length of risk factor discussions and managerial miscalibration, respectively.

[21] Dyer et al. (2017) estimate the length of risk factor discussions in 10-K filings using a natural language processing technique called Latent Dirichlet Allocation (LDA). LDA is especially well-suited for analyzing complicated documents, such as 10-K filings, that discuss multiple topics in different places. In addition, the format and location of the voluntary risk factor disclosures in the pre-mandate period were highly arbitrary and idiosyncratic, making it difficult to capture the disclosures using a general textual analysis approach.

In Appendix B, I validate the research design and the definition of treatment in two ways. First, I show that treated firms are the ones that significantly increase their risk factor disclosures after the mandate (Figure A1). Second, using Smith and So's (2022) risk information measure,[22] I provide evidence that treated firms' risk information conveyed in 10-K filings increased significantly after the mandate, compared to control firms (Column 3 of Table A1). Additionally, as an alternative approach to defining treated and control firms, I exploit *changes* in risk factor disclosures around the mandate. Noting that this measure can have some endogeneity concerns—since it is subject to changes in firms' riskiness during the period—I tabulate the main results using this alternative treatment definition in Appendix C.

One important thing to note is that the identifying assumption of my research design does *not* require the random assignment of treated and control firms. As shown in the descriptive statistics in Table 2, the two groups differ across several characteristics. Instead, the assumption is that, in the absence of the mandate, the risk-taking behavior of treated and control firms would have followed a similar trend. Although the parallel trends assumption is not directly testable, I provide evidence, in Panel B of Table 3, that the risk-taking behavior of the two groups exhibits similar trends in the pre-mandate period.

To identify firms with high shareholder-debtholder conflicts, I use firms' bankruptcy risk because high bankruptcy risk exacerbates risk-shifting problems and leads to more risk-taking than is preferred by debtholders (Jensen and Meckling 1976). For manager-shareholder conflicts, I examine whether managers started their careers during recession vs. non-recession periods. Prior literature shows that managers who entered the labor market during recessions (i.e., recession managers) tend to manage their firms in a more conservative way (Schoar and Zuo 2017). Thus, firms with recession managers are more likely to be susceptible to managerial risk aversion. To

[22] Using an option-pricing model, Smith and So (2022) estimate how information events, such as 10-K filings or earnings announcements, impact investors' perception of a firm's future riskiness.

capture firms with a high chance of managerial learning, I focus on managers' tendency to systematically underestimate the volatility of their firms' future outcomes using the managerial miscalibration measure from Ben-David et al. (2013). Exploiting those variations, I estimate the following triple-difference regression model:

$$
\begin{aligned}
y_{i,t} = {} & \beta_1 POST_t + \beta_2 TREAT_i + \beta_3 POST_t \times TREAT_i + \beta_4 CS\ VARIABLE_{i,t-1} \\
& + \beta_5\ POST_t \times CS\ VARIABLE_{i,t-1} + \quad \beta_6\ TREAT_i \times CS\ VARIABLE_{i,t-1} \\
& + \beta_7\ POST_t \times TREAT_i \times CS\ VARIABLE_{i,t-1} + (\alpha_i\ or\ \alpha_{ind}) + \alpha_t + \gamma' X \\
& + \varepsilon_{i,t}\ , \qquad (1)
\end{aligned}
$$

where *i*, *ind*, and *t* index firms, industries, and years, respectively. My main variable of interest is *POST* × *TREAT* × *CS VARIABLE*. The dependent variable, $y_{i,t}$, represents earnings volatility (*ROA VOLATILITY*). *POST* is an indicator variable that equals one for the post-mandate period (i.e., December 2005 and beyond). *TREAT* is the natural logarithm of the length of risk factor disclosures *right before* the mandate (I validate the variable in Appendix B), measured by Dyer et al. (2017) using a natural language processing technique called LDA. I multiply the variable by -1 so that a greater value represents a greater degree of "treatment" (i.e., more affected by the mandate). *CS VARIABLE* is based on either (i) Ohlson (1980)'s O-score (*BANKRUPTCY RISK*), (ii) the economic condition at the time of managers' career entry (*RECESSION IN EARLY CAREER*),[23] or (iii) the managerial miscalibration measure constructed by Ben-David et al. (2013) (*MANAGER MISCALI.*). α_i, α_{ind}, and α_t represent firm, industry, and year fixed effects, respectively.[24,25] I include market-to-book, leverage, firm size, sales growth, annual stock return,

[23] I assume that managers enter the labor market at the age of 24 or 25 (Schoar and Zuo 2017). *RECESSION IN EARLY CAREER* equals one if there was a recession when a firm's CEO was 24 or 25. A calendar year is defined as a recession year if a trough occurs during the year or if the entire year falls into a recession period. The results remain consistent when assuming managers' job market entrances at age 24 (Schoar and Zuo 2017).

[24] Year fixed effects do not subsume the *POST* variable because they are defined based on calendar years. When year fixed effects are instead defined relative to the mandate (i.e., each year starts in December), they do subsume the *POST* variable. I present the results using the calendar-year definition, but the results are very similar when using the latter definition.

[25] When testing the agency conflict channels (***H1*** and ***H2***), I include firm fixed effects. However, when testing the managerial learning channel (***H3***), I include industry fixed effects (based on the two-digit SIC code) instead. This is

and cash surplus as control variables (e.g., Ljungqvist et al. 2018; Yost 2018; Armstrong, Glaeser, Huang, and Taylor 2019). Standard errors are clustered at the firm level. Appendix A provides detailed variable definitions.

Table 2, Panel A reports descriptive statistics for the full sample. Before taking natural logarithms, the average earning volatility is 3.4%. The average length of risk factor disclosures was 951 words before the mandate and increased by 41% to 1,338 words after the mandate (not tabulated). Panel B separately presents the statistics for firms with high vs. low *TREAT* variable. Treated firms—those with high *TREAT*—exhibit lower earnings volatility, bankruptcy risk, and managerial miscalibration. They also have a lower market-to-book ratio, higher leverage, greater sales, lower sales growth, and a greater cash surplus.

5.2. Empirical Results

Panel A of Table 3 presents the results from estimating Equation (1).[26] Columns 1–4 examine the agency conflict channel (***H1*** and ***H2***) and present results when *CS VARIABLE* is *BANKRUPTCY RISK* and *RECESSION IN EARLY CAREER*, respectively. Columns 5–6 test the managerial learning channel (***H3***) using the *MANAGER MISCALIB.* variable as *CS VARIABLE*. Regarding shareholder-debtholder conflicts, the coefficient on *POST* × *TREAT* × *BANKRUPTCY RISK* in Column 2 is negative and statistically significant (coef.=-0.094; t-stat.=-2.01), supporting ***H1*** that treated firms with high bankruptcy risk reduce their risk-taking once risk factor disclosures become mandated. The findings are also economically meaningful. A one-standard-deviation

mainly due to the limited availability of the managerial miscalibration variable, which covers 464 unique firms and 776 observations, making it difficult to exploit within-firm variation.

[26] Before proceeding with the main regression, I examine the overall impact of the mandate on treated firms by running regressions only with two-way interactions (i.e., without *CS VARIABLE*). The coefficient of interest, *POST* × *TREAT*, is statistically insignificant (coef.=0.001; t-stat.=0.028; untabulated), suggesting that the mandate does not significantly affect the average treated firm. One possible explanation could be the countervailing effects of the mandate across different firm types (e.g., firms subject to shareholder-debtholder conflicts vs. manager-shareholder conflicts).

increase in the exposure to the mandate leads to a 9.4% relative decrease in earnings volatility in the post-period for a firm with high bankruptcy risk.

With respect to manager-shareholder conflicts, the coefficient on *POST* × *TREAT* × *RECESSION IN EARLY CAREER* in Column 4 is statistically insignificant (coef.=0.000; t-stat.=0.01). The insignificant coefficient suggests that the mandate does not lead to a distinct change in the risk-taking of firms with recession managers (i.e., firms whose managers are likely to be risk-averse), relative to those without. Thus, I do not find evidence supporting ***H2***.[27] The null results may stem from the nature of risk factor disclosures. Because these disclosures are designed to highlight risks a firm faces, their tone is inherently negative, making them well-suited for revealing additional risk exposures—information that debtholders may use to discourage excessive risk-taking—but less suited for signaling that firms are taking too little risk. As such, it may be difficult for shareholders to infer from these disclosures that managers are being overly conservative or underinvesting in risky but value-enhancing projects.

For the managerial learning channel, the coefficient on *POST* × *TREAT* × *MANAGER MISCALIB.* in Column 6 is negative and statistically significant (coef.=-0.318; t-stat.= -1.76), supporting ***H3*** that treated firms with miscalibrated managers learn from preparing risk disclosures and reduce their risks after the mandate. Economically, a one-standard-deviation increase in the exposure to the mandate leads to a 31.8% relative decrease in earnings volatility in the post-period for a firm with a miscalibrated manager. Overall, these results in Table 3, Panel A are consistent with my hypotheses that mandated risk factor disclosures reduce the risk-taking of firms (1) that are susceptible to shareholder-debtholder conflicts (***H1***) and (2) whose managers systematically underestimate the volatility of their firms' future outcomes (***H3***). In Appendix D, I provide

[27] As an alternative measure of managerial risk aversion, I use firms' insider ownership and find similar insignificant results (untabulated). The rationale behind using the insider ownership proxy is that firms whose managers hold a larger fraction of the firms' shares are more likely to avoid risks, especially from the perspective of well-diversified shareholders (Panousi and Pananikolaou 2012).

evidence that the results are generally robust to using alternative risk-taking proxies, such as return on invested capital (ROIC) volatility, cash flow volatility, and R&D expenses.[28]

Panel B of Table 3 presents the results from the dynamic analyses, where *POST* is replaced with year indicators representing each year in my sample period. Note that *YEAR2005* is excluded as it serves as the base year. Columns 1–2 indicate that while there is no evidence of a pre-treatment trend in earnings volatility, a distinct divergence appears in the relative trend post-mandate between high bankruptcy-risk firms and low bankruptcy-risk firms. This suggests that the parallel trends assumption is unlikely to be violated in my sample. A similar trend appears when I compare firms with miscalibrated managers to those without in Columns 5–6. Similar to the results from static analyses, the results in Columns 3–4 indicate that there are no changes in the relative trend in earnings volatility regardless of whether firms have recession managers or not, in both the pre- and post-periods. Figure 1 plots the coefficient estimates of *YEAR× TREAT × CS VARIABLE* and graphically presents the results.

5.3. Additional Analyses

5.3.1. Mechanism: Shareholder-Debtholder Conflict (*H1*)

In this section, I conduct additional tests to substantiate the argument that the mandate leads to a decrease in the corporate risk-taking of firms susceptible to shareholder-debtholder conflicts (***H1***). The key assumptions of the shareholder-debtholder conflict channel are twofold: debtholders (1) learn new information from risk disclosures and (2) are able to exert influence on corporate decisions. To provide support for these assumptions, I proceed with the following cross-sectional tests.

[28] While another often-used proxy for corporate risk-taking is stock return volatility (e.g., Armstrong et al. 2019), this proxy is not well-suited for my research question, which relates disclosures to corporate risk-taking. This is mainly because it is difficult to disentangle whether changes in return volatility result from investors' reactions to disclosures or from changes in firms' behavior. Relatedly, studies on risk factor disclosures often use stock return volatility to capture changes in investors' risk *perceptions* (e.g., Kravet and Muslu 2013; Campbell et al. 2014). Therefore, I rely on proxies from firms' financial statements, so that I can more clearly attribute the results to changes in firms' behavior.

First, banks typically have superior access to a firm's information compared to bondholders, because of their frequent private communication with the firm (e.g., Bharath et al. 2008). If the findings of the shareholder-debtholder conflict channel are indeed driven by debtholders learning from risk disclosures, then such a relation should be stronger when bondholders account for a large proportion of debtholders. To test this, I partition my sample into two groups based on firms' proportion of bank debt vs. public debt and re-estimate Equation (1).

Columns 1 and 2 of Table 4 present the results. Consistent with my prediction, the coefficient on *POST* × *TREAT* × *BANKRUPTCY RISK* is negative and significant (coef.= -0.293; t-stat.=-2.55) only in the subsample of firms that have a high proportion of public debt (i.e., a low proportion of bank debt). On the other hand, the coefficient is insignificant (coef.=0.030; t-stat.=0.31) when firms have a low proportion of public debt. The difference in the coefficients is significant (p-value=0.031). The results imply that the decrease in risk-taking of high bankruptcy-risk firms in response to the mandate is driven by firms whose debtholders are likely to learn from the disclosures, supporting my first assumption.

Second, considering that debtholders generally have limited control rights unless firms go into default (Shleifer and Vishny 1997; Nini, Smith, and Sufi 2012), my main findings do not address whether and how debtholders can induce firms to reduce their risks. To identify cases when debtholders likely have greater influence, I exploit cross-sectional variation in firms' long-term debt maturity around the implementation of the mandate (Almeida et al. 2012).[29] Theory suggests that debtholders are likely to have a stronger influence when a firm's debt matures and needs to be refinanced (Shleifer and Vishny 1997). Thus, I predict that the decrease in risk-taking for high bankruptcy-risk firms is more pronounced for the sample of firms that have a large fraction of long-term debt maturing during the first year of the mandate. I partition my sample into two

[29] The underlying assumption of the analysis is that the fraction of long-term debt maturing in the first year of the mandate is independent of factors influencing corporate risk-taking.

based on whether the fraction of total long-term debt (i.e., debt with a maturity of 12 months or more) maturing during the first year of the mandate exceeds 15%.

Columns 3 and 4 of Table 4 present the results. As predicted, the coefficient on *POST* × *TREAT* × *BANKRUPTCY RISK* is negative and significant (coef.= -0.272; t-stat.=-2.25) in the subsample of firms that have a significant portion of long-term debt maturing in the year of the mandate. On the other hand, the coefficient is negative but insignificant (coef.=-0.079; t-stat.=-0.95) in the subsample of firms with little long-term debt maturing in the first year of the mandate. In an untabulated test, I use the leverage ratio as an alternative proxy for debtholders' disciplinary power and find that the decrease in risk-taking is concentrated among the sample of firms with a high leverage ratio. Taken together, the results of these cross-sectional tests validate my assumptions for the shareholder-debtholder conflict channel by showing that risk disclosures inform debtholders and that debtholders exert influence on firms' risk-related decisions.

5.3.2. Mechanism: Managerial Learning (*H3*)

Evidence of Managerial Learning

To corroborate my findings that risk disclosures affect corporate risk-taking through managerial learning (***H3***), I present more direct evidence of the learning process. Specifically, I examine changes in managerial miscalibration around the mandate and predict that the degree of miscalibration will decrease (i.e., managers will be better able to predict future volatility/riskiness) after the mandate. To test this prediction, I regress the raw values of *MANAGER MISCALIB.* on *POST*, *TREAT*, and the interaction term between the two variables.

Table 5, Panel A presents the results. Consistent with my prediction, the coefficient on *POST*× *TREAT* is negative and statistically significant (coef.=-0.106; t-stat.=-1.72). Economically, a one-standard-deviation increase in the exposure to the mandate leads to a 0.11 standard-deviation decrease in managerial miscalibration in the post-period. The results indicate that mandated risk disclosures reduce miscalibration among treated managers, suggesting an improvement in these managers' ability to predict volatility.

Alternative Proxy: Management Forecast Quality

While the managerial miscalibration measure is well-suited for testing the managerial learning channel, the results should be understood with caution as the measure is available for only 5% of the full sample. To address potential concerns regarding its limited coverage, I re-examine the managerial learning channel using an alternative proxy—management forecast quality—which reflects managers' forecasting ability as well as firms' internal information quality. For instance, Goodman et al. (2013) suggest that the quality of management forecasts serves as an observable signal of their broader forecasting ability, reflecting managers' understanding of the firm's economic environment and their ability to predict future business prospects. Also, firms with high internal information quality are known to produce more accurate forecasts (e.g., Cassar and Gibson 2008, Dorantes, Li, Peters, and Richardson 2013, Gallemore and Labro 2015). Therefore, firms with low management forecast quality are likely to have a higher propensity of managerial learning from the disclosure mandate and change their risk-taking behavior accordingly. Although the measure nicely complements the managerial miscalibration measure, especially in terms of coverage, one caveat is that it is relatively less direct for testing the managerial learning channel compared to the managerial miscalibration measure.

Using the management forecast quality measure, I re-estimate the main regression (i.e., Equation (1)). My prediction is that firms with low management forecast quality (i.e., those with low internal information quality so that managers are more likely to learn from complying with the risk disclosure requirement) reduce their risk-taking after the mandate. Column 1 of Table 5, Panel B presents the results. As predicted, the coefficient on *POST* × *TREAT* × *MEF INACCURACY* is negative and statistically significant (coef.=-0.128; t-stat.=-2.20). Additionally, to provide more direct evidence of managerial learning, I examine whether management forecast quality improves after the mandate. Column 2 reports the regression results using the *raw* values of *MEF INACCURACY* as the dependent variable. The result indicates that, after the mandate, the improvement in forecasting quality is concentrated among treated firms that previously had low forecast quality. Collectively, the results in Table 5, Panel B confirm the managerial learning channel using a proxy that covers a broader sample of firms.

6. Power Plant Analyses

6.1. The Electric Power Industry and Power Plants

While the analyses using the full sample in Section 5 suggest that the mandate leads to changes in corporate risk-taking behavior, they do not explain *whether* and *how* firms alter their operations and investments to adjust their risks. Identifying the real effects of disclosures on corporate risk-taking is empirically challenging, mainly because the riskiness of corporate decisions is difficult to observe (Kim et al. 2022). Studies generally compare the levels of different investment types (e.g., R&D expenses, capital expenditure; Hayes, Lemmon, and Qiu 2012) to estimate investment riskiness. While these proxies have implications for investment riskiness, it remains difficult to tell whether disclosures directly affect investment *riskiness* or whether the changes in investment riskiness are a manifestation of changing investment *levels* (Roychowdhury et al. 2019). I complement prior studies by exploiting granular operational data from the power plant industry and observing firms' decisions that closely relate to the material risks covered in their disclosures.

The electric power industry and the power plant setting are especially well-suited for my research question for three reasons. First, the industry publicly discloses very granular operational data at the production unit (i.e., generation; power plant) level. Due to their essential nature, operating facilities must provide relevant information to the U.S. Energy Information Administration (EIA) under Section 13(b) of the Federal Energy Administration Act of 1974

(FEAA).[30] For instance, owners of power plants have to disclose an operation report (e.g., EIA-423) about each of their power plants' electric power generation, energy source consumption, fossil fuel stocks, and fossil fuel receipts. The detailed power plant level data allows me to observe their various operational decisions closely tied to risks.

Another advantage comes from the homogeneity of operations across plants. A power plant receives fuel (e.g., coal, natural gas, and petroleum) from its suppliers, produces electricity in its generators, and transmits electricity to customers through the grid. This uniformity allows me to observe and compare changes in various operational decisions across power plants around the mandate, which is relatively more challenging in a general firm setting. For instance, a typical manufacturer's inventory account consists of many different parts and products, but the amounts are provided only at an aggregated level, making it difficult to compare them with those of other firms. On the other hand, since power plants report inventories of the *same* product (i.e., fossil fuels), I can compare the inventory management of different power plants. Also, fuel stocks are reported at the monthly level, which allows me to exploit within-year changes in stocks.

Lastly, the granularity of the data allows me to include a variety of fixed effects so that I can identify the effect of the mandate on corporate risk-taking, holding other effects constant. By including state-year fixed effects, for instance, I control for the heterogeneity in electricity supply and demand across different state-years. Similarly, by including the fuel type-year fixed effects, I control for the difference in supply of and demand for fuels across different fuel type-years.

To understand operational decisions of power plants tied to the material risks they face, I analyze every risk factor disclosure submitted by power plant owners in their first 10-K filings after the mandate. I find that 98.6%, 89%, and 46.6% of disclosures mention risk factors related to geographic risk, fuel procurement and shortage, and supplier relationships, respectively. Therefore,

[30] Also, the EIA is required to publicize the data that is "necessary to keep the public fully and currently informed as to" the status of energy suppliers (Section 14(b) of FEAA).

I focus on (1) geographic diversification, (2) inventory management, and (3) supplier base of power plants and examine whether the mandate leads to changes in these operational decisions of power plants in a way that affects their exposure to risks.[31,32] Appendix E provides examples of power plants' risk factor disclosures that mention those risks.

6.2. Power Plant Sample and Data

Data on the operations of the U.S. power plants is obtained from the S&P Global, EIA, and Federal Energy Regulatory Commission (FERC) databases. I use Demirer and Karaduman's (2024) power plant ownership data to identify the ultimate owner of each plant. Following Demirer and Karaduman (2024), I limit my sample to plants that use natural gas or coal as their primary energy sources, which account for 70% of the total electricity generation in the U.S. during my sample period. Monthly fuel stock data is from Forms EIA-920, 960, and 923. Data on fuel suppliers are collected from Forms EIA-423 and FERC-423. Because Form FERC-423 only provides the redacted lists of suppliers from January to May 2004, I exclude year 2004 from my sample period for the fuel supplier sample. I manually match slightly different names of the same suppliers. The fuel stock data only includes coal-fired power plants because gas-fired plants do not report natural gas stocks.[33] Other characteristics of the power plant owners are from Compustat and CRSP.

6.3. Research Design of the Power Plant Analyses

Using the power plant data, I investigate whether plants' investment and operational decisions related to their risk exposure change in response to the mandate. As previously mentioned, those decisions relate to power plants' (1) geographic diversification, (2) inventory

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[31] Inventory management and geographic diversification are typical operational hedging strategies for firms (e.g., Geczy, Minton, and Schrand 2001).

[32] Note that power plants consider the balance between risk and efficiency when making those decisions. For example, while increasing fuel storage can mitigate shortage risks, it might elevate storage expenses. Similarly, expanding their supplier network can decrease delivery disruptions but may lead to higher administrative costs or the loss of benefits (e.g., price discounts) from maintaining close relationships with select suppliers.

[33] Unlike other power plants, gas-fired power plants receive just-in-time delivery of their natural gas through pipelines. Therefore, gas-fired plants are not required to report their stocks of natural gas.

management, and (3) supplier base. The regression model is almost identical to that of the full sample, except for the dependent variables and fixed effect structures. Specifically, I estimate the following regression model:

$$
\begin{aligned}
y_{p,i,t} = \; & \beta_1 POST_t + \beta_2 POST_t \times TREAT_i + \beta_3 CS\,VARIABLE_{i,t-1} \\
& + \beta_4\, POST_t \times CS\,VARIABLE_{i,t-1} + \; \beta_5\, TREAT_i \times CS\,VARIABLE_{i,t-1} \\
& + \beta_6\, POST_t \times TREAT_i \times CS\,VARIABLE_{i,t-1} + Fixed\ Effects + \; \gamma' X \\
& + \; \varepsilon_{i,t}\ , \qquad\qquad (2)
\end{aligned}
$$

where p, i, and t indicate plants, firms (i.e., owners of power plants), and years, respectively. Again, the main variable of interest is *POST* × *TREAT* × *CS VARIABLE*, where *CS VARIABLE* is based on either Ohlson's (1980) O-score (*BANKRUPTCY RISK)* or management forecast inaccuracy (*MEF INACCURACY*).[34]

For the analyses of geographic diversification, the Herfindahl-Hirschman Index (HHI) based on the capacity concentration of a firm's power plants (*CAPACITY CONCENT.*, *COUNTY COCENT.*, and *STATE CONCENT.*) is used as the dependent variable. The analyses are at the firm-year level, and I include firm and year fixed effects. Standard errors are clustered at the firm level. For the fuel stock analyses, the dependent variable represents either a power plant's average stock level (*AVG. FUEL STOCK)*, its minimum to maximum ratio of fuel stock (*MIN TO MAX RATIO*), or an indicator for fuel stock shortage (*FUEL SHORTAGE*). Here, the analyses are at the plant-coal type-year level, and I include plant, firm, coal type-year, and state-year fixed effects.[35] Coal type-year fixed effects control for the difference in the supply and demand for coals across different coal type-years.[36] And state-year fixed effects control for the difference in electricity

[34] In the power plant sample, *MEF INACCURACY* is used to test the managerial learning channel. This is mainly because the managerial miscalibration variable is available for only a limited number of firms (63 firm-years; 35 unique firms) in the power plant sample.
[35] Plant and firm fixed effects can be included in a single equation because a plant's ownership can change over time. Indeed, M&As happen frequently in the power generation industry (Demirer and Karaduman 2024).
[36] Coal is classified into four types—anthracite, bituminous, subbituminous, and lignite—each varying significantly in the amount of carbon and heat energy produced.

supply and demand across state-years. Standard errors are clustered at the plant level. Lastly, for the analyses of the fuel supplier base, the number of suppliers from which a power plant receives fuel (*#SUPPLIERS*) and the HHI index based on the quantity of fuel received from each supplier (*SUPPLIER CONCENT.*) are used as the dependent variables. The analyses are at the plant-fuel type-year level, and plant, firm, fuel type-year, and state-year fixed effects are included. Standard errors are clustered at the plant level. The other variables are defined as previously described. See Appendix A for variable definitions. Table 6 presents descriptive statistics for each sample.

6.4. Empirical Results from the Power Plant Sample

Before delving into the effect of the mandate on power plants' operational and investment decisions, I rerun the main regression (i.e., Equation (1)) using the power plant sample. The results, presented in Appendix F, indicate that the main findings from the full sample are applicable to the power plant sample. Specifically, the results indicate that treated power plants with high bankruptcy risk or low management forecast quality reduce their risk-taking (proxied by earnings volatility) following the implementation of the mandate.

I then examine whether and how power plants are adjusting their operational and investment decisions to manage risks, focusing on decisions related to geographic diversification, fuel stock, and supplier base. Table 7 shows how the mandate affects the geographic diversification of power plants. Columns 1–3 examine the shareholder-debtholder conflict channel, and Columns 4–6 test the managerial learning channel. In Columns 1–3, the coefficients on *POST* × *TREAT* × *BANKRUPTCY RISK* are all negative and are statistically significant when HHI is defined at the plant or the state levels. Economically, in the post-period, a one-standard-deviation increase in the exposure to the mandate for a firm with high bankruptcy risk results in a 14.5% (10.4%) relative decrease in the degree of capacity concentration estimated at the plant (state) level. This suggests that, following the mandate, owners of power plants close to bankruptcy tend to be geographically more diversified.

In Columns 4–6 of Table 7, the coefficients on *POST* × *TREAT* × *MEF INACCURACY* are all negative and statistically significant when HHI is defined at the plant or the county levels. In the post-period, for a firm with low management forecast accuracy, a one-standard-deviation increase in the exposure to the mandate leads to a 12.1% (9.3%) relative decrease in the degree of capacity concentration estimated at the plant (county) level. The results imply that, after the mandate, owners of power plants with low management forecast quality decrease their exposure to geographic risk by reducing the degree of their capacity concentration.

Table 8 documents the results of the fuel stock analyses. Again, Columns 1–3 explore the shareholder-debtholder conflict channel while Columns 4–6 examine the managerial learning channel. In Column 1, the coefficient on *POST* × *TREAT* × *BANKRUPTCY RISK* is positive and significant (coef.=73.59; t-stat.=1.69), showing that power plants susceptible to shareholder-debtholder conflicts increase their average fuel stock level after the mandate. Economically, a one-standard-deviation increase in the exposure to the mandate results in a 24.6% relative increase in the average fuel stock level for a power plant with high bankruptcy risk in the post-period. In a similar vein, the significantly positive coefficient on *POST* × *TREAT* × *BANKRUPTCY RISK* in Column 2 indicates that the power plants' minimum to maximum ratio of fuel stock increases after the mandate. The results suggest that the inventory management of power plants with high bankruptcy risk becomes more stable after the mandate.

In Column 4, I find that the coefficient on *POST* × *TREAT* × *MEF INACCURACY* is significantly positive (coef.=90.70; t-stat.=2.62), implying that power plants with low management forecast quality are likely to hold more fuel stock after the mandate. Economically, for a firm with low management forecast ability, a one-standard-deviation increase in the exposure to the mandate leads to a 30.3% relative increase in the average fuel stock level post-period. In Column 6, the significantly negative coefficient on *POST* × *TREAT* × *MEF INACCURACY* (coef.=-0.166; t-stat.=-2.35) indicates that those plants also experience less frequent fuel shortages after the mandate. Collectively, the results imply that, in response to the mandate, power plants likely to learn from preparing risk disclosures adjust their fuel stock management in ways that reduce fuel shortage and disruptions.

Table 9 presents the results of the fuel supplier analyses. Columns 1–2 investigate the shareholder-debtholder conflict channel, while Columns 3–4 explore the managerial learning channel. The significantly positive coefficient on *POST* × *TREAT* × *BANKRUPTCY RISK* (coef.=0.501; t-stat.=1.66) in Column 1 indicates that power plants with high bankruptcy risk expand their upstream supplier bases after the mandate. The negative coefficient in Column 2 suggests that power plants with high bankruptcy risk reduce the degree of supplier concentration after the mandate (coef.=-0.115; t-stat=-2.88). The results suggest that plants facing a high bankruptcy risk diversify their supplier bases and reduce their dependence on individual fuel suppliers following the mandate, supporting the shareholder-debtholder conflict channel. For the managerial learning channel, the positive coefficient on *POST* × *TREAT* × *MEF INACCURACY* in Column 3 (coef.=0.469; t-stat.=1.83) suggests that plants with low management forecast ability expand their supplier bases in response to the mandate. In terms of economic magnitude, the coefficients in Columns 1 and 3 correspond to 18.8% and 17.3% increases in the number of suppliers from which a power plant receives fuel, respectively.

Taken together, the collective evidence from the power plant sample suggests that the impact of mandated risk disclosures on corporate risk-taking involves changes in operations and investments, which illuminates how the mandate leads to changes in corporate risk-taking. The findings confirm the two channels I documented with the full sample and show that, after the mandate, power plants that are susceptible to shareholder-debtholder conflicts or with a higher chance of managerial learning adjust their risks downward by geographically diversifying their operations, reducing fuel shortages and disruptions, and expanding their supplier bases.

7. Conclusion

This paper examines whether mandated risk disclosures affect corporate risk-taking by informing outsider investors and managers. First, I hypothesize that mandated risk disclosures influence corporate risk-taking by mitigating risk-related agency conflicts between different stakeholders. I predict that mandated risk disclosures reduce (increase) the risk-taking of firms that are particularly susceptible to shareholder-debtholder conflicts (manager-shareholder conflicts). Additionally, even in the absence of agency conflicts, I argue that mandated risk disclosures affect corporate risk-taking by prompting managerial learning. Allowing managers to be better informed of and alerted to risks their firms face, I predict that mandated risk disclosures reduce the risk-taking of firms whose managers underestimate their firms' future volatility and risks.

Exploiting the 2005 risk factor disclosure mandate, I test my predictions using two different samples. First, I use a broad sample of firms from different industries. Using earnings volatility as a proxy for corporate risk-taking, I find that treated firms with (1) high bankruptcy risk and (2) miscalibrated managers respond to the mandate by reducing their risks. However, I do not find evidence consistent with the idea that the mandate mitigates the conflict between managers and shareholders. My findings collectively suggest that mandated risk disclosures influence corporate risk-taking behavior by mitigating shareholder-debtholder conflicts during financial distress and inducing managerial learning for managers who systematically underestimate risk.

While earnings volatility is often used in the literature to capture corporate risk-taking, it does not explain whether and how firms change their operational and investment decisions to adjust risks. To address this limitation, I move on to the power plant sample, where I can observe granular operational decisions that relate closely to material risks firms are exposed to.

Focusing on power plants' geographic diversification, inventory management, and supplier relationships, I find that power plant owners with high bankruptcy risk or low management forecast ability change their operations in a way that attenuates risk exposure after the mandate. For instance, those plants become more geographically diversified, increase their fuel stock level, and broaden their supplier bases. Overall, the collective evidence from the power plant data shows that firms prone to shareholder-debtholder conflicts or managerial learning respond to the mandate by changing their actual operations and investments, which sheds light on the specific mechanisms through which financial reporting requirements affect corporate risk-taking behavior.

My paper adds to the literature on the effect of financial reporting on investment decisions by documenting its impact on corporate risk-taking. Taking advantage of the power plant data, I overcome the challenge of estimating risk-related behaviors and investigate whether and how financial reporting alters firms' operational and investment decisions closely tied to their material risks. Second, my question contributes to the literature on the economic consequences of risk disclosures. My study provides evidence that mandated risk disclosures mitigate agency conflicts and prompt managerial learning, thereby influencing corporate risk-taking.

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Appendix A
Variable Definitions

This table provides a detailed description of the procedure used to compute each variable used in the analyses. All continuous variables are winsorized at 1% and 99% of the distribution. If the source is not specified, it is from Compustat. The variables are listed in alphabetical order.

Variable	***Definition***
#SUPPLIERS	The number of suppliers from which a power plant receives fuel for each fuel type in year t. (Sources: Forms EIA 423, FERC 423)
ANNUAL RETURN	Cumulative annual returns over the 12 month period at the fiscal year-end. (Source: CRSP)
AVG. FUEL STOCK	A power plant's average stock level of each coal-type in year t. (Sources: Forms EIA 920, 960, 923)
BANKRUPTCY RISK	An indicator that equals one if a firm's O-score is in the top quartile. O-score is computed as -1.32 -0.407 × log(AT) + 6.03 × (LT / AT) - 1.43 × (ACT - LCT)/AT + 0.076 × (LCT / ACT) -1.72 × (1 if LT > AT) -2.37 × (NI / AT) -1.83 × (OANCF / LT) + 0.285 × (1 if NI and lag NI is both negative) - 0.521 × (NI - lag NI) / (absolute value of NI + absolute value of lag NI). Ranks are defined by industry (3-digit SIC code) and year for the full sample and by year for the power plant sample.
CAPACITY CONCENT.	The natural logarithm of the Herfindahl-Hirschman Index based on the distribution of a firm's power plants' electricity generation capacity. (Sources: S&P Global, Demirer and Karaduman (2024)'s plant ownership data)
CASH SURPLUS	Cash from assets in place (OANCF - DPC + XRD) scaled by total assets (AT).
COUNTY CONCENT.	The natural logarithm of the Herfindahl-Hirschman Index based on the distribution of a firm's power plants' electricity generation capacity across different counties. (Sources: S&P Global, Demirer and Karaduman (2024)'s plant ownership data)
FUEL SHORTAGE	An indicator that equals one if a power plant's minimum fuel stock of a coal-type is lower than 40% of its average level in year t. (Sources: Forms EIA 920, 960, 923)
LEVERAGE	Long-term debt (DLTT) plus debt in current liabilities (DLC) scaled by total assets (AT).
MANAGER MISCALIB.	An indicator that equals one if the managerial miscalibration measure of a firm's CFO is above the median. The measure captures the extent to which a firm's CFO underestimates the volatility of S&P returns in year t+1. (Source: Ben-David et al. 2013)
MEF INACCURACY	An indicator that equals one if a firm's average management forecast inaccuracy is above the median. Management forecast inaccuracy is measured as the absolute value of the difference between the management forecasted EPS minus the actual EPS divided by the stock price at the fiscal-year end (PRCC_F). Ranks are defined by industry (3-digit SIC code) and year for the full sample and by year for the power plant sample. (Source: I/B/E/S)
MIN TO MAX RATIO	A power plant's minimum coal stock level divided by its maximum stock level for each coal-type in year t. (Sources: Forms EIA 920, 960, 923)
MTB	Market value of equity (PRCC_F * CSHO) scaled by total assets (AT).
POST	An indicator that equals one for firm-years ending after December 15, 2005.
RECESSION IN EARLY CAREER	An indicator that equals one if there was a recession during the year when a firm's CEO was 24 or 25. A calendar year is defined as a recession year if a trough

	occurs during the year or if the entire year fully falls into a recession period. (Source: Execucomp, NBER)
ROA VOLATILITY	The natural logarithm of the standard deviation of the difference between quarterly ROA and ROA for the same quarter of the previous year, computed in year t. ROA is defined as NIQ divided by ATQ.
SALES GROWTH	(SALE - lag SALE) / lag SALE.
SIZE	The natural logarithm of total sales (SALE).
STATE CONCENT.	The natural logarithm of the Herfindahl-Hirschman Index based on the distribution of a firm's power plants' electricity generation capacity across different states. (Sources: S&P Global, Demirer and Karaduman (2024)'s plant ownership data)
SUPPLIER CONCENT.	The natural logarithm of the Herfindahl-Hirschman Index based on the amount of fuel a power plant receives from each supplier for each fuel type in year t. (Sources: Forms EIA 423, FERC 423)
TREAT	The natural logarithm of the length of risk factor disclosures right before the mandate multiplied by -1. (Source: Dyer et al. 2017)
YEAR2003	An indicator that equals one for firm-years ending between January 2003 - November 2003.
YEAR2004	An indicator that equals one for firm-years ending between December 2003 - November 2004.
YEAR2006	An indicator that equals one for firm-years ending between December 2005 - November 2006.
YEAR2007	An indicator that equals one for firm-years ending between December 2006 - November 2007.
YEAR2008	An indicator that equals one for firm-years ending between December 2007 - December 2008.

Appendix B
Validation Tests of the Setting and Research Design

Validating TREAT: Examining changes in risk factor disclosures around the mandate

To validate the *ex-ante* approach I use to define *TREAT*, I show that firms with high *TREAT* are the ones significantly affected by the mandate. I partition the sample based on whether the *TREAT* variable is above or below the median and examine how the length of risk factor disclosures changes around the mandate. As explained in the draft, treated firms are those that did not voluntarily disclose much risk information before the mandate. Figure A1 shows that treated firms (denoted by the blue line) significantly increased their risk factor disclosures following the mandate, while control firms (denoted by the orange line) did not exhibit a significant change.

Validating the research design: Smith and So's (2022) risk information measure

To further validate that financial statement users find risk factor disclosures useful, I examine whether the amount of risk information conveyed in 10-Ks increases after the mandate, using Smith and So's (2022) risk information measure.[37] Column 1 of Table A1 indicates that the amount of risk information conveyed in 10-K filings is positively associated with the length of risk factor disclosures. Column 2 shows that the risk-relevant information conveyed in 10-K filings significantly increases after the mandate. Most importantly, Column 3 presents that the increase in the risk information in the post-mandate period is concentrated in treated firms (i.e., those that did not provide sufficient risk factor disclosures before the mandate).

The results above collectively validate the research design and lend credence to an important underlying assumption of the paper—the risk factor disclosure mandate led firms to provide additional risk-relevant information, which helped outside investors better understand firms' risk exposure.

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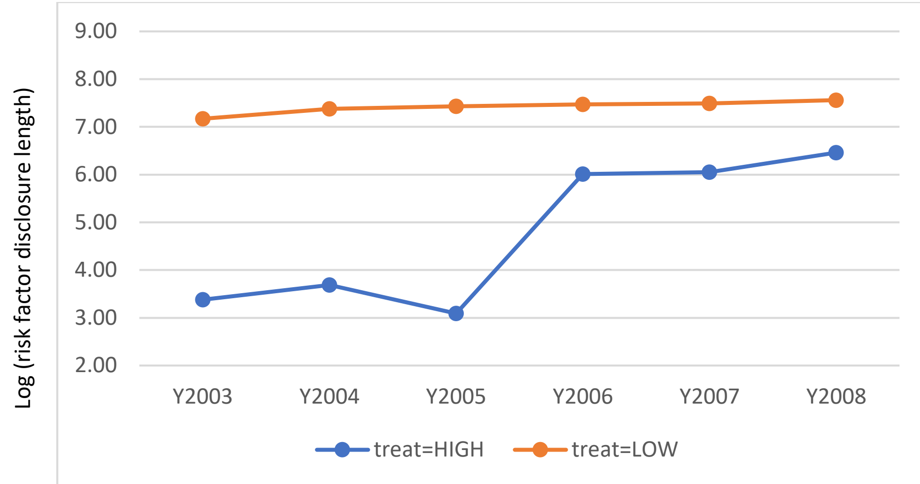
[37] Examining option prices, Smith and So (2022) estimate how information events impact investors' perception of a firm's future riskiness. This measure is specifically constructed to capture the amount of risk information conveyed in anticipated information events, such as 10-K filing dates in my analyses.

Figure A1. Changes in the Risk Factor Disclosures around the Mandate



Notes: This figure plots the median logged length of risk factor disclosure for two groups over the sample period. The groups are defined based on whether the *TREAT* variable is above or below the median. The blue (orange) line represents firms with *TREAT* values above (below) the median.

Table A1. Effects of the 2005 Risk Factor Disclosure Mandate on the amount of Risk Information conveyed in 10-K filings

Dependent Variable:		***RISK INFO*** $_{i,t}$	
	(1) Coef. *t*-Stat.	(2) Coef. *t*-Stat.	(3) Coef. *t*-Stat.
RF DISC. LENGTH $_{i,t}$	**0.059**** **(2.206)**		
POST $_t$		**0.245***** **(5.265)**	**0.236***** **(5.017)**
TREAT $_i$ **×** ***POST*** $_t$			**0.156***** **(4.159)**
Firm-level control variables	Included	Included	Included
Fixed effects			
Year	Included	Not Included	Not Included
Firm	Included	Included	Included
R-Squared	0.499	0.407	0.409
No. of Observations	7,267	7,383	7,383

Notes: This table presents the results from regressions of the natural logarithm of the absolute value of risk information measure in year t on (1) the natural logarithm of the length of risk factor disclosures in year t, (2) an indicator variable that equals one for the post-mandate period, (3) and the natural logarithm of the length of risk factor disclosures right before the mandate multiplied by -1, an indicator variable that equals one for the post-mandate period, and interaction terms between these variables. The risk information measure is from Smith and So (2022) and is measured around the 10-K filing dates using 122-day options. Standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent levels, respectively.

Appendix C
Main Results with the Full Sample – Alternative Treatment Variable

Dependent Variable:	(1) Coef. *t*-Stat.	(2) Coef. *t*-Stat.	(3) Coef. *t*-Stat.
		CS VARIABLE =	
	BANKRUPTCY RISK	***RECESSION IN EARLY CAREER***	***MANAGER MISCALIB.***
$POST_t$	-0.034	-0.028	0.045
	(-0.900)	(-0.447)	(0.146)
$\Delta RISKFACTOR_i$			-0.314***
			(-3.156)
$POST_t \times \Delta RISKFACTOR_i$	0.054**	0.093**	0.270**
	(2.513)	(2.351)	(2.284)
$CS\ VARIABLE_{i,\,t\text{-}1}$	0.354***	-0.016	0.051
	(8.425)	(-0.226)	(0.301)
$POST_t \times CS\ VARIABLE_{i,\,t\text{-}1}$	-0.100**	0.000	-0.070
	(-2.146)	(0.004)	(-0.355)
$\Delta RISKFACTOR_i \times CS\ VARIABLE_{i,\,t\text{-}1}$	0.069*	-0.026	0.308**
	(1.669)	(-0.390)	(2.146)
$\boldsymbol{POST_t \times \Delta RISKFACTOR_i \times CS\ VARIABLE_{i,\,t\text{-}1}}$	**-0.102****	**-0.019**	**-0.346***
	(-2.043)	**(-0.297)**	**(-1.959)**
Firm-level control variables	Included	Included	Included
Fixed effects			
Firm	Included	Included	Not Included
Industry	Not Included	Not Included	Included
Year	Included	Included	Included
R-Squared	0.637	0.611	0.361
No. of Observations	15,066	6,982	756

Notes: This table presents the results from regressions of the natural logarithm of the standard deviation of return on volatility (ROA) on the *change* of the natural logarithm of the risk factor disclosure length around the mandate, an indicator variable that equals one for the post-regulation period (i.e., December 2005), indicators based on (1) Ohlson (1980)'s O-score, (2) the timing of managers' entry into the labor market, or (3) managerial miscalibration, interaction terms between these variables and control variables. Standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent levels, respectively.

Appendix D
Main Results with the Full Sample – Alternative Risk-Taking Proxies

Dependent Variable:	***ROIC VOLATILITY*** $_{i,t}$	***OCF VOLATILITY*** $_{i,t}$	***ICF VOLATILITY*** $_{i,t}$	***FCF VOLATILITY*** $_{i,t}$	***R&D EXPENSE*** $_{i,t}$	***ROIC VOLATILITY*** $_{i,t}$	***OCF VOLATILITY*** $_{i,t}$	***ICF VOLATILITY*** $_{i,t}$	***FCF VOLATILITY*** $_{i,t}$	***R&D EXPENSE*** $_{i,t}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.
	t-Stat.	*t*-Stat.	*t*-Stat.	*t*-Stat.	*t*-Stat.	*t*-Stat.	*t*-Stat.	*t*-Stat.	*t*-Stat.	*t*-Stat.
					CS VARIABLE =					
			BANKRUPTCY RISK					***MANAGER MISCALIB.***		
POST $_t$	-0.063**	-0.000	0.049	0.110**	-0.003*	-0.343	-0.109	-0.099	0.026	-0.009
	(-2.165)	(-0.007)	(1.224)	(2.521)	(-1.770)	(-1.542)	(-0.653)	(-0.363)	(0.100)	(-0.632)
TREAT $_i$						-0.294***	-0.183***	-0.139	-0.190*	-0.012*
						(-3.532)	(-3.069)	(-1.446)	(-1.835)	(-1.805)
POST $_i$ × *TREAT* $_t$	0.036**	-0.021	0.017	0.057**	-0.002**	0.268***	0.211***	0.147	0.286**	0.008
	(2.364)	(-1.606)	(0.791)	(2.400)	(-2.567)	(2.794)	(2.803)	(1.271)	(2.359)	(1.124)
CS VARIABLE $_{i,t-1}$	0.229***	0.092***	-0.040	0.083*	0.007***	-0.228*	-0.083	-0.149	-0.076	0.007
	(7.076)	(3.515)	(-0.888)	(1.882)	(3.095)	(-1.708)	(-0.853)	(-0.900)	(-0.464)	(0.661)
POST $_t$ × *CS VARIABLE* $_{i,t-1}$	-0.027	0.004	-0.042	-0.048	0.002	0.197	0.076	0.178	0.133	0.000
	(-0.753)	(0.120)	(-0.835)	(-0.975)	(0.609)	(1.226)	(0.617)	(0.881)	(0.702)	(0.017)
TREAT $_i$ × *CS VARIABLE* $_{i,t-1}$	0.033	0.014	0.014	0.067	-0.000	0.240**	0.166*	0.182	0.181	0.005
	(1.014)	(0.549)	(0.319)	(1.599)	(-0.208)	(1.982)	(1.898)	(1.157)	(1.191)	(0.575)
POST $_t$ **×** ***TREAT*** $_i$ **×** ***CS VARIABLE*** $_{i,t-1}$	**-0.076****	**-0.027**	**-0.098***	**-0.081***	**-0.004***	**-0.293****	**-0.294*****	**-0.243**	**-0.303***	**-0.010**
	(-2.064)	**(-0.884)**	**(-1.887)**	**(-1.673)**	**(-1.757)**	**(-2.051)**	**(-2.591)**	**(-1.310)**	**(-1.769)**	**(-0.985)**
Firm-level control variables	Included	Included	Included	Included	Included	Included	Included	Included	Included	Included
Fixed effects										
Firm	Included	Included	Included	Included	Included	Not Included	Not Included	Not Included	Not Included	Not Included
Industry	Not Included	Not Included	Not Included	Not Included	Not Included	Included	Included	Included	Included	Included
Year	Included	Included	Included	Included	Included	Included	Included	Included	Included	Included
R-Squared	0.718	0.664	0.574	0.524	0.895	0.358	0.393	0.216	0.153	0.472
No. of Observations	15,890	15,850	15,850	15,850	15,909	776	772	772	772	776

Notes: This table presents the results from regressions of alternative risk-taking proxies on the natural logarithm of the length of risk factor disclosures right before the mandate multiplied by -1, an indicator variable that equals one for the post-regulation period (i.e., December 2005), indicators based on (1) Ohlson (1980)'s O-score, or (2) managerial miscalibration, interaction terms between these variables and control variables. Risk-taking proxies used in this table are (1) the natural logarithm of the standard deviation of return on invested capital (ROIC), operating cash flows (OCF), investment cash flows (ICF), financial cash flows (FCF), and (2) R&D expenses scaled by total assets. Standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent levels, respectively.

Appendix E
Examples of Power Plant Owners' Risk Factor Disclosures

1. Risks related to Geographic Factors
AES Corp's 10-K filings in 2005 (emphasis added)

> […] We have also sought to reduce our credit risk by locating our plants in different geographic areas in order to mitigate the effects of regional economic downturns.

Great Plains Energy Inc's 10-K filings in 2005 (emphasis added)

> **The Company has Regulatory Risks**
> The Company is subject to extensive federal and state regulation […]
>
> **KCP&L and Strategic Energy are Affected by Demand, Seasonality and Weather**
> The results of operations of KCP&L and Strategic Energy can be materially affected by changes in weather and customer demand.

2. Risks related to Fuel Stock Storage
Empire District Electric Co's 10-K filings in 2005 (emphasis added)

> **We have recently experienced, and may continue to experience, coal delivery shortfalls which could require us to reduce the output of our coal-fired generating facilities and lead to increases in our fuel and purchased power costs.**
>
> We depend upon regular deliveries of coal as fuel for our Riverton, Asbury and Iatan plants, and as fuel for the facility which supplies us with purchased power under our contract with Westar Energy. Substantially all of this coal comes from mines in the Powder River Basin of Wyoming and is delivered to the plants by railroad. In recent months, due to widespread railroad congestion problems, the railroads have been unable to achieve the delivery cycle times required to maintain our plants' inventory levels. As a result, inventory levels at our plants have declined. We expect that the railroads' congestion problems and resulting delivery delays will continue for an indefinite period. As a result, we have implemented coal conservation and supply replacement measures to retain adequate reserve inventories at our facilities.

3. Risks related to Relationships with Fuel Suppliers
Oge Energy Corp's 10-K filings in 2005 (emphasis added)

> We have certain coal supply contracts in place; however, there can be no assurance that the counterparties to these agreements will fulfill their obligations to supply coal to us. The suppliers under these agreements may experience financial or technical problems which inhibit their ability to fulfill their obligations to us. […]Coal delivery may be subject to short-term interruptions or reductions due to various factors, including transportation problems, weather and availability of equipment. Failure or delay by our suppliers of coal deliveries could disrupt our ability to deliver electricity and require us to incur additional expenses to meet the needs of our customers. In addition, as agreements with our suppliers expire, we may not be able to enter into new agreements for coal delivery on equivalent terms.

AES Corp's 10-K filings in 2005 (emphasis added)

> ***Most of our contract generation businesses are dependent to a large degree on one or a limited number of customers and a limited number of fuel suppliers.***
> Most of our contract generation businesses rely on power sales contracts with one or a limited number of customers for the majority of, and in some case all of, the relevant plant's output and revenues over the term of the power sales contract. The remaining term of the power sales contracts related to our contract generation power plants ranges from 1 to 25 years. Many of these businesses also limit their exposure to fluctuations in fuel prices by entering into long term contracts for fuel with a limited number of suppliers. The cash flows and results of operations of such businesses are dependent on the continued ability of their customers and suppliers to meet their obligations under the relevant power sales contract or fuel supply contract, respectively.

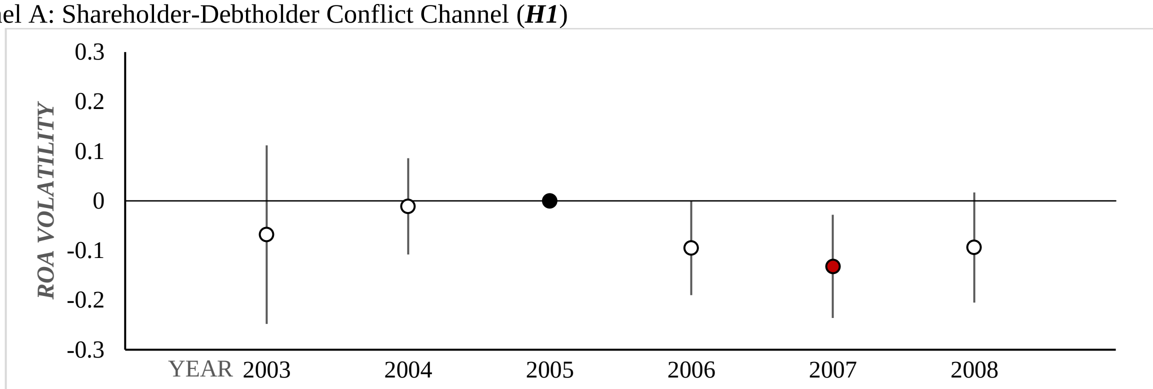
Appendix F
Main Results with the Power Plant Sample – Earnings Volatility (Equation (1))

Dependent Variable:	***ROA VOLATILITY*** $_{i,t}$		
	(1)	(2)	(3)
	Coef.	Coef.	Coef.
	t-Stat.	*t*-Stat.	*t*-Stat.
		CS VARIABLE =	
	BANKRUPTCY RISK	***RECESSION IN EARLY CAREER***	***MEF INACCURACY***
POST $_t$	1.050**	0.691	0.890
	(2.498)	(1.414)	(1.256)
POST $_i$ × *TREAT* $_t$	0.139	-0.062	0.271**
	(1.295)	(-0.384)	(2.316)
CS VARIABLE $_{i,t-1}$	0.373*	0.241	0.137
	(1.892)	(0.969)	(0.769)
POST $_t$ × *CS VARIABLE* $_{i,t-1}$	-0.169	0.411**	0.071
	(-0.715)	(2.084)	(0.396)
TREAT $_i$ × *CS VARIABLE* $_{i,t-1}$	0.279	0.231	0.324
	(1.440)	(0.955)	(1.645)
POST $_t$ **×** ***TREAT*** $_i$ **×** ***CS VARIABLE*** $_{i,t-1}$	**-0.625****	**0.065**	**-0.356***
	(-2.614)	**(0.287)**	**(-1.836)**
Firm-level control variables	Included	Included	Included
Fixed effects			
Firm	Included	Included	Included
Year	Included	Included	Included
R-Squared	0.656	0.679	0.651
No. of Observations	418	379	312

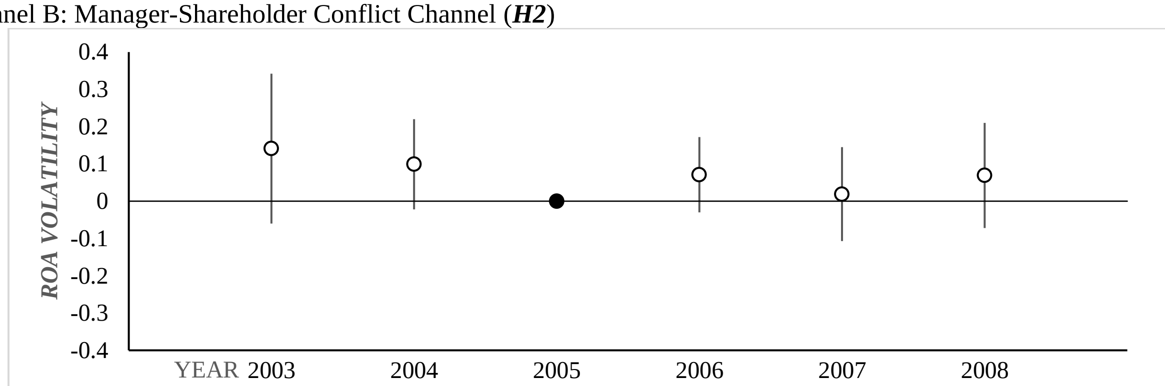
Notes: Using the power plant sample, this table presents the results from regressions of the natural logarithm of the standard deviation of return on volatility (ROA) on the natural logarithm of the length of risk factor disclosures right before the mandate multiplied by -1, an indicator variable that equals one for the post-regulation period (i.e., December 2005), indicators based on (1) Ohlson (1980)'s O-score, (2) the timing of managers' entry into the labor market, or (3) management forecast inaccuracy, interaction terms between these variables and control variables. See Appendix A for variable definitions. Standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent levels, respectively.

Figure 1. Dynamic Effect of the Mandate on Corporate Risk Taking

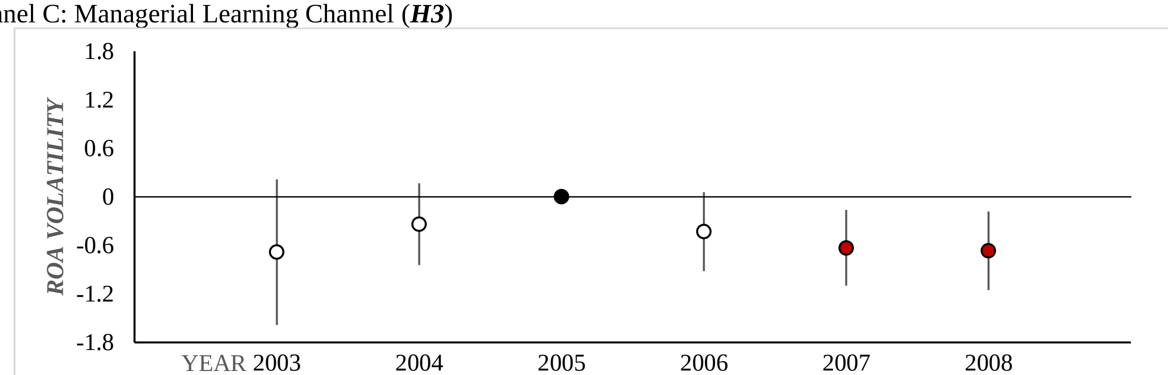
Panel A: Shareholder-Debtholder Conflict Channel (***H1***)



Panel B: Manager-Shareholder Conflict Channel (***H2***)



Panel C: Managerial Learning Channel (***H3***)



Notes: In the figures above, the x-axes represent years centered around the implementation of the risk factor disclosure mandate in 2005. The y-axes represent the natural logarithm of the standard deviation of seasonally adjusted quarterly returns on assets (ROA) in year t. The coefficient estimates in Panel A capture the percentage change in the earnings volatility of treated firms with high bankruptcy risk relative to that of treated firms with low bankruptcy risk in each period. The coefficient estimates in Panel B capture the percentage change in the earnings volatility of treated firms with recession managers relative to that of treated firms without in each period. The coefficient estimates in Panel C capture the percentage change in the earnings volatility of treated firms with miscalibrated managers relative to that of treated firms without in each period. The figure in Panel A corresponds to the regression tabulated in Column 2 in Table 3, Panel B. The figure in Panel B corresponds to the regression tabulated in Column 4 in Table 3, Panel B. The figure in Panel C corresponds to the regression tabulated in Column 6 in Table 3, Panel B. Each unshaded (red) circle on the graph represents an insignificant (significant) regression coefficient. The figures also plot the two-tailed 90% confidence intervals around each point estimate.

Table 1. Sample Selection – the Full Sample

N	Sample composition	Observations dropped	Observations remaining
(1)	Sample of firm-years in Compustat from 2001 to 2010 with positive total assets, non-missing historical SIC codes, and non-missing permno variables		55,582
(2)	*Less:* Firms that belong to the financial or public sectors (SIC=6 or 9)	11,012	44,570
(3)	*Less:* Observations with fiscal periods not ending between 2003 and 2008	18,120	26,450
(4)	*Less:* Observations with missing data needed to construct the main variables used in the regressions	9,644	16,806
(5)	*Less:* Observations with missing data of all the cross-sectional variables	418	16,388
(6)	*Less:* Observations with missing control variables	272	16,116
	Final sample of firm-year observations available for analysis		16,116

Notes: This table presents the sample selection procedure I followed to construct the full sample.

Table 2. Descriptive Statistics – the Full Sample

Panel A: the Full Sample

	Mean	SD	P25	P50	P75	N
RF DISC. LENGTH_PRE	996.39	1085.17	20.86	668.57	1693.40	16,116
TREAT^{+}	-5.338	2.570	-7.434	-6.505	-3.038	16,116
POST	0.580	0.494	0.000	1.000	1.000	16,116
ROA VOLATILITY (unlogged)	0.034	0.057	0.005	0.012	0.035	16,116
BANKRUPTCY RISK (raw)	-1.502	2.459	-2.988	-1.602	-0.245	15,909
RECESSION IN EARLY CAREER	0.437	0.496	0.000	0.000	1.000	7,221
MANAGER MISCALIB. (raw)	-0.028	0.963		0.244		776
MEF INACCURACY (raw)	0.040	0.170	0.002	0.004	0.012	5,839
MTB	3.079	3.179	1.400	2.175	3.586	16,116
LEVERAGE	0.155	0.168	0.000	0.107	0.263	16,116
SIZE	5.830	2.109	4.386	5.846	7.268	16,116
SALES GROWTH	0.143	0.370	-0.008	0.089	0.215	16,116
ANNUAL RETURN	0.218	0.680	-0.166	0.097	0.409	16,116
CASH SURPLUS	0.059	0.126	0.009	0.063	0.125	16,116

Panel B: Comparing the characteristics of firms with a high vs. low TREAT variable

	High *TREAT*	Low *TREAT*	Diff. in Mean
	Mean	Mean	
RF DISC. LENGTH_PRE	132.83	1861.03	-1728.20***
TREAT^{+}	-3.267	-7.412	4.145***
POST	0.574	0.586	-0.012
ROA VOLATILITY (unlogged)	0.026	0.042	-0.016***
BANKRUPTCY RISK (raw)	-1.662	-1.343	-0.319***
RECESSION IN EARLY CAREER	0.436	0.440	-0.004
MANAGER MISCALIB. (raw)	-0.085	0.070	-0.155**
MEF INACCURACY (raw)	0.034	0.048	-0.014***
MTB	2.988	3.171	-0.183***
LEVERAGE	0.167	0.144	0.023***
SIZE	6.299	5.361	0.938***
SALES GROWTH	0.110	0.177	-0.067***
ANNUAL RETURN	0.214	0.221	-0.007
CASH SURPLUS	0.067	0.052	0.015***

Notes: This table presents the descriptive statistics for the variables used in my analyses of the full sample. Panel A reports descriptive statistics for the entire sample. Panel B presents the mean values for firms whose *TREAT* variable is above vs. below median, respectively, as well as the differences between the two groups. + indicates that the variable is standardized to have a mean of zero and a standard deviation of one in my regression analyses. ***, **, and * in Panel B denote statistical significance at the two-tailed 1, 5, and 10 percent levels, respectively. To protect the confidentiality of the data, P25 and P75 of the *MANAGER MISCALI.* variable are not provided. *RF DISC. LENGTH_PRE* indicates the length of risk factor disclosures right before the mandate for each firm, which is used to define the *TREAT* variable. See Appendix A for other variable definitions.

Table 3. Effect of the Risk Factor Disclosure Mandate on Corporate Risk Taking

Panel A: Static Analyses

Dependent Variable:	***ROA VOLATILITY*** $_{i,t}$					
	(1)	(2)	(3)	(4)	(5)	(6)
	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.
	t-Stat.	*t*-Stat.	*t*-Stat.	*t*-Stat.	*t*-Stat.	*t*-Stat.
	CS VARIABLE =					
	BANKRUPTCY RISK		***RECESSION IN EARLY CAREER***		***MANAGER MISCALIB.***	
POST $_t$	-0.002	-0.024	0.007	-0.023	0.304	0.141
	(-0.060)	(-0.640)	(0.119)	(-0.374)	(0.991)	(0.459)
TREAT $_i$					-0.339***	-0.217**
					(-3.347)	(-2.116)
POST $_t$ × *TREAT* $_i$	-0.001	0.009	0.027	0.041	0.126	0.080
	(-0.069)	(0.441)	(0.689)	(1.109)	(1.037)	(0.661)
CS VARIABLE $_{i,t-1}$	0.434***	0.371***	-0.030	-0.035	-0.006	0.013
	(11.094)	(9.336)	(-0.402)	(-0.480)	(-0.033)	(0.077)
POST $_t$ × *CS VARIABLE* $_{i,t-1}$	-0.145***	-0.118***	-0.007	0.002	-0.055	-0.092
	(-3.240)	(-2.652)	(-0.105)	(0.034)	(-0.261)	(-0.461)
TREAT $_i$ × *CS VARIABLE* $_{i,t-1}$	0.039	0.061	0.036	0.043	0.364**	0.298*
	(1.001)	(1.569)	(0.480)	(0.580)	(2.359)	(1.953)
POST $_t$ **×** ***TREAT*** $_i$ **×** ***CS VARIABLE*** $_{i,t-1}$	**-0.086***	**-0.094****	**0.002**	**0.000**	**-0.413****	**-0.318***
	(-1.826)	**(-2.009)**	**(0.037)**	**(0.005)**	**(-2.196)**	**(-1.758)**
MTB $_{i,t-1}$		-0.004		-0.011		0.021
		(-1.070)		(-1.553)		(1.318)
LEVERAGE $_{i,t-1}$		-0.229*		-0.135		-0.215
		(-1.894)		(-0.698)		(-0.560)
SIZE $_{i,t-1}$		0.126***		0.223***		-0.179***
		(4.239)		(3.320)		(-6.069)
SALES GROWTH $_{i,t-1}$		-0.163***		-0.417***		-0.341
		(-6.302)		(-5.799)		(-1.433)
ANNUAL RETURN $_{i,t-1}$		-0.053***		-0.124***		-0.228**
		(-4.230)		(-4.380)		(-1.974)
CASH SURPLUS $_{i,t-1}$		-0.626***		-1.149***		-1.584***
		(-5.650)		(-4.965)		(-3.011)
Fixed effects						
Firm	Included	Included	Included	Included	Not Included	Not Included
Industry	Not Included	Not Included	Not Included	Not Included	Included	Included
Year	Included	Included	Included	Included	Included	Included
R-Squared	0.647	0.651	0.608	0.617	0.267	0.353
No. of Observations	15,909	15,909	7,221	7,221	776	776

Table 3 – *continued*

Panel B: Dynamic Analyses

Dependent Variable:	***ROA VOLATILITY*** $_{i,t}$					
	(1)	(2)	(3)	(4)	(5)	(6)
	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.
	t-Stat.	*t*-Stat.	*t*-Stat.	*t*-Stat.	*t*-Stat.	*t*-Stat.
	CS VARIABLE =					
	BANKRUPTCY RISK		***RECESSION IN EARLY CAREER***		***MANAGER MISCALIB.***	
TREAT $_i$					-0.589***	-0.479**
					(-3.382)	(-2.547)
CS VARIABLE $_{i,t-1}$	0.378***	0.336***	-0.079	-0.074	-0.131	-0.117
	(7.925)	(6.993)	(-0.968)	(-0.919)	(-0.461)	(-0.419)
TREAT $_i$ × *CS VARIABLE* $_{i,t-1}$	0.058	0.073	-0.032	-0.032	0.527**	0.574**
	(1.155)	(1.447)	(-0.395)	(-0.395)	(2.112)	(2.289)
YEAR2003 $_t$ **×** ***TREAT*** $_i$ **×** ***CS VARIABLE*** $_{i,t-1}$	**-0.084**	**-0.068**	**0.142**	**0.141**	**-0.387**	**-0.684**
	(-0.752)	**(-0.620)**	**(1.129)**	**(1.158)**	**(-0.768)**	**(-1.253)**
YEAR2004 $_t$ **×** ***TREAT*** $_i$ **×** ***CS VARIABLE*** $_{i,t-1}$	**-0.024**	**-0.011**	**0.080**	**0.099**	**-0.176**	**-0.338**
	(-0.399)	**(-0.183)**	**(1.104)**	**(1.353)**	**(-0.550)**	**(-1.100)**
YEAR2006 $_t$ **×** ***TREAT*** $_i$ **×** ***CS VARIABLE*** $_{i,t-1}$	**-0.096***	**-0.095**	**0.069**	**0.071**	**-0.366**	**-0.430**
	(-1.656)	**(-1.637)**	**(1.125)**	**(1.151)**	**(-1.178)**	**(-1.454)**
YEAR2007 $_t$ **×** ***TREAT*** $_i$ **×** ***CS VARIABLE*** $_{i,t-1}$	**-0.130****	**-0.132****	**0.015**	**0.019**	**-0.611****	**-0.630****
	(-2.044)	**(-2.093)**	**(0.199)**	**(0.250)**	**(-2.124)**	**(-2.219)**
YEAR2008 $_t$ **×** ***TREAT*** $_i$ **×** ***CS VARIABLE*** $_{i,t-1}$	**-0.093**	**-0.094**	**0.060**	**0.069**	**-0.668****	**-0.667****
	(-1.378)	**(-1.400)**	**(0.689)**	**(0.813)**	**(-2.247)**	**(-2.265)**
Firm-level Control Variables	Not Included	Included	Not Included	Included	Not Included	Included
YEAR, YEAR × TREAT, YEAR × CS VARIABLE Indicators	Included	Included	Included	Included	Included	Included
Fixed effects						
Firm	Included	Included	Included	Included	Not Included	Not Included
Industry	Not Included	Not Included	Not Included	Not Included	Included	Included
Year	Included	Included	Included	Included	Included	Included
R-Squared	0.649	0.653	0.611	0.62	0.277	0.364
No. of Observations	15,909	15,909	7,221	7,221	776	776

Notes: This table presents the results from regressions of the natural logarithm of the standard deviation of return on volatility (ROA) on the natural logarithm of the length of risk factor disclosures right before the mandate multiplied by -1, an indicator variable that equals one for the post-regulation period (i.e., December 2005), indicators based on (1) Ohlson (1980)'s O-score, (2) the timing of managers' entry into the labor market, or (3) managerial miscalibration, interaction terms between these variables and control variables. The regressions in Panel A use a single post-treatment indicator for the period after the regulation and the regressions in Panel B use indicators representing each year in my sample period. See Appendix A for variable definitions. Standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent levels, respectively.

Table 4. Cross-Sectional Tests Based on Debt Characteristics

Dependent Variable:	*ROA VOLATILITY* $_{i,t}$			
	(1) Coef. t-Stat.	(2) Coef. t-Stat.	(3) Coef. t-Stat.	(4) Coef. t-Stat.
	% of public debt		**% of debt maturing in the first year of the mandate**	
	HIGH	LOW	HIGH	LOW
POST $_t$	0.102	-0.097	-0.015	-0.022
	(0.971)	(-1.143)	(-0.165)	(-0.364)
POST $_t$ × *TREAT* $_i$	0.038	-0.044	0.048	0.013
	(0.907)	(-0.865)	(1.004)	(0.394)
BANKRUPTCY RISK $_{i,t-1}$	0.256***	0.415***	0.352***	0.379***
	(2.736)	(5.261)	(3.508)	(5.291)
POST $_t$ × *BANKRUPTCY RISK* $_{i,t-1}$	-0.090	-0.222**	-0.121	-0.127
	(-0.886)	(-2.436)	(-1.081)	(-1.535)
TREAT $_i$ × *BANKRUPTCY RISK* $_{i,t-1}$	0.270***	-0.072	0.228**	0.032
	(2.718)	(-0.954)	(2.243)	(0.450)
POST $_t$ **×** ***TREAT*** $_i$ **×** ***BANKRUPTCY RISK*** $_{i,t-1}$	**-0.293****	**0.030**	**-0.272****	**-0.079**
	(-2.553)	**(0.310)**	**(-2.245)**	**(-0.948)**
p-value for difference in the coefficients on: *POST × TREAT × BANKRUPTCY RISK*	0.031		0.190	
Firm-level control variables	Included	Included	Included	Included
Fixed effects				
Firm	Included	Included	Included	Included
Year	Included	Included	Included	Included
R-Squared	0.725	0.697	0.575	0.603
No. of Observations	3,889	3,943	3,187	6,133

Notes: This table presents the results from regressions of the natural logarithm of the standard deviation of return on volatility (ROA) on the natural logarithm of the length of risk factor disclosures right before the mandate multiplied by -1, an indicator variable that equals one for the post-regulation period (i.e., December 2005), indicators based on Ohlson (1980)'s O-score, interaction terms between these variables and control variables. Columns 1 and 2 partition the sample into two groups based on whether or not the ratio of public debt to total debt is above the median. Columns 3 and 4 partition the sample into two groups based on whether or not the percentage of long-term debt maturing in the first year of the mandate exceeds 15%. See Appendix A for variable definitions. Standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent levels, respectively.

Table 5. Additional Tests for the Managerial Learning Channel

Panel A: Effects of the Risk Factor Disclosure Mandate on Managerial Miscalibration

Dependent Variable:	*MANAGER MISCALIB.* $_{i, t}$
	(1)
	Coef.
	t-Stat.
POST $_t$	0.101
	(0.669)
TREAT $_i$	-0.034
	(-0.594)
POST $_t$ **×** ***TREAT*** $_i$	**-0.106***
	(-1.721)
Firm-level control variables	Included
Fixed effects	
Industry	Included
Year	Included
R-Squared	0.156
No. of Observations	812

Panel B: An Alternative Proxy: Management Forecast Quality

Dependent Variable:	*ROA VOLATILITY* $_{i, t}$	*MEF INACCURACY* $_{i, t}$
	(1)	(2)
	Coef.	Coef.
	t-Stat.	*t*-Stat.
POST $_t$	0.033	-0.012
	(0.447)	(-1.340)
POST $_t$ × *TREAT* $_i$	0.119***	0.002
	(2.667)	(0.836)
MEF INACCURACY $_{i, t-1}$	0.107**	-0.019***
	(1.997)	(-3.102)
POST $_t$ × *MEF INACCURACY* $_{i, t-1}$	0.054	0.035***
	(0.830)	(4.018)
TREAT $_i$ × *MEF INACCURACY* $_{i, t-1}$	0.110**	0.008
	(2.155)	(1.313)
POST $_t$ **×** ***TREAT*** $_i$ **×** ***MEF INACCURACY*** $_{i, t-1}$	**-0.128****	**-0.015***
	(-2.195)	**(-1.711)**
Firm-level control variables	Included	Included
Fixed effects		
Firm	Included	Included
Year	Included	Included
R-Squared	0.629	0.723
No. of Observations	5,839	4,852

Notes: Panel A in this table presents the results from regressions of managerial miscalibration on the natural logarithm of the length of risk factor disclosures right before the mandate multiplied by -1, an indicator variable that equals one for the post-regulation period (i.e., December 2005), interaction terms between these variables and control variables. Panel B presents the results from regressions of (1) the natural logarithm of the standard deviation of return on volatility and (2) management forecast inaccuracy on the natural logarithm of the length of risk factor disclosures right before the mandate multiplied by -1, an indicator variable that equals one for the post-regulation period (i.e., December 2005), an indicator based on management forecast inaccuracy in the previous year, interaction terms between these variables and control variables. See Appendix A for variable definitions. Standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent levels, respectively.

Table 6. Descriptive Statistics – the Power Plant Sample

Panel A: Sample for Geographic Diversification Analyses

	Mean	SD	P25	P50	P75	N
TREAT#	-4.113	2.606	-6.412	-3.814	-1.816	425
POST	0.649	0.478	0.000	1.000	1.000	425
ROA VOLATILITY (unlogged)	0.010	0.014	0.003	0.005	0.011	425
CAPACITY CONCENT.	-1.032	0.793	-1.603	-1.085	-0.245	417
COUNTY CONCENT.	-0.907	0.770	-1.534	-0.898	-0.010	417
STATE CONCENT.	-0.405	0.533	-0.718	-0.090	0.000	417
BANKRUPTCY RISK (raw)	-1.125	1.028	-1.495	-0.994	-0.523	418
MEF INACCURACY (raw)	0.015	0.050	0.002	0.004	0.008	312
MTB	2.187	1.412	1.419	1.766	2.456	425
LEVERAGE	0.305	0.118	0.244	0.296	0.372	425
SIZE	8.726	1.287	7.889	8.862	9.413	425
SALES GROWTH	0.058	0.258	-0.019	0.059	0.147	425
ANNUAL RETURN	0.159	0.298	0.022	0.136	0.279	425
CASH SURPLUS	0.041	0.049	0.013	0.033	0.054	425

Panel B: Sample for Fuel Stock Analyses

	Mean	SD	P25	P50	P75	N
TREAT#	-5.042	2.681	-7.438	-5.863	-2.752	1,343
POST	0.659	0.474	0.000	1.000	1.000	1,343
FUEL SHORTAGE	0.159	0.365	0.000	0.000	0.000	1,343
MIN TO MAX RATIO	0.451	0.204	0.318	0.456	0.594	1,343
AVG. FUEL STOCK	299.264	304.458	83.403	195.359	405.162	1,343
BANKRUPTCY RISK (raw)	-0.920	0.665	-1.411	-1.052	-0.630	1,340
MEF INACCURACY (raw)	0.021	0.100	0.002	0.004	0.007	1,210
MTB	1.948	1.223	1.380	1.675	2.130	1,343
LEVERAGE	0.341	0.084	0.285	0.323	0.380	1,343
SIZE	8.828	0.754	8.426	9.063	9.402	1,343
SALES GROWTH	0.015	0.219	-0.045	0.042	0.111	1,343
ANNUAL RETURN	0.133	0.275	0.051	0.131	0.208	1,343
CASH SURPLUS	0.024	0.022	0.013	0.028	0.037	1,343

Panel C: Sample for Fuel Supplier Base Analyses

	Mean	SD	P25	P50	P75	N
TREAT#	-4.731	2.700	-7.243	-5.108	-2.567	4,122
POST	0.789	0.408	1.000	1.000	1.000	4,122
SUPPLIER CONCENT.	-0.276	0.488	-0.424	0.000	0.000	4,122
#SUPPLIERS	2.709	3.940	1.000	1.000	2.000	4,122
BANKRUPTCY RISK (raw)	-0.983	0.667	-1.420	-1.102	-0.747	4,085
MEF INACCURACY (raw)	0.015	0.050	0.002	0.004	0.007	3,547
MTB	2.055	1.245	1.464	1.727	2.335	4,122
LEVERAGE	0.330	0.092	0.277	0.313	0.377	4,122
SIZE	8.913	0.841	8.472	9.154	9.433	4,122
SALES GROWTH	0.022	0.236	-0.041	0.042	0.115	4,122
ANNUAL RETURN	0.112	0.277	0.042	0.129	0.221	4,122
CASH SURPLUS	0.028	0.025	0.015	0.029	0.042	4,122

Notes: This table presents the descriptive statistics for the variables used in the power plant sample. Panels A, B, and C present the descriptive statistics for the variables used in geographic diversification, fuel stock, and fuel supplier base analyses, respectively. # indicates that the variable is standardized to have a mean of zero and a standard deviation of one in my regression analyses. See Appendix A for variable definitions.

Table 7. Effect of the Risk Factor Disclosure Mandate on Geographic Diversification of Power Plants

Dependent Variable:	*CAPACITY CONCENT.* $_{i,t}$	*COUNTY CONCENT.* $_{i,t}$	*STATE CONCENT.* $_{i,t}$	*CAPACITY CONCENT.* $_{i,t}$	*COUNTY CONCENT.* $_{i,t}$	*STATE CONCENT.* $_{i,t}$
	(1)	(2)	(3)	(4)	(5)	(6)
	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.
	t-Stat.	t-Stat.	t-Stat.	t-Stat.	t-Stat.	t-Stat.
	CS VARIABLE =					
	BANKRUPTCY RISK			***MEF INACCURACY***		
$POST_t$	-0.048	-0.053	-0.021	0.026	0.002	-0.003
	(-1.131)	(-1.435)	(-0.641)	(0.497)	(0.042)	(-0.069)
$POST_t \times TREAT_i$	0.031	0.034	0.027	0.080	0.061	0.005
	(0.789)	(1.156)	(0.887)	(1.377)	(1.351)	(0.141)
$CS\ VARIABLE_{i,t-1}$	-0.068	-0.067*	-0.112**	0.038	0.010	-0.016
	(-1.490)	(-1.880)	(-2.377)	(0.742)	(0.214)	(-0.371)
$POST_t \times CS\ VARIABLE_{i,t-1}$	0.090	0.081	0.092	-0.040	-0.013	0.020
	(1.359)	(1.406)	(1.553)	(-0.657)	(-0.225)	(0.374)
$TREAT_i \times CS\ VARIABLE_{i,t-1}$	0.056	0.044	0.061	0.111**	0.088*	0.034
	(1.071)	(1.104)	(1.585)	(2.089)	(1.997)	(1.066)
$\boldsymbol{POST_t \times TREAT_i \times CS\ VARIABLE_{i,t-1}}$	**-0.145***	**-0.092**	**-0.104***	**-0.121***	**-0.093***	**-0.016**
	(-1.723)	**(-1.259)**	**(-1.976)**	**(-1.850)**	**(-1.834)**	**(-0.381)**
Firm-level control variables	Included	Included	Included	Included	Included	Included
Fixed effects						
Firm	Included	Included	Included	Included	Included	Included
Year	Included	Included	Included	Included	Included	Included
R-Squared	0.948	0.955	0.913	0.937	0.944	0.903
No. of Observations	410	410	410	307	307	307

N*otes:* This table presents the results from regressions of the Herfindahl-Hirschman Index based on the capacity concentration of a firm's power plants in year t on the natural logarithm of the length of risk factor disclosures right before the mandate multiplied by -1, an indicator variable that equals one for the post-regulation period (i.e., December 2005), indicators based on (1) Ohlson (1980)'s O-score or (2) management forecast inaccuracy, interaction terms between these variables and control variables. See Appendix A for variable definitions. Standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent levels, respectively.

Table 8. Effect of the Risk Factor Disclosure Mandate on Fuel Stock Management Behavior of Power Plants

Dependent Variable:	***AVG. FUEL STOCK*** $_{p, i, t}$	***MIN TO MAX RATIO*** $_{p, i, t}$	***FUEL SHORTAGE*** $_{p, i, t}$	***AVG. FUEL STOCK*** $_{p, i, t}$	***MIN TO MAX RATIO*** $_{p, i, t}$	***FUEL SHORTAGE*** $_{p, i, t}$
	(1)	(2)	(3)	(4)	(5)	(6)
	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.
	t-Stat.	*t*-Stat.	*t*-Stat.	*t*-Stat.	*t*-Stat.	*t*-Stat.
	CS VARIABLE =					
	BANKRUPTCY RISK			***MEF INACCURACY***		
$POST_t \times TREAT_i$	23.622	-0.005	0.032	-35.609	0.011	0.132**
	(1.224)	(-0.250)	(0.853)	(-1.109)	(0.401)	(2.364)
$CS\ VARIABLE_{i, t-1}$	-2.428	-0.031	0.126*	55.374	-0.006	-0.039
	(-0.062)	(-1.023)	(1.953)	(1.473)	(-0.225)	(-0.736)
$POST_t \times CS\ VARIABLE_{i, t-1}$	30.350	0.031	-0.168**	-94.890**	-0.020	0.031
	(0.637)	(0.982)	(-2.558)	(-2.576)	(-0.672)	(0.494)
$TREAT_i \times CS\ VARIABLE_{i, t-1}$	-70.172*	0.000	-0.007	-73.076**	-0.007	0.140**
	(-1.812)	(0.012)	(-0.124)	(-2.200)	(-0.214)	(2.054)
$\boldsymbol{POST_t \times TREAT_i \times CS\ VARIABLE_{i, t-1}}$	**73.585***	**0.064****	**-0.088**	**90.699*****	**-0.009**	**-0.166****
	(1.689)	**(2.025)**	**(-1.222)**	**(2.619)**	**(-0.241)**	**(-2.349)**
Firm-level control variables	Included	Included	Included	Included	Included	Included
Fixed effects						
Plant	Included	Included	Included	Included	Included	Included
Firm	Included	Included	Included	Included	Included	Included
Coal-type × Year	Included	Included	Included	Included	Included	Included
State × Year	Included	Included	Included	Included	Included	Included
R-Squared	0.872	0.631	0.508	0.872	0.644	0.531
No. of Observations	1,340	1,340	1,340	1,210	1,210	1,210

Notes: This table presents the results from regressions of (1) a power plant's average fuel stock level, (2) its minimum fuel stock level divided by its maximum level, and (3) an indicator that equals one if a power plant experience a fuel emergency in year t on the natural logarithm of the length of risk factor disclosures right before the mandate multiplied by -1, an indicator variable that equals one for the post-regulation period (i.e., December 2005), indicators based on (1) Ohlson (1980)'s O-score or (2) management forecast inaccuracy, interaction terms between these variables and control variables. See Appendix A for variable definitions. Standard errors are clustered at the plant level. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent levels, respectively.

Table 9. Effect of the Risk Factor Disclosure Mandate on Supplier Base of Power Plants

Dependent Variable:	***#SUPPLIERS*** $_{p,i,t}$	***SUPPLIER CONCENT.*** $_{p,i,t}$	***#SUPPLIERS*** $_{p,i,t}$	***SUPPLIER CONCENT.*** $_{p,i,t}$
	(1)	(2)	(3)	(4)
	Coef.	Coef.	Coef.	Coef.
	t-Stat.	*t*-Stat.	*t*-Stat.	*t*-Stat.
	CS VARIABLE =			
	BANKRUPTCY RISK		***MEF INACCURACY***	
$POST_t \times TREAT_i$	-0.424***	0.072***	-0.732***	0.082***
	(-3.190)	(4.029)	(-3.706)	(2.763)
$CS\ VARIABLE_{i,t-1}$	-0.183	0.033	0.088	-0.007
	(-0.401)	(0.588)	(0.303)	(-0.213)
$POST_t \times CS\ VARIABLE_{i,t-1}$	-0.066	-0.009	-0.277	0.003
	(-0.145)	(-0.150)	(-0.870)	(0.071)
$TREAT_i \times CS\ VARIABLE_{i,t-1}$	-0.119	0.024	-0.471**	0.023
	(-0.438)	(0.695)	(-2.011)	(0.650)
$\boldsymbol{POST_t \times TREAT_i \times CS\ VARIABLE_{i,t-1}}$	**0.501***	**-0.115*****	**0.469***	**-0.031**
	(1.663)	**(-2.883)**	**(1.825)**	**(-0.795)**
Firm-level control variables	Included	Included	Included	Included
Fixed effects				
Plant	Included	Included	Included	Included
Firm	Included	Included	Included	Included
Fuel-type × Year	Included	Included	Included	Included
State × Year	Included	Included	Included	Included
R-Squared	0.561	0.575	0.571	0.577
No. of Observations	4,085	4,085	3,547	3,547

Notes: This table presents the results from regressions of (1) the number of suppliers from which a power plant receives fuel for each fuel type, and (2) the Herfindahl-Hirschman Index based on the amount of fuel a power plant receives from each supplier for each fuel type in year t on the natural logarithm of the length of risk factor disclosures right before the mandate multiplied by -1, an indicator variable that equals one for the post-regulation period (i.e., December 2005), indicators based on (1) Ohlson (1980)'s O-score or (2) management forecast inaccuracy, interaction terms between these variables and control variables. See Appendix A for variable definitions. Standard errors are clustered at the plant level. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent levels, respectively.