Three Essays in Corporate Finance and Accounting

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Yi Zhang
Alliance Manchester Business School

List of Contents

This thesis contains 46,321 words, including title page, tables and footnotes.

Abstract

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Yi Zhang
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This thesis consists of three self-contained essays studying the topics of in the field of financial reporting frauds, cost of equity, and cost of bank credit in various contexts.

In the first essay, I examine the impact of CEOs' political promotion incentives on firms' financial reporting fraud using a sample of state-owned enterprises in China. Based on a unique dataset of preceding CEOs' promotions, I find that CEOs' political promotion incentives significantly decrease the propensity of firms' financial reporting misconduct. This effect persists after the preceding CEOs' promotion. In addition, the effect is more pronounced when the current CEOs have higher promotion prospects and when the firms' information environments are more transparent. The findings shed light on the mitigating effect of CEO's non-pecuniary incentives on agency conflicts and corporate fraud.

In the second essay, I study the association between government subsidies and the cost of equity using a sample of U.S. listed firms. I find a negative association between government subsidies and firms' cost of equity. And the results are robust to a battery of robustness tests. I also find evidence that government subsidies decrease firms' cost of equity by improving firms' information environment and their fundamental performance. Additionally, both tax-related and non-tax-related subsidies have significant impacts on the cost of equity and state-level subsidies have a more significant impact than federal and local-level subsidies. Overall, the evidence is consistent with government subsidies having a significant impact on firms' financing costs.

In the third essay, I investigate the intra-industry spillover effects of industry rivals' ESG incidents on firms' loan spreads. Exploiting the event-based ESG records of European firms, I find that industry rivals' ESG incidents lower a firm's subsequent loan spreads. This intra-industry spillover effect pertains to both the aggregate ESG records and various subcategories. It is stronger when rivals are listed firms or bigger firms, consistent with the significance of an incident's salience in ESG spillover process. Importantly, this effect is more pronounced among borrowing firms with better ESG conduct and those that operate in more competitive industries, suggesting the competition effect dominates the contagion effect during spillover.

Declaration

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Chapter 1

Introduction

1.1 Overview of the Thesis

Governments' intervention in firms in various ways, although its roles have been a long-standing and unsettled debate. Critics of government intervention advocate the market taking the leading role and argue that government interventions can lead to market distortion and economic inefficiency because of the multiplicity of governments' objectives (Hayek 1945; Shleifer and Vishny 1994; Banerjee 1997; Wang 2018). However, proponents of government intervention argue that governments can alleviate negative externalities and correct market failures that firms cannot properly deal with their own initiative (Millward 1976; Hart 1997; Sapienza 2004). Moreover, the empirical findings on government interventions' impact on firms are also mixed and inconclusive (Hall and Jorgenson 1967; Kneller et al. 1999; Cull and Xu 2005; Lin and Man-lai Wong 2013; Shao et al. 2015; Wang 2018; Deng et al. 2020; Huang and Yuan 2021).

Despite the ample discussion on the role of government intervention, very few studies have provided evidence on the impact of government intervention on firms under different institutional settings. The reasons include the complexity of institutions, the institutional variation across countries, and the various degrees of government intervention (Kneller et al. 1999; Wang 2018; Lin and Man-lai Wong 2013). This thesis aims to explore the impact of government intervention on various economies and provide policy suggestions.

This thesis consists of three self-contained essays in Chapters 2, 3, and 4, respectively. Chapter 2 examines how government intervention in firms' personnel decisions affects firms' financial reporting behaviour, using a sample of state-owned enterprises (SOEs) in China. Chapter 3 focuses on the effect of government subsidies on firms' equity cost, using a sample of U.S. listed firms. In Chapter 4, I investigate the influence of firms' adverse Environmental, Social, and Governance (ESG) incidents on bank-loan contracting in European countries to inform governments' ESG policies regarding the banking sector. I briefly introduce each of the chapters below.

In Chapter 2, I study the impact of CEOs' political promotion incentives on firms' financial reporting misconduct using the setting of CEO promotion in SOEs in China. Prior literature documents evidence that executives' career concerns provide implicit incentives (Fama 1980; Holmström 1999). Yet, how CEOs might behave when the external career

opportunities are limited remains less examined. China offers a unique setting to test this question. In contrast with professional managers in western countries, CEOs in Chinese SOEs are considered implicit government officials working in a relatively closed internal labour market (Chen et al. 2018). Given the control of the central and local governments over their personnel decisions, SOE executives in China have limited outside managerial labour market opportunities but ample upward potential in the SOE system and the political arena. In the context of Chinese SOEs, CEOs have strong incentives to pursue political promotion, an upward move to a higher political position either in the SOEs or in the government (Cao et al. 2019). Unlike prior studies using CEOs' *ex post* career paths to measure their promotion incentives (Cao et al. 2019; Kong et al. 2020), I use the predecessor's promotion to measure the current CEO's promotion incentives. The predecessors' promotion not only serves as a strong predictor of their successor' promotion prospects in Chinese SOEs but also is more exogenous compared with the *ex post* measures.

Based on manually collected data on CEOs' political promotion incentives in Chinese SOEs, I begin by validating that preceding CEOs' promotion is predictive of current CEOs' promotion probability. The results show that a current CEO's promotion is significantly positively related to their predecessors' promotion status. Next, I find a robust negative association between preceding CEOs' promotion and firms' likelihood of financial reporting frauds. This suggests that preceding CEOs' promotion has a role-model effect on successors, leading to a lower likelihood of the current CEOs conducting fraudulent behaviour. The negative association between preceding CEOs' promotion and financial reporting fraud is more pronounced for current CEOs with better promotion prospects and SOEs with more transparent information environments.

This paper adds to the growing literature on CEOs' career concerns. As CEOs are already at the top of the corporate ladder and have limited promotion incentives (Kale et al. 2009), prior literature has paid limited attention to CEOs' career concerns (Pae et al. 2016; Baginski et al. 2018; Pae 2021). This study contributes to this strand of literature by utilizing a unique setting in Chinese SOEs and providing novel evidence that CEOs' political promotion, incentivized by their predecessors' promotions, has an impact on the successors' financial reporting behaviour. Secondly, this study contributes to an emerging literature on CEOs' political promotion incentives (Chen et al. 2018; Cao et al. 2019), enabling us to understand better the financial reporting consequences of CEOs' political promotion incentives. Lastly, this study adds to the literature examining the determinants of financial

reporting frauds (Agrawal and Chadha 2005; Miller 2006; Dyck et al. 2010) by revealing that CEOs' political promotion incentives deter firms' fraudulent behaviour.

In Chapter 3, I investigate the relationship between government subsidies and the cost of equity using a sample of U.S. listed firms. In the U.S., billions of dollars in subsidies are granted to specific firms each year. Despite a growing literature examining the economic consequences of government subsidies (Almus and Czarnitzki 2003; Lee et al. 2014; Lee et al. 2017; Howell 2017; Aobdia et al. 2019; Criscuolo et al. 2019; De Simone et al. 2019; Rotemberg 2019; Raghunandan 2021; Pappas et al. 2021; Huang 2022), little attention has been paid to investors' perception of government subsidies. I find that firms receiving more government subsidies have significantly lower implied cost of equity, suggesting that investors perceive subsidized firms as less risky. I also find that firms with more government subsidies have a lower level of analyst forecast dispersion, better future operating performance, and lower performance volatility, constituting the channels underlying the baseline results. Taken together, the results suggest that government subsidies reduce the cost of equity by both improving firms' information environment and improving their fundamental performance. In addition, I find that both tax-related and non-tax-related subsidies are negatively associated with firms' cost of equity, and subsidies awarded by state-level government agencies have a greater impact on the cost of equity than those granted by federal and local government.

This study contributes to the literature in several ways. First, it adds to the emerging literature studying the capital market consequences of government subsidies (Aobdia et al. 2019; Raghunandan 2021; Pappas et al. 2021; Huang 2022) by providing novel evidence on the relationship between government subsidies and the cost of equity. Second, this study contributes to the literature on the cost of equity (Dhaliwal et al. 2006; Hail and Leuz 2006; Hail and Leuz 2009; Chen et al. 2016; Dhaliwal et al. 2016) by showing that government subsidy is one of its material determinants. Last, this study is timely as the Government Accounting Standards Board (GASB) and Financial Accounting Standards Board (FASB) have recently required more disclosures of the government subsidies received by firms. The findings of this study therefore have policy implications for regulators.

In Chapter 4, I study the intra-industry spillover effects of rivals' ESG incidents on firms' loan spreads. Despite existing literature showing that borrowing firms' ESG practices have essential impacts on various firm aspects (Kim et al. 2014; Chava 2014; Cheung et al. 2018; Albuquerque et al. 2019), there is little knowledge of whether and how firms might be affected by the ESG practices of their peer firms. This study attempts to fill the research gap

by examining how industry rivals' adverse ESG incidents affect firms' loan spreads using a sample of European firms. The results show that industry rivals' adverse ESG incidents reduce firms' loan spreads, suggesting the intra-industry competitive effect prevails over the contagion effect (Lang and Stulz 1992). This intra-industry competitive effect is stronger when rivals are listed firms or larger firms, highlighting the significance of an incident's salience in ESG spillover process. And the competitive effect is present for both the aggregate ESG records and various subcategories.

Although this study identifies the domain of the competitive effects, not all firms are equally likely to benefit from rivals' adverse incidents. I find that the negative association between industry rivals' ESG incidents and firms' loan spread is more pronounced among firms with better ESG performance, indicating firms with better ESG profiles are more attractive to lenders and in a better position to attract more favourable loan contracting terms after rivals' ESG incidents. Besides, the competitive effect is stronger for firms located in more competitive industries, suggesting ESG practices play a more significant role where product-market competition is more intense (Fernández-Kranz and Santaló 2010; Dupire and M'Zali 2018) so that a firm can better differentiate itself from industry peers and exploit product-market opportunities by taking advantage of rivals' adverse ESG incidents (Guo et al. 2020; Kang et al. 2021).

This study makes two main contributions. First, it contributes to the literature that examines the impact of firms' ESG performance (Chava 2014; Masulis and Reza 2015; Lins et al. 2017; Buchanan et al. 2018) by providing the novel evidence regarding how industry rivals' adverse ESG incidents spillover to borrowing firms cost of bank credit. Second, it adds to the literature on intra-industry spillover effects (Lang and Stulz 1992; Gleason et al. 2008; Hertzel and Officer 2012; Paruchuri and Misangyi 2015) by providing evidence on the relative importance of the competitive effect vis-à-vis the contagion effect (Lang and Stulz 1992). By focusing on ESG incidents' impact on industry rival's borrowing costs, this study adds to the literature on the wider externality of ESG conduct. My findings show that because firms can exploit product-market opportunities arising from rivals' ESG misconduct, the competition effect prevails over the contagion effect.

The three essays study widely debated topics in accounting and corporate finance: financial reporting frauds, cost of equity, and cost of bank loans. The first and the second essays show government intervention in firms can lead to positive outcomes, such as decreasing the likelihood of financial reporting fraud and cost of equity. The third essay highlights the importance of firms' ESG records, and contributes to governments' on-going

consideration on ESG policies.

1.2 Thesis Structure

The thesis structure follows the format consistent with the requirement at Alliance Manchester Business School, The University of Manchester. It allows chapters to be incorporated into a format suitable for submission and publication in peer-review academic journals. Therefore, this thesis is structured into three original empirical essays in Chapters 2, 3, and 4. The chapters are self-contained, each having a separate literature review, answering unique and original questions, and employing distinct analyses with different datasets. The equations, footnotes, tables, and figures are independent and are numbered from the beginning of each chapter. Page numbers, titles, and subtitles have a sequential order throughout the thesis.

The thesis continues as follows. Chapter 2 examines the impact of CEOs' political promotion incentives on firms' financial reporting misconduct using the setting of SOEs in China. Chapter 3 investigates the relationship between government subsidies and the cost of equity using a sample of U.S. listed firms. Chapter 4 studies the intra-industry spillover effects of rivals' ESG incidents on firms' loan spreads using a sample of European firms. Chapter 5 concludes. In Chapters 2-4, I use the third person (we or our) rather than the first person (I or my) as these chapters are in the form of working or submitted papers co-authored with my supervisors.

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Chapter 2

Preceding CEOs' Political Promotion and Financial Reporting Fraud: Evidence from China

Abstract

This paper examines the impact of CEOs' political promotion incentives on firms' financial reporting misconduct using the setting of state-owned enterprises in China. Utilizing a unique dataset of preceding CEOs' promotions, we find that CEOs' political promotion incentives significantly decrease the propensity of firms to engage in financial reporting fraud. This effect persists across the years after the preceding CEOs are promoted. In addition, the effect is more pronounced when the CEOs have higher promotion prospects and when the firms' information environments are more transparent. Overall, our findings shed new light on political promotion incentives' mitigation of agency conflict and reduction of corporate fraud incidence.

Keywords: Political promotion; financial reporting fraud; preceding CEO; state-owned enterprises

2.1 Introduction

The past decades have witnessed an upsurge in highly publicized allegations of accounting irregularities and fraudulent financial reporting. It is acknowledged that corporate fraud damages the reputations of firms (Karpoff and Lott Jr 1993; Karpoff et al. 2008; Johnson et al. 2014), decreases shareholder value (Bhagat et al. 1998; Gande and Lewis 2009), stifles investors' confidence (Gurun et al. 2018), and threatens the efficiency of capital markets (Amiram et al. 2018). A number of studies have investigated factors affecting the likelihood of fraud and its detection (Beasley 1996; Farber 2005; Khanna et al. 2015; Shi et al. 2017). Yet, how executives' career concerns affect corporate financial fraudulent behaviour remains under-examined.

Prior research argues that top executives' career concerns can reduce the agency problem by influencing the decision-making process (Fama 1980; Holmström 1999). Despite limited room for CEOs to be promoted within their organizations, external opportunities create career incentives for CEOs to behave in a way that is aligned with their firms' interests. For instance, Gibbons and Murphy (1992) show that, as CEOs approach retirement, the pay-performance sensitivity increases. Likewise, Coles et al. (2018) find that firm performance is positively associated with the pay disparity between the firm's CEO and the top-paid CEOs in the same industry. Although career opportunities improve CEOs' incentive, less is known about how CEOs might behave when the external career opportunities are limited. Our study seeks to fill the gap and examine how CEOs' political promotion incentives affect firms' propensity to engage in financial reporting fraud.

China offers a good setting in which to test our research question. While China has been moving towards a market-based economy since 1978, the government is still playing a predominant role in the economy. This is evidenced by China's huge apparatus of state-owned enterprises (SOEs). In contrast with professional managers in western countries, CEOs in Chinese SOEs are considered informal government officials with political ranks and have strong incentives for political promotion. This is because, firstly, significant salary disparities existed between CEOs in SOEs and non-SOEs. For example, using a sample of Chinese listed firms from 1997 to 2000, Firth et al. (2007) find that CEO pay is lower when the state controls the firm, suggesting that the political promotion incentives could substitute for pecuniary incentives. Secondly, the government retains ultimate control over personnel decisions in Chinese SOEs, such as the appointment, transfer, or dismissal of executives of SOEs (Fan et al. 2007; Cao et al. 2019). CEOs in Chinese SOEs work in a relatively closed

internal labour market and have fewer opportunities compared to other CEOs, strengthening the importance of pursuing a higher political position. The CEOs in SOE sectors enjoy non-transferable benefits within the system, and departure from this system can leave them with no comparably prestigious employment opportunities (Chen et al. 2018). Getting promotion within this system means more benefits in power, status, reputation, pecuniary and non-pecuniary rewards (Li and Zhou 2005). The lock-in effect of the closed labour market of Chinese SOEs, coupled with the large difference in terms of personal benefits between staying power and relinquishing power, reinforces CEOs' incentives to pursue a political promotion, an upward move to a higher political position either in the SOEs or in the government (Chen et al. 2018; Cao et al. 2019).

Unlike prior studies using the *ex post* career paths of CEOs to measure their promotion incentives (Cao et al. 2019; Kong et al. 2020), we use the predecessor's promotion as our measurement of the current CEO's promotion incentives. In Chinese SOEs, a CEO's promotion serves as a strong predictor of their successor's promotion prospects. Moreover, political promotion is endogenous, because it can be determined by CEO and firm characteristics, which may simultaneously affect financial reporting fraud. Relative to the *ex post* measure, our proxy is more exogenous while not losing its validity for capturing a CEO's political promotion incentives.

It is unclear *ex ante* how preceding CEOs' political promotion affects firms' financial reporting fraud behaviour. On the one hand, previous literature suggests that firms with politically connected executives have a lower hazard rate in relation to fraud detection (Yu and Yu 2011; Correia 2014; Jagolinzer et al. 2020). This is because political connections with preceding CEOs help firms gain favourable treatment, such as exemption from investigation or receiving lower regulatory penalties. In addition, promoted preceding CEOs do not want their names tarnished by any scandals in the firms they used to lead. As such, they have incentives to push enforcement agencies to turn a blind eye to connected firms engaging in fraud.

On the other hand, preceding CEOs' promotion may have role-model effects on their successors. Promotion-minded CEOs are likely to be cautious about their professional reputations and avoid involvement in any unethical behaviour. Moreover, corporate misconduct often results in negative financial outcomes, such as abnormally negative market reactions and falls in shareholder value (Bhagat et al. 1998; Desai et al. 2006; Hennes et al. 2008; Gande and Lewis 2009). To the extent that firm performance is an important criterion

for CEO promotion and compensation (Cao et al. 2019), the negative impact of corporate misconduct can also threaten CEOs' promotion prospects. Taking these arguments together, the relationship between preceding CEOs' political promotion and firms' current financial reporting fraud is an open empirical question.

Based on manually collected data on CEOs' political promotion incentives in Chinese SOEs over the period 2008-2016, we begin by validating that preceding CEOs' promotion is predictive of current CEOs' promotion likelihood. The results estimated from both Cox hazard and Logit models consistently show that a current CEO's promotion is positively associated with their predecessors' promotions. This provides us with substantial confidence in our measurement of political promotion incentives.

Next, we find a strong negative association between preceding CEOs' political promotion and firms' current financial reporting fraud likelihood, consistent with the role-model effect of a promoted predecessor leading to a lower probability of the current CEO conducting fraudulent behaviour. Notably, the economic significance is also sizable. The probability of engaging in financial reporting fraud for firms with promoted predecessors is 16.2 percentage points lower than that for firms without promoted predecessors. Our results are robust to a battery of sensitivity analyses.

To ensure the robustness of our findings, we further investigate the effects of preceding CEOs' promotions in each year after they are promoted. We find that the impact of preceding CEOs' promotions is persistent across the years after they are promoted, with the effect most pronounced in the second and third years afterwards.

Moreover, one may be concerned that firms with and without promoted predecessors may be systematically different. These observed heterogeneities may drive the different likelihoods of the commitment of financial reporting fraud. To mitigate this concern, we employ entropy balancing matching (EBM) and propensity score matching (PSM) to construct matched samples. We replicate our main regression using the matched samples and find similar results.

We next perform several cross-sectional variation tests to substantiate the relationship between preceding CEOs' promotion and firms' current financial reporting fraud. Given that our results suggest that preceding CEOs' promotion paves the way towards an upward career movement for their successors, which constrains their unethical behaviour, this effect should be more noticeable when the successors' promotion is more likely. Thus, we test whether the role-model effect varies among CEOs with different promotion prospects. Consistent

with our prediction, the negative association between preceding CEOs' promotion and financial reporting fraud is more pronounced for current CEOs with better promotion prospects, measured by their education level and political rank.

In addition, we expect that the impact of preceding CEOs' promotion on financial reporting fraud is conditional on firms' information environments. Firms with more transparent information environments are less likely to engage in fraudulent behaviour (Miller 2006; Yu 2008; Dyck et al. 2010). As such, the negative association between preceding CEOs' promotion and financial reporting fraud is expected to be more pronounced for SOEs with more transparent information environments. We follow previous research and use media coverage and analysts' forecast dispersion as proxies for information transparency (Leuz 2003; Frankel and Li 2004; Maskara and Mullineaux 2011; Cui et al. 2018). We find results consistent with our expectations.

Our study contributes to the literature in several ways. First, our findings add to the growing literature on CEOs' career concerns (Song and Thakor 2006; Chen 2015; Pae et al. 2016; Li et al. 2017; Pae 2021). Although a few studies have examined the role of career concerns of executives in corporate governance (Song and Thakor 2006) and reporting quality (Pae 2021), relatively less is known about the impact of CEOs' career concerns. This is mainly because CEOs are already at the top of the corporate ladder and have limited promotion incentives (Kale et al. 2009). We contribute to this stream of literature by utilizing a unique setting in China, where there is a relatively closed internal political labour market for CEOs of Chinese SOEs. We provide novel evidence that CEOs' political promotion, incentivized by their predecessors' promotions, impacts the successors' financial reporting behaviour.

Second, our study is related to an emerging literature on CEOs' political promotion incentives. Despite the existing literature on the political promotion of government officials (Li and Zhou 2005; Piotroski and Zhang 2014; Kong et al. 2020), the research on CEOs' political promotion incentives is relatively scarce. Among the few exceptions, Cao et al. (2019) find that political promotion incentives, as a substitute for monetary incentives, positively relate to firms' financial performance. Chen et al. (2018) provide evidence that there is a negative association between CEOs' political ranks and firms' stock price crash risk. Our finding adds to this literature by highlighting the consequences of CEOs' political promotion incentives on their tendency of conducting disclosure fraud.

Last but not least, we advance the understanding of the determinants of financial reporting fraud. Prior research suggests that financial reporting fraud is a significant threat to the existence and efficiency of capital markets (Amiram et al. 2018). It impairs the trust between corporations, gatekeepers, and market participants, and undermines capital markets' core role of efficiently allocating resources. A significant body of research shows that both internal and external characteristics limit financial misconduct behaviour (Agrawal and Chadha 2005; Miller 2006; Yu 2008; Zhao and Chen 2008; Dyck et al. 2010). To the best of our understanding, our study is the first to reveal that CEOs' political promotion incentives help deter firms' fraudulent behaviour.

The rest of the paper is organized as follows. Section 2.2 provides the background. Section 2.3 reviews relevant literature and develops hypotheses. Section 2.4 describes the data, model, and descriptive statistics. Section 2.5 provides empirical results. Section 2.6 concludes.

2.2 Institutional Background

China began the corporatization and privatization of SOEs in 1978. Since then, the SOEs have enjoyed greater freedom in decision making about aspects such as profit-sharing schemes than ever before (Cao et al. 2019). Despite the decentralization of SOEs, the government has retained substantial control over personnel decisions, including appointments and dismissals of SOE executives (Fan et al. 2007). Specifically, the personnel decisions in central and local SOEs are made by central and local State-Owned Assets Supervision and Administration Commissions of the State Council (SASACs), respectively. Given governments' full control over SOE executive personnel decisions, it is very common in China for SOE executives to enter the political arena at both central and local levels. For example, Yaqing Xiao, the former director of the SASAC, served as the CEO of Chinalco (a centrally owned enterprise) during 2004 to 2009. He was promoted to deputy secretary of the State Council in 2009 and then appointed as the director of SASAC in 2016. Another example is Qishan Wang, current vice president of China, who was promoted from the vice president of China Construction Bank (CBC) to the provincial level of the Politburo. Given the control of the central and local government over their personnel decisions, SOE executives in China have limited outside managerial labour market opportunities, but ample upward potential in the SOE system and the political arena (Cao et al. 2019).

The political promotion has always been a critical incentive for CEOs in SOEs. Using a sample of Chinese listed firms from 1997 to 2000, Firth et al. (2007) find that CEO pay is

lower when the firm is controlled by the state, suggesting there are salary disparities between CEOs in SOEs and non-SOEs. More importantly, CEOs in Chinese SOEs work in a relatively closed internal labour market and enjoy non-transferable benefits within the system (Chen et al. 2018). Getting promotion within this system means more benefits in power, status, reputation, pecuniary and non-pecuniary rewards (Li and Zhou 2005). Recently, China has introduced a number of regulations with regard to SOE executives' payments. For example, the Payment Regulation Reform Plan for Central Government-owned Enterprises in 2015 put restrictions on the basic salaries and bonuses of central SOEs' top executives. When their pecuniary incentives are constrained, executives have stronger motivation to pursue implicit forms of compensation, such as political promotion, to compensate for their loss of pecuniary payment.

There are two parallel systems in the personnel management of Chinese SOEs: the regular corporate system and the party system. The management team in the corporate system is similar to that commonly found in non-SOEs, including, for instance, the CEO, vice CEOs and Chairman of the Board. The leadership team in the party system includes secretaries, deputy secretaries, and other members of the party committee (Lin 2013; Hu and Xu 2022). A CEO's promotion could be a dual promotion in both the corporate and the party system. For instance, Shaoqun Chen served as CEO and director at Shenzhen Agricultural Products Group during 2009. Then, he was promoted to the Chairman of the Board and Secretary of the Communist Party of China (CPC) Committee in the firm. A CEO could also be promoted in either the corporate or the party system. One example is Huaxiong Wang, who served as CEO and director of Hubei Yihua Chemical Industry Co., Ltd. from 2001 to 2006. In 2006, he was promoted to Vice-chairman of the parent firm, with no ranking change in the party system.

2.3 Related Literature and Hypothesis Development

2.3.1 Executives' Incentives to Commit Misconduct

Some corporate misconduct activities are power-based rent-seeking behaviours, which are closely related to firms' internal rights allocation. Although managers' rights to control are beneficial in order for them to give full play to their professional skills and improve the effectiveness of decision-making in the firm, they can also induce unethical executive behaviour. Powerful managers can take advantage of their control rights to create personal benefits such as perks paid by the company (Rajan and Wulf 2006; Yermack 2006), excessive compensation (Boyd 1994; Bebchuk et al. 2002; Wade et al. 2006), and rigged

incentive pay (Morse et al. 2011; Abernethy et al. 2015). Executives may violate their fiduciary responsibilities and hollow firms out, rather than maximizing the shareholders' interests (Haß et al. 2015; Bianchi and Mohliver 2016; Li et al. 2022).

Economic incentives are important for aligning the interests of the executives with those of the shareholders and thereby deterring executives' unethical behaviour (Jensen and Murphy 1990; Beatty and Zajac 1994; Conyon and He 2016). As an effective way to reduce agency costs, stock-based compensation contracts encourage executives to take appropriate risks and benefit from business growth (Smith Jr and Watts 1992; Baber et al. 1996). However, a growing number of studies have cast doubt on the assumption that options are used solely to align the interests of management and shareholders. Stock-based compensation is a double-edged sword. On the one hand, it encourages managers' work efforts and risk-taking. On the other hand, it may lead to costly information manipulation activities (Goldman and Slezak 2006), financial misreporting (Burns and Kedia 2006; Efendi et al. 2007), earnings manipulation (Bergstresser and Philippon 2006; Zhang et al. 2008), and corporate fraud (O'Connor et al. 2006; Peng and Röell 2008; Johnson et al. 2009).

Aside from pecuniary incentives, several non-pecuniary incentives, such as promotion expectations and career concerns, can also affect wrongdoing. Few studies have investigated how executives' non-pecuniary incentives affect corporate unethical behaviour. Among the few exceptions, Swalm (1966) argues that managers tend to engage in activities that are not in the shareholders' interests due to career concerns. Several studies find that CEOs' career concerns are related to less efficient information (Song and Thakor 2006; Pae et al. 2016; Baginski et al. 2018; Bochkay et al. 2019; Pae 2021). For instance, Pae et al. (2016) find that CEOs with career concerns use more conservative earnings guidance to increase their likelihood of meeting expectations. And Song and Thakor (2006) suggest CEOs' career concerns lead to less precession information used in the communication between CEOs and board members. Besides, a few studies also suggest that managers with stronger tournament incentives are more likely to engage in misreporting fraud (Haß et al. 2015) and sabotage activities (Harbring and Irlenbusch 2011). On the other hand, few previous studies show external opportunities create career incentives for CEOs to behave in a way that is aligned with their firms' interests. For instance, Gibbons and Murphy (1992) suggest the pay-performance sensitivity increases as CEOs approach retirement. Also, Coles et al. (2018) document evidence that firm performance is positively related to the pay disparity between the firm's CEO and the top-paid CEOs in the same industry.

2.3.2 Hypothesis Development

It is unclear *ex ante* how preceding CEOs' political promotion affects firms' current misconduct behaviour. On the one hand, firms may take advantage of political connections with former CEOs to gain favourable treatment, such as exemption from an investigation or receiving lower penalties from regulatory agencies. Previous literature suggests that politically connected firms have a significantly lower hazard rate in relation to fraud detection (Yu and Yu 2011; Correia 2014). More recent research shows that executives in politically connected firms are more likely to engage in insider trading (Jagolinzer et al. 2020). From the viewpoint of the promoted former CEOs, they do not want their names tarnished by any scandals at the firms they used to lead, because this may adversely affect their subsequent promotion prospects. Therefore, the former CEOs have incentives to push enforcement agencies to turn a blind eye to connected firms engaging in fraud. The above arguments predict a positive relation between preceding CEOs' political promotion and the likelihood of firms' current fraudulent behaviour.

On the other hand, it is also reasonable to predict that firms with politically promoted former CEOs are less likely to engage in misconduct. First, preceding CEOs' promotion tends to have strong role-model effects on successors and to positively affect the latter's promotion likelihood. Promotion-minded CEOs are likely to be cautious about their professional reputations. Unethical behaviour can lead to a severe loss of reputation. For example, Karpoff et al. (2008) find that most executives who had lost their jobs did so due to regulatory enforcements following financial misreporting. Executives can also bear substantial financial losses through restrictions on their future employment and their shareholdings. Desai et al. (2006) also document that the subsequent employment prospects of executives exposed as unethical are poorer. Thus, with their promotions in mind, CEOs are less likely to engage in fraudulent activities. Second, corporate misconduct often results in adverse financial consequences, for example, triggering abnormally negative market reactions and decreasing shareholders' value (Bhagat et al. 1998; Desai et al. 2006; Hennes et al. 2008; Gande and Lewis 2009). To the extent that firm performance is an important criterion for CEO promotion and compensation (Cao et al. 2019), the negative impact of corporate misconduct can also threaten CEOs' promotion prospects. These arguments predict a negative association between former CEOs' political promotion and corporate fraud.

Given the above competing arguments, we formulate a null hypothesis:

Hypothesis: There is no relation between preceding CEOs' political promotion and firms' current misconduct behaviour.

2.4 Data and Research Design

2.4.1 Data and Sample

Our empirical tests are based on SOEs listed on the Shanghai and Shenzhen Stock Exchanges. The sample period is 2008 to 2016. We manually collect CEOs' political promotion information from various sources, including firms' annual reports and appointment statements, and news on the internet, covering the period of 2008-2016. We obtain information on financial reporting fraud and firms' financial characteristics from the China Stock Market and Accounting Research (CSMAR) Database.

We exclude (1) firms operating in financial industries,[1] (2) observations without information about former CEOs, and (3) observations with insufficient data with which to calculate the variables used in our main analysis. Our final sample for the main analysis thus consists of 2,228 firm-year observations (317 unique firms).[2]

2.4.2 Variables and Economic Specifications

2.4.2.1 Measuring CEOs' Political Promotion

As discussed in Section 2.2, there are two types of promotion in Chinese SOEs: that in the corporate system and that in the party system. In our context, a CEO is deemed to be politically promoted if he/she is appointed to one of the following positions: (1) the Chairman of the Board in the firm or another SOE at the same administrative level,[3, 4](2) the CEO or Chairman of the Board in the parent firm or another SOE at a higher administrative level, (3) secretary or vice secretary of the CPC committee in the firm or another SOE at the same administrative level, (4) secretary or vice secretary of the CPC committee in the parent firm or another SOE at a higher administrative level, or (5) governmental official. We define *CEO Promotion* as an indicator variable equal to one if a CEO is promoted in that year, and

[1] Following the conventions in prior literature studying executives' incentives in Chinese SOEs (Feng and Johansson 2018; Cao et al. 2019), we excluded the firms operating in the financial industries because their financial practices are different from nonfinancial firms (Cao et al. 2019).

[2] The above procedure yields a sample of 6,293 firm-year observations. Since we use a logistic model, controlling for firm fixed effects in the main analysis, 4,065 firm-year observations are further dropped because of perfect collinearity (i.e., there is no within-firm variation in the dependent variable).

[3] The administrative rankings of SOEs include (from high to low): (1) national level, (2) provincial level and municipal level, (3) city level, and (4) county level.

[4] Typically, the top managers themselves are also Communist Party members reporting to the Party Secretary, who is often the Chairman of the board and who, in turn, reports to the higher levels of Party organization in the firm group. Therefore, the Chairman of the board is supposed to be in a higher political position than the CEO in the same firm (Hu and Xu 2022).

zero otherwise. We define *ExCEO Promotion* as our main proxy for former CEOs' promotion status. *ExCEO Promotion* is an indicator variable that equals one if any former CEO has been promoted in the last five years, and zero otherwise. In robustness checks, we also provide alternative measures of former CEOs' promotion status.

2.4.2.2 Measuring Financial Reporting Fraud

Following Chen et al. (2013), we define financial reporting fraud as fraudulent activities in the following categories: manipulation of earnings, manipulation of assets, false statement, delayed disclosure, significant omission from disclosure, fraudulent disclosure, fraudulent disclosure in public offerings, and non-material accounting errors. We define *Fraud* as an indicator variable that equals one if a firm commits financial reporting fraud in that year, and zero otherwise. In robustness checks, we also use the average number of fraudulent activities during a CEO's tenure and discretionary accruals as alternative proxies for financial reporting misconduct.[5]

2.4.2.3 Descriptive Statistics

Table 1 presents the summary statistics of the variables used for testing the effects of former CEOs' promotion on successors' promotion likelihood. Panel A presents the summary statistics for CEOs and their predecessors' promotion. On average, there are 8.3% observations with promoted predecessors and 5.5% with promoted CEOs. Panels B and C present the summary statistics for the firm and CEO characteristics, respectively.

[Insert Table 1 here]

Table 2 presents the summary statistics of the variables used for testing the effects of former CEOs' promotion on the likelihood of firms committing financial reporting fraud.[6] About 13% of the observations in our sample feature promoted predecessors and 31.8% feature the commitment of financial reporting fraud. In Panel E, we summarize the details of CEOs' status at the end of the year of observation. 703 individual CEOs are covered in our analysis. Among them, 17% are promoted, 9% are demoted, 15% move to similar positions (lateral moves), 35% are serving as CEO at the end of the year of observation, and 25% leave the CEO position for other reasons (e.g., involved in legal cases, retirement, personal reasons etc.).

[5] Different types of frauds and the corresponding codes in CSMAR database can be found in Appendix B.
[6] In Appendix Table C1, we report the summary statistics for all 6,293 observations, and the distributions of the variables are largely the same.

[Insert Table 2 here]

2.5 Empirical Findings

2.5.1 The Effect of Preceding CEOs' Promotion on Successors' Promotion

The predicted relationship between former CEOs' promotion and firms' financial misconduct is premised on the assumption that promoted preceding CEOs increase the promotion likelihood of their successors via the role-model effect. Therefore, we begin validating this assumption by testing the impact of preceding CEOs' promotion on successors' promotion. We first use the Cox Proportional Hazard (Cox PH) model. The Cox PH model is the most commonly used model in survival analysis, and is widely used in finance and accounting research (Zmijewski 1984; Lane et al. 1986; Shumway 2001). There are several advantages to using it: first, alternative models like the logistic and probit models assume a static state for a CEO's promotion, and do not provide any estimation of the time to promotion. However, the hazard model allows a CEO's probability of promotion to change over time, and can estimate both the occurrence and timing of the promotion (Lane et al. 1986; Shumway 2001). Second, the Cox PH model, as a semi-parametric estimation model, has less restrictive assumptions about the distribution of the baseline hazard function (Lane et al. 1986).[7] Third, the hazard model incorporates time-varying covariates, thus accommodating variables such as a CEO's age (Shumway 2001).

The hazard function is as follows:

$$h(t, X(t)) = h_0(t)\exp\left[\sum_{i=1}^{p_1}\beta_i X_i + \sum_{j=1}^{p_2}\gamma_j X_j(t)\right] \quad (1)$$

where $h(t, X(t))$ is the expected hazard at time t for a CEO with a given set of time-invariant explanatory variables X_i and time-variant explanatory variables $X_j(t)$. $h_0(t)$ is assumed to be the unspecified baseline hazard rate (LeClere 2000). The model does not require particular probability distribution assumptions to carry out the estimation of β, γ, or $h_0(t)$. In our analysis, the original time a CEO enters the analysis is the year in which he/she takes office, and the exit time is the year in which he/she departs. The event of interest is CEOs' promotion.

[7] Parametric models like the logit and probit models require the specification of a probability distribution over time. The logit model assumes a cumulative logistic distribution while the probit model assumes a cumulative normal distribution.

ExCEO Promotion, as one of our explanatory variables, is an indicator variable that equals one if a former CEO has been promoted in the past five years, and zero otherwise. As for control variables, we first control for a set of firm fundamentals including firm size (*Ln Market Cap*), return on assets (*ROA*), leverage (*Leverage*), and the number of employees (*Ln Employee*). We also include the effective tax rate (*Effective Tax Rate*) as Wang et al. (2018) suggest that CEOs may be aggressive in their tax payment to stay on good terms with the government. In addition, we control for a bunch of CEO characteristics. Specifically, we control for CEO and board chairman duality (*Duality*) and years of tenure (*Tenure Years*), as a CEO with dual roles and longer tenure tends to be more capable and experienced, and thus more likely to be promoted. We control for CEO's age (*Ln Age*) as age is a critical determinant of CEO turnover (Li and Zhou 2005). We also include CEO's salary (*Ln Salary*) and stock ownership (*CEO Ownership*) as those pecuniary compensations may offset CEOs' motivation to pursue higher political positions. We also include the firm's industry classification as a control variable and we expect CEOs working in industries that are strategically more important to the national economy to be more likely to be promoted.

In addition to the Cox PH model, we conduct a static analysis using a logistic model and the data on CEOs' year of departure The model is as follows:

$$CEO\ Promotion_{i,t} = \alpha + \beta_1 ExCEO\ Promotion_{i,t} + \gamma Controls_{i,t} + Industry\ FE + \varepsilon_{i,t} \quad (2)$$

where *CEO Promotion* is an indicator variable that equals one if a CEO is promoted in that year, and zero otherwise. All the control variables are the same as in the Cox PH model.

Table 3 reports the regression results. Columns (1) and (2) report the coefficients and hazard ratios for the Cox PH model given in Eq. (1). The coefficient of *ExCEO Promotion* is significantly positive at the 1% level (*z*-stat = 3.01), suggesting that current CEOs' promotions are positively associated with their predecessors' promotions. The hazard ratio of *ExCEO Promotion* is 1.928, meaning that there is a 9.28% increase in the probability of the current CEO gaining a promotion if he/she has a promoted predecessor.

Turning to the controls, the coefficient on *ROA* is positive and significant at the 10% level (*z*-stat = 1.84), suggesting that better-performing CEOs are more likely to be promoted, consistent with the findings of Cao et al (2019). Also, we find that CEOs of SOEs with higher leverage and those who serve as chairman are more likely to receive promotions.

Columns (3) and (4) present the static analysis using the logistic model from Eq. (2) and the data on CEOs' year of departure. The coefficient of *ExCEO Promotion* is

significantly positive at the 5% level (*z*-stat = 2.30), which is in line with the results of the Cox PH model. The marginal effects show that the probability of a CEO's promotion is 8.9% higher if he/she has a promoted predecessor. Again, we find that *ROA*, *Leverage*, and *Duality* are significantly positively related to *CEO Promotion* in the logistic regression. In addition, the results in Column (3) suggest that younger CEOs and CEOs with longer tenures are more likely to be promoted.

[Insert Table 3 here]

2.5.2 The Effect of Preceding CEOs' Promotion on Financial Reporting Fraud

Our baseline model for testing the relation between former CEOs' promotion and financial reporting fraud is as follows:

$$Fraud_{i,t} = \alpha + \beta ExCEO\ Promotion_{i,t} + \gamma Controls_{i,t} + Year\ FE + Firm\ FE + \varepsilon_{i,t} \quad (3)$$

where the dependent variable *Fraud* is an indicator variable that equals one if a firm commits financial reporting fraud in the year, and zero otherwise. The independent variable of interest to us is *ExCEO Promotion*.

As for control variables, we include several firm and CEO characteristics that are potentially related to the likelihood of a firm committing financial reporting fraud. First, we control for firm fundamentals including firm size (*Ln Market Cap*), return on assets (*ROA*), and leverage (*Leverage*). Second, we include a set of variables related to firms' internal and external governance, including the number of directors on the board (*Ln Board Size*) and the ratio of independent directors to board size (*Independent Ratio*). Prior literature suggests that larger boards play a less effective role in monitoring (Jensen 1993; Yermack 1996; Eisenberg et al. 1998), and independent directors have a positive impact on internal governance (Weisbach 1988; Beasley 1996) and improve information transparency (Ferreira et al. 2011; Armstrong et al. 2014). Further, we control for analyst coverage (*Ln Analyst Coverage*) as analysts contribute to fraud detection (Dyck et al. 2010; Chen et al. 2016b). We also include an indicator for the "big four" auditors (*Big Auditor*) as they are more stringent and can serve as a deterrent against reporting misconduct (Francis 2004; Lennox and Pittman 2010). Moreover, we control for a set of CEO characteristics, including CEO and board chairman duality (*Duality*), CEO's age (*Ln Age*), years of tenure year (*Tenure Years*), salary (*Ln Salary*), and share ownership (*CEO Ownership*). Prior literature indicates that older CEOs, CEOs with longer tenure, and CEOs serving as chairmen of the board tend to be more

experienced and powerful, and thus more likely to evade fraud detection (Dechow et al. 1996; Khanna et al. 2015). Also, CEOs' economic incentives, including salary and share ownership are important in promoting corporate governance and deterring executives' unethical behaviour (Chen et al. 2016a; Conyon and He 2016). Finally, we control for year and firm fixed effects to account for macroeconomic fluctuations and unobservable firm- and time-invariant characteristics.

Table 4 reports the results of estimating Eq. (3). In Columns (1) and (2), the coefficients (*z*-stats) of *ExCEO Promotion* are −0.528 (−2.30) and −0.682 (−2.89), consistent with the role-model effect of a promoted predecessor leading to a lower probability of the firm engaging in fraudulent behaviour. Notably, the marginal effect in Column (3) suggests that the probability of committing financial reporting fraud of firms with promoted former CEOs is 16.2 percentage points lower than that of firms without that characteristic.

With regard to the control variables, firms with poorer financial performance are more likely to commit fraud. Consistent with prior literature, we also find that larger boards are less effective at monitoring fraud (Jensen 1993; Yermack 1996; Eisenberg et al. 1998) and CEOs' equity ownership deters misconduct by aligning their self-interests with those of their firms (Alexander and Cohen 1999).

[Insert Table 4 here]

To ensure the robustness of our results, we perform several sensitivity analyses and report the results in Appendix Tables C2-C5. First, in Appendix Table C2, we report the results of excluding firm-year observations in the CEO transition year. We exclude those observations because CEOs' impact in the transition year is not clear.[8] After excluding the transition year observations, we still observe a significantly negative coefficient on *ExCEO Promotion*. Second, we repeat the baseline model with standard errors clustered at different levels. In Columns (1) to (3) of Appendix Table C3, we report the *z*-statistics with non-clustered standard errors, and standard errors clustered at the province and province-industry levels, respectively. The coefficients on *ExCEO Promotion* remain significant across all columns. Third, since prior literature suggests that institutional ownership, board meetings, and media coverage influence corporate governance (Vafeas 1999; Hartzell and Starks 2003;

[8] For example, Shenzhen Zhenye (Group) Co., Ltd. (stock code 000006 SZ) announced a CEO turnover in May 2013. In that month, the former CEO Chuanfu Li left, while Canming Jiang was appointed as the new CEO. There were thus two CEOs in the transition year of 2013, and it is not clear who was the dominant CEO in that year. Therefore, we drop such CEO transition years.

Johnson et al. 2005; Core et al. 2008; Bednar 2012; Wu et al. 2016), we further control for institutional ownership (*Institutional Ownership*), the number of board meetings (*Ln Board Meetings*), and media coverage (*Ln Media Coverage*) in the regression model. The results shown in Appendix Table C4 suggest that *ExCEO Promotion* continues to be negatively associated with the likelihood of financial reporting fraud after controlling for the above factors. In addition, as discussed in prior literature (Neyman and Scott 1948; Lancaster 2000; Greene 2004), non-linear logistic or probit models with fixed effects can produce biased coefficient estimates due to the incidental parameters problem. Thus, we re-estimate the baseline regression using linear probability regression. In Appendix Table C5, the coefficients on *ExCEO Promotion* remain significantly negative. Overall, the above results show that our findings are robust to a battery of sensitivity tests.

2.5.3 Dynamic Impact of Preceding CEOs' Promotion

Table 5 reports the results regarding the dynamic impact of former CEOs' promotion on financial reporting fraud. We estimate the baseline regression model by replacing *ExCEO Promotion* with a set of dummy variables, namely *After1*, *After2*, *After3* and *After4+*, to investigate the effects of former CEOs' promotion each year after their promotion. The four indicators are set to one for the first year (*After1*), the second year (*After2*), the third year (*After3*) and the fourth and fifth years (*After4+*) after a former CEO has been promoted, respectively, and zero otherwise.

Column (1) reports the results without control variables, while Column (2) reports the results with their inclusion. Column (2) shows that the coefficients on *After1*, *After2*, and *After3* are negative and statistically significant. Consistent with our baseline results, the impact of former CEOs' promotions appears to be persistent in the years following such promotions, with the effect most pronounced in the second and third years after the promotions.

[Insert Table 5 here]

2.5.4 Robustness Checks

2.5.4.1 Alternative Measures of Preceding CEOs' Promotions

As well as using *ExCEO Promotion* as the dependent variable, we also provide robustness checks by replacing it in Eq. (3) with several alternative measures of predecessors' promotions. Table 6 reports the results. In Columns (1) and (2), we use *ExCEO Promotion 4 Years* and *ExCEO Promotion 6 Years*, i.e., indicators that equal one a former

CEO has been promoted in the past four or six years respectively, and zero otherwise, as the independent variables. The coefficients on *ExCEO Promotion 4 Years* and *ExCEO Promotion 6 Years* are significantly negative, suggesting our findings are not sensitive to that change. To provide a cleaner test of promoted predecessors without the influence of earlier CEOs' promotions, in Column (3) we define our dependent variable, only considering the promotion of the immediate predecessor to the current CEO. Specifically, we define an indicator *ExCEO Promotion Immediate* that equals one in the five years following the immediate predecessor's promotion and zero otherwise. The coefficient on *ExCEO Promotion Immediate* is negative and significant at the 5% level, indicating that the immediate predecessor's promotion also has a significant impact in deterring financial reporting fraud. In Column (4), we examine the effect of the first promotion of a former CEO in our sample. We define *ExCEO Promotion First* as equal to one in the five years following the first promoted former CEO in our sample period, and zero otherwise. The coefficient on *ExCEO Promotion First* is negative and significant at the 1% level, consistent with our baseline again.

[Insert Table 6 here]

2.5.4.2 Alternative Measures of Financial Reporting Fraud

In Table 7, we report the results of robustness checks using alternative measurements of the dependent variable. We use the following OLS regression:

$$Avg.Fraud_{i,t} \text{ or } Earnings\ Management_{i,t} = \alpha + \beta ExCEO\ Promotion_{i,t} + \gamma Controls_{i,t} + Year\ FE + Firm\ FE + \varepsilon_{i,t} \quad (4)$$

In Column (1), we use *Avg. Fraud*, i.e., the average number of cases of financial reporting fraud during a CEO's tenure, as the dependent variable. The coefficient on *ExCEO Promotion* is −0.085 and significant at the 5% level (*t*-stat = −2.00), suggesting that predecessors' promotions decrease the amount of financial reporting fraud.

We also use accruals-based earnings management as an alternative dependent variable. Existing literature suggests that the extent of earnings management is negatively related to disclosure quality (Jo and Kim 2007; Lobo and Zhou 2001; Katmon and Farooque 2017). We estimate the extent of earnings management for each firm following Kothari et al (2005):

$$TA_{i,t} = \alpha + \beta_1 \left(\frac{1}{Assets_{i,t-1}} \right) + \beta_2 (\Delta Sales_{i,t} - \Delta AR_{i,t}) + \beta_3 PPE_{i,t} + \beta_4 ROA_{i,t} + \varepsilon_{i,t} \quad (5)$$

where $TA_{i,t}$ is total accruals, calculated as the difference between income before extraordinary items and operating cash flows adjusted for extraordinary items and discontinued operations, scaled by lagged total assets ($Assets_{i,t-1}$). The variables $\Delta Sales_{i,t}$, $\Delta AR_{i,t}$ and $PPE_{i,t}$ are the change in sales, change in account receivables, and net property, plant, and equipment, all of which are scaled by $Assets_{i,t-1}$. The discretionary accruals are the absolute value of the residuals estimated from the regression. We estimate the regression by each industry and year cluster and require the number of observations in each cluster to be above ten. As discretionary accruals reverse over time, we use the absolute value of the residuals as an alternative proxy for financial reporting fraud, with a higher absolute value of discretionary accruals suggesting a greater extent of information manipulation (Griffin et al. 2016).

In Column (2) we use the absolute value of estimated accrual-based earnings management as the dependent variable. The coefficient on *ExCEO Promotion* is −0.007 and significant at the 5% level (*t*-stat = −2.03), indicating that earnings management is lower for firms with promoted former CEOs.

[Insert Table 7 here]

Together, the results in Tables 6 and 7 show that our findings are robust to different definitions of the independent and dependent variables.

2.5.5 Using Matched Samples

To alleviate the concern that systematic differences between firms with and without promoted former CEOs may drive the different likelihoods of the commitment of financial reporting fraud, we use entropy balancing matching (EBM) and propensity-score-matching (PSM) to construct matched samples.

We first use a sample based on EBM. The control sample consists of firms without any promoted former CEOs. We assign weights to the control group based on the first two moments, i.e., the mean and variance, of the firm-level and CEO-level covariates used in the baseline regression in Table 4. By using entropy balancing, we can construct a matched sample based on higher moments of the covariates and allow for non-linear relations (Hainmueller 2012; King and Nielsen 2019). In Panel A of Appendix Table C7, we report

the means and variances of the firm-level and CEO-level characteristics, and show that entropy balance has been achieved. In Column (1) of Table 8, we repeat the baseline regression based on the EBM sample. We continue to observe a significantly negative coefficient on *ExCEO Promotion*, in line with our baseline findings.

We next construct an event-based PSM sample. We begin by retaining all firm-year observations with promoted CEOs in that year as the treated group, and the observations without any promoted CEOs during our sample period as the control group. We then estimate a logistic model to compute the propensity score. The logistic model used in the PSM is as follows:

$$CEO\ Promotion_{i,t} = \alpha + \beta Controls_{i,t} + Industry\ FE + Year\ FE + \varepsilon_{i,t} \quad (6)$$

where the control variables are the same as in the baseline regression in Table 4. We match the treatment group to the control group using a caliper of 0.05, and without replacement. We successfully match 61 CEO promotion events with the valid control group. Panel B of Appendix Table C7 reports the covariate balance tests, and shows that the firm-level and CEO-level characteristics are quite similar across the two groups. We then construct an indicator variable *Treat* that equals one if a firm has a promoted CEO, and zero otherwise, and an indicator variable *Post* that equals one for the one to three years after a CEO is promoted and zero for the one to three years before a CEO is promoted. In Column (2) of Table 8, we repeat the baseline regression using the PSM sample and replacing *ExCEO Promotion* with the interaction term *Treat* × *Post* and the term *Post*. The equation used for Column (2) is as follows:

$$Fraud_{i,t} = \alpha + \beta_1 Treat_{i,t} \times Post_{i,t} + \beta_2 Post_{i,t} + \gamma Controls_{i,t} + Year\ FE + Firm\ FE + \varepsilon_{i,t} \quad (7)$$

The coefficient of *Treat* × *Post* is negative and significant at the 1% level, suggesting that, compared with the control group (i.e., firms without promoted CEOs), the treated group (i.e., firms with promoted former CEOs) are less likely to commit financial reporting fraud after the CEOs receive the promotion. This finding is consistent with our baseline results.[9]

[Insert Table 8 here]

[9] In Appendix Table C8, we report the results of using the EBM sample constructed using the first moment (i.e., the mean) and three moments (i.e., the mean, the variance and the skewness), and the results of using PSM one year before a CEO is promoted. We obtain similar results when using these alternative matching procedures.

2.5.6 Cross-sectional Variation Analyses

2.5.6.1 Impact of CEOs' Political Promotion Prospects

Our empirical results have so far provided support for the role-model view of preceding CEOs' promotion, i.e., that preceding CEOs' promotions point the way towards an upward career pathway for their successors, and thus the successors have stronger motivation to behave well to seek promotion. Following this line of reasoning, successors' promotion motivation can also be affected by their promotion prospects that are partially determined by attributes such as education, work experience, political ranking etc. In this subsection, we test whether the role-model effect varies among CEOs with varying promotion prospects. We expect the negative association between preceding CEOs' promotions and financial reporting fraud to be more pronounced for CEOs with better promotion prospects.

We first test the moderating effect of CEOs' educational background. Prior literature documents that managers with better educational backgrounds display higher intelligence, greater managerial ability (Datta and Rajagopalan 1998; Frey and Detterman 2004; King et al. 2016), and better performance (Chevalier and Ellison 1999). Therefore, education is an important factor to consider in recruiting a CEO (Bhagat et al. 2015). Moreover, education has become a critical criterion in the CEO appointments of Chinese SOEs since the professionalization and marketization reforms. Since 2003, SASAC and the Organization Department of the CPC have launched several professionalization and marketization reforms to improve the quality of the executive teams in SOEs.[10] As Lin (2013) discusses, the professionalization reforms impose more requirements on CEOs' age, education, and specialization work. In terms of education, the executive reforms require executives to have a minimum of a bachelor's degree.[11] As the reforms proceed, CEO candidates with better educational backgrounds are more likely to be promoted. We expect that the role-model effect of preceding CEOs is stronger for CEOs with better educational backgrounds.

To test this prediction, we interact *ExCEO Promotion* with *High Education* (an indicator variable that equals one if a CEO has a Ph.D. degree, and zero otherwise). We report the results in Column (1) of Table 9. The coefficient on the interaction term *High Education* × *ExCEO Promotion* is significantly negative at the 1% level, suggesting

[10] SASAC and the Party's Organization Department have announced several regulatory schemes on executive recruitment, for example, Provisional Rules on Corporate Leaders of Central SOEs (2009), Provisional Measures on the Comprehensive Evaluation of Corporate Leadership Teams and Leaders of Central SOEs (2009), and Guidance on Public Recruitments for Senior Managers of Central SOEs (2004). Local SASACs have similar rules.

[11] Provisional Rules on Corporate Leaders of Central SOEs (2009).

that the negative association between preceding CEOs' promotion and financial reporting fraud is more pronounced among CEOs with higher educational levels.

We then consider the moderating impact of CEOs' political ranking. In Chinese SOEs, the government retains substantial control over personnel decisions such as selections, appointments, and dismissals (Fan et al. 2007). Under the strong intervention of the government, executives in Chinese SOEs work in a closed pyramidal managerial market, with fewer people higher up in the political ranking hierarchy (Chen et al. 2018). CEOs with higher political rankings are more senior in the party system, and tend to have a richer experience in the political arena. Thus, CEOs with higher political rankings have better promotion prospects than their counterparts with lower political rankings. And we expect the role-model effect of preceding CEOs to be stronger for CEOs with higher political rankings.

In Column (2) of Table 9, we interact *ExCEO Promotion* with *High Political Rank* (an indicator variable that equals one if a CEO's political ranking is one of the following posts: chief at the state level, deputy at the state level, chief at the provincial or ministerial level, or deputy at the provincial or ministerial level, and zero otherwise). The coefficient on the interaction term *High Political Rank* × *ExCEO Promotion* is significantly negative at the 1% level, indicating that the negative association between preceding CEOs' promotion and financial reporting fraud is more pronounced among CEOs with higher political rankings.

Overall, the above empirical findings are in line with our conjecture that the fraud-deterring effect of preceding CEOs' promotion is more pronounced for CEOs with better promotion prospects.[12]

[Insert Table 9 here]

2.5.6.2 Impact of Information Environment

In this subsection, we examine the impact of former CEOs' promotions on financial reporting fraud, conditional on firms' information environments. Firms with more transparent information environments are less likely to engage in fraudulent behaviour (Miller 2006; Yu 2008; Dyck et al. 2010). Therefore, we expect that the negative association between former CEOs' promotions and financial reporting fraud will be more (less) pronounced for SOEs with more (less) transparent information environments. We follow previous research and use media coverage and analysts' forecast dispersion as proxies for

[12] We also report robustness checks of cross-sectional analysis using a linear probability model. The results for the impact of CEOs' promotion prospects are reported in Columns (1) and (2) of Table C6 in the appendix.

information transparency (Leuz 2003; Frankel and Li 2004; Maskara and Mullineaux 2011; Cui et al. 2018).

Table 10 reports the results. In Column (1), we multiply *ExCEO Promotion* by *High Media Coverage*, an indicator variable that equals one (zero) if a firm's number of media reports is in the top (bottom) tercile. A higher level of media coverage indicates a more transparent information environment. The coefficient on the interaction term *High Media Coverage* × *ExCEO Promotion* is significantly negative at the 5% level. In Column (2), we multiply *ExCEO Promotion* by *High Forecast Dispersion*, an indicator variable that equals one (zero) if a firm's analyst forecast dispersion is in the top (bottom) tercile. A higher level of analyst forecast dispersion suggests a less transparent information environment. The coefficient on the interaction term *High Forecast Dispersion* × *ExCEO Promotion* is significantly positive at the 1% level. Overall, the above findings suggest that the impact of former CEOs' promotions on financial reporting fraud is more pronounced for SOEs with more transparent information environments.[13]

[Insert Table 10 here]

2.6 Conclusion

In this paper, we examine the influence of preceding CEOs' promotions on successors' likelihood of committing financial reporting fraud, using Chinese listed SOEs. We find that preceding CEOs' promotions significantly decrease the incumbent CEOs' probability of committing financial reporting fraud. In Chinese SOEs, promoted predecessors serve as role models for their successors, thereby effectively deterring financial fraud. Our findings are robust to alternative measurements of preceding CEOs' promotion, various measures of financial reporting fraud, and alternative samples.

Our subsequent cross-sectional analysis shows that the negative association between preceding CEOs' promotions and financial reporting fraud is more pronounced for current CEOs with better promotion prospects (better education and higher political rankings) and for firms with more transparent information environments (more media coverage and less dispersed analyst forecasts).

This study makes several contributions to the literature. First, our study contributes to the growing literature on incentives and career approaches of executives, by showing that preceding CEOs can affect their successors' financial reporting incentives and behaviour

[13] The results for the impact of the information environment when using a linear probability model are reported in Columns (3) and (4) of Table C6 in the Appendix.

(Lin 2013; Feng and Johansson 2017; Cao et al. 2019). To the best of our knowledge, our study is the first to provide evidence on the relation between preceding CEOs' promotion and successors' financial misconduct. Second, our study adds to the research on corporate fraud (Beasley 1996; Fich and Shivdasani 2007; Wang et al. 2021), revealing that former CEOs' promotions can deter firms' fraudulent behaviour.

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Table 1

Summary Statistics for Effects of Preceding CEOs' Promotion on Successors' Promotion

This table reports the summary statistics for variables used in Table 3. The sample period is 2008-2016. All variables are defined in Appendix A. The continuous variables are winsorized at the 1st and 99th percentiles.

	Mean	P25	P50	P75	Std.Dev.
Panel A: CEO Promotion (N = 3,765)					
ExCEO Promotion	0.083	0.000	0.000	0.000	0.276
CEO Promotion	0.055	0.000	0.000	0.000	0.228
Panel B: Firm Attributes (N = 3,765)					
Market Cap (in billion RMB)	9.844	2.363	4.157	8.512	18.250
Ln Market Cap	22.310	21.580	22.150	22.860	1.044
ROA	0.029	0.008	0.028	0.057	0.061
Leverage	0.541	0.391	0.559	0.688	0.202
Employee (in thousand)	6.025	1.089	2.541	5.904	11.010
Ln Employee	7.809	6.993	7.840	8.683	1.382
Effective Tax Rate	0.174	0.077	0.178	0.260	0.214
Panel C: CEO Attributes (N = 3,765)					
Duality	0.090	0.000	0.000	0.000	0.286
Age	49.110	45.000	49.000	53.000	5.480
Ln Age	3.888	3.807	3.892	3.970	0.112
Tenure Years	3.992	1.852	3.247	5.515	2.829
Salary (in thousand RMB)	536.100	263.800	426.000	670.200	446.300
Ln Salary	12.920	12.480	12.960	13.420	0.745
CEO Ownership	0.210	0.000	0.000	0.000	0.407

Table 2

Summary Statistics for Effects of Preceding CEOs' Promotion on Financial Reporting Frauds

This table reports the summary statistics for variables used in the main analysis. The sample period is 2008-2016. All variables are defined in Appendix A. The continuous variables are winsorized at the 1st and 99th percentiles.

	Mean	P25	P50	P75	Std.Dev.
Panel A: CEO Promotion (N = 2,228)					
ExCEO Promotion	0.130	0.000	0.000	0.000	0.336
Panel B: Financial Reporting Fraud (N = 2,228)					
Fraud	0.318	0.000	0.000	1.000	0.466
Panel C: Firm Attributes (N = 2,228)					
Market Cap (in billion RMB)	7.096	2.197	3.780	7.216	11.430
Ln Market Cap	22.160	21.510	22.050	22.700	0.915
ROA	0.018	0.004	0.019	0.043	0.061
Leverage	0.578	0.437	0.595	0.718	0.203
Board Size	9.290	9.000	9.000	10.000	1.894
Ln Board Size	2.209	2.197	2.197	2.303	0.202
Independent Ratio	0.366	0.333	0.333	0.375	0.050
Analysts Coverage	20.920	1.000	6.000	24.000	36.530
Ln Analysts Coverage	2.028	0.693	1.946	3.219	1.504
Big Auditor	0.146	0.000	0.000	0.000	0.353
Panel D: CEO Attributes (N = 2,228)					
Duality	0.106	0.000	0.000	0.000	0.308
Age	49.020	45.000	49.000	53.000	5.481
Ln Age	3.886	3.807	3.892	3.970	0.113
Tenure Years	4.074	1.725	3.129	5.663	3.112
Salary (in thousand RMB)	492.900	244.300	400.000	612.000	395.600
Ln Salary	12.850	12.410	12.900	13.320	0.729
CEO Ownership	0.200	0.000	0.000	0.000	0.400

Panel E: CEOs' Status at the End of the Year of Observation (CEO-level)

Number of CEOs	Promoted	Demoted	Lateral Move	Incumbent	Other
703	119	62	103	246	173
	17%	9%	15%	35%	25%

Table 3

The Effect of Preceding CEOs' Promotion on Successors' Promotion

The table below reports the results of the effect of preceding CEOs' promotion on the likelihood of successors' promotion. The dependent variable *CEO Promotion* is an indicator variable that equals one if a CEO is promoted in that year, and zero otherwise. The independent variable *ExCEO Promotion* is an indicator variable that equals one if there is a promoted preceding CEO in the past five years, and zero otherwise. All variables are defined in Appendix A. The *z*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent= *CEO Promotion*			
	Cox PH Model		Logit Model	
	Coefficients	Hazard Ratios	Coefficients	Marginal Effects at Means
	(1)	(2)	(3)	(4)
ExCEO Promotion	0.656***	1.928	0.585**	0.089
	(3.01)		(2.30)	
Ln Market Cap	0.123	1.131	−0.018	−0.002
	(1.32)		(−0.17)	
ROA	3.201*	24.552	5.335***	0.688
	(1.84)		(3.42)	
Leverage	1.223***	3.398	1.382***	0.178
	(2.86)		(2.97)	
Ln Employee	0.074	1.077	0.065	0.008
	(1.03)		(0.84)	
Effective Tax Rate	0.334	1.397	0.477	0.062
	(1.04)		(1.22)	
Duality	0.695***	2.003	0.502**	0.065
	(3.21)		(2.26)	
Ln Age	−0.456	0.634	−1.865***	−0.240
	(−0.65)		(−2.73)	
Tenure Years	−0.020	0.980	0.054*	0.007
	(−0.51)		(1.95)	
Ln Salary	−0.123	0.885	0.083	0.011
	(−1.06)		(0.70)	
CEO Ownership	−0.147	0.864	−0.010	−0.001
	(−0.77)		(−0.05)	
Constant			2.067	
			(0.60)	
Industry FE	Yes		Yes	
N	2,522		1,218	
Pseudo R^2	0.029		0.056	

Table 4

The Effect of Preceding CEOs' Promotion on Financial Reporting Frauds

The table below reports the results of the effect of preceding CEO's promotion on firms' financial reporting frauds. The dependent variable *Fraud* is an indicator variable that equals one if a firm commits financial reporting fraud in that year, and zero otherwise. The independent variable *ExCEO Promotion* is an indicator variable equals one if there is a promoted preceding CEO in the past five years, and zero otherwise. All variables are defined in Appendix A. The *z*-statistics are in parentheses, computed using robust standard errors clustered at the firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Fraud*		
	Coefficients	Coefficients	Marginal Effects at Means
	(1)	(2)	(3)
ExCEO Promotion	−0.528**	−0.682***	−0.162
	(−2.30)	(−2.89)	
Ln Market Cap		−0.301	−0.075
		(−1.40)	
ROA		−2.548**	−0.632
		(−2.15)	
Leverage		−0.996	−0.247
		(−1.48)	
Ln Board Size		2.138***	0.530
		(2.75)	
Independent Ratio		3.578*	0.888
		(1.66)	
Ln Analyst Coverage		−0.072	−0.018
		(−0.71)	
Big Auditor		0.044	0.011
		(0.18)	
Duality		0.414	0.103
		(1.40)	
Ln Age		0.322	0.080
		(0.33)	
Tenure Years		−0.051	−0.013
		(−1.38)	
Ln Salary		0.025	0.006
		(0.17)	
CEO Ownership		−0.585**	−0.145
		(−2.14)	
Year FE	Yes	Yes	
Firm FE	Yes	Yes	
N	2,228	2,228	
Pseudo R^2	0.018	0.043	

Table 5
Dynamic Impact of Preceding CEOs' Promotion

The table below reports the results of the dynamic impact of preceding CEOs' promotion. The four indicators *After1*, *After2*, *After3*, and *After4+* are set to one for the first year (*After1*), the second year (*After2*), the third year (*After3*) and the fourth and fifth years (*After4+*) after a preceding CEO is promoted respectively, and zero otherwise. The control variables are the same as Table 4, including *Ln Market Cap*, *ROA*, *Leverage*, *Ln Board Size*, *Independent Ratio*, *Ln Analyst Coverage*, *Big Auditor*, *Duality*, *Ln Age*, *Tenure Years*, *Ln Salary* and *CEO Ownership*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *z*-statistics are in parentheses, computed using robust standard errors clustered at the firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Fraud*	
	(1)	(2)
After1	−0.352	−0.542*
	(−1.19)	(−1.74)
After2	−0.559*	−0.755**
	(−1.67)	(−2.22)
After3	−1.199***	−1.394***
	(−2.95)	(−3.44)
After4+	−0.556	−0.629
	(−1.38)	(−1.57)
Controls	No	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes
N	2,185	2,185
Pseudo R^2	0.021	0.046

Table 6

Robustness: Alternative Measures of Preceding CEOs' Promotion

The table below reports the results of using alternative measures of preceding CEOs' promotion. The control variables are the same as Table 4, including *Ln Market Cap*, *ROA*, *Leverage*, *Ln Board Size*, *Independent Ratio*, *Ln Analyst Coverage*, *Big Auditor*, *Duality*, *Ln Age*, *Tenure Years*, *Ln Salary* and *CEO Ownership*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *z*-statistics are in parentheses, computed using robust standard errors clustered at the firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Fraud*			
	(1)	(2)	(3)	(4)
ExCEO Promotion 4 Years	−0.600***			
	(−2.62)			
ExCEO Promotion 6 Years		−0.635**		
		(−2.54)		
ExCEO Promotion Immediate			−0.499**	
			(−2.00)	
ExCEO Promotion First				−0.640***
				(−2.59)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	2,228	2,228	2,228	2,228
Pseudo R^2	0.042	0.042	0.040	0.042

Table 7

Robustness: Alternative Measures of Financial Reporting Misconducts

The table below reports the results of using alternative measures of financial reporting misconducts. The dependent variable in Column (1) is *Avg. Fraud*, the average number of financial reporting frauds during a CEO's tenure. The dependent variable in Column (2) is *Earnings Management*, it is the absolute value of accrual-based earnings management estimated following Kothari et al. (2005). The control variables are the same as Table 4, including *Ln Market Cap*, *ROA*, *Leverage*, *Ln Board Size*, *Independent Ratio*, *Ln Analyst Coverage*, *Big Auditor*, *Duality*, *Ln Age*, *Tenure Years*, *Ln Salary* and *CEO Ownership*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at the firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Avg. Fraud*	Dependent = *Earnings Management*
	(1)	(2)
ExCEO Promotion	−0.085**	−0.007**
	(−2.00)	(−2.03)
Controls	Yes	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes
N	6,235	6,187
R^2	0.692	0.291

Table 8

Using Entropy Balanced and Propensity-score Matched Sample

The table below reports the results of using entropy balanced and propensity-score matched sample. *Treat* is an indicator variable that equals one if a firm has a promoted CEO, and zero otherwise. *Post* is an indicator variable that equals one for one to three years after a CEO is promoted and zero for one to three years before a CEO is promoted. The control variables are the same as Table 4, including *Ln Market Cap*, *ROA*, *Leverage*, *Ln Board Size*, *Independent Ratio*, *Ln Analyst Coverage*, *Big Auditor*, *Duality*, *Ln Age*, *Tenure Years*, *Ln Salary* and *CEO Ownership*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *z*-statistics are in parentheses, computed using robust standard errors clustered at the firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Fraud*	
	Entropy Balanced Sample	Propensity-score Matched Sample
	(1)	(2)
ExCEO Promotion	−0.793**	
	(−2.15)	
Treat × *Post*		−2.530***
		(−3.06)
Post		1.063
		(1.33)
Controls	Yes	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes
N	2,228	234
Pseudo R^2	0.280	0.195

Table 9
The Effect of CEOs' Promotion Prospects

The table below reports the results of the effect of the CEOs' promotion prospects on firms' financial reporting frauds. The control variables are the same as Table 4, including *Ln Market Cap*, *ROA*, *Leverage*, *Ln Board Size*, *Independent Ratio*, *Ln Analyst Coverage*, *Big Auditor*, *Duality*, *Ln Age*, *Tenure Years*, *Ln Salary* and *CEO Ownership*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *z*-statistics are in parentheses, computed using robust standard errors clustered at the firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Fraud*	
	(1)	(2)
High Education × *ExCEO Promotion*	−14.165***	
	(−19.39)	
High Education	−0.088	
	(−0.18)	
High Political Rank × *ExCEO Promotion*		−14.665***
		(−14.80)
High Political Rank		0.705
		(0.89)
ExCEO Promotion	−0.646***	−0.649***
	(−2.74)	(−2.76)
Controls	Yes	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes
N	2,228	2,228
Pseudo R^2	0.047	0.046

Table 10
The Effect of Information Environment

The table below reports the results of the effect of the information environment on firms' financial reporting frauds. The control variables are the same as Table 4, including *Ln Market Cap*, *ROA*, *Leverage*, *Ln Board Size*, *Independent Ratio*, *Ln Analyst Coverage*, *Big Auditor*, *Duality*, *Ln Age*, *Tenure Years*, *Ln Salary* and *CEO Ownership*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *z*-statistics are in parentheses, computed using robust standard errors clustered at the firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Fraud*	
	(1)	(2)
High Media Coverage × ExCEO Promotion	−1.092**	
	(−2.07)	
High Media Coverage	0.315	
	(0.96)	
High Forecast Dispersion × ExCEO Promotion		1.820***
		(2.73)
High Forecast Dispersion		−0.122
		(−0.43)
ExCEO Promotion	0.197	−1.675***
	(0.51)	(−2.83)
Controls	Yes	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes
N	1,191	753
Pseudo R^2	0.048	0.103

Appendix A: Variable Definitions

Variable Definitions

Panel A: Variables Appearing in the Main Body of the Paper	
Variable	**Definition**
After1	An indicator variable that equals one if it is the first year after a preceding CEO's promotion, and zero otherwise.
After2	An indicator variable that equals one if it is the second year after a preceding CEO's promotion, and zero otherwise.
After3	An indicator variable that equals one if it is the third year after a preceding CEO's promotion, and zero otherwise.
After4+	An indicator variable that equals one if it is the fourth or fifth year after a preceding CEO's promotion, and zero otherwise.
ΔAR	The changes in account receivables scaled by lagged total assets.
Assets	Total assets.
Avg. Fraud	The average number of occurrences of financial reporting fraud during a CEO's tenure.
Big Auditor	An indicator variable that equals one if the auditor is one of the "big four", and zero otherwise.
CEO Ownership	An indicator variable that equals one if the CEO holds stock ownership in the firm.
CEO Promotion	An indicator variable that equals one if a CEO is promoted in that year, and zero otherwise. A CEO is defined as receiving a promotion if they are appointed to one of the following positions: (1) the Chairman of the Board in the firm or another SOE at the same administrative level, (2) the CEO or Chairman of the Board in the parent firm or another SOE at a higher administrative level, (3) secretary or vice secretary of the CPC committee in the firm or another SOE at the same administrative level, (4) secretary or vice secretary of the CPC committee in the parent firm or another SOE at a higher administrative level, and (5) governmental official.
Duality	An indicator variable that equals one if a firm's CEO also serves as the board's chairman.
Earnings Management	Accrual-based earnings management that is estimated following Kothari et al. (2005) and using the absolute value of the estimated residuals.
Effective Tax Rate	Total current income tax expense divided by pretax income.
ExCEO Promotion	An indicator variable that equals one if a former CEO has been promoted in the past five years, and zero otherwise.
ExCEO Promotion 4 Years	An indicator variable that equals one if a former CEO has been promoted in the past four years, and zero otherwise.
ExCEO Promotion 6 Years	An indicator variable that equals one if a former CEO has been promoted in the past six years, and zero otherwise.
ExCEO Promotion First	An indicator variable that equals one in the five years following the first promotion in our sample of the firm's former CEOs, and zero otherwise.

ExCEO Promotion Immediate	An indicator variable that equals one in the five years following the promotion of the immediate predecessor to the current CEO, and zero otherwise.
Fraud	An indicator variable that equals one if a firm commits financial reporting fraud in that year, and zero otherwise. We define financial reporting fraud as any fraudulent activity in the following categories: manipulation of earnings, manipulation of assets, false statement, delayed disclosure, significant omission in disclosure, fraudulent disclosures, fraudulent disclosure in public offerings, and non-material accounting errors.
High Education	An indicator variable that equals one if a CEO has a Ph.D. degree, and zero otherwise.
High Forecast Dispersion	An indicator variable that equals one (zero) if a firm's analyst forecasting dispersion is in the top (bottom) tercile.
High Media Coverage	An indicator variable that equals one (zero) if a firm's number of media reports is in the top (bottom) tercile.
High Political Rank	An indicator variable that equals one if a CEO's political ranking is one of the following: chief at the state level, deputy at the state level, chief at the provincial or ministerial level, or deputy at the provincial or ministerial level, and zero otherwise.
Independent Ratio	The proportion of independent board directors.
Leverage	Total liabilities scaled by total assets.
Ln Age	The natural logarithm of the CEO's age.
Ln Analysts Coverage	The natural logarithm of the number of analysts following a firm.
Ln Board Size	The natural logarithm of the number of board directors.
Ln Employee	The natural logarithm of the number of employees.
Ln Market Cap	The natural logarithm of the firm's market capitalization.
Ln Salary	The natural logarithm of the CEO's total salary.
Post	An indicator variable that equals one in the one to three years after a CEO is promoted, and zero in the one to three years before a CEO is promoted.
PPE	Net property, plant, and equipment scaled by lagged total assets.
ROA	Net income scaled by total assets.
ΔSales	Changes in sales scaled by lagged total assets.
TA	Total accruals, calculated as the difference between income before extraordinary items and operating cash flows adjusted for extraordinary items and discontinued operations, scaled by lagged total assets.
Tenure Years	The tenure of the CEO in years.
Treat	An indicator variable that equals one if a firm has a promoted former CEO, and zero otherwise.
Panel B: Variables Appearing in the Appendix	
Institutional Ownership	The percentage of shares held by institutional shareholders.

Ln Board Meetings	The natural logarithm of one plus the number of board meetings.
Ln Media Coverage	The natural logarithm of one plus the number of media reports about the firm.

Appendix B: Different Types of Frauds

Details on Different Types of Frauds

Categories	Codes	Details
Financial Frauds	P2501	Manipulation of earnings
	P2502	Manipulation of asset
	P2503	False statement (misleading description)
	P2504	Delayed disclosure
	P2505	Significant omission in disclosures
	P2506	Fraudulent disclosures (other)
	P2507	Fraudulent disclosure in public offerings
	P2515	Non-material accounting errors
Market-related Frauds	P2511	Trading on inside information
	P2512	Violations of stock purchase rules
	P2513	Manipulations of stock prices
Capital-related Frauds	P2508	Violations of capital contribution
	P2509	Abusing the contributed capital
	P2510	Expropriations of firm assets
	P2514	Violations of warranty rules
Other Frauds	P2599	Frauds associated with other activities

Appendix C: Robustness Checks and Additional Analysis

Table C1
Summary Statistics for Variables without Missing

This table reports the summary statistics for variables without missing. The sample period is 2008-2016. All variables are defined in Appendix A. The continuous variables are winsorized at the 1st and 99th percentiles.

	Mean	P25	P50	P75	Std.Dev.
Panel A: CEO Promotion (N = 6,293)					
ExCEO Promotion	0.108	0.000	0.000	0.000	0.311
Panel B: Financial Reporting Fraud (N = 6,293)					
Fraud	0.121	0.000	0.000	0.000	0.326
Panel C: Firm Attributes (N = 6,293)					
Market Cap (in billion RMB)	10.900	2.679	4.873	10.150	18.860
Ln Market Cap	22.450	21.710	22.310	23.040	1.042
ROA	0.031	0.009	0.029	0.056	0.058
Leverage	0.535	0.385	0.548	0.685	0.203
Board Size	9.291	9.000	9.000	10.000	1.825
Ln Board Size	2.210	2.197	2.197	2.303	0.194
Independent Ratio	0.367	0.333	0.333	0.375	0.051
Analysts Coverage	33.080	2.000	12.000	43.000	48.360
Ln Analysts Coverage	2.481	1.099	2.565	3.784	1.591
Big Auditor	0.196	0.000	0.000	0.000	0.397
Panel D: CEO Attributes (N = 6,293)					
Duality	0.090	0.000	0.000	0.000	0.286
Age	49.130	46.000	49.000	53.000	5.267
Ln Age	3.889	3.829	3.892	3.970	0.108
Tenure Years	3.978	1.737	3.131	5.521	2.958
Salary (in thousand RMB)	602.400	297.500	478.200	724.600	508.600
Ln Salary	13.040	12.600	13.080	13.490	0.742
CEO Ownership	0.236	0.000	0.000	0.000	0.424

Panel E: CEOs' Status at the End of the Year of Observation (CEO-level)

Number of CEOs	Promoted	Demoted	Lateral Move	Incumbent	Other
1,995	303	178	328	741	445
	15%	9%	16%	37%	22%

Table C2
Robustness: Excluding CEO Transition Year

The table below reports the results of using the sample excluding the observations of CEO transition year. The control variables are the same as Table 4, including *Ln Market Cap*, *ROA*, *Leverage*, *Ln Board Size*, *Independent Ratio*, *Ln Analyst Coverage*, *Big Auditor*, *Duality*, *Ln Age*, *Tenure Years*, *Ln Salary* and *CEO Ownership*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *z*-statistics are in parentheses, computed using robust standard errors clustered at the firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent= *Fraud*	
	(1)	(2)
ExCEO Promotion	−0.566**	−0.701***
	(−2.20)	(−2.68)
Controls	No	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes
N	2,146	2,146
Pseudo R^2	0.017	0.039

Table C3

Robustness: Standard Errors Clustered at Different Levels

The table below reports the results of using standard errors clustered at different levels. The control variables are the same as Table 4, including *Ln Market Cap*, *ROA*, *Leverage*, *Ln Board Size*, *Independent Ratio*, *Ln Analyst Coverage*, *Big Auditor*, *Duality*, *Ln Age*, *Tenure Years*, *Ln Salary* and *CEO Ownership*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *z*-statistics are in parentheses, computed using robust standard errors clustered at different levels. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Fraud*		
	(1)	(2)	(3)
ExCEO Promotion	−0.682***	−0.682***	−0.682***
	(−2.89)	(−3.45)	(−2.82)
Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
N	2,228	2,228	2,228
Pseudo R^2	0.043	0.043	0.043
Level of Cluster	None	Province	Province-industry

Table C4
Robustness: Additional Controls

The table below reports the results of including additional control variables. The other control variables are the same as Table 4, including *Ln Market Cap*, *ROA*, *Leverage*, *Ln Board Size*, *Independent Ratio*, *Ln Analyst Coverage*, *Big Auditor*, *Duality*, *Ln Age*, *Tenure Years*, *Ln Salary* and *CEO Ownership*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *z*-statistics are in parentheses, computed using robust standard errors clustered at the firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Fraud*		
	(1)	(2)	(3)
ExCEO Promotion	−0.660***	−0.689***	−0.670***
	(−2.78)	(−2.91)	(−2.86)
Institutional Ownership	−0.022		
	(−1.44)		
Ln Board Meetings		0.129	
		(0.56)	
Ln Media Coverage			0.123
			(1.05)
Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
N	2,228	2,228	2,228
Pseudo R^2	0.044	0.043	0.043

Table C5

Robustness: Baseline using Linear Probability Model

The table below reports the results of linear probability models. The control variables are the same as Table 4, including *Ln Market Cap*, *ROA*, *Leverage*, *Ln Board Size*, *Independent Ratio*, *Ln Analyst Coverage*, *Big Auditor*, *Duality*, *Ln Age*, *Tenure Years*, *Ln Salary* and *CEO Ownership*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at the firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Fraud*	
	(1)	(2)
ExCEO Promotion	−0.043**	−0.057***
	(−2.26)	(−2.80)
Controls	No	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes
N	6,235	6,235
R^2	0.404	0.410

Table C6

Robustness: Cross-sectional Analysis using Linear Probability Model

The table below reports the results of cross-sectional analysis of using linear probability models. The control variables are the same as Table 4, including *Ln Market Cap*, *ROA*, *Leverage*, *Ln Board Size*, *Independent Ratio*, *Ln Analyst Coverage*, *Big Auditor*, *Duality*, *Ln Age*, *Tenure Years*, *Ln Salary* and *CEO Ownership*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *z*-statistics are in parentheses, computed using robust standard errors clustered at the firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Fraud*			
	CEO Background		Information Environment	
	(1)	(2)	(3)	(4)
High Education × ExCEO Promotion	−0.053***			
	(−2.61)			
High Education	−0.004			
	(−0.17)			
High Political Rank × ExCEO Promotion		−0.265**		
		(−2.15)		
High Political Rank		0.067		
		(0.73)		
High Media Coverage × ExCEO Promotion			−0.092**	
			(−2.17)	
High Media Coverage			0.023	
			(0.93)	
High Forecast Dispersion × ExCEO Promotion				0.106***
				(2.62)
High Forecast Dispersion				−0.005
				(−0.31)
ExCEO Promotion	−0.053***	−0.053***	0.024	−0.101***
	(−2.61)	(−2.60)	(0.68)	(−3.16)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	6,235	6,235	4,090	3,132
Pseudo R^2	0.410	0.410	0.472	0.503

Table C7

Covariates Balancing of Entropy Balanced and Propensity-score Matched Sample

The table below reports the covariates balancing tests for the matched sample used in Table 8. Panel A presents the mean and variance of entropy balanced sample Panel B presents the means of propensity-score matched sample.

Panel A: Entropy Balanced Sample

	Group without Promoted Preceding CEO (Obs.=1,939)		Group with Promoted Preceding CEO (Obs.=289)		Difference in Means	Difference in Variance
	Mean	Variance	Mean	Variance		
Ln Market Cap	22.300	0.783	22.300	0.783	0.000	0.000
ROA	0.003	0.004	0.003	0.004	0.000	0.000
Leverage	0.616	0.042	0.616	0.042	0.000	0.000
Ln Board Size	2.173	0.047	2.173	0.047	0.000	0.000
Independent Ratio	0.372	0.003	0.372	0.003	0.000	0.000
Ln Analyst Coverage	1.802	1.954	1.802	1.954	0.000	0.000
Big Auditor	0.184	0.150	0.183	0.150	0.001	0.000
Duality	0.069	0.065	0.069	0.065	0.000	0.000
Ln Age	3.865	0.012	3.866	0.012	-0.001	0.000
Tenure Years	2.126	2.162	2.126	2.156	0.000	0.006
Ln Salary	12.760	0.477	12.760	0.477	0.000	0.000
CEO Ownership	0.128	0.112	0.128	0.112	0.000	0.000

Panel B: Propensity-score Matched Sample

	Group without Promoted Preceding CEO (Obs.=61)	Group with Promoted Preceding CEO (Obs.=61)	Difference in Means
	Mean	Mean	
Ln Market Cap	22.240	22.480	−0.233
ROA	0.041	0.046	−0.005
Leverage	0.577	0.567	0.010
Ln Board Size	2.179	2.178	0.002
Independent Ratio	0.370	0.368	0.002
Ln Analyst Coverage	2.332	2.753	−0.421
Big Auditor	0.164	0.164	0.000
Duality	0.115	0.131	−0.016
Ln Age	3.897	3.903	−0.006
Tenure Years	5.288	5.581	−0.293
Ln Salary	13.150	13.210	−0.068
CEO Ownership	0.180	0.295	−0.115

Table C8

Robustness: Alternative Entropy Balanced and Propensity-score Matching Analysis

The table below reports the results of using alternative entropy balanced and propensity-score matched samples. In Panel A we balance the firms with and without promoted preceding CEOs using the first moment (i.e., the mean) and three moments (i.e., the mean, the variance, and the skewness). Panel B reports the results using a propensity-score matched sample one year before a CEO was promoted. The control variables are the same as Table 4, including *Ln Market Cap*, *ROA*, *Leverage*, *Ln Board Size*, *Independent Ratio*, *Ln Analyst Coverage*, *Big Auditor*, *Duality*, *Ln Age*, *Tenure Years*, *Ln Salary* and *CEO Ownership*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *z*-statistics are in parentheses, computed using robust standard errors clustered at the firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

Panel A: Alternative EBM Sample		
	Dependent = *Fraud*	
	Entropy Balancing on the First Moment	Entropy Balancing on the Three Moments
ExCEO Promotion	−0.789**	−0.834**
	(−2.15)	(−2.24)
Controls	Yes	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes
N	2,228	2,228
Pseudo R^2	0.278	0.293
Panel B: Alternative PSM Sample		
	Dependent = *Fraud*	
Treat × Post	−2.316***	−2.801***
	(−3.03)	(−3.04)
Post	2.492**	2.439*
	(2.18)	(1.90)
Controls	No	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes
N	180	180
Pseudo R^2	0.201	0.347

Chapter 3

Government Subsidies and Cost of Equity: Evidence from U.S. Listed Firms

Abstract

This study examines the relationship between government subsidies and the cost of equity using a sample of U.S. listed firms. Our results show a negative association between government subsidies and firms' cost of equity. The results are robust to a battery of sensitivity tests. We also find evidence that government subsidies decrease firms' cost of equity by improving firms' information environment and their fundamental performance. In addition, both tax-related and non-tax-related subsidies significantly impact the cost of equity. State-level subsidies have a more significant impact than federal and local-level subsidies. Overall, our evidence is consistent with government subsidies having a significant impact on firms' financing costs.

Keywords: Government subsidy; implied cost of equity; financing cost

3.1 Introduction

Government subsidies represent a widely used policy tool worldwide. In the U.S., billions of dollars in subsidies are awarded to firms each year. For example, the U.S. federal government spent about $100 billion on corporate welfare in 2012 (DeHaven 2012). State and local-level subsidies are also considerably large. For instance, Boeing received a megadeal subsidy from Washington State in 2013, with a value of $8.7 billion. Despite a growing literature examining the economic consequences of government subsidies (Almus and Czarnitzki 2003; Lee et al. 2014; Howell 2017; Lee et al. 2017; Lim et al. 2018; Aobdia et al. 2019; Criscuolo et al. 2019; De Simone et al. 2019; Rotemberg 2019; Blank et al. 2021; Raghunandan 2021; Pappas et al. 2021; Huang 2022), very little attention has been paid to investors' perceptions of government subsidies. This study attempts to fill this gap by examining the association between government subsidies and the cost of equity.

Government subsidies can affect firms' cost of equity via two channels: firms' information asymmetry and their operating performance. Prior literature largely reaches a consensus that an increase in firms' information asymmetry results in a higher cost of equity (Botosan 1997; Leuz and Verrecchia 2000; Easley et al. 2002; Easley and O'Hara 2004; Francis et al. 2005; Fu et al. 2012; Levi and Zhang 2015). This is because investors demand a higher return for bearing greater information risk (Barry and Brown 1985; Barry and Brown 1986), and it is costly to gather, process, and transmit information (Meton 1987). Uninformed investors' cannot efficiently allocate stocks into portfolios and require higher returns to compensate for their information disadvantage (Easley and O'Hara 2004). Besides, information asymmetry can dampen stock liquidity (Copeland and Galai 1983; Glosten and Milgrom 1985), and investors demand higher expected returns when liquidity is low (Amihud and Mendelson 1986; Brennan and Subrahmanyam 1996; Amihud 2002; Pástor and Stambaugh 2003). Recent literature on government subsidies indicates subsidies have significant impacts on firms' information environment. On the one hand, as a subsidy is a major form of wealth redistribution, firms receiving subsidies are subject to greater public scrutiny, especially when they are politically connected. As such, subsidized firms may intentionally withhold subsidy information (Li et al. 2021) and obfuscate reported earnings via income smoothing (Pappas et al. 2021). Moreover, Raghunandan (2021) documents evidence that subsidy-receiving firms tend to engage more frequently in fraudulent activities. The above findings, taken together, suggest that subsidies may increase the receiving firms' information asymmetry, subsequently leading to an increase in their cost of equity. On the

other hand, instead of being "silent", subsidized firms can mitigate potential public criticism through voluntary disclosures to show their social legitimacy and thus right to receive subsidies. For example, subsidized firms can disclose more information, including general corporate information, subsidy-goal-related information (Huang 2022), and corporate social responsibility information (Lee et al. 2017). If firms voluntarily provide more information to the market to show their qualifications for receiving subsidies, the cost of equity will decrease.

Moreover, government subsidies can impact firms' cost of equity by fuelling the recipients' innovation (Almus and Czarnitzki 2003; Howell 2017), improving their corporate social responsibility (Criscuolo et al. 2019; De Simone et al. 2019) and increasing their market share (Rotemberg 2019). Firms with better social responsibility performance and market power tend to have lower costs of equity (Sullivan 1978; Sharfman and Fernando 2008; El Ghoul et al. 2011). On top of this, receiving government subsidies may signal that the receiving firm is politically connected (Aobdia et al. 2019). Since politically connected firms are more likely to be bailed out when in financial distress (Faccio 2006) and are less exposed to market risk in economic downturns (Boubakri et al. 2012), their cost of equity is expected to be lower. Following the above line of research, we expect a negative relationship between government subsidies and the cost of equity.

To test our research question, we use a sample of the U.S. listed firms from 2003 to 2014. We find that firms with more government subsidies have significantly lower implied costs of equity, suggesting that investors perceive firms with subsidies as less risky. This effect is significant both statistically and economically. According to our baseline results, a one-standard-deviation change in the government subsidy amount will decrease the cost of equity by about 0.15%, which is about 1.44% of the sample mean of the implied cost of equity. We also conduct a battery of robustness checks, and the results are consistent with the baseline findings.

One might reasonably be concerned that government subsidies are endogenously determined, which would lead to biased coefficient estimates. To circumvent the endogeneity concern that the relation between subsidies and the cost of equity is determined by the differences in the observed covariates between subsidized and unsubsidized firms, we repeat our baseline regression using (1) a propensity-score-matched (PSM) sample and (2) an entropy-balanced sample. The results remain qualitatively the same. To further mitigate the endogeneity caused by the omitted variable issue, we examine the robustness of our main

findings using a quasi-natural experiment that captures the changes in subsidies resulting from changes in the subsidy providers' fiscal surplus. Motivated by Poterba (1994) and Clemens and Miran (2012), we construct subsidy provider states' surplus shocks, which indicate whether a firm's subsidy-provider state has an unexpected surplus. Intuitively, if a firm's subsidy provider state has an unexpected surplus, the firm is more likely to receive subsidies from the state government. The results indicate that firms' cost of equity is significantly lower after a surplus shock of its subsidy providers.

We then test channels through which government subsidies could affect firms' cost of equity. We find firms receiving more government subsidies have a lower level of analyst forecast dispersion, better future operating performance, and lower performance volatility. Taken together, these findings show that government subsidies reduce the cost of equity by both decreasing firms' information asymmetry and improving performance. Further, we find that tax-related and non-tax-related subsidies are both negatively associated with firms' cost of equity, and state-level subsidies have a more significant impact than federal and local-level subsidies.

Our study contributes to the literature in several ways. First, while there is a substantial literature examining the economic consequences of government subsidies (Almus and Czarnitzki 2003; Howell 2017; Aobdia et al. 2019; Criscuolo et al. 2019; De Simone et al. 2019; Rotemberg 2019; Raghunandan 2021; Pappas et al. 2021; Huang 2022), to the best of our knowledge, our study is the first that provides evidence on the relationship between government subsidies and the cost of equity. Secondly, our study further adds to the literature on the cost of equity (Attig et al. 2008; Hail and Leuz 2006; Hail and Leuz 2009; Chen et al. 2011; Dhaliwal et al. 2011; El Ghoul et al. 2011; Chen et al. 2016; Dhaliwal et al. 2016) by showing that government subsidies form one of its material determinants. Lastly, this research is timely as the Government Accounting Standards Board (GASB) and Financial Accounting Standards Board (FASB) have recently required more disclosure of the government subsidies received by firms. Our results suggest that a decrease in the cost of equity could be one of the important implications of the disclosure of government subsidies.

The rest of the paper proceeds as follows: Section 3.2 provides the background on government subsidies in the U.S., Section 3.3 reviews related literature and proposes the hypothesis, Section 3.4 presents the data and research design, Section 3.5 presents the empirical findings, and Section 3.6 concludes.

3.2 Background

In the U.S., governments at various levels grant billions of dollars of subsidies to the corporate sector, every year, to bolster the social welfare. There are different forms of government subsidies such as cash grants, low-cost loans, tax abatement, tax credits, and others. Prior literature broadly classifies subsidies into tax-related and non-tax-related subsidies (Raghunandan 2021; Pappas et al. 2021). Tax-related subsidies are tax liabilities that the government forgoes, resulting in a decrease in government revenues. Non-tax-related subsidies are often offered in forms such as cash grants and loans, which increases government expenditure. According to the data provided by Subsidy-Tracker, the largest tax-related subsidy during our sample period was awarded to Sempra Energy, with a value of about $2.2 billion, by the state of Louisiana in 2013, while the largest non-tax-related subsidy was granted to Ameren, with a value of about $589.74 million, by the federal government in 2010. Megadeal subsidies may include both tax-related and non-tax-related subsidies. Boeing received the largest megadeal subsidy from Washington State in 2013, with a value of $8.7 billion.

Given the pervasiveness of government subsidies made to specific firms in the U.S., it is important for investors to obtain information on them. This is because, firstly, investors need to know the extent to which a firm is depending on government subsidies. During our sample period, many firms received materially large subsidies. 34 firms received more than $1 billion in subsidies, while Boeing received $14 billion in subsidies in total, the largest amount during the sample period. Secondly, many subsidies require firms' commitment to specific aspects such as job creation and R&D investment. For example, in 2012, the state of Texas granted a $269 million sales tax rebate to Amazon, and in exchange, Amazon agreed to invest $200 million in new facilities and create 2,500 jobs.[1] Firms' commitments along with the government subsidies could have a substantial impact on their operations. Thirdly, studies also show government subsidies can lead to changes in firms' behaviour, for example, disclosure and reporting (Huang 2022; Pappas et al. 2021), and fraudulent activities (Raghunandan 2021). Those potentially indirect consequences of government subsidies are also critical to investors' perceptions of subsidized firms.

Recently, both the GASB and the FASB called for more disclosures on government subsidies. In 2015, the GASB announced Statement No.77, on Tax Abatement Disclosure. GASB No.77 requires state and local governments to disclose information such as that on

[1] https://subsidytracker.goodjobsfirst.org/subsidy-tracker/tx-amazoncom

tax abatement programmes, eligibility criteria, the commitments of recipients, and the gross amount of any tax abatement. In 2015, the FASB considered requiring firms to disclose information on the government assistance they received, although it has not yet issued a codification requirement. Our finding that government subsidies are associated with a lower cost of equity speaks to the importance of disclosing information on government subsidies.

3.3 Related Literature and Hypothesis Development

3.3.1 Related Literature on Government Subsidies

There is a growing body of literature exploring the economic consequences of firm-specific subsidies. On the one hand, prior literature suggests positive outcomes for firms receiving government subsidies. For example, Howell (2017) finds government subsidies in the early stages have attract subsequent venture capital investment, and elevate firms' patenting and revenue. Almus and Czarnitzki (2003) examine the impact of R&D subsidies in Germany and find firms with R&D subsidies increase their innovation activities. Criscuolo et al. (2019) suggest subsidies stimulate employment, but the effect only exists for small firms. Similarly, De Simone et al. (2019) suggest that state and local firm-specific subsidies are positively related to various employment measures, and that the local information environment plays an important role in the achievement of the goals of government subsidies. Blank et al. (2021) show that subsidies are related to higher firm value for firms without reputation concerns. In recent work, Huang (2022) examines the association between government subsidies and firm disclosure behaviour and finds subsidized firms provide more voluntary disclosures regarding general business information and subsidy-goal-related information. Besides, some studies in the context of developing economies provide evidence that government subsidies have positive impacts on subsidized firms. Using a sample of Indian firms, Rotemberg (2019) documents that subsidies have two countervailing effects on firms: direct gains for subsidized firms and indirect losses for unsubsidized firms. The newly subsidized firms have large sales increases and almost completely crowd out competitors' products that are less internationally traded. In the Chinese context, prior literature shows that subsidized firms have higher firm value (Lee et al. 2014), more voluntary disclosure on corporate social responsibility (Lee et al. 2017) and a lower cost of debt (Lim et al. 2018).

On the other hand, some recent studies provide a critical view of firm-specific subsidies. In the U.S. context, for example, Aobdia et al. (2019) argue that government subsidies are disproportionately awarded to politically connected companies. Their study shows that

politically connected firms are more likely to receive subsidies, and that grants received by those firms are more valuable. However, politically connected government subsidies are associated with lower county-level economic growth. Raghunandan (2021) find firms with tax-related subsidies are more likely to engage in fraud than unsubsidized firms, but this association can be mitigated by third-party monitoring. Pappas et al. (2021) argue that subsidized firms aggressively smooth their earnings to avoid public scrutiny, and this earnings-smoothing behaviour is more pronounced for firms receiving non-tax-related subsidies.

Collectively, the existing literature has examined different outcomes of firm-specific subsidies. However, little attention has been paid to investors' perceptions of government subsidies. Our study tries to fill this gap by examing how government subsidies can affect the implied cost of equity.

3.3.2 Hypothesis Development

We argue that government subsidies can affect firms' cost of equity by affecting their information asymmetry and operating performance.

There are two contrasting views on how subsidies affect firms' information asymmetry. On the one hand, as a subsidy is a major form of wealth redistribution, firms receiving subsidies are subject to public scrutiny. In the U.S., politically connected firms are more likely to receive government subsidies, and the subsidies granted to politically connected firms are more valuable (Aobdia et al. 2019). Besides, the majority of government subsidies are awarded to large firms as opposed to small and start-up businesses in the U.S. (Pappas et al. 2021). The preferential treatment in terms of firms' eligibility to receive subsidies can draw public criticism that taxpayers' money is disproportionately used to benefit politically connected and big firms. To mitigate public criticism, firms have incentives to obfuscate their information environment. For example, Pappas et al. (2021) document that subsidized firms aggressively smooth their earnings to avoid public scrutiny. Meanwhile, Raghunandan (2021) finds firms with tax-related subsidies engage more frequently in fraudulent activities. The above findings suggest that subsidies may adversely affect the receiving firms' information environment, leading to higher information asymmetry. And increase in firms' information asymmetry leads to a higher cost of equity (Botosan 1997; Leuz and Verrecchia 2000; Easley et al. 2002; Easley and O'Hara 2004; Francis et al. 2005; Fu et al. 2012; Levi and Zhang 2015) because investors demand a higher return for bearing greater information risk (Barry and Brown 1985; Barry and Brown 1986), and information asymmetry can hinder

investors' ability to efficiently allocate stocks into portfolios (Easley and O'Hara 2004) and dampen stock liquidity (Copeland and Galai 1983; Glosten and Milgrom 1985). Thus, investors may demand a larger risk premium for subsidized firms and hence raise cost of equity.

On the other hand, instead of being "silent", subsidized firms may mitigate potential public criticism through voluntary disclosures to show their social legitimacy and thus right to receive subsidies. For example, subsidized firms can disclose more information, including general corporate information, subsidy-goal-related information (Huang 2022) and corporate social responsibility information (Lee et al. 2017). If firms voluntarily provide more information to the market to show their qualifications for receiving subsidies, the cost of equity will decrease.

Besides affecting firms' reporting and disclosure behaviour, the prior literature also suggests government subsidies can impact firms' cost of equity by affecting their operating performance. Government subsidies can fuel recipients' innovation (Almus and Czarnitzki 2003; Howell 2017), improve their corporate social responsibility (Criscuolo et al. 2019; De Simone et al. 2019) and increase their market share (Rotemberg 2019). Firms with better social responsibility performance and market power tend to have lower costs of equity (Sullivan 1978; Sharfman and Fernando 2008; El Ghoul et al. 2011). On top of this, receiving government subsidies may signal that a firm is politically connected (Aobdia et al. 2019). Since politically connected firms are more likely to be bailed out when in financial distress (Faccio 2006) and are less exposed to market risk in economic downturns (Boubakri et al. 2012), the cost of equity is expected to be lower for them.

Based on the above discussion, we formulate a null hypothesis:

Hypothesis: There is no relation between government subsidies and firms' cost of equity.

3.4 Data and Research Design

3.4.1 Measuring Government Subsidies

We use the Subsidy-Tracker database to obtain information on government subsidies in the U.S.[2] Subsidy-Tracker is a database provided by Good Job First (GJF), a national policy resource centre aimed at promoting corporate and government accountability in the U.S. Subsidy-Tracker provides details on federal, local and government subsidies awarded to

[2] http://www.goodjobsfirst.org/subsidy-tracker

specific firms since 1983. For each subsidy documented in Subsidy-Tracker, it provides basic information on the recipient firm, the awarding body, the amount of the subsidy, the awarding year, and the type of the subsidy. Subsidy-Tracker has been widely used in empirical research studying government subsidies recently (Jansa and Gray 2017; Wang 2018; Aobdia et al. 2019; De Simone et al. 2019; Pappas et al. 2021; Raghunandan 2021; Huang 2022). To obtain the amount a firm receives in subsidies each year, we aggregate the subsidies to the firm-year level. The main dependent variable *Subsidy* is the total amount in government subsidies a firm received in that year scaled by the firm's total assets.

3.4.2 Measuring Cost of Equity

We use an *ex-ante* approach to measure the cost of equity, based on the discounted cash flow valuation models. The basic idea is that the implied cost of equity is the discount rate that makes the present value of expected future cash flows per share equal to the current share price. The *ex-ante* cost of equity has been widely used in accounting and finance research recently (Hail and Leuz 2006; Attig et al. 2008; Hail and Leuz 2009; Chen et al. 2011; Dhaliwal et al. 2011; Chen et al. 2016; Dhaliwal et al. 2016; Boubaker et al. 2018; Gupta et al. 2018). Estimating the implied cost of equity offers several advantages. Firstly, the implied cost of equity is a forward-looking measure, while measures such as realized returns are poor alternatives (Elton 1999). Secondly, other traditional approaches, like multi-factor asset pricing models, rely heavily on historical return data in estimating the expected return, while the implied cost of equity circumvents the use of noisy realized returns (El Ghoul et al. 2011). Lastly, the implied cost of equity incorporates the effects of future growth and cash flow, making it suitable for capturing changes in the cost of equity (Hail and Leuz 2006; Hail and Leuz 2009).

Since there is little consensus on which model performs best, following prior literature (Hail and Leuz 2006; Hail and Leuz 2009; Li 2010; Dhaliwal et al. 2016), we measure the implied cost of equity using the equally weighted average of the four measures implemented by Gebhardt et al. (2001), Claus and Thomas (2001), Easton (2004), and Ohlson and Juettner-Nauroth (2005). Averaging the four proxies reduces measurement errors across the different models. The implied cost of equity estimates of the above four models are denoted by *COE GLS*, *COE CT*, *COE MPEG*, and *COE OJ* respectively. Gebhardt et al. (2001) and Claus and Thomas (2001) use the residual income valuation model, while Easton (2004) and Ohlson and Juettner-Nauroth (2005) adopt the abnormal earnings growth valuation model. The individual models differ in assumptions such as forecasting horizon and short-term and

long-term growth. Following Li (2010) and Hail and Leuz (2006), we use stock prices and analyst forecasts seven months after the fiscal year-end to ensure the financial data (e.g., book value of equity) are publicly available and priced at the time of estimation.[3] Details on the estimation of each model can be found in Appendix B.

3.4.3 Data and Sample

Our study utilizes data from multiple sources. We obtain the data on government subsidies awarded to specific firms from Subsidy-Tracker, provided by GJF. To estimate the implied cost of equity and construct the control variables, we obtain stock returns and market returns data from CRSP, firm-level accounting data from Compustat, and analyst forecast data from I/B/E/S. Since Raghunandan (2021) suggests the subsidy data from Subsidy-Tracker are much more complete from 2003 onward, we use 2003 as the beginning year of our sample period. To estimate the implied cost of equity, we need the financial data in the next few years after the estimation year. Thus, our sample period ends in 2014. We exclude (1) financial firms (SIC 6000-6900) and utility firms (SIC 4900-4999), (2) observations without valid estimates of the cost of equity, and (3) observations with insufficient data for calculating the variables used in our main analysis. Our final sample consists of 13,541 firm-year observations and 2,625 unique firms.

3.4.4 Baseline Specification

To test the association between government subsidies and the implied cost of equity, we use the following multivariate regression:

$$\begin{aligned} COE_{i,t} = {} & \alpha_1 + \beta_1 Subsidy_{i,t} + \beta_2 Ln\ Asset_{i,t} + \beta_3 Book\ to\ Market_{i.t} + \beta_4\ Leverage_{i,t} \\ & + \beta_5 Beta_{i,t} + \beta_6 Idiosyncratic\ Risk_{i,t} + \beta_7 Momentum_{i,t} \\ & + \beta_8 Analyst\ Forecast\ Bias_{i,t} + \beta_9 Long\ Term\ Growth_{i,t} \\ & + \beta_{10} Fixed\ Effects + \varepsilon_{i,t} \end{aligned} \tag{1}$$

where *COE* is the equally weighted average of the four estimates of the implied cost of equity, following Gebhardt et al. (2001), Claus and Thomas (2001), Easton (2004), and Ohlson and Juettner-Nauroth (2005). The independent variable of interest to us is *Subsidy*, which is defined as the amount of government subsidies received by a firm in the fiscal year, scaled by the firm's total assets. Our control variables include financial variables that are commonly

[3] We also repeat our analysis using price and analyst forecasts ten months after the fiscal year-end and obtain similar results.

found in prior studies of the cost of equity (Attig et al. 2008; Chen et al. 2011; Dhaliwal et al. 2011; El Ghoul et al. 2011; Chen et al. 2016; Dhaliwal et al. 2016; Boubakri et al. 2012). Specifically, we control for firm size (*Ln Asset*) and the book-to-market ratio (*Book to Market*). As Fama and French (1992) suggest, the cost of equity is expected to be negatively associated with firm size and positively associated with the book-to-market ratio. We control for leverage (*Leverage*), and the costs of equity are expected to be higher for firms with higher leverage ratios (Modigliani and Miller 1958). We then include beta (*Beta*) and idiosyncratic risk (*Idiosyncratic Risk*), both of which are expected to be positively related to the cost of equity (Dhaliwal et al. 2011; Chen et al. 2016; Dhaliwal et al. 2016). We also include the compounded stock return over the previous 12 months (*Momentum*) to control for the stickiness of analyst forecasts (Guay et al. 2011; Nekrasov and Ogneva 2011), analyst forecast bias (*Analyst Forecast Bias*) to control for forecast optimism (Easton and Sommers 2007) and analysts' long-term earnings growth (*Long Term Growth*) to control for potential bias in the estimation of the cost of equity (Chen et al. 2011). The implied cost of equity is expected to be negatively related to *Momentum* and positively related to *Analyst Forecast Bias* and *Long Term Growth* (Hail and Leuz 2006; Hail and Leuz 2009; Dhaliwal et al. 2016). Finally, we control for different sets of fixed effects, including industry and year fixed effects and industry × year fixed effects. Industry is defined according to the Fama-French 48 indudstry classifications. The values of all continuous variables are winsorized at their 1st and 99th percentiles. The estimated standard errors in all regressions are corrected for heteroscedasticity and clustered at the firm level to correct for serial correlation within firms.

3.4.5 Descriptive Statistics

Table 1 presents the summary statistics of the sample used in our baseline analysis. Panel A shows those four different estimates of the implied cost of equity. The largest estimated implied cost of equity is *COE MEPG*, with a mean of 12.87% and a median of 11.44%. The smallest estimated implied cost of equity is *COE GLS*, with a mean of 7.98% and a median of 7.61%. The mean of the equally weighted average of the four estimates, *COE*, is 10.61% and the median is 9.66%. Our estimates of the implied cost of equity are comparable to those in prior studies (Chen et al. 2011; Dhaliwal et al. 2011; Hail and Leuz 2009). Panel B reports the summary statistics of control varibales use in our main tests. In Panel C, we present the pairwise correlations between the different estimates of the implied cost of equity, all of which are highly correlated.

Panel D of Table 1 shows the univariate tests of the cost of equity and the control

variables, for subsidized and non-subsidized firms. The subsidized firms have significantly lower costs of equity than their unsubsidized counterparts. Taking *COE* as an example, investors require 0.56% higher returns from non-subsidized firms than their subsidized peers. The univariate tests of the control variables suggest that the firm characteristics differ greatly between the subsidized and non-subsidized groups. Compared with non-subsidized firms, subsidized firms have larger firm sizes, higher leverage, and lower book-to-market ratios, market betas, idiosyncratic risk, analyst forecast bias, and long-term growth rates.

[Insert Table 1 here]

Table 2 presents detailed summary statistics of the subsidies. Panel A shows the summary statistics for the full sample. The average amount of subsidies firms in our sample received was $0.83 million, while the maximum amount was $29.70 million. About 19.92% of the observations in our sample received subsidies. Panel C reports the summary statistics of the different types of subsidies. The mean of the tax-related (non-tax-related) subsidies is $2.85 million ($2.15 million). Panel D suggests that the state government is the main source of subsidies (2,277 out of 2,698), and federal-level subsidies tend to be larger (mean = $3.46 million) than state-level (mean = $2.20 million) and local-level (mean = $0.75 million) subsidies.

[Insert Table 2 here]

3.5 Empirical Findings

3.5.1 The Effect of Government Subsidies on Implied Cost of Equity

We report the baseline results in Table 3. In Columns (1), (3), and (5), we control for industry and year fixed effects, and in Columns (2), (4), and (6), we control for industry × year fixed effects. In Columns (1) and (2), the coefficients on *Subsidy* are −2.546 (*t*-stat = −4.23) and −2.653 (*t*-stat = −4.27), which are significantly negative at the 1% level. This suggests that firms receiving more in government subsidies have lower costs of equity. Taking Column (1) as an example, a one-standard-deviation change in *Subsidy* decreases the cost of equity by about 0.15% (−2.546 × 0.0006), which is approximately 1.44% of the sample mean of *COE* (10.61%).

As for the control variables, the cost of equity is significantly positively related to *Book to Market*, *Leverage*, *Beta*, *Idiosyncratic Risk*, *Analyst Forecast Bias*, and *Long Term Growth* and significantly negatively related to *Momentum*. The signs of the estimated coefficients are largely consistent with the prior literature (Hail and Leuz 2006; Hail and Leuz 2009;

Chen et al. 2011; Dhaliwal et al. 2011; Chen et al. 2016; Dhaliwal et al. 2016; Boubaker et al. 2018).

To test whether receiving or not receiving subsidies impacts the cost of equity, in Columns (3) and (4) we regress *COE* on *Subsidy Dummy*. *Subsidy Dummy* is an indicator variable that equals one if the firm receives subsidies in that year, and zero otherwise. The coefficients of *Subsidy Dummy* are insignificant. This indicates there is no effect of simply receiving government subsidies on firms' cost of equity, with an impact appearing only when the subsidies are materially large enough.

As a large proportion of our sample consists of non-subsidized firms, we include *Subsidy Dummy* as an additional control variable in Columns (5) and (6). As seen in Columns (5) and (6), the coefficients of *Subsidy* are still negative and significant at the 1% level. The results are consistent with the finding that investors require a lower return on equity from firms receiving more in government subsidies.

[Insert Table 3 here]

3.5.2 Robustness Checks

To ensure the robustness of our findings, we conduct several robustness checks by changing various aspects of our specification. Firstly, we replace the dependent variable *COE* with the four individual estimates, i.e., *COE GLS*, *COE CT*, *COE MPEG*, and *COE OJ*. The results are reported in Table 4. The coefficients on *Subsidy* are negative and significant at the 1% level across all columns, showing that the negative association between government subsidies and the implied cost of equity is not sensitive to the methods we use to estimate the implied cost of equity.

[Insert Table 4 here]

Secondly, following Chen et al. (2016) and Hail and Leuz (2009), we use a Fama-MacBeth regression (Fama and MacBeth 1973) as an alternative regression specification. The Fama-MacBeth regression removes the effects of the cross-sectional correlation in the error terms. The results are reported in Table 5; the coefficients on *Subsidy* are still significantly negative at the 1% level.

[Insert Table 5 here]

Results of further robustness checks are reported in Appendix Tables C1-C4. Firstly, in Table C1, we repeat the baseline model with standard errors that are clustered at different levels. Columns (1) and (3) report the *t*-statistics when standard errors are clustered at the

industry level, and Columns (2) and (4) those when standard errors are clustered at the industry-state level. The coefficients on *Subsidy* remain significant across all columns. Secondly, we report results when controlling for different sets of fixed effects, in Table C2. In Columns (1), (2), and (3), we control for industry, year, and state fixed effects, industry × year and state fixed effects, and industry and state × year fixed effects, respectively. The results in Table C2 suggest that *Subsidy* continues to be significantly negatively related to the cost of equity. Thirdly, we repeat the Fama-MacBeth regression using the four individual estimates of the cost of equity (i.e., *COE GLS*, *COE CT*, *COE MPEG*, and *COE OJ*), and present the results in Table C3. The coefficients on *Subsidy* are still significantly negative. Lastly, we replace the dependent variable with the cost of equity estimated using price and analyst forecasts ten months after the fiscal year-end, as shown in Table C4. In Panel A, we repeat the pooled OLS regression and Fama-MacBeth regression using the average of the four individual estimates of the cost of equity. In Panel B, we use the four individual estimates as the dependent variables. The coefficients on *Subsidy* remain significantly negative.

3.5.3 Dealing with Endogeneity

3.5.3.1 Using Matched Samples

As discussed in the summary of the descriptive statistics, the firm characteristics of subsidized and unsubsidized firms are quite different. To address the concern that the receipt of subsidies may not be random, we use propensity-score-matched (PSM) and entropy-balancing matched (EBM) samples.

Following Huang (2022) and Pappas et al. (2021), we create a PSM sample to ensure the subsidized and unsubsidized firms are similar in terms of firm attributes, except for the likelihood of receiving government subsidies. To calculate the propensity score, we estimate the following logistic model:

$$\begin{aligned} Pr\,(Subsidy\ Dummy_{i,t} = 1) \\ &= \alpha_1 + \beta_1 Ln\ Asset_{i,t} + \beta_2 Book\ to\ Market_{i.t} + \beta_3\ Leverage_{i,t} \\ &+ \beta_4 Beta_{i,t} + \beta_5 Idiosyncratic\ Risk_{i,t} + \beta_6 Momentum_{i,t} \\ &+ \beta_7 Analyst\ Forecast\ Bias_{i,t} + \beta_8 Long\ Term\ Growth_{i,t} + \varepsilon_{i,t} \end{aligned} \quad (2)$$

where *Subsidy Dummy* is an indicator variable that equals one if the firm receives subsidies in that year, and zero otherwise. The control variables are the same as in the baseline

regression. We match each subsidized firm with unsubsidized counterparts in the same industry and year, and we require a caliper width of 0.1 and perform the matching without replacement. We successfully match 869 subsidized observations with unsubsidized counterparts. Panel A of Table C5 in the appendix reports the covariate balance tests, and the differences in firm characteristics between the subsidized and unsubsidized firms are statistically insignificant. We then repeat the baseline regression using the PSM sample, with the results reported in Panel A of Table 6. We continue to observe significantly negative coefficients on *Subsidy*, consistent with the baseline findings.[4]

Then we construct an entropy-balanced sample. The control sample consists of firms without government subsidies. We assign weights to the control group based on the first moments, i.e., the means, of all the control variables used in the baseline regression. Panel B of Table C5 in the appendix reports the covariate balance tests, and shows that entropy balance has been achieved. In Panel B of Table 6, we repeat the baseline regression using the entropy-balanced sample. In line with our previous findings, the coefficients on *Subsidy* are significantly negative at the 1% level, showing firms receiving more in government subsidies have lower implied costs of equity.[5]

[Insert Table 6 here]

3.5.3.2 The Effect of Subsidy Providers' Surplus Shocks

While the above analysis based on matched samples alleviates the endogeneity concerns arising from observable confounders, it is still possible that endogeneity arising from unobservable variables could bias our findings. To deal with this issue, we conduct an analysis using a quasi-natural experiment that captures the changes in subsidies resulting from subsidy providers' fiscal surplus changes. As subsidies are part of government spending, firms are likely to receive more subsidies if the subsidy providers, i.e., governments, have fiscal surpluses. Motivated by Poterba (1994) and Clemens and Miran (2012), we construct unexpected surplus shocks of state-level governments. In this test, we only focus on the subsidies received from state-level governments. Because the unexpected

[4] We provide robustness checks for PSM analysis in appendix. In Table C6, we use the Fama-MacBeth regression in Panel A and replace the dependent variable with the four individual estimates of the cost of equity in Panel B. In Table C7, we use alternative PSM samples with different caliper widths (0.05 and 0.2). In both Table C6 and Table C7, the coefficients on *Subsidy* remain significantly negative.

[5] We provide robustness check for EMB analysis in appendix. In Table C8, we use Fama-Macbeth regression in Panel A, and replace the dependent variable with the four individual estimates of the cost of equity in Panel B. In Table C9 of the appendix, we use alternative entropy-balanced samples using the first two moments (i.e., the means and variances) and all three moments (the means, variances, and skewness). In both Table C8 and Table C9, the coefficients on *Subsidy* remain significantly negative.

surplus shocks of state governments are less likely to be related to individual firms' cost of equity, the surplus shocks could capture the exogenous changes in subsidies. We use fiscal surveys from the National Association of State Budget Officers (NASBO) to construct unexpected surplus shocks of state governments. Since 1979, NASBO has provided fiscal surveys of individual states in each fiscal year. The fiscal surveys provide detailed information on states' forecasted revenues and expenditures at the beginning of the fiscal year, actual revenues and expenditures at the end of the fiscal year, and spending cuts and tax changes that have been enacted in the current fiscal year.[6]

The unexpected surplus shocks of state governments have two components: unexpected revenue and unexpected expenditure. The unexpected revenue is the difference between the forecasted revenue at the beginning of the fiscal year and the actual revenue at the end of the fiscal year. The forecasted revenue is based on the tax system at the beginning of the fiscal year. If a state changes its tax system during the fiscal year, then the difference between the actual revenue and the forecasted revenue will not fully capture the unexpected revenue shock (Poterba 1994). Although we cannot directly observe the revenue under a constant law, we can correct for tax changes within the fiscal year by subtracting any tax changes that occur during that fiscal year (Poterba 1994; Clemens and Miran 2012). Therefore, the unexpected revenue for state *s* in fiscal year *t* is:

$$Unexpcted\ Revenue_{s,t} = Actual\ Revenue_{s,t} - \Delta Tax_{s,t} - Forecast\ Revenue_{s,t} \quad (3)$$

where *Actual Revenue* is the actual revenue of state *s* during the fiscal year *t*, ΔTax is the change in revenue that results from any tax changes that occurred during the fiscal year, and *Forecast Revenue* is the projected revenue at the beginning of the fiscal year.

Similarly, unexpected expenditure can be defined as:

$$Unexpected\ Expend_{s,t} = Actual\ Expend_{s,t} - \Delta Expend_{s,t} - Forecast\ Expend_{s,t} \quad (4)$$

where *Actual Expend* is the actual expenditure of state *s* during the fiscal year *t*, $\Delta Expend$ measures the spending cuts enacted after the initial budget during that fiscal year, and *Forecast Expend* is the forecasted expenditure at the beginning of the fiscal year.

Then, we can calculate the difference between the unexpected revenue and unexpected expenditure of state *s* in fiscal year *t* as follows:

[6] State fiscal surveys can be download at: https://www.nasbo.org/mainsite/reports-data/fiscal-survey-of-states/fiscal-survey-archives.

$$Unexpected\ Surplus_{s,t} = Unexpected\ Revenue_{s,t} - Unexpected\ Expend_{s,t} \quad (5)$$

where *Unexpected Surplus* is the unexpected amount of government surplus. We define a state to have experienced a surplus shock if the calculated *Unexpected Surplus* is positive for that state in that year. As a firm can receive subsidies from multiple states, we aggregate the surplus shocks of the firms' subsidy providers each year. A state is defined as a firm's subsidy provider if it provided subsidies to the firm during our sample period. We then define *Provider Surplus Shock* as an indicator variable that equals one if at least one of a firm's subsidy provider states is exposed to a surplus shock in that year, and zero otherwise. In the regression analysis, we use the one-year-lagged *Provider Surplus Shock* to avoid the possibility that subsidy granting may impact a state's fiscal surplus in the same year. Intuitively, if a firm's subsidy provider states have an unexpected surplus in year $t-1$, the firm is more likely to receive subsidies in year t. We confirm this conjecture empirically in Table C10 of the appendix. The results show that firms receive more in subsidies if their subsidy providers experienced a surplus shock in the previous fiscal year. This indicates that subsidy providers' surplus shocks could serve as a valid shock to capture the changes in government subsidies.

As we can only measure the surplus shocks of state governments, we exclude observations with subsidies from federal and local governments. To test the effect of subsidy providers' unexpected surplus shocks on the implied cost of equity, we estimate the following regression:

$$\begin{aligned} COE_{i,t} = {} & \alpha_1 + \beta_1 Provider\ Surplus\ Shock_{\ i,t-1} + \beta_2 Ln\ Asset_{i,t} + \beta_3 Book\ to\ Market_{i.t} \\ & + \beta_4\ Leverage_{i,t} + \beta_5 Beta_{i,t} + \beta_6 Idiosyncratic\ Risk_{i,t} \\ & + \beta_7 Momentum_{i,t} + \beta_8 Analyst\ Forecast\ Bias_{i,t} \\ & + \beta_9 Long\ Term\ Growth_{i,t} + \beta_{10} Fixed\ Effects + \varepsilon_{i,t} \end{aligned} \quad (6)$$

The results are presented in Table 7. In Columns (1) and (2), we control for industry and year fixed effects, and in Columns (3) and (4), we control for industry × year fixed effects. To rule out the possibility that any changes in state-level economic conditions could affect both states' fiscal surplus and firms' cost of equity, we further control for state fixed effects in Columns (5) and (6) and state × year fixed effects in Columns (7) and (8). The coefficients on *Provider Surplus Shock* are significantly negative across all columns in Table 7. This suggests that firms have a lower cost of equity if their subsidy providers experienced

unexpected surplus shocks, consistent with the conclusion that firms receiving more in government subsidies have a lower cost of equity.

[Insert Table 7 here]

We also provide robustness checks of the above test in the appendix. We test the effect of subsidy providers' extreme surplus shocks in Table C11. A surplus shock is defined as an extreme surplus shock if the state government surplus is in the top tercile in that fiscal year. We define *Extreme Provider Surplus Shock* as an indicator variable that equals one if at least one of a firm's subsidy providers experiences an extreme surplus shock, and zero otherwise. We replace the independent variable *Provider Surplus Shock* in Eq. (6) with *Extreme Provider Surplus Shock* and find its coefficients to be significantly negative. Then, we test the effect of subsidy providers' surplus shocks over the previous three years, in Table C12. We define *Three Year Provider Surplus Shock* as an indicator variable that equals one if at least one of the firm's subsidy providers experienced surplus shocks during the prior three years. We regress *COE* on *Three Year Provider Surplus Shock* and find the coefficients on *Three Year Provider Surplus Shock* also to be significantly negative. The results in Table C11 and Table C12 show that the results of our tests of the effect of subsidy providers' surplus shocks are robust to different definitions of subsidy providers' surplus shocks.

3.5.4 Channel Tests

As discussed earlier, government subsidies can decrease firms' cost of equity by affecting their information asymmetry and operating performance. In this subsection, we provide evidence on the two underlying channels. Specifically, we test whether subsidies are related to firms' analyst forecast properties, financial reporting quality, operating performance, and the volatility of their future performance.

3.5.4.1 Effect of Subsidies on Analyst Forecasts

First, we use analyst forecast dispersion and forecast bias as proxies for the firms' information asymmetry (Barron et al. 1998; Thomas 2002; Leuz 2003; Autore and Kovacs 2010; Cui et al. 2018), and then test the effect of subsidies on analyst forecasts by estimating the following regression:

$$Analyst\ Forecast\ Dispersion_{i,t}\ or\ Analyst\ Forecast\ Bias_{i,t}$$
$$= \alpha_1 + \beta_1 Subsidy_{i,t} + \beta_2 Ln\ Asset_{i,t} + \beta_3 Leverage_{i,t}$$
$$+ \beta_4 Ln\ Analyst\ Following_{i,t} + \beta_5 Sale\ STD_{i,t} + \beta_6 Cash\ Flow\ STD_{i,t}$$
$$+ \beta_7 ROA\ STD_{i,t} + \beta_8 Fixed\ Effect_{i,t} + \varepsilon_{i,t} \quad (7)$$

where the dependent variable is analyst forecast dispersion (*Analyst Forecast Dispersion*) or analyst forecast bias (*Analyst Forecast Bias*), respectively. Following Behn et al. (2008), we define *Analyst Forecast Dispersion* as the one-year-ahead analyst forecast standard deviation, scaled by the stock price at the beginning of the fiscal year. *Analyst Forecast Bias* is the one-year-ahead analyst forecast error scaled by the stock price at the beginning of the fiscal year. The forecast error is calculated as the mean consensus earnings forecast minus the actual earnings. Following prior literature (Chen et al. 2016; Behn et al. 2008), we control for a set of firm characteristics, including firm size (*Ln Asset*), leverage (*Leverage*), number of analysts following (*Ln Analyst Following*), past volatility of sales (*Sale STD*), past volatility of cash flow (*Cash Flow STD*) and past volatility of ROA (*ROA STD*).

The results are reported in Table 8. The coefficients on *Subsidy* are negative and significant at the 5% level in Columns (1) and (2), suggesting that subsidies can significantly decrease analyst forecast dispersion and hence decrease firms' information asymmetry. However, we observe that subsidies have an insignificant impact on analyst forecast bias.

[Insert Table 8 here]

3.5.4.2 Effect of Subsidies on Earnings Management

We then examine the association between subsidies and discretionary accruals to identify whether subsidies reduce firms' egregious earnings management. Prior literature has widely documented that earnings management reduces the quality of accounting information, thus increasing information asymmetry (Lobo and Zhou 2001; Jo and Kim 2007; Katmon and Farooque 2017). We estimate accrual-based earnings management following Kothari et al. (2005) and real earnings management following Cohen et al. (2008) as proxies for firms' accounting information quality.[7] Then, we estimated the following regression:

[7] The details on the estimation of accrual-based earnings management and real earnings management can be found in Appendix B2.

$$Abs\ Discretionary\ Accruals_{i,t}\ or\ Real\ Earnings\ Management_{i,t}$$
$$= \alpha_1 + \beta_1 Subsidy_{i,t} + \beta_2 Ln\ Asset_{i,t} + \beta_3 Book\ to\ Market_{i,t}$$
$$+ \beta_4 Leverage_{i,t} + \beta_5 R\&D_{i,t} + \beta_6 ROA\ STD_{i,t} + \beta_7 Cash\ Flow\ STD_{i,t}$$
$$+ \beta_8 Real\ Earnings\ Management_{i,t}\ or\ Abs\ Discretionary\ Accruals_{i,t}$$
$$+ \beta_9 Fixed\ Effects + \varepsilon_{i,t} \quad (8)$$

where the dependent variable *Abs Discretionary Accruals* is the absolute value of discretional accruals, and *Real Earnings Management* is abnormal production costs minus abnormal discretionary expenses and abnormal operating flows (Kim et al. 2012).[8] The higher values of *Abs Discretionary Accruals* and *Real Earnings Management* indicate lower level of accounting information quality. Following prior literature (Ali et al. 2007; Kim et al. 2012; Chen et al. 2016), we control for firm size (*Ln Asset*), book-to-market ratio (*Book to Market*), leverage (*Leverage*), research and development expenditure (*R&D*), [9] performance (*ROA STD*), cash flow volatility (*Cash Flow STD*). As real activities manipulation and accrual-based earnings management can substitute for each other (Zang 2012), we further control for *Real Earnings Management* (*Abs Discretionary Accruals*) when the dependent variable is *Abs Discretionary Accruals* (*Real Earnings Management*). The results are reported in Table 9, and the coefficients on *Subsidy* are insignificant, suggesting government subsidies do not reduce firms' egregious earnings management.

[Insert Table 9 here]

3.5.4.3 Effect of Subsidies on Operating Performance

Next, we test whether subsidies decrease the cost of equity by improving firms' future operating performance. We measure firms' operating performance by industry-adjusted ROA (Core et al. 2006; Chen et al. 2016), and estimate the following regression:

[8] We also use alternative proxies for real earnings management following Cohen and Zarowin (2010). The first alternative proxy is calculated as abnormal production costs minus abnormal discretionary expenses. And the second alternative proxy is calculated using the aggregate value of abnormal cash flows from operation multiple by negative one and abnormal discretionary expenses multiple by negative one. The higher amount of those two measures, the more likely the firm engaged in real earnings management activities. And we still find the coefficients on *Subsidy* are insignificant.

[9] Missing values of for R&D are replaced with zeroes.

$$Adj\ ROA_{i,t+N} = \alpha_1 + \beta_1 Subsidy_{i,t} + \beta_2 Ln\ Asset_{i,t} + \beta_3 Leverage_{i,t} + \beta_4 R\&D_{i,t} + \beta_5 Dividend\ Yield_{i,t} + \beta_6 Return\ STD_{i,t} + \beta_7 Sale\ Growth_{i,t} + \beta_8 Institution\ Ownership_{i,t} + \beta_9 Ln\ Firm\ Age_{i,t} + \beta_{10} Fixed\ Effects + \varepsilon_{i,t+N} \quad (9)$$

where the dependent variable $Adj\ ROA_{i,t+N}$ is the industry-adjusted ROA in the fiscal year $t+N$ (N = 0, 1, and 2) and the independent variable of interest to us is *Subsidy*. Following Chen et al. (2016), we control for firm size (*Ln Asset*), leverage (*Leverage*), research and development expenditure (*R&D*), dividend yield (*Dividend Yield*), and stock return volatility (*Return STD*). In addition, we control for sales growth (*Sale Growth*), institutional ownership (*Institution Ownership*), and firm age (*Ln Firm Age*) as they are likely to affect firms' performance (Chaganti and Damanpour 1991; Brush et al. 2000; Loderer and Waelchli 2010). We test the impact of subsidies on firms' operating performance in the year of receiving subsidies (i.e., year *t*) and the following two years (i.e., years *t*+1 and *t*+2). The results are reported in Table 10. *Subsidy* is significantly positively associated with $Adj\ ROA_t$, $Adj\ ROA_{t+1}$ and $Adj\ ROA_{t+2}$, suggesting that improving firms' operating performance could be one of the channels through which subsidies decrease the cost of equity.

[Insert Table 10 here]

3.5.4.4 Effect of Subsidies on Performance Volatility

As well as improving firms' operating performance, subsidies could also decrease the cost of equity by reducing the volatility of future performance. To test this conjecture, we replace the dependent variable in Eq. (9) with a proxy for the volatility of firms' future performance, i.e., *ROA Volatility*. We define *ROA Volatility* as the standard deviation of a firm's subsequent 20 quarterly industry-adjusted ROAs (Fu et al. 2017). The results are reported in Table 11. The coefficients on *Subsidy* are negative and significant at the 5% level. This suggests that firms have lower performance volatility after receiving subsidies, and thus investors view those firms as less risky and require lower costs of equity from them.

[Insert Table 11 here]

3.5.5 Further Analysis

3.5.5.1 Analysis of Tax-related and Non-tax-related Subsidies

Prior literature shows tax-related and non-tax-related subsidies have different impacts on firms. For example, Raghunandan (2021) finds that firms receiving tax-related subsidies

are more likely to engage in fraud. Pappas et al. (2021) find subsidized firms tend to smooth earnings, and the earnings-smoothing behaviour is more pronounced among firms with non-tax-related subsidies. In this subsection, we test whether tax-related and non-tax-related subsidies have different impacts on firms' implied cost of equity.

We define two variables, *Tax Subsidy*, and *Non-tax Subsidy,* to measure the amounts of tax-related and non-tax-related subsidies, respectively. We re-estimate the baseline regression in Eq. (1), replacing the dependent variable with *Tax Subsidy* and then *Non-tax Subsidy*, and report the results in Columns (1) to (4) of Table 12. The coefficients on both *Tax Subsidy* and *Non-tax Subsidy* are significantly negative at the 1% level, showing that receiving tax-related or non-tax-related subsidies can reduce firms' cost of equity. In Columns (5) and (6), we run horse-race tests by including *Tax Subsidy* and *Non-tax Subsidy* in one regression. Again, we find that both types of subsidies can significantly decrease firms' cost of equity. Economically, taking Column (5) as an example, one-standard-deviation changes in *Tax Subsidy* and *Non-tax Subsidy* decrease the cost of equity by 0.17% (−2.830 × 0.0006) and 0.26% (−5.169 × 0.0005) respectively, representing 1.60% and 2.44% of the sample mean of *COE*.

[Insert Table 12 here]

3.5.5.2 Analysis of Subsidies from Different Sources

Lastly, we examine whether the subsidies granted by the different levels of government have different impacts on firms' cost of equity. We define three variables, *Federal Subsidy, State Subsidy,* and *Local Subsidy,* to measure the amounts of subsidies awarded by federal, state, and local governments. We re-estimate the baseline regression in Eq. (1), replacing the dependent variable with *Federal Subsidy, State Subsidy,* and *Local Subsidy*, respectively, and the results are reported in Table 13. The results in Columns (1) to (6) suggest that only the subsidies granted by the state government significantly impact firms' cost of equity. In Columns (7) and (8), we run horse-race tests by including *Federal Subsidy, State Subsidy,* and *Local Subsidy* in one regression, and we only observe the coefficients on *State Subsidy* to be significantly negative, at the 1% level.

[Insert Table 13 here]

3.6 Conclusion

In this paper, we examine the impact of government subsidies on firms' cost of equity in the U.S. We find firms with more government subsidies have lower costs of equity.

Specifically, a one-standard-deviation change in the government subsidy amount will decrease the cost of equity by about 0.15%, which is about 1.44% of the sample mean of the cost of equity. To address the concern that government subsidies are endogenously determined, we construct a PSM sample and an EBM sample. We find consistent results using the matched samples. To further mitigate endogeneity caused by omitted variables, we examine the robustness of the results using subsidy providers' unexpected surplus shocks. We find the firms' cost of equity to be significantly lower following subsidy providers' surplus shocks.

We then test channels through which government subsidies might affect firms' cost of equity. Our results show firms receiving more in government subsidies have a lower level of analyst forecast dispersion, better future operating performance, and lower performance volatility. The findings suggest government subsidies reduce firms' cost of equity by both decreasing firms' information asymmetry and improving their operating performance. In addition, we find both tax-related and non-tax-related subsidies can significantly reduce firms' cost of equity and that subsidies from state governments have the most pronounced effect in decreasing firms' cost of equity.

Our study contributes to the growing literature studying the economic consequences of firm-specific subsidies (Almus and Czarnitzki 2003; Lee et al. 2014; Howell 2017; Lee et al. 2017; Lim et al. 2018; Aobdia et al. 2019; Criscuolo et al. 2019; De Simone et al. 2019; Rotemberg 2019; Blank et al. 2021; Raghunandan 2021; Pappas et al. 2021; Huang 2022), and the literature examining the determinants of the cost of equity (Attig et al. 2008; Hail and Leuz 2006; Hail and Leuz 2009; Chen et al. 2011; Dhaliwal et al. 2011; El Ghoul et al. 2011; Chen et al. 2016; Dhaliwal et al. 2016). In addition, this study speaks to the importance of the GASB's and FASB's calls for the disclosure of government subsidies by showing that a decreased cost of equity could be one of the important results of such disclosure.

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Table 1
Summary Statistics of Key Variables

This table reports the summary statistics for variables used in the main analysis. All variables are defined in Appendix A. The values of all continuous variables are winsorized at their 1st and 99th percentiles. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

Panel A: Estimates of Implied Cost of Equity (N = 13,541)

	Mean	Std. dev.	Min	P50	Max
COE	0.1061	0.0558	0.0356	0.0966	0.4563
COE GLS	0.0798	0.0392	0.0145	0.0761	0.2908
COE CT	0.0947	0.0644	0.0196	0.0842	0.5047
COE MPEG	0.1287	0.0714	0.0358	0.1144	0.5459
COE OJ	0.1199	0.0557	0.0426	0.1106	0.4513

Panel B: Control Variables (N = 13,541)

	Mean	Std. dev.	Min	P50	Max
Total Asset (million $)	4679.8420	11289.5300	39.5800	983.2000	77888.0000
Ln Asset	7.0341	1.6340	3.7030	6.8920	11.2600
Book to Market	0.4679	0.2944	0.0503	0.4031	1.6052
Leverage	0.4656	0.2027	0.0774	0.4699	0.9253
Beta	1.2591	0.7354	−0.0666	1.1497	3.7603
Idiosyncratic Risk	0.1141	0.0521	0.0392	0.1036	0.3088
Momentum	0.1634	0.4547	−0.7341	0.1174	1.8852
Analyst Forecast Bias	0.0051	0.0305	−0.0626	−0.0001	0.2097
Long -term Growth	0.1576	0.0729	0.0231	0.1500	0.4630

Panel C: Correlation Matrix for the Estimates of Implied Cost of Equity

	COE	*COE GLS*	*COE CT*	*COE MPEG*	*COE OJ*
COE	1.0000				
COE GLS	0.8538***	1.0000			
COE CT	0.9413***	0.8140***	1.0000		
COE MPEG	0.9393***	0.7204***	0.8011***	1.0000	
COE OJ	0.9677***	0.7525***	0.8798***	0.9509***	1.0000

Panel D: Univariate Test

	Subsidized N=2,698	Non-subsidized N=10,843	Mean Diff	Std Diff
	Mean	Mean		
COE	0.1016	0.1072	−0.0056***	−0.1046
COE GLS	0.0744	0.0811	−0.0068***	−0.1751
COE CT	0.0915	0.0955	−0.0040***	−0.0648
COE MPEG	0.1227	0.1302	−0.0075***	−0.1088
COE OJ	0.1170	0.1206	−0.0036***	−0.0665
Ln Asset	8.7104	6.6170	2.0934***	1.50848
Book to Market	0.4227	0.4792	−0.0565***	−0.1996
Leverage	0.5467	0.4455	0.1012***	0.5342
Beta	1.1340	1.2902	−0.1562***	−0.2241
Idiosyncratic Risk	0.0833	0.1217	−0.0384***	−0.8497
Momentum	0.1631	0.1635	−0.0004	−0.0009
Analyst Forecast Bias	0.0011	0.0061	−0.0050***	−0.1880
Long Term Growth	0.1321	0.1639	−0.0318***	−0.4700

Table 2
Summary Statistics of Government Subsidies

The table below reports the summary statistics for government subsidies. All variables are defined in Appendix A. The values of all continuous variables are winsorized at their 1st and 99th percentiles.

Panel A: Subsidies in Full Sample						
	N	Mean	Std. dev.	Min	P50	Max
Subsidy (million $)	13,541	0.8324	3.8166	0.0000	0.0000	29.6995
Subsidy	13,541	0.0001	0.0006	0.0000	0.0000	0.0049
Subsidy Dummy	13,541	0.1992	0.3994	0.0000	0.0000	1.0000
Panel B: Subsidies in Subsidized Sample						
	N	Mean	Std. dev.	Min	P50	Max
Subsidy (million $)	2,698	4.1779	7.6907	0.0001	0.6925	29.6995
Subsidy	2,698	0.0006	0.0012	0.0000	0.0001	0.0049
Panel C: Tax-related and Non-tax-related Subsidies						
	N	Mean	Std. dev.	Min	P50	Max
Tax Subsidy (million $)	2,109	2.8493	5.2654	0.0001	0.4620	18.9383
Tax Subsidy	2,109	0.0003	0.0006	0.0000	0.0001	0.0023
Non-Subsidy (million $)	1,610	2.1544	3.3270	0.0001	0.4970	10.7686
Non-tax Subsidy	1,610	0.0003	0.0005	0.0000	0.0001	0.0015
Panel D: Subsidies from Different Sources						
	N	Mean	Std. dev.	Min	P50	Max
Federal Subsidy (million $)	588	3.4643	3.1196	0.0020	2.1490	8.0408
Federal Subsidy	588	0.0004	0.0004	0.0000	0.0002	0.0009
State Subsidy (million $)	2,277	2.1977	3.8455	0.0001	0.4310	14.1561
State Subsidy	2,277	0.0003	0.0006	0.0000	0.0001	0.0021
Local Subsidy (million $)	758	0.7505	0.9189	0.0002	0.2361	2.4388
Local Subsidy	758	0.0001	0.0001	0.0000	0.0000	0.0002

Table 3
The Effect of Subsidies on Implied Cost of Equity

The table below reports the results of the effect of government subsidies on implied cost of equity. The dependent variable *COE* is the average of the four estimates of the implied cost of equity. The independent variable *Subsidy* is the amount of government subsidies scaled by total assets. *Subsidy Dummy* is an indicator variable that equals one if the firm receives government subsidies in that year, and zero otherwise. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

		Dependent = COE_t					
	Pred. Sign	(1)	(2)	(3)	(4)	(5)	(6)
$Subsidy_t$	±	−2.546***	−2.653***			−3.056***	−3.138***
		(−4.23)	(−4.27)			(−4.36)	(−4.40)
$Subsidy\ Dummy_t$	±			0.000	−0.000	0.002	0.002
				(0.07)	(−0.01)	(0.98)	(0.91)
$Ln\ Asset_t$	−	0.001	0.001	0.001	0.001	0.001	0.001
		(1.64)	(1.55)	(1.37)	(1.32)	(1.18)	(1.12)
$Book\ to\ Market_t$	+	0.039***	0.040***	0.039***	0.040***	0.039***	0.040***
		(14.72)	(14.78)	(14.42)	(14.49)	(14.45)	(14.52)
$Leverage_t$	+	0.042***	0.043***	0.042***	0.043***	0.042***	0.043***
		(9.89)	(9.88)	(9.90)	(9.88)	(9.91)	(9.90)
$Beta_t$	+	0.005***	0.005***	0.005***	0.006***	0.005***	0.006***
		(4.24)	(4.20)	(4.26)	(4.22)	(4.26)	(4.23)
$Idiosyncratic\ Risk_t$	+	0.152***	0.151***	0.151***	0.150***	0.152***	0.151***
		(7.54)	(7.20)	(7.49)	(7.15)	(7.53)	(7.20)
$Momentum_t$	−	−0.017***	−0.017***	−0.017***	−0.017***	−0.017***	−0.017***
		(−11.37)	(−11.11)	(−11.39)	(−11.12)	(−11.38)	(−11.11)
$Analyst\ Forecast\ Bias_t$	+	0.681***	0.684***	0.680***	0.684***	0.680***	0.684***
		(13.17)	(13.08)	(13.15)	(13.05)	(13.17)	(13.08)
$Long\ Term\ Growth_t$	+	0.064***	0.062***	0.064***	0.062***	0.064***	0.062***
		(5.83)	(5.59)	(5.82)	(5.59)	(5.82)	(5.59)
Industry FE		Yes	No	Yes	No	Yes	No
Year FE		Yes	No	Yes	No	Yes	No
Industry × Year FE		No	Yes	No	Yes	No	Yes
Constant		0.027***	0.026***	0.027***	0.027***	0.028***	0.027***
		(4.24)	(4.01)	(4.47)	(4.22)	(4.64)	(4.40)
N		13,541	13,524	13,541	13,524	13,541	13,524
Adj R^2		0.346	0.346	0.345	0.345	0.346	0.346

Notes.
There are 17 singleton observations dropped when we control for industry × year fixed effects.

Table 4
Robustness: Using Implied Cost of Equity Estimated from Different Methods

The table below reports the results of using implied cost of equity estimated from different methods. The dependent variables *COE GLS*, *COE CT*, *COE MPEG*, and *COE OJ* are implied cost of equity estimated by adopting different methods following Gebhardt et al. (2001), Claus and Thomas (2001), Easton (2004) and Ohlson and Juettner-Nauroth (2005) respectively. The control variables are the same as Table 3, including *Ln Asset*, *Book to Market*, *Leverage*, *Beta*, *Idiosyncratic Risk*, *Momentum*, *Analyst Forecast Bias* and *Long Term Growth*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = $COE\ GLS_t$		Dependent = $COE\ CT_t$		Dependent = $COE\ MPEG_t$		Dependent = $COE\ OJ_t$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Subsidy_t$	−1.786***	−1.737***	−2.643***	−2.800***	−2.844***	−3.001***	−2.574***	−2.685***
	(−4.22)	(−4.08)	(−3.91)	(−4.10)	(−2.99)	(−3.09)	(−3.51)	(−3.54)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	No	Yes	No	Yes	No	Yes	No
Year FE	Yes	No	Yes	No	Yes	No	Yes	No
Industry × Year FE	No	Yes	No	Yes	No	Yes	No	Yes
N	13,541	13,524	13,541	13,524	13,541	13,524	13,541	13,524
Adj R^2	0.416	0.423	0.287	0.287	0.329	0.334	0.305	0.305

Table 5
Robustness: Using Fama-MacBeth Regression

The table below reports the results of Fama-MacBeth regression. The control variables are the same as Table 3, including *Ln Asset*, *Book to Market*, *Leverage*, *Beta*, *Idiosyncratic Risk*, *Momentum*, *Analyst Forecast Bias* and *Long Term Growth*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *t*-statistics are in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = COE_t	
	(1)	(2)
$Subsidy_t$	−4.651***	−2.664***
	(−6.66)	(−5.44)
Controls	No	Yes
Industry FE	Yes	Yes
N	13,541	13,541
R^2	0.080	0.386

Table 6

Dealing with Endogeneity: Propensity Score Matched and Entropy Balanced Sample

The table below reports the results of using propensity-score matched and entropy balanced sample. The control variables are the same as Table 3, including *Ln Asset*, *Book to Market*, *Leverage*, *Beta*, *Idiosyncratic Risk*, *Momentum*, *Analyst Forecast Bias* and *Long Term Growth*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

Panel A: PSM Sample				
	Dependent = COE_t			
	(1)	(2)	(3)	(4)
$Subsidy_t$	−3.620***	−2.496***	−3.556***	−2.457***
	(−3.33)	(−3.19)	(−3.16)	(−3.08)
Controls	No	Yes	No	Yes
Industry FE	Yes	Yes	No	No
Year FE	Yes	Yes	No	No
Industry × Year FE	No	No	Yes	Yes
N	1,738	1,738	1,738	1,738
Adj R^2	0.033	0.337	0.031	0.337
Panel B: Entropy Balanced Sample				
	Dependent = COE_t			
	(1)	(2)	(3)	(4)
$Subsidy_t$	−2.508***	−1.622***	−2.845***	−1.837***
	(−2.69)	(−2.62)	(−2.91)	(−2.88)
Controls	No	Yes	No	Yes
Industry FE	Yes	Yes	No	No
Year FE	Yes	Yes	No	No
Industry × Year FE	No	No	Yes	Yes
N	13,541	13,541	13,524	13,524
Adj R^2	0.105	0.324	0.135	0.358

Table 7

Dealing with Endogeneity: The Effect of Subsidies Providers' Surplus Shocks on Implied Cost of Equity

The table below reports the results of the effect of subsidies providers' surplus shocks on implied cost of equity. The independent variable *Provider Surplus Shock* is an indicator variable that equals one if there is at least one of a firm's subsidies providers experience surplus shock in that year, and zero otherwise. And we use lagged one-year *Provider Surplus Shock* in all columns. The control variables are the same as Table 3, including *Ln Asset*, *Book to Market*, *Leverage*, *Beta*, *Idiosyncratic Risk*, *Momentum*, *Analyst Forecast Bias* and *Long Term Growth*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent Variable= COE_t							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Provider Surplus Shock $_{t-1}$	−0.016***	−0.006***	−0.017***	−0.007***	−0.016***	−0.006***	−0.017***	−0.006***
	(−7.96)	(−3.25)	(−8.00)	(−3.23)	(−8.01)	(−3.21)	(−7.96)	(−3.26)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Industry FE	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	Yes	Yes	No	No
Industry × Year FE	No	No	Yes	Yes	No	No	No	No
State FE	No	No	No	No	Yes	Yes	No	No
State × Year FE	No	No	No	No	No	No	Yes	Yes
N	12,083	12,083	12,050	12,050	12,081	12,081	12,036	12,036
Adj R^2	0.068	0.351	0.066	0.352	0.078	0.355	0.065	0.345

Table 8
The Effect of Subsidies on Analysts Forecasting

The table below reports the results of the effect of government subsidies on analysts forecast properties. In Columns (1) and (2) the dependent variable is *Analyst Forecast Dispersion*. In Columns (3) and (4) the dependent variable is *Analyst Forecast Bias*. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Analyst Forecast Dispersion* $_t$		Dependent = *Analyst Forecast Bias* $_t$	
	(1)	(2)	(3)	(4)
Subsidy $_t$	−0.171**	−0.171**	0.189	0.260
	(−2.35)	(−2.23)	(0.41)	(0.59)
Ln Asset $_t$	−0.000	−0.000	−0.001***	−0.001***
	(−0.15)	(−0.31)	(−2.60)	(−2.77)
Leverage $_t$	0.005***	0.005***	0.009***	0.009***
	(8.74)	(8.74)	(3.85)	(3.85)
Ln Analyst Following $_t$	−0.002***	−0.002***	−0.005***	−0.005***
	(−9.44)	(−9.42)	(−6.71)	(−6.61)
Sale STD $_t$	−0.001	−0.001	−0.007**	−0.007**
	(−1.01)	(−1.12)	(−2.38)	(−2.33)
Cash Flow STD $_t$	0.010***	0.011***	−0.050***	−0.049***
	(2.72)	(2.77)	(−3.56)	(−3.43)
ROA STD $_t$	0.022***	0.021***	0.060***	0.058***
	(10.24)	(9.86)	(7.71)	(7.47)
Industry FE	Yes	No	Yes	No
Year FE	Yes	No	Yes	No
Industry × Year FE	No	Yes	No	Yes
Constant	0.004***	0.004***	0.018***	0.019***
	(6.87)	(6.98)	(8.61)	(8.65)
N	12,612	12,596	13,054	13,037
Adj R^2	0.169	0.164	0.055	0.058

Table 9
The Effect of Subsidies on Firms' Earnings Management

The table below reports the results of the effect of government subsidies on firms' earnings management. In Columns (1) and (2) the dependent variable is *Abs Discretionary Accruals*. In Columns (3) and (4), the dependent variable is *Real Earnings Management*. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent =*Abs Discretionary Accruals* $_t$		Dependent =*Real Earnings Management* $_t$	
	(1)	(2)	(3)	(4)
Subsidy $_t$	0.596	0.692	6.216	5.123
	(0.72)	(0.80)	(0.87)	(0.69)
Ln Asset $_t$	−0.002***	−0.002***	−0.003	−0.004
	(−4.07)	(−3.92)	(−0.59)	(−0.71)
Book to Market $_t$	0.011***	0.009***	0.377***	0.381***
	(4.77)	(3.89)	(15.41)	(14.91)
Leverage $_t$	0.015***	0.013***	0.411***	0.413***
	(4.23)	(3.68)	(10.09)	(9.88)
R&D $_t$	0.057***	0.073***	−3.825***	−3.858***
	(2.66)	(3.41)	(−22.89)	(−22.51)
ROA STD $_t$	0.209***	0.217***	−0.048	−0.127
	(12.14)	(12.48)	(−0.42)	(−1.07)
Cash Flow STD $_t$	0.294***	0.300***	−0.094	−0.216
	(9.84)	(9.90)	(−0.40)	(−0.91)
Real Earning Management $_t$	−0.009***	−0.004**		
	(−4.37)	(−2.21)		
Abs Discretionary Accruals $_t$			−0.341***	−0.175**
			(−4.41)	(−2.20)
Industry FE	Yes	No	Yes	No
Year FE	Yes	No	Yes	No
Industry × Year FE	No	Yes	No	Yes
Constant	0.029***	0.029***	−0.345***	−0.340***
	(7.18)	(7.22)	(−7.19)	(−6.91)
N	12698	12687	12698	12687
Adj R^2	0.201	0.262	0.351	0.357

Table 10
The Effect of Subsidies on Firm Performance

The table below reports the results of the effect of government subsidies on firms' operating performance. The dependent variable *Adj ROA* is the industry median adjusted ROA. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Adj ROA*					
	Adj ROA $_t$		*Adj ROA* $_{t+1}$		*Adj ROA* $_{t+2}$	
	(1)	(2)	(3)	(4)	(5)	(6)
Subsidy $_t$	6.183***	6.305***	5.008***	4.888***	5.046***	5.132***
	(4.02)	(4.11)	(3.11)	(3.02)	(2.88)	(2.98)
Ln Asset $_t$	−0.003**	−0.003***	0.000	0.000	0.001	0.001
	(−2.34)	(−2.71)	(0.36)	(0.16)	(0.80)	(0.71)
Leverage $_t$	−0.065***	−0.062***	−0.054***	−0.054***	−0.048***	−0.049***
	(−9.04)	(−8.39)	(−6.81)	(−6.58)	(−5.71)	(−5.72)
R&D $_t$	−0.058*	−0.060*	−0.079**	−0.078**	−0.075**	−0.073*
	(−1.75)	(−1.79)	(−2.21)	(−2.17)	(−1.98)	(−1.90)
Dividend Yield $_t$	0.256***	0.223**	0.240**	0.242**	0.224**	0.219**
	(2.88)	(2.46)	(2.44)	(2.39)	(2.23)	(2.13)
Return STD $_t$	−0.246***	−0.284***	−0.276***	−0.304***	−0.299***	−0.323***
	(−8.89)	(−9.55)	(−9.12)	(−9.36)	(−9.53)	(−9.59)
Sale Growth $_t$	0.112***	0.118***	0.074***	0.078***	0.042***	0.045***
	(17.91)	(17.95)	(11.25)	(11.33)	(6.46)	(6.49)
Institution Ownership $_t$	0.009**	0.009**	0.016***	0.016***	0.018***	0.019***
	(2.39)	(2.40)	(3.68)	(3.61)	(4.03)	(3.96)
Ln Firm Age $_t$	0.008***	0.008***	0.007***	0.006***	0.007***	0.006***
	(4.70)	(4.62)	(3.27)	(3.06)	(3.19)	(2.94)
Industry FE	Yes	No	Yes	No	Yes	No
Year FE	Yes	No	Yes	No	Yes	No
Industry × Year FE	No	Yes	No	Yes	No	Yes
Constant	0.073***	0.080***	0.048***	0.054***	0.043***	0.048***
	(7.33)	(7.75)	(4.37)	(4.71)	(3.82)	(4.11)
N	13,501	13,484	12,802	12,785	12063	12044
Adj R^2	0.433	0.447	0.385	0.396	0.393	0.401

Table 11
The Effect of Subsidies on Firm Performance Volatility

The table below reports the results of the effect of government subsidies on firms' performance volatility. The dependent variable *ROA Volatility* is the standard deviation of firms' future *ROA*. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent Variable=*ROA Volatility* $_t$	
	(1)	(2)
Subsidy $_t$	−0.905**	−0.793**
	(−2.41)	(−2.06)
Ln Asset $_t$	−0.001***	−0.001***
	(−3.73)	(−3.41)
Leverage $_t$	−0.000	−0.000
	(−0.00)	(−0.01)
R&D $_t$	0.046***	0.046***
	(4.79)	(4.72)
Dividend Yield $_t$	0.015	0.017
	(0.65)	(0.76)
Return STD $_t$	0.091***	0.099***
	(11.63)	(11.73)
Sale Growth $_t$	−0.004**	−0.004***
	(−2.47)	(−2.67)
Institution Ownership $_t$	−0.008***	−0.008***
	(−7.39)	(−6.99)
Ln Firm Age $_t$	−0.002***	−0.002***
	(−3.36)	(−3.32)
Industry FE	Yes	No
Year FE	Yes	No
Industry × Year FE	No	Yes
Constant	0.026***	0.024***
	(9.41)	(8.54)
N	12,415	12,397
Adj R^2	0.189	0.196

Table 12
Analysis of Tax-related and Non-tax-related Subsidies

The table below reports results of the analysis of tax-related and non-tax-related subsidies. The dependent variable *COE* is the mean of the four estimates of the implied cost of equity. The control variables are the same as Table 3, including *Ln Asset*, *Book to Market*, *Leverage*, *Beta*, *Idiosyncratic Risk*, *Momentum*, *Analyst Forecast Bias* and *Long Term Growth*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = COE_t					
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Subsidy $_t$	−3.472***	−3.822***			−2.830**	−3.192**
	(−2.86)	(−3.03)			(−2.29)	(−2.50)
Non-tax Subsidy $_t$			−5.906***	−5.878***	−5.169**	−5.062**
			(−2.81)	(−2.78)	(−2.41)	(−2.36)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	No	Yes	No	Yes	No
Year FE	Yes	No	Yes	No	Yes	No
Industry × Year FE	No	Yes	No	Yes	No	Yes
N	13,541	13,524	13,541	13,524	13,541	13,524
Adj R^2	0.346	0.345	0.346	0.345	0.346	0.345

Table 13
Analysis of Subsidies from Different Sources

The table below reports the results of the analysis of government subsidies from different sources. The dependent variable *COE* is the mean of the four estimates of the implied cost of equity. The control variables are the same as Table 3, including *Ln Asset*, *Book to Market*, *Leverage*, *Beta*, *Idiosyncratic Risk*, *Momentum*, *Analyst Forecast Bias* and *Long Term Growth*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = COE_t							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Federal Subsidy $_t$	−6.038	−6.759*					−5.605	−6.299
	(−1.54)	(−1.69)					(−1.44)	(−1.59)
State Subsidy $_t$			−4.444***	−4.561***			−4.508***	−4.586***
			(−3.12)	(−3.11)			(−3.11)	(−3.08)
Local Subsidy $_t$					21.671	19.278	27.119	24.914
					(0.98)	(0.84)	(1.22)	(1.07)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	No	Yes	No	Yes	No	Yes	No
Year FE	Yes	No	Yes	No	Yes	No	Yes	No
Industry × Year FE	No	Yes	No	Yes	No	Yes	No	Yes
N	13,541	13,524	13,541	13,524	13,541	13,524	13,541	13,524
Adj R^2	0.346	0.345	0.346	0.345	0.345	0.345	0.346	0.345

Appendix A: Variable Definitions

Variable Definitions

Variable	Definition
Abs Discretionary Accruals	Performance-adjusted discretional accruals, estimated following Kothari et al. (2005). We estimate discretional accruals within each year and industry as defined by the Fama-French 48-industry classification.
Adj ROA	Firms' ROA minus the industry-median ROA.
Analyst Forecast Bias	One-year-ahead analyst forecast error, scaled by the stock price at the beginning of the fiscal year, where forecast error is defined as the mean consensus earnings forecast minus the actual earnings.
Analyst Forecast Dispersion	One-year-ahead analyst forecast standard deviation scaled by the stock price at the beginning of the fiscal year.
Momentum	Compounded stock return during the previous 12 months.
Beta	Market beta, estimated by the market model using the previous 60 monthly returns, where we require a minimum of 24 months of observations.
Cash Flow STD	Standard deviation of a firm's previous five years of cash flow from operations, scaled by total assets.
Dividend Yield	Total dividends scaled by the stock price at the end of the fiscal year.
Federal Subsidy	Amount of federal subsidies scaled by total assets.
Federal Subsidy (million $)	Amount of federal subsidies in millions of dollars.
COE CT	Implied cost of equity estimated following Claus and Thomas (2001).
COE GLS	Implied cost of equity estimated following Gebhardt et al. (2001).
COE	Equally weighted average of the four estimates of the cost of equity: *COE GLS*, *COE CT*, *COE MPEG,* and *COE OJ*.
COE MPEG	Implied cost of equity estimated by the modified PEG model, following Easton (2004).
COE OJ	Implied cost of equity estimated following Ohlson and Juettner-Nauroth (2005).
Idiosyncratic Risk	Standard deviation of residuals, estimated by the market model using the previous 60 monthly returns, where we require a minimum of 24 months of observations.
Institution Ownership	The number of institutional shares divided by the total number of outstanding shares.
Leverage	The ratio of the firm's total debt to total assets.
Ln Analyst Following	Natural logarithm of the number of analysts following.
Ln Asset	Natural logarithm of total assets in millions of dollars measured at the beginning of the fiscal year.
Ln Employee	Natural logarithm of the number of employees.
Ln Firm Age	Natural logarithm of the number of years a firm has existed in Compustat.
Local Subsidy	Amount of local subsidies scaled by total assets.

Local Subsidy (million $)	Amount of local subsidies in millions of dollars.
Long Term Growth	The median analyst forecast of the long-term earnings growth rate.
Book to Market	The firm's book value divided by its market capitalization.
Non-tax Subsidy	Amount of non-tax-related subsidies scaled by total assets.
Non-tax Subsidy (million $)	Amount of non-tax-related subsidies in millions of dollars.
Political Contribution	Natural logarithm of political contribution amount over previous four years.
Provider Surplus Shock	An indicator variable that equals one if at least one of a firm's subsidy providers is exposed to a surplus shock in that year, and zero otherwise.
R&D	R&D expenditure scaled by total assets, with missing values set to zero.
Real Earnings Management	Real earnings management following Cohen et al. (2008). Abnormal production costs minus abnormal discretionary expenses and abnormal operating cash flows. Abnormal production costs, abnormal discretionary expenses, and abnormal operating cash flows are estimated within each year and industry as defined by the Fama-French 48-industry classification.
Return STD	The standard deviation of a firm's previous 60 monthly returns.
ROA	Firm earnings before extraordinary and discontinued items, scaled by total assets.
ROA STD	The standard deviation of a firm's previous five years of ROA.
ROA Volatility	The standard deviation of a firm's following 20 quarterly ROAs.
Sale Growth	A firm's growth of sales.
Sale STD	The standard deviation of a firm's previous five years of sales, scaled by total assets.
State Subsidy	Amount of state subsidies, scaled by total assets.
State Subsidy (million $)	Amount of state subsidies in millions of dollars.
Subsidy	The amount of government subsidies received by a firm during the fiscal year, scaled by total assets measured at the beginning of the fiscal year.
Subsidy (million $)	Amount of subsidies in millions of dollars.
Subsidy Dummy	An indicator variable that equals one if a firm received subsidies in that year, and zero otherwise.
Tax Subsidy	Amount of tax-related subsidies scaled by total assets.
Tax Subsidy (million $)	Amount of tax-related subsidies in millions of dollars.
Total Asset (million $)	Amount of total assets in millions of dollars.

Appendix B: Estimation of Variables

Appendix B1: Estimation of Implied Cost of Equity

The descriptions of models are largely taken from existing literature, such as (Hail and Leuz 2006; Hail and Leuz 2009; Dhaliwal et al. 2006; Boubakri et al. 2012)

$r_{Subscript}$ = Cost of equity estimated from the model identified in subscript.
P_t = Firms stock price at *t*
B_t = Book value per share at *t*
$FEPS_{t+i}$ = I/B/E/S consensus earnings forecast for the *i*th year from the estimation year
D_{t+i} = Net dividends per share for the *i*th year from the estimation year, $FEPS_{t+i}$ × Dividend pay-out ratio

Model 1: r_{CT} following Claus and Thomas (2001)

$$P_t = B_t + \frac{FEPS_{t+1} - r_{CT}B_t}{(1+r_{CT})} + \cdots + \frac{FEPS_{t+5} - r_{CT}B_{t+4}}{(1+r_{CT})^5} + \frac{(FEPS_{t+5} - r_{CT}B_{t+4})(1+g)}{(r_{CT}-g)(1+r_{CT})^5}$$

Claus and Thomas (2001) use residual income valuation model, and assume the residual income grows at a constant rate, i.e., *g*, beyond 5 years. We use 10-year annualized U.S. Treasury bill yield minus 3% as the long-term growth rate beyond five years.

Model 2: r_{GLS} following Gebhardt et al. (2001)

$$P_t = B_t + \sum_{i=1}^{T} \frac{FEPS_{t+i} - r_{GLS}B_{t+i-1}}{(1+r_{GLS})^i} + \frac{FEPS_{t+T+1} - r_{GLS}B_{t+T}}{r_{GLS}(1+r_{GLS})^T}$$

Gebhardt et al. (2001) also use residual income valuation model. After three years, *FEPS* are forecasted to be linearly converged to industry median *ROE* in the 12th year. From $T = 12$ on, the growth in earnings is assume to be zero.

Model 3: r_{OJ} following Ohlson and Juettner-Nauroth (2005)

$$r_{OJ} = A + \sqrt{A^2 + \frac{FEPS_{t+1}}{P_t}\left(g_2 - \left(r_{rf} - 3\%\right)\right)}$$

where

$$A = \frac{1}{2}\left(\left(r_{rf} - 3\%\right) + \frac{D_{t+1}}{P_t}\right)$$

$$g_2 = \frac{FEPS_{t+2} - FEPS_{t+1}}{FEPS_{t+1}}$$

Ohlson and Juettner-Nauroth (2005) use abnormal earnings growth valuation model. The long-term growth rate is 10-year annualized U.S. Treasury bill yield (r_{rf}) minus 3%. And this model requires $FEPS_{t+1} > 0$ and $FEPS_{t+2} > 0$

Model 4: r_{MEPG} following Easton (2004)

$$P_T = \left(\frac{FEPS_{t+2} + r_{MEPG}D_{t+1} - FEPS_{t+1}}{{r_{MEPG}}^2}\right)$$

Easton (2004) use abnormal earnings growth valuation model. And it requires $FEPS_{t+2} \geq FEPS_{t+1} > 0$

Appendix B2: Estimation of Earnings Management

Estimation of discretional accruals following Kothari et al (2005)

Abs Discretionary Accruals is the absolute value of discretional accruals following Kothari et al (2005). To obtain the discretional accruals, we estimated the following regression for firm within each year and industry defined by Fama-French 48 industry classification:

$$TA_{i,t} = \beta_1 \left(\frac{1}{Assets_{i,t-1}} \right) + \beta_2 (\Delta Sales_{i,t} - \Delta AR_{i,t}) + \beta_3 PPE_{i,t} + \beta_4 ROA_{i,t} + \varepsilon_{i,t}$$

where $TA_{i,t}$ is total accruals calculated as the difference between income before extraordinary items and operating cash flows adjusted for extraordinary items and discontinued operations, scaled by lagged total assets ($Assets_{i,t-1}$). The variable $\Delta Sales_{i,t}$, $\Delta AR_{i,t}$ and $PPE_{i,t}$ are changes in sales, changes in account receivables, and net property, plant and equipment. $\Delta Sales_{i,t}$, $\Delta AR_{i,t}$ and $PPE_{i,t}$ are all scaled by $Assets_{i,t-1}$. The discretional accruals is the difference between total accruals and the fitted normal accruals.

Estimation of real earning management following Cohen (2008)

Following Kim et al (2012), *Real Earnings Management* is defined as abnormal production costs minus abnormal discretionary expenses and abnormal operating cash flows. Following Cohen (2008), to obtain abnormal production costs, abnormal discretionary expenses and abnormal operating cash flows, we estimated the following three regressions for firms within each year and industry defined by Fama-French 48 industry classification:

$$Prod_{i,t} = \beta_1 \left(\frac{1}{Assets_{i,t-1}} \right) + \beta_2 Sales_{i,t} + \beta_3 \Delta Sales_{i,t} + \beta_4 \Delta Sales_{i,t-1} + \varepsilon_{i,t}$$

$$Disc\ Exp_{i,t} = \beta_1 \left(\frac{1}{Assets_{i,t-1}} \right) + \beta_2 Sales_{i,t-1} + \varepsilon_{i,t}$$

$$CFO_{i,t} = \beta_1 \left(\frac{1}{Assets_{i,t-1}} \right) + \beta_2 Sales_{i,t} + \beta_3 \Delta Sales_{i,t} + \varepsilon_{i,t}$$

where $Prod_{i,t}$ is the production cost calculated as the sum of cost of goods and the change in inventories scaled by $Assets_{i,t-1}$. $Disc\ Exp_{i,t}$ is the discretionary expenditure calculated as the sum of advertising expenses, R&D expenses, and selling, general and administrative expenses scaled by $Assets_{i,t-1}$. $CFO_{i,t}$ is the cash flow from operations scaled by $Assets_{i,t-1}$. $Sales_{i,t}$, $Sales_{i,t-1}$ and $\Delta Sales_{i,t}$ are all scaled by $Assets_{i,t-1}$. The abnormal production costs, abnormal discretionary expenses, and abnormal operating flows are the difference between actual $Prod_{i,t}$, $Disc\ Exp_{i,t}$ and $CFO_{i,t}$ minus the corresponding fitted values.

Appendix C: Additional Results

Table C1
Robustness: Standard Errors Clustered at Different Levels

The table below reports the results of using standard errors clustered at different levels. The control variables are the same as Table 3, including *Ln Asset*, *Book to Market*, *Leverage*, *Beta*, *Idiosyncratic Risk*, *Momentum*, *Analyst Forecast Bias* and *Long Term Growth*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at different levels. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = COE_t			
	(1)	(2)	(3)	(4)
$Subsidy_t$	−2.546***	−2.546***	−2.653***	−2.653***
	(−5.78)	(−4.49)	(−6.00)	(−4.55)
Controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	No	No
Year FE	Yes	Yes	No	No
Industry × Year FE	No	No	Yes	Yes
N	13,541	13,541	13,524	13,524
Adj R^2	0.346	0.346	0.346	0.346
Level of Cluster	Industry	Industry-State	Industry	Industry-State

Table C2
Robustness: Different Fixed Effects

The table below reports the results of controlling for different fixed effects. The control variables are the same as Table 3, including *Ln Asset*, *Book to Market*, *Leverage*, *Beta*, *Idiosyncratic Risk*, *Momentum*, *Analyst Forecast Bias* and *Long Term Growth*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = COE_t		
	(1)	(2)	(3)
$Subsidy_t$	−2.441***	−2.547***	−2.626***
	(−4.14)	(−4.21)	(−4.25)
Controls	Yes	Yes	Yes
Industry FE	Yes	No	Yes
Year FE	Yes	No	No
Industry × Year FE	No	Yes	No
State FE	Yes	Yes	No
State × Year FE	No	No	Yes
N	13,539	13,522	13,500
Adj R^2	0.351	0.350	0.341

Table C3

Robustness: Fama-MacBeth Regression Using Implied Cost of Equity Estimated from Different Methods

The table below reports the results of Fama-MacBeth regression and using implied cost of equity estimated from different methods. The dependent variables are implied cost of equity *COE GLS*, *COE CT*, *COE MPEG*, and *COE OJ* that are implied cost of equity estimated by adopting different methods following Gebhardt et al. (2001), Claus and Thomas (2001), Easton (2004) and Ohlson and Juettner-Nauroth (2005) respectively. The control variables are the same as Table 3, including *Ln Asset*, *Book to Market*, *Leverage*, *Beta*, *Idiosyncratic Risk*, *Momentum*, *Analyst Forecast Bias* and *Long Term Growth*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *t*-statistics are in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = $COE\ GLS_t$		Dependent = $COE\ CT_t$		Dependent = $COE\ MPEG_t$		Dependent = $COE\ OJ_t$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Subsidy_t$	−3.941***	−1.791***	−4.307***	−2.883***	−5.821***	−2.958***	−4.180***	−2.602***
	(−6.38)	(−4.36)	(−4.72)	(−3.98)	(−6.77)	(−4.96)	(−6.11)	(−4.94)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	13,541	13,541	13,541	13,541	13,541	13,541	13,541	13,541
Adj R^2	0.112	0.462	0.068	0.331	0.088	0.372	0.077	0.339

Table C4

Robustness: Using Implied Cost of Equity Estimated at Ten Month after Fiscal Year-end

The table below reports the results of using implied cost of equity estimated using price and analyst forecasts at ten months after fiscal year-end. The control variables are the same as Table 3, including *Ln Asset*, *Book to Market*, *Leverage*, *Beta*, *Idiosyncratic Risk*, *Momentum*, *Analyst Forecast Bias* and *Long Term Growth*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *t*-statistics are in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

Panel A: The Mean Implied Cost of Equity

	Pooled OLS Regression		Fama-MacBeth Regression	
	Dependent = COE_t		Dependent = COE_t	
	(1)	(2)	(3)	(4)
$Subsidy_t$	−2.009***	−2.048***	−4.009***	−1.986**
	(−2.97)	(−2.99)	(−4.72)	(−2.97)
Controls	Yes	Yes	No	Yes
Industry FE	Yes	No	Yes	No
Year FE	Yes	No	Yes	No
Industry × Year FE	No	Yes	No	Yes
N	13,153	13,136	13,153	13,153
Adj R^2 or R^2	0.290	0.291	0.091	0.329

Panel B: The Implied Cost of Equity Estimated from Different Methods

	Dependent = $COE\ GLS_t$		Dependent = $COE\ CT_t$		Dependent = $COE\ MPEG_t$		Dependent = $COE\ OJ_t$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Subsidy_t$	−1.383***	−1.295***	−1.972***	−2.003***	−2.246**	−2.413**	−2.021***	−2.105***
	(−3.20)	(−2.94)	(−2.63)	(−2.67)	(−2.16)	(−2.31)	(−2.60)	(−2.66)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	No	Yes	No	Yes	No	Yes	No
Year FE	Yes	No	Yes	No	Yes	No	Yes	No
Industry × Year FE	No	Yes	No	Yes	No	Yes	No	Yes
N	13,153	13,136	13,153	13,136	13,153	13,136	13,153	13,136
Adj R^2	0.415	0.426	0.234	0.237	0.270	0.272	0.257	0.257

Table C5

Covariates Balancing of Propensity-score Matched and Entropy Balanced Sample

The table below reports the covariates balancing tests for the matched sample used in Table 6. Panel A presents the statistics of propensity-score matched sample. Panel B presents the statistics of entropy balanced sample.

Panel A: Propensity-score Matched Sample			
	Subsidized Firms (Obs.= 869)	Unsubsidized Firms (Obs.= 869)	
	Mean	Mean	Std Diff
Ln Asset	7.968	8.033	−0.050
Book to Market	0.430	0.436	−0.022
Leverage	0.491	0.508	−0.089
Beta	1.238	1.242	−0.006
Idiosyncratic Risk	0.094	0.094	−0.001
Annual Ret	0.150	0.156	−0.015
Analyst Forecast Bias	0.001	0.001	0.035
Long Term Growth	0.142	0.140	0.032
Panel B: Entropy Balanced Sample			
	Subsidized Firms (Obs.=2,698)	Unsubsidized Firms (Obs.= 10,803)	
	Mean	Mean	Diff
Ln Asset	8.710	8.710	0.000
Book to Market	0.547	0.547	0.000
Leverage	0.031	0.031	0.000
Beta	0.014	0.014	0.000
Idiosyncratic Risk	0.101	0.101	0.000
Annual Ret	0.097	0.097	0.000
Analyst Forecast Bias	0.708	0.708	0.000
Long Term Growth	3.178	3.178	0.000

Table C6

Robustness: PSM Sample Using Fama-MacBeth Regression and Implied Cost of Equity Estimated from Different Methods

The table below reports the results of robustness checks using the PSM sample in Table 6. The control variables are the same as Table 3, including *Ln Asset*, *Book to Market*, *Leverage*, *Beta*, *Idiosyncratic Risk*, *Momentum*, *Analyst Forecast Bias* and *Long Term Growth*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *t*-statistics are in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

Panel A: Fama-Macbeth Regression

	Dependent = COE_t	
	(1)	(2)
$Subsidy_t$	−3.617***	−2.037***
	(−3.75)	(−3.22)
Controls	No	Yes
Industry FE	Yes	Yes
N	1,738	1,738
R^2	0.099	0.460

Panel B: The Implied Cost of Equity Estimated from Different Methods

	Dependent = $COE\ GLS_t$		Dependent = $COE\ CT_t$		Dependent = $COE\ MPEG_t$		Dependent = $COE\ OJ_t$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Subsidy_t$	−1.643***	−1.678***	−2.885***	−2.902***	−2.840**	−2.655**	−2.334**	−2.322**
	(−3.33)	(−3.33)	(−3.44)	(−3.39)	(−2.13)	(−1.97)	(−2.34)	(−2.29)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	No	Yes	No	Yes	No	Yes	No
Year FE	Yes	No	Yes	No	Yes	No	Yes	No
Industry × Year FE	No	Yes	No	Yes	No	Yes	No	Yes
N	1,738	1,738	1,738	1,738	1,738	1,738	1,738	1,738
Adj R^2	0.479	0.484	0.279	0.277	0.305	0.309	0.270	0.272

Table C7

Robustness: Using Propensity-score Matched Sample with Different Caliper Width

The table below reports the results of using PSM sample with different caliper width. In Panels A and B we match firms with and without government subsidies based on the closet propensity score with caliper width of 0.05 and 0.2, and without replacement. The control variables are the same as Table 3, including *Ln Asset*, *Book to Market*, *Leverage*, *Beta*, *Idiosyncratic Risk*, *Momentum*, *Analyst Forecast Bias* and *Long Term Growth*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

Panel A: PSM Sample with Caliper Width of 0.05				
	Dependent = COE_t			
	(1)	(2)	(3)	(4)
$Subsidy_t$	−3.877***	−2.392***	−4.029***	−2.570***
	(−3.61)	(−3.00)	(−3.48)	(−3.10)
Controls	No	Yes	No	Yes
Industry FE	Yes	Yes	No	No
Year FE	Yes	Yes	No	No
Industry × Year FE	No	No	Yes	Yes
N	1,538	1,538	1,538	1,538
Adj R^2	0.026	0.299	0.027	0.304
Panel A: PSM Sample with Caliper Width of 0.2				
	Dependent = COE_t			
	(1)	(2)	(3)	(4)
$Subsidy_t$	−3.550***	−2.340***	−3.732***	−2.415***
	(−3.32)	(−3.21)	(−3.23)	(−3.22)
Controls	No	Yes	No	Yes
Industry FE	Yes	Yes	No	No
Year FE	Yes	Yes	No	No
Industry × Year FE	No	No	Yes	Yes
N	1,832	1,832	1,832	1,832
Adj R^2	0.031	0.339	0.025	0.343

Table C8

Robustness: Entropy Balanced Sample Using Fama-MacBeth Regression and Implied Cost of Equity Estimated from Different Methods

The table below reports the results of using the entropy balanced sample in Table 6. The control variables are the same as Table 3, including *Ln Asset*, *Book to Market*, *Leverage*, *Beta*, *Idiosyncratic Risk*, *Momentum*, *Analyst Forecast Bias* and *Long Term Growth*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *t*-statistics are in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

Panel A: Fama-MacBeth Regression

	Dependent = COE_t	
	(1)	(2)
$Subsidy_t$	−3.130***	−1.846***
	(−4.08)	(−4.02)
Controls	No	Yes
Industry FE	Yes	Yes
N	13,541	13,541
R^2	0.149	0.389

Panel B: The Implied Cost of Equity Estimated from Different Methods

	Dependent = $COE\ GLS_t$		Dependent = $COE\ CT_t$		Dependent = $COE\ MPEG_t$		Dependent = $COE\ OJ_t$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Subsidy_t$	−1.441***	−1.421***	−1.582**	−1.827***	−1.648*	−2.034**	−1.506**	−1.708**
	(−3.29)	(−3.14)	(−2.33)	(−2.70)	(−1.75)	(−2.14)	(−2.02)	(−2.25)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	No	Yes	No	Yes	No	Yes	No
Year FE	Yes	No	Yes	No	Yes	No	Yes	No
Industry × Year FE	No	Yes	No	Yes	No	Yes	No	Yes
N	13,541	13,524	13,541	13,524	13,541	13,524	13,541	13,524
Adj R^2	0.427	0.473	0.290	0.329	0.329	0.281	0.273	0.308

Table C9
Robustness: Using Different Entropy Balanced Sample

The table below reports the results of using different entropy balanced sample. In Panel A, we balance the firms with and without government subsidies using the first two moments (i.e., the mean and variance) of all controls. In Panel B, we balance the firms with and without government subsidies using the all three moments (i.e., the mean, variance and skewness) of all controls. The control variables are the same as Table 3, including *Ln Asset*, *Book to Market*, *Leverage*, *Beta*, *Idiosyncratic Risk*, *Momentum*, *Analyst Forecast Bias* and *Long Term Growth*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

Panel A: Entropy Balanced Sample Using Mean and Variance				
	Dependent = COE_t			
	(1)	(2)	(3)	(4)
$Subsidy_t$	−2.464***	−1.379**	−2.789***	−1.561**
	(−2.64)	(−2.23)	(−2.85)	(−2.45)
Controls	No	Yes	No	Yes
Industry FE	Yes	Yes	No	No
Year FE	Yes	Yes	No	No
Industry × Year FE	No	No	Yes	Yes
N	13,541	13,541	13,524	13,524
Adj R^2	0.107	0.328	0.129	0.354
Panel B: Entropy Balanced Sample Using Mean, Variance and Skewness				
	Dependent = COE_t			
	(1)	(2)	(3)	(4)
$Subsidy_t$	−2.376***	−1.279**	−2.663***	−1.443**
	(−2.59)	(−2.10)	(−2.78)	(−2.33)
Controls	No	Yes	No	Yes
Industry FE	Yes	Yes	No	No
Year FE	Yes	Yes	No	No
Industry × Year FE	No	No	Yes	Yes
N	13,541	13,541	13,524	13,524
Adj R^2	0.109	0.338	0.131	0.364

Table C10
The Determinants for Government Subsidies

The table below reports the results of testing the determinants for government subsidies. The dependent variable *Subsidy* is the amount of government subsidies scaled by total assets in thousands. The independent variable *Provider Surplus Shock* is an indicator variable that equal one if there is at least one of a firm's subsidies providers experience surplus shock in that year, and zero otherwise. And we use lagged one-year *Provider Surplus Shock* in all columns. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent Variable=$Subsidy_t$			
	(1)	(2)	(3)	(4)
$Provider\ Surplus\ Shock_{t-1}$	0.222***	0.227***	0.219***	0.441***
	(8.80)	(8.74)	(8.96)	(28.08)
$Ln\ Asset_t$	−0.004	−0.003	−0.005	0.012***
	(−0.81)	(−0.65)	(−0.93)	(2.67)
$Book\ to\ Market_t$	−0.040***	−0.042***	−0.042***	−0.011
	(−3.54)	(−3.66)	(−3.59)	(−1.05)
$Leverage_t$	−0.030	−0.034	−0.039	−0.028
	(−1.28)	(−1.42)	(−1.61)	(−1.61)
$Beta_t$	−0.004	−0.004	−0.002	−0.003
	(−0.65)	(−0.56)	(−0.22)	(−0.58)
$Idiosyncratic\ Risk_t$	0.059	0.061	0.046	0.079
	(0.60)	(0.63)	(0.48)	(1.20)
$Momentum_t$	0.002	0.009	0.003	−0.007
	(0.30)	(1.04)	(0.33)	(−1.29)
$Analyst\ Forecast\ Bias_t$	−0.017	0.002	−0.037	0.003
	(−0.29)	(0.03)	(−0.66)	(0.06)
$Long\ Term\ Growth_t$	0.023	0.030	0.029	0.072**
	(0.41)	(0.57)	(0.52)	(1.99)
$Political\ Contribution_t$	0.001	0.001	0.001	0.004***
	(0.68)	(0.43)	(0.95)	(3.64)
$Ln\ Employee_t$	−0.006	−0.007	−0.008*	0.012**
	(−1.32)	(−1.48)	(−1.70)	(2.16)
Industry FE	Yes	No	Yes	Yes
Year FE	Yes	No	Yes	No
Industry × Year FE	No	Yes	No	No
State FE	No	No	Yes	No
State × Year FE	No	No	No	Yes
Constant	0.071*	0.065*	0.077**	−0.087***
	(1.80)	(1.68)	(2.09)	(−3.05)
N	11,928	11,894	11,926	11,872
Adj R^2	0.055	0.067	0.061	0.413

Table C11

Robustness: Using Subsidies Providers' Extreme Surplus Shocks

The table below reports the results of the effect of subsidy providers' extreme surplus shocks on implied cost of equity. *Extreme Provider Surplus Shock* is an indicator variable that equals one if there is at least one of a firm's subsidies providers experience extreme surplus shocks, and zero otherwise. We use lagged one-year *Extreme Provider Surplus Shock* in all columns. The other control variables are the same as Table 3, including *Ln Asset*, *Book to Market*, *Leverage*, *Beta*, *Idiosyncratic Risk*, *Momentum*, *Analyst Forecast Bias* and *Long Term Growth*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent Variable=COE_t							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Extreme Provider Surplus Shock $_{t-1}$	−0.015***	−0.005**	−0.016***	−0.005**	−0.015***	−0.005**	−0.015***	−0.005**
	(−7.78)	(−2.56)	(−7.86)	(−2.47)	(−7.71)	(−2.51)	(−7.60)	(−2.47)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Industry FE	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	Yes	Yes	No	No
Industry × Year FE	No	No	Yes	Yes	No	No	No	No
State FE	No	No	No	No	Yes	Yes	No	No
State × Year FE	No	No	No	No	No	No	Yes	Yes
N	12,083	12,083	12,050	12,050	12,081	12,081	12,036	12,036
Adj R^2	0.064	0.350	0.061	0.351	0.074	0.354	0.061	0.344

Table C12

Robustness: Using Subsidies Providers' Surplus Shocks Over Previous Three Years

The table below reports the results of the impact of subsidy providers' surplus shocks over previous three years on implied cost of equity. *Three Year Provider Surplus Shock* is an indicator variable that equals to one if there is at least one of firm's subsidies providers experience surplus shocks during the prior there years, and zero otherwise. We use lagged one-year *Three Year Provider Surplus Shock* in all columns. The other control variables are the same as Table 3, including *Ln Asset*, *Book to Market*, *Leverage*, *Beta*, *Idiosyncratic Risk*, *Momentum*, *Analyst Forecast Bias* and *Long Term Growth*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent Variable=COE_t							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Three Year Provider Surplus Shock $_{t-1}$	−0.017***	−0.006***	−0.017***	−0.007***	−0.016***	−0.006***	−0.017***	−0.006***
	(−8.05)	(−3.27)	(−8.19)	(−3.31)	(−8.07)	(−3.22)	(−8.00)	(−3.20)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Industry FE	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	Yes	Yes	No	No
Industry × Year FE	No	No	Yes	Yes	No	No	No	No
State FE	No	No	No	No	Yes	Yes	No	No
State × Year FE	No	No	No	No	No	No	Yes	Yes
N	12,083	12,083	12,050	12,050	12,081	12,081	12,036	12,036
Adj R^2	0.069	0.351	0.067	0.352	0.079	0.355	0.066	0.345

Chapter 4

Intra-industry ESG Spillover Effects on Loan Spread: Evidence from European Countries

Abstract

This paper investigates the intra-industry spillover effects of rivals' negative environmental, social, and governance (ESG) incidents on firms' loan spreads. Exploiting the event-based ESG records of European firms, we find that industry rivals' ESG incidents lower a firm's subsequent loan spreads. This intra-industry spillover effect pertains to both the aggregate ESG records and various subcategories. It is more pronounced when rivals are listed firms or larger firms, highlighting the significance of an incident's salience in the ESG spillover process. Importantly, it is also more pronounced among borrowing firms with better ESG conduct and those operating in more competitive industries, suggesting the competition effect dominates the contagion effect during spillover.

Keywords: ESG; loan spread; spillover effect

4.1 Introduction

The rising awareness of firms' environmental, social, and governance (ESG) practices in recent years has fuelled studies on the economic consequences of such practices (Di Giuli and Kostovetsky 2014; Masulis and Reza 2015; Flammer 2015a; Ferrell et al. 2016; Lins et al. 2017; Buchanan et al. 2018; Albuquerque et al. 2019). Particularly, a line of literature focuses on whether firms' ESG practices affect decisions on bank loans. The relevant studies show that firms with better ESG profiles are associated with lower systematic risk (Hong and Kacperczyk 2009; Benabou and Tirole 2010; El Ghoul et al. 2011; Oikonomou et al. 2012; Lins et al. 2017), lower credit risk (Jiraporn et al. 2014), and lower litigation risk (Hong and Liskovich 2015; Schiller 2018). Therefore, firms with better ESG performance have lower costs of loans (Chava 2014; Kim et al. 2014; Hauptmann 2017; Cheng et al. 2017; Cheung et al. 2018; Delis et al. 2019; Anginer et al. 2020).

While existing literature shows that borrowing firms' ESG practices impact various firm aspects significantly (Chava 2014; Kim et al. 2014; Flammer 2015a; Cheng et al. 2017; Cheung et al. 2018; Albuquerque et al. 2019), little knowledge exists on whether or how firms are affected by the ESG practices of economically linked firms. Prior literature documents that significant corporate events influence economically linked firms such as customers and suppliers (Fee and Thomas 2004; Cohen and Frazzini 2008; Hertzel et al. 2008), geographic neighbours (Dougal et al. 2015; Engelberg et al. 2018), strategic alliance partners (Boone and Ivanov 2012), and technology peers (Cai et al. 2019). In particular, many studies find that significant corporate events have substantial impacts on industry peers (Foster 1981; Han et al. 1989; Lang and Stulz 1992; Song and Walkling 2000; Jorion and Zhang 2007; Thomas and Zhang 2008; Gleason et al. 2008; Hertzel and Officer 2012; Beatty et al. 2013; Servaes and Tamayo 2014). In this paper, we attempt to fill the research gap by examining the intra-industry spillover effects of adverse ESG incidents on firms' loan spreads.

There are two contrasting views on intra-industry spillover effects: the contagion effect and the competitive effect (Lang and Stulz 1992). On the one hand, a firm's adverse event could convey negative information about industry peers since their businesses will be quite similar. Thus, a firm's adverse event can lead to a negative impact on industry peers, i.e., the contagion effect (Ferris et al. 1997; Xu et al. 2006; Gleason et al. 2008; Benmelech and Bergman 2011; Hertzel and Officer 2012; Paruchuri and Misangyi 2015; Garcia-Appendini 2018). If the adverse ESG incidents convey information of common interest about shared

ESG fundamentals within an industry, lenders may expect other non-event firms in the same industry to be exposed to credit risk induced by similar ESG concerns. Thus, the loan spreads will be higher for firms with industry rivals in adverse ESG incidents.

On the other hand, a firm can derive economic benefits from a change in the competitive balance due to industry rivals' adverse events, i.e., competitive effects (Jorion and Zhang 2007; Goldman et al. 2012; Burchard et al. 2021). Following rivals' adverse ESG incidents, industry competitors will be in a unique position to respond to and benefit from their rivals' misconduct strategically. For example, the non-event firms can exploit product-market opportunities (Kang et al. 2021) and initiate competitive actions (Guo et al. 2020) to create competitive advantages. Besides, lenders specilizing in a particular industry could prefer borrowers without concerns over related ESG issues, and thus willing to offer favourable loan contracts to the non-event firms. Therefore, banks are expected to require lower spreads if a firm can gain ground in a reshaped competitive landscape following rivals' adverse ESG incidents.

Our study focuses on European firms because ESG issues are increasingly important in EU countries' bank lending. Specifically, the EU has recently introduced several regulatory policies for industrial companies and banks to foster awareness of ESG issues. For example, the EU Taxonomy for Sustainable Activities, which came into force in 2020, established criteria for classifying business activities based on their sustainability. The European Banking Authority (EBA) also published a consultation paper in 2021 that discussed the implementation of technical standards on disclosures on ESG risks for financial institutions. Our research highlights the importance of ESG issues for bank loans in the EU context by showing a significant intra-industry effect of borrowers' ESG incidents.

Using a unique sample of European firms from 2010 to 2019, we find that industry rivals' adverse ESG incidents lead to a decrease in firms' loan spreads, consistent with the competitive effect prevailing over the contagion effect. Specifically, the spreads on firms' loans, on average, decrease by about 38 basis points following the publicity of industrial rivals' adverse ESG incidents. The negative association between rivals' adverse ESG incidents and loan spreads remains economically and statistically significant when using different test specifications and alternative measurements. Next, we test how the industry rival's type alters the effect of their ESG incidents on loan spreads. We find the ESG incidents of public firms and large firms have stronger impacts on borrowing firms' loan spreads. Then, we test the effect of industry rivals' high-profile ESG incidents, and find that

firms enjoy lower loan spreads after such incidents.

Although we identify the domain of the competitive effects, not all firms are equally likely to benefit from rivals' adverse incidents. We expect firms with better ESG profiles to be more attractive to lenders and better positioned to attract more favourable loan contracting terms after rivals' adverse ESG incidents. Indeed, we find the negative association between industry rivals' adverse ESG incidents and firms' loan spreads to be more pronounced among firms with better ESG performance. Prior literature on bankruptcy suggests the intra-industry competitive effect is more significant among concentrated industries, since companies in such industries are more likely to benefit from the exit of an industry-dominant competitor (Lang and Stulz 1992; Jorion and Zhang 2007). However, we find the competitive effect arising from rivals' ESG incidents stronger for firms located in more competitive industries. This could be because ESG practices play a more significant role in markets where the competition is intense (Flammer 2015b; Fernández-Kranz and Santaló 2010; Dupire and M'Zali 2018). The efficient implementation of ESG practices can differentiate firms from competitors and attract customers in a competitive market where the price competition is strong (Baron 2001; Fisman et al. 2006; Siegel and Vitaliano 2007; Flammer 2015b). Following rivals' adverse ESG incidents, a firm could further differentiate itself in the industry and exploit product-market opportunities. The stronger market power could decrease credit risk (Bonfim 2009) and the cost of capital (Sullivan 1978). In addition, we test whether firms benefit from rivals' adverse ESG incidents by gaining a greater market share. Our results suggest there are increases in firms' market shares due to rivals' adverse ESG incidents.

Lastly, we examine the impact of various types of incidents and find rivals' environmental, social, governance, and cross-cutting incidents all reduce borrowing firms' loan spreads. We also find that all subcategories of rivals' incidents significantly reduce firms' loan spreads.

This research makes two main contributions. First, our study contributes to the growing literature that examines the economic consequences of firms' ESG performance (Turban and Greening 1996; Fisman et al. 2006; Siegel and Vitaliano 2007; Edmans 2011; Di Giuli and Kostovetsky 2014; Flammer 2015a; Masulis and Reza 2015; Ferrell et al. 2016; Lins et al. 2017; Buchanan et al. 2018; Albuquerque et al. 2019). Although recent studies show firms' ESG performance is an important determinant of bank loan contracting (Goss and Roberts 2011; Chava 2014; Kim et al. 2014; Cheng et al. 2017; Hauptmann 2017; Delis et al. 2019;

Anginer et al. 2020; Shin 2021), we provide novel evidence regarding how industry rivals' adverse ESG incidents affect the cost of bank loans. To our best knowledge, to date, no study has been done on how a firm's industrial ESG environment impacts its loan contracting. Second, our paper adds to the literature on intra-industry spillover effects. Prior literature has investigated the intra-industry spillover effects of bankruptcy (Lang and Stulz 1992; Ferris et al. 1997; Jorion and Zhang 2007; Benmelech and Bergman 2011; Hertzel and Officer 2012), restatement (Gleason et al. 2008), fraud (Goldman et al. 2012; Paruchuri and Misangyi 2015), and top manager turnover (Burchard et al. 2021). By focusing on ESG incidents' impact on industry rival's borrowing costs, we add to the literature on the broader externality of ESG conducts. Our findings show that because firms can exploit product-market opportunities arising from rivals' ESG misconduct, the competition effect prevails over the contagion effect.

The rest of this paper is organized as follows: Section 4.2 provides a review of the related literature and hypothesis development; Section 4.3 presents the data and sample construction and elaborates the empirical design; Section 4.4 presents the empirical findings; and Section 4.5 concludes.

4.2 Related Literature and Hypothesis Development

Our study is motivated by two streams of research. One research stream documents the impact of ESG practices on firms' cost of capital. The second stream of research documents intra-industry spillover effects. This subsection briefly summarizes and discusses related literature and proposes a hypothesis based on prior research.

4.2.1 ESG Practices and Economic Consequences

Despite a growing body of literature examining the economic consequences of firms' ESG practices, the findings are inconclusive. A strand of literature posits that agency costs manifest themselves in inefficient ESG practices. Some studies argue ESG increases a firm's unnecessary costs, putting the firm at a competitive disadvantage (Aupperle et al. 1985; Jensen 2002). Other studies argue that excessive resources on corporate social responsibilities reflect managerial preferences rather than shareholders' value (Brammer and Millington 2008; Benabou and Tirole 2010; Masulis and Reza 2015). In line with this agency view, prior literature shows ESG activities are related to poor firm performance and lower firm value (Di Giuli and Kostovetsky 2014; Masulis and Reza 2015; Buchanan et al. 2018).

However, the emerging perception is that firms have multiple obligations to various entities such as its employees and communities, and ESG activities promote firms'

accountability to a wide range of stakeholders (Freeman and Evan 1990; Freeman 1994; Freeman and Phillips 2002; Freeman et al. 2004). Besides, positive engagement in ESG activities help firms to gain social legitimacy (Patten 1992; O'Donovan 2002; Cho and Patten 2007), a perception that firms' actions are aligned with the social norms, values, and beliefs (Suchman 1995). Prior literature shows ESG activities can help firms to obtain and sustain competitive advantages (Hart 1995; Jones 1995; Aragón-Correa 1998), differentiate themselves from their competitors (Fisman et al. 2006; Siegel and Vitaliano 2007; Flammer 2015b), engage their employees (Turban and Greening 1996; Edmans 2011; Flammer and Luo 2017), enhance their performance (Flammer 2015a; Ferrell et al. 2016; Lins et al. 2017; Albuquerque et al. 2019), and gain better access to finance (Cheng et al. 2014).

Among the literature examining the economic consequences of ESG practices, we pay particular attention to how ESG practices affect firms' risks and whether the risks induced by ESG practices are priced in capital markets. Many studies show that firms with better ESG profiles are exposed to lower systematic risk (Hong and Kacperczyk 2009; Benabou and Tirole 2010; El Ghoul et al. 2011; Oikonomou et al. 2012; Lins et al. 2017; Albuquerque et al. 2019). A few studies also indicate that firms with stronger ESG performance have lower credit risk. For example, Jiraporn et al. (2014) show that responsible firms enjoy favourable credit ratings, while Seltzer et al. (2020) find firms with poor environmental profiles to have lower credit ratings. Better ESG practices can also reduce litigation risk. Hong and Liskovich (2015) find socially responsible firms face fewer fines for misconduct. Moreover, Schiller (2018) shows the adoption of environmental and social policies at customer firms is related to a lower likelihood of lawsuits and lower penalty payments.

Prior literature also shows that the risks induced by ESG practices are priced in both equity and debt markets. The findings largely agree that firms with poor ESG profiles have a higher cost of capital. For example, Sharfman and Fernando (2008), El Ghoul et al. (2011), and Breuer et al. (2018) find firms with better ESG performance to have lower costs of equity. Hong and Kacperczyk (2009) show firms in "sin" industries (alcohol, tobacco, and gaming) to have higher costs of equity. Chava (2014) documents that investors demand higher stock returns on firms with poor environmental practices.

Regarding the impact of firms' ESG performance on the cost of debt, Bauer and Hann (2010) conclude that ESG concerns increase the cost of bonds and decrease credit ratings. Ge and Liu (2015) show that firms with better ESG performance have lower yields on corporate bonds. Some papers also examine how ESG performance affects loan contracting.

Most of the literature suggests borrowers with better ESG performance tend to have lower loan spreads (Goss and Roberts 2011; Kim et al. 2014; Cheng et al. 2017; Hauptmann 2017; Cheung et al. 2018). Some studies on the environmental dimension also provide consistent results. For example, Chava (2014) suggests firms with environmental concerns have higher credit risk, leading to higher loan spreads. Delis et al. (2019) find banks have priced climate-related risk since the Paris Climate Agreement. Anginer et al. (2020) find that loans originating after firms' adverse climate-related incidents have higher spreads, shorter maturities, and more restrictions.

4.2.2 Intra-industry Spillover Effects

Prior literature documents significant information-diffusion effects among economically related firms (Fee and Thomas 2004; Cohen and Frazzini 2008; Hertzel et al. 2008; Menzly and Ozbas 2010; Boone and Ivanov 2012; Dougal et al. 2015; Engelberg et al. 2018; Cai et al. 2019), showing the importance of assessments of market information produced by economically linked firms. In particular, a stream of literature examines the impact of various corporate events (e.g., bankruptcy, fraud, accounting restatements, earnings announcements, and mergers and acquisitions) on peer firms in the same industry (Foster 1981; Han et al. 1989; Lang and Stulz 1992; Song and Walkling 2000; Shahrur 2005; Jorion and Zhang 2007; Gleason et al. 2008; Thomas and Zhang 2008; Hertzel and Officer 2012; Bradley and Yuan 2013; Paruchuri and Misangyi 2015; Kang et al. 2021).

Beginning with the study of Lang and Stulz (1992), the literature on intra-industry spillover effects mainly investigates two types of effects: the contagion effect and the competitive effect. On the one hand, a firm-specific event not only reveals information related to the event firm but also conveys information that can be generalized to other non-event firms in the same industry. Consistent with this argument, many researchers find evidence of contagion effect. For example, firms experience losses in the equity market after industry rivals' bankruptcy announcements (Ferris et al. 1997), accounting restatements (Gleason et al. 2008; Xu et al. 2006), and exposure of their financial misconduct (Paruchuri and Misangyi 2015). The contagion effects are also observed in the debt market. Hertzel and Officer (2012) find the spreads on bank loans taken out after industry rivals' bankruptcy filings to be significantly higher. Similarly, Benmelech and Bergman (2011) show that a firm's bankruptcy reduces competitors' collateral value, leading to increases in the cost of debt.

On the other hand, the way corporate events alter the industrial competitive balance can lead to competitive effects. Erwin and Miller (1998) find firms' announcements of open-market repurchases produce positive reactions in the stock market for the firms themselves, while their industry rivals experience negative market reactions. Burchard et al. (2021) find that, during the periods of stagnation that affect companies with top-manager turnover, their industry rivals take actions to gain competitive advantages, which result in positive abnormal returns and accounting performance for the rival firms. Some literature also documents mixed findings. For example, Lang and Stulz (1992) find that the intra-industry contagion effects of bankruptcy announcements prevails, but competitive effects emerge in concentrated industries. Goldman et al. (2012) examine the intra-industry spillover effects of fraudulent financial misrepresentation. They also observe competition effects in less competitive industries, while contagion effects play a more significant role in competitive industries. Jorion and Zhang (2007) investigate the intra-industry spillover effects of different types of credit events. They find that Chapter 11 bankruptcies result in contagion effects, while Chapter 7 bankruptcies are associated with competitive effects.

4.2.3 Hypothesis Development

As discussed above, there could be two contrasting effects of firms' ESG incidents on industry peers: the contagion effect and the competitive effect (Lang and Stulz 1992). On the one hand, a firm's adverse ESG incident could convey information of common interest about shared ESG fundamentals within the whole industry. Lenders could worry that other firms in the same industry may commit similar ESG misconduct and be exposed to potential regulation and litigation risks due to having businesses similar to that of the event firm. Thus, from the perspective of contagion effect (Ferris et al. 1997; Xu et al. 2006; Gleason et al. 2008; Benmelech and Bergman 2011; Hertzel and Officer 2012), banks should require higher interest rates from borrowers whose industry rivals have experienced adverse ESG incidents.

On the other hand, lenders and investors may hold favourable views of firms whose rivals are exposed to adverse ESG events because the non-event firm can derive economic benefits from the change in the competitive balance (Lang and Stulz 1992; Jorion and Zhang 2007; Goldman et al. 2012; Burchard et al. 2021). The disclosure of adverse ESG incidents will undermine a firm's legitimacy with its customers, suppliers, lenders, and investors (Karpoff et al. 2008), leading to difficulties accessing essential resources and putting it at a competitive disadvantage. However, non-incident firms in the same industry can exploit

opportunities arising from this. For example, a firm may obtain product-market opportunities as customers are likely to switch to products offered by competitors that are free from misconduct (Kang et al. 2021). Besides, Guo et al. (2020) show that firms can initiate predatory actions, e.g., new market entry and pricing change, to create and maintain competitive advantages during the stagnation periods their rivals face after adverse events. Moreover, lenders specializing in transactions in a particular industry may be concerned about the risk related to ESG issues after the exposure of an ESG incident, prefer borrowers without those concerns, and thus be willing to offer favourable spreads in their loan contracts. Thus, banks are expected to require lower spreads.

Based on the above discussion, we formulate the following null hypothesis:

Hypothesis: There is no relation between industry rivals' adverse ESG incidents and firms' loan spreads.

4.3 Data, Sample Construction and Research Design

To test our hypothesis, we construct a unique dataset of ESG incidents and bank loans made to public and private firms in Europe using three primary data sources: (1) incident-based data on firms' ESG practices from the RepRisk database; (2) the Thomson Reuters DealScan database of loans made to firms; and (3) firm-specific financial information data from Bureau van Dijk (BvD)'s Amadeus database, covering both private and public firms. The following subsections introduce the databases and explain the sample construction in detail.

4.3.1 Data

RepRisk is an event-driven database that tracks both private and public firms' ESG performance from 2007 to the present. On a daily basis, RepRisk screens over 90,000 public sources and stakeholders in 20 languages to identify negative ESG incidents.[1] Here are some examples of negative ESG incidents recorded in RepRisk. On 30th of April 2010, Transocean Ltd, the largest offshore drilling contractor based in Switzerland, was reported to have been involved in an oil spill incident in the Gulf of Mexico. On 6th of December 2019, Ericsson AB, a Swedish telecommunication company, was reported to have paid bribes between 2000 to 2017 in order to build its business in Asia, Africa, and the Middle East. For each incident, RepRisk removes duplicates and analyses the nature of the incidents. Each incident is broadly classified into either environmental, social, governance, or cross-cutting issues.

[1] RepRisk's news sources include print media, online media, social media, government bodies, regulators, newsletters etc. These sources range from the international to the regional, national, and local levels.

Cross-cutting issues span multiple categories of ESG issues. RepRisk has also constructed a list of 31 specific ESG issues, and every incident in RepRisk is linked to at least one of them.[2] Each incident is also assigned three parameters to indicate its degree of influence: *Severity*, *Reach*, and *Novelty*.[3] *Severity* reflects the harshness of the incident, with three levels: low severity (*Severity* = 1), severe (*Severity* = 2), and high severity (*Severity* = 3). *Reach* represents the influence of the incident based on the readership and importance of the information source. It also has three levels: limited reach (*Reach* = 1), medium reach (*Reach* = *2*), and high reach (*Reach* = 3). *Novelty* shows the newness of the issues for the firm. *Novelty* equals 2 if a company is linked to a particular issue in a particular country for the first time, and 1 if the event is not novel for the firm.

We obtain firm-level ESG incidents for all public and private firms in Europe from 2007 to 2019. In Appendix B, we report a summary of the RepRisk incidents that could be successfully matched to Amadeus. As is reported in Table B1, we find 9,144 European firms covered by Amadeus with ESG incidents recorded in RepRisk. Public firms account for 12.72% (1,163), and non-public firms for 87.28% (7,981). The three main categories, i.e., environmental issues, social issues, and governance issues, are well represented, and social issues constitute the largest group with 30,067 incidents. We also report the breakdown into the 31 issues in Panel C. Among those 31 issues, violation of national legislation (28,620 events), impact on communities (13,250 events), impacts on landscapes, ecosystems, and biodiversity (13,141 events), corruption, bribery, extortion, and money laundering (11,145 events), and human rights abuses and corporate complicity (10,210 events) are the most frequently represented incidents.

We retrieve the data on loan facilities from the Thomson Reuters DealScan database, which provides data on deals in the global commercial loan market. DealScan covers detailed information on loan contracts, borrower identities, and lender identities. A loan package in DealScan contains one or more loan facilities. The loan facilities in a loan package can differ in price and non-price terms. As DealScan provides detailed information on the global commercial loan market, prior works such as Acharya et al. (2018) and Saunders and Steffen (2011) have used it to conduct global research on bank loans.

Finally, we extract firms' financial information from BvD's Amadeus database. Amadeus provides comprehensive financial information for approximately 19 million public

[2] The details of the 31 ESG types can be found in Appendix B.
[3] For more details on the definitions of *Severity*, *Reach,* and *Novelty,* please refer to the methodology overview of RepRisk: https://www.reprisk.com/news-research/resources/methodology.

and private companies in Europe. Amadeus groups firms into four different categories based on their size: “Very Large”, “Large”, “Medium”, and “Small”. As mentioned by Acharya et al. (2018), the firms in the intersection of Amadeus and DealScan are all classified as either “Very Large” or “Large”. Therefore, we only use “Very Large” and “Large” firms in Amadeus to construct our sample. A firm is classified as “Very Large” if it satisfies at least one of the following criteria: revenue of at least 100 million Euros, total assets of at least 200 million Euros, or at least 1,000 employees. A firm is classified as “Large” if it satisfies at least one of the following criteria: revenue of at least 10 million Euros, total assets of at least 20 million Euros, at least 150 employees, or publicly listed.

4.3.2 Linking Multiple Databases

As there is no common identifier across RepRisk, DealScan, and Amadeus, we implement a matching procedure following Li and Wu (2020), based on the similarity of firm names. We first standardize the firm names covered by RepRisk, DealScan, and Amadeus to increase the matching quality. Then, we calculate the firm-name-matching score using the partial ratio method from the fuzzy-wuzzy Python package (Chava and Roberts 2008). The matching score ranges from 0 (no match) to 100 (perfect match). Following Li and Wu (2020), we define a match if we obtain any of the following: (1) an exact match of the standardized firm names and headquarter countries; (2) a fuzzy string match of the standardized company names with a match score greater than or equal to 99 and an exact match of headquarter countries. To ensure matching accuracy, we manually review all the matched pairs with matching scores greater than or equal to 99 and require the differences between the lengths of matched names to be less than 15 characters. To check the likelihood that we are omitting firm pairs that would have been matched had we not imposed the above thresholds, we also randomly select 1% of the matched pairs with matching scores below 99 and differences between the lengths of matched names of more than 15 characters. Not surprisingly, there are scarcely any acceptable matched pairs.[4] More details on the matching process can be found in Appendix C.

4.3.3 Sample

Because our research focuses on intra-industry spillover effects on bank loans, our sample is composed of loan-dependent firms. Specifically, we use firms that have borrowed

[4] We randomly select 1% of the matched pairs with matching scores below 99 and differences between the lengths of the matched names of more than 15 characters, and only 2 out of 3,999 matched pairs are acceptable matched pairs.

via bank loans during the past three years. Our sample period is from 2010 to 2019. The sample period starts in 2010 because the data in RepRisk begin in 2007 and our main explanatory variable (i.e., *Ln Rival Incident Count*) is computed using the number of reported ESG incidents of a firm's industry rivals during the past three years. We exclude (1) firms operating in the financial sector (SIC codes 6000-6999) and (2) observations with insufficient data to calculate the variables used in our baseline analysis. Our final sample consists of 706 firms and 2,083 facility-level observations. As displayed in Table D2 of Appendix D, firms from France, Germany, Spain, and the United Kingdom make up a large portion of our sample.

4.3.4 Baseline Specification

We examine the impact of industry rivals' ESG incidents on loan pricing using multivariate ordinary least squares (OLS) regressions, following prior literature (e.g., Lin et al. 2013; Pappas and Xu 2021). The baseline model is as follows:

$$\begin{aligned} Ln\ AISD_{i,t} = {} & \alpha_1 + \beta_1 Ln\ Rival\ Incident\ Count_{i,t} + \beta_2 Ln\ Total\ Assets_{i,t-1} \\ & + \beta_3 Leverage_{i,t-1} + \beta_4 ROA_{i,t-1} + \beta_5 Loss_{i,t-1} + \beta_6 Tangibility_{i,t-1} \\ & + \beta_7 Ln\ Maturity_{i,t} + \beta_8 Ln\ Loan\ Amount_{i,t} \\ & + \beta_9 Performance\ Pricing_{i,t} + \beta_{10} Number\ of\ Lenders_{i,t} \\ & + \beta_{11} Syndication_{i,t} + \beta_{12} Financial\ Covernant\ Count_{i,t} \\ & + Loan\ Types\ Fixed\ Effects + Loan\ Purpose\ Fixed\ Effects \\ & + Year\ Fixed\ Effects + Firm\ Fixed\ Effects + \varepsilon_{i,t} \end{aligned} \quad (1)$$

In regression Eq (1), each observation represents a single loan facility *i*. The dependent variable is the natural logarithm of all-in-drawn spread (*AISD*). All-in-drawn spread is a widely used measure for loan pricing (Graham et al. 2008; Lin et al. 2011; Aslan and Kumar 2012; Hertzel and Officer 2012; Valta 2012; Lin et al. 2013). It is an all-inclusive measure of the loan price (Bharath et al. 2011), measuring what the borrower pays in basis points over the London Interbank Offered Rate (LIBOR) or equivalent for each dollar drawn. To mitigate the potential skewness in the loan spread data, we take the natural logarithm of the all-in-drawn spread (*Ln AISD*) (Graham et al. 2008; Chava et al. 2009; Lin et al. 2011).

To capture the effect of industry rivals' ESG incidents, we define a variable *Ln Rival Incident Count*, the natural logarithm of one plus the number of reported ESG incidents of a firm's industry rivals during the past three years. Following Li and Wu (2020) and Wang and Li (2019), the ESG incidents are weighted by the *Severity* and *Reach* scores from RepRisk.

Meanwhile, a firm's industry rivals are those firms with the same three-digit SIC code, provided by Amadeus.

Following prior literature, we include firm-specific and loan-specific factors that might affect the loan spread (Graham et al. 2008; Chava et al. 2009; Lin et al. 2011; Saunders and Steffen 2011; Chan et al. 2013; Lin et al. 2013; Huang et al. 2016; Franco et al. 2016; Hasan et al. 2017; Francis et al. 2017). We also include year and firm fixed effects to control for unmeasured macro factors in the time series and time-invariant firm characteristics, respectively. Regarding time-varying firm-specific characteristics, we control for borrowers' firm size (*Ln Total Assets*), leverage (*Leverage*), return on assets (*ROA*), the presence of aggregate loss (*Loss*), and asset tangibility (*Tangibility*). Small firms' information is opaquer, leading to increased loan spreads. Low-leverage and profitable firms are likely to have stable cash flow and lower default risk, and thus lower loan spreads (Lin et al. 2011). Also, asset tangibility increases recovery rates in default, and firms with higher tangibility ratios are expected to have lower loan spreads (Bae and Goyal 2009). All firm-specific variables in our regression are calculated at the end of the fiscal year immediately before the loan origination, to ensure those characteristics are at least exogenous in time (Lin et al. 2013).

Along with firm-specific characteristics, we also control for a set of loan-specific variables. We control for loan maturity (*Ln Maturity*) and loan amount (*Ln Loan Amount*). Loans with longer maturities expose banks to borrowers' financial conditions for a longer time, and are thus expected to have higher spreads (Pappas and Xu 2021). Loans with larger amounts are expected to have lower spreads because of the scale-economy effect in lending (Berger and Udell 1990). We include an indicator variable for performance pricing provision as a control variable. As Asquith et al. (2005) indicate, interest-decreasing performance pricing reduces the spread, whereas interest-increasing performance pricing increases the spread. We further control for the existence of several lenders (*Number of Lenders*) and an indicator for syndicated loans (*Syndication*). As Francis et al. (2017) discussed, the decision to lend through a syndicated loan is a function of the size, complexity, and risk of the loan. The loan spread is expected to be higher for syndicated loans. However, the larger number of lenders in a syndicated loan means each lender is exposed to less risk, and consequently the number of lenders is expected to be negatively related to the loan spread. We also control for the number of financial covenants in the loan facility (*Financial Covenant Count*). Financial covenants reduce the agency cost of debt, which means loans with financial covenants have lower spreads (Bradley and Roberts 2015). Following Campello et al. (2011),

we also include a set of indicator variables regarding loan purpose (*Loan Purpose Indicators*, including corporate purpose, working capital, debt repayment, acquisition, backup line for commercial paper) and loan type (*Loan Type Indicators*, including term loan, revolver greater than one year, revolver less than one year, and 364-day facilities). More details of the variable definitions are provided in Appendix A.

4.3.5 Descriptive Statistics

Table 1 reports the summary statistics for the main variables.[5] Our sample period ranges from 2010 to 2019 and there are 2,083 facility-level observations in the main analysis.[6] As presented in Panel A, the average number of industry rivals' adverse ESG incidents during the past three years, weighted by *Severity* and *Reach* (*Rival Incident Count*), is 98.85. *Rival Incident* is an indicator that equals one if a firm's industry rivals have reported at least one ESG incident during the past three years, and zero otherwise. The mean of *Rival Incident* is 0.66, indicating that about 66% of our sample firms have rivals exposed to ESG incidents. *Rival Severe Incident* is an indicator that equals one if a firm's industry rivals have reported at least one ESG incident with high severity (*Severity*≥2) during the past three years, and zero otherwise. The mean of *Rival Severe Incident* is 0.55, suggesting that about 55% of our sample have a rival that has experienced a severe ESG incident. *Borrowers Incident Count* is the number of reported ESG incidents of a firm during the past three years, weighted by *Severity* and *Reach*. The mean of *Borrowers Incident Count* is 4.42. *Borrower Incident* is an indicator variable that equals one if a borrowing firm has at least one reported ESG incident during the past three years. The mean of *Borrower Incident* is 0.22, suggesting that 22% of our sample has been engaged in adverse ESG incidents.

Panel B presents the summary statistic of the firm characteristics. The average of total assets (*Total Assets*) is 3,383.00 million US$. As our sample is constructed using loan-dependent firms (i.e., firms with loans during the past three years), the mean leverage (*Leverage*, defined as total liabilities scaled by total assets) is 0.65. The means of the return on assets (*ROA*), the indicator of negative net income (*Loss*), and asset tangibility (*Tangibility*) are 0.04, 0.25, and 0.19, respectively.

As shown in Panel C, the mean loan spread (*AISD*) is 299.98 basis points. The mean loan maturity (*Maturity*) is 66.05 months, and the mean loan size (*Loan Amount*) is 435.04

[5] We report the sample distributions by year and by country in Tables D1 and D2 in the appendix.
[6] The number of observations without missing variables is 2,291, and there are 208 singleton observations that are dropped in the regression.

million US$. And 6% of the loans in our sample have performance pricing provisions.

[Insert Table 1 here]

4.4 Empirical Findings

4.4.1 The Effect of Rivals' ESG Incidents on Firms' Loan Spreads

We report the baseline regression in Table 2. Column (1) reports the regression of firms' loan spreads on the weighted number of industry rivals' ESG incidents, without any control variable. The coefficient on *Ln Rival Incident Count* is −0.051 (*t*-stat = −2.80), significant at the 1% level. After adding a set of firm-specific and loan-specific controls, as shown in Column (2), the coefficient on *Ln Rival Incident Count* is −0.055 (*t*-stat = −3.15), still significant at the 1% level. The significantly negative association between industry rivals' ESG incidents and firms' loan spreads is consistent with the competitive effect, showing firms benefit from rivals' adverse ESG incidents. Specifically, taking Column (2) as an example, a one-standard-deviation increase in the logarithm of the number of industry rivals' ESG incidents decreases firms' loan spreads by 12.60% (0.055×2.29), about 38 basis points.[7] Consistent with prior literature, we also find that loans with longer maturities and fewer lenders have lower spreads (Francis et al. 2017; Pappas and Xu 2021).

In Columns (3) and (4), we use the indicator of the presence of rivals' adverse ESG incidents as the independent variable of interest. We find the coefficient on *Rival Incident* in Column (3) to be insignificant, whereas the coefficient on *Rival Severe Incident* is significant at 5% (coefficient = −0.170, *t*-stat = −2.28). This indicates industry rivals' adverse ESG incidents decrease firms' loan spreads only if the incidents have severe impacts (*Severity*≥2). Economically, the presence of rivals' severe incidents, i.e., *Rival Severe Incident Dummy* = 1, reduces the loan spread by 17%, equivalent to 51 basis points.

To mitigate the confounding effect of borrowers' adverse ESG incidents, we include *Ln Borrower Incident Count* and *Borrower Incident* as control variables in Columns (5) and (6). In both columns, the coefficients on *Ln Rival Incident Count* are still significantly negative at the 1% level after controlling for borrowers' adverse ESG incidents. Moreover, the coefficient on the number of borrowers' negative ESG incidents, i.e., *Ln Borrower Incident Count*, is insignificant, whereas the coefficient on the indicator of borrowers' negative ESG incidents, i.e., *Borrower Incident*, is significantly positive at the 5% level (coefficient = 0.125,

[7] Since the dependent variable (*Ln AISD*) of the regression is a logarithm, the coefficient on an independent variable can be interpreted as the percentage change in the loan spread as the independent variable increases by one unit.

t-stat = 2.45). This suggests that the existence of borrowers' ESG incidents signals negative information to the loan market, increasing the borrowing cost, but the negative signal effect of borrowers' ESG incidents is not sensitive to the number of ESG incidents. Economically, the presence of borrowing firms' ESG incidents, i.e., *Borrower Incident* = 1, reduces the loan spread by 12.5%, equivalent to 37 basis points.

In Column (7), we conduct a placebo test by replacing industry rivals' negative ESG incidents with non-rival firms' negative ESG incidents. *Ln Non-rival Incident Count* is defined as the natural logarithm of one plus the number of reported ESG incidents of non-rival firms (i.e., firms without the same three-digit SIC code) during the past three years, weighted by the *Severity* and *Reach* of the news. The coefficient on *Ln Non-rival Incident* is insignificant, suggesting that adverse ESG incidents that happen to non-rival firms have no significant impact on firms' loan spreads.

[Insert Table 2 here]

We conduct a battery of robustness checks by changing various aspects of the test specification. The results of these robustness tests are tabulated in Appendix D. First, in Table D3, we use an alternative sample period from 2007 to 2019. As the start year of the data in RepRisk is 2007, there is insufficient data to calculate rivals' ESG incidents during the past three years before 2010. In this robustness check, we expand our sample period to start in 2007, and calculate rivals' ESG incidents using the limited incident records that exist before 2010. Second, we repeat the baseline analysis using robust standard errors clustered at the industry level and country-industry level. The results are presented in Table D4. Third, we use alternative measures of industry rivals' ESG incidents. In Table D5, we replace the independent variable with alternatives using rivals' negative incidents without weight, only weighted by *Severity*, and weighted by *Severity*, *Reach*, and *Novelty*. In Table D6, we use the yearly, quarterly, monthly, and industry-level average number of rivals' ESG incidents as the independent variables. Fourth, in Table D7, we use the original value of the all-in-drawn spread as the dependent variable. Fifth, in Table D8, we further control for a few factors that may affect loan pricing. Specifically, we control for cash holding (*Cash*) in Column (1) as Acharya et al. (2012) suggest that cash is inversely related to the likelihood of default over the short term while positively related to the likelihood of default over the long term. In Column (2), we include an indicator for top 10 lead arrangers (*Top 10 Arranger*) to capture the reputations of lead arrangers. Reputable arrangers have a certification effect that attenuates the moral hazard problem and adverse selection, leading to lower loan spreads

(Do and Vu 2010; Ross 2010; Chaudhry and Kleimeier 2015; Bushman et al. 2016). In Column (3), we control for any previous lending relations between lenders and borrowing firms (*Lender Relation*) as a lending relationship can mitigate the information asymmetry between lenders and borrowers, leading to lower loan spreads (Bharath et al. 2007). Sixth, in Table D9, we replace year fixed effects and firm fixed effects with country × year fixed effects and country × year × industry fixed effects. In all the robustness checks provided in Appendix D, the results are consistent with the baseline results.

Overall, the results of our regression analysis indicate that industry rivals' adverse ESG incidents are significantly negatively related to loan spreads. The negative association between rivals' ESG incidents remains economically and statistically significant even after controlling for various firm-specific and loan-specific characteristics, as well as time and firm fixed effects. The results are robust to a series of different test specifications and alternative measurements. The significantly negative effect of rivals' ESG incidents on loan spreads is consistent with the competitive effect hypothesis, showing that borrowers benefit from rivals' adverse ESG incidents.

4.4.2 Rival Types and the Intra-industry Spillover Effect

We next test how rival type alters the effect of industry rivals' ESG incidents on loan spreads. We expect incidents affecting more salient rivals to have a greater impact on firms' loan spreads. First, we decompose industry rivals' negative ESG incidents into incidents affecting public and private rivals, respectively. Specifically, we define a variable *Ln Public Rival Incident Count* as the natural logarithm of one plus the number of ESG incidents involving a firm's public rivals during the past three years, weighted by the *Severity* and *Reach* of the news. Similarly, *Ln Private Rival Incident Count* is the natural logarithm of one plus the number of ESG incidents involving a firm's private rivals during the past three years, weighted by the *Severity* and *Reach* of the news. In Table 3, we show the results of regressing loan spreads on *Ln Public Rival Incident* and *Ln Private Rival Incident* in Columns (1) and (2), respectively.

The results show that the coefficient on *Ln Public Rival Incident* is significantly negative, while that on *Ln Private Rival Incident* is insignificant. In Column (3), we run a horse race test by including *Ln Public Rival Incident* and *Ln Private Rival Incident* in one regression. Again, we only find the coefficient on *Ln Public Rival Incident* to be significantly negative. The results suggest public rivals' adverse ESG incidents have stronger impact on loan spreads.

We then decompose industry rivals' negative ESG incidents into incidents involving big and small rivals, respectively. We define *Ln Big Rival Incident Count* (*Ln Small Rival Incident Count*) as the natural logarithm of one plus the number of ESG incidents involving a firm's big (small) rivals during the past three years, weighted by the *Severity* and *Reach* of the news. A rival is defined as big (small) if their total assets are above (below) the industry median in that year. We present the results of regressing loan spreads on *Ln Big Rival Incident Count* and *Ln Small Rival Incident Count* in Columns (4) and (5) of Table 3, respectively, and we run a horse race test in Column (6). We only find the coefficient on *Ln Big Rival Incident Count* in Column (6) to be negatively significant at the 10% level, showing big rivals' negative incidents have a stronger impact on borrowers' loan spreads.

[Insert Table 3 here]

4.4.3 The Effect of Rivals' Influential ESG Incidents

We then conduct a robustness check using a different specification. In this test, we compare non-event firms' loan spreads before and after their industry rivals' influential ESG incidents. We only focus on influential ESG incidents because there are multiple ESG incidents in each industry, and the impact of those influential ones is more noticeable, which enhances the power of test. Existing literature documents that investors have limited attention (Penman 1987; Hirshleifer and Teoh 2003; Peng and Xiong 2006; Barber and Odean 2008; Dellavigna and Pollet 2009; Hirshleifer et al. 2009), and investors' inattention on corporate event can lead to a muted market response (Dellavigna and Pollet 2009; Louis and Sun 2010). Consistent with this view, the unimportant ESG incidents could raise limited attention among stakeholders, leading to an underreaction in the loan market. Therefore, this section focuses on influential ESG incidents.

Specifically, we begin by identifying influential ESG incidents. We define an adverse ESG incident as influential if *Severity*≥2, *Reach*≥2, and *Novelty*=2. To alleviate confounding impacts, we require at least a three-year clean gap between each influential incident in the same industry. We finally identify 69 valid influential ESG incidents. Next, we construct a sample using all the loans that originate in the three years before or three years after the influential ESG incidents in the same industry. We define an indicator *Post* which equals one if a loan originates within the three years after industry rivals' influential ESG incidents, and zero if it originates within the three years before industry rivals' influential ESG incidents. In Columns (1) and (2) of Table 4, we present results from testing the differences in the loan spreads before and after rivals' influential ESG incidents. The dependent variable is the loan

spread (*Ln AISD*). The independent variable *Post* captures the different effect on the loan spreads before and after industry rivals' influential ESG incidents. All the firm-specific and loan-specific characteristics used as control variables are the same as in Table 2, and we also control for year and firm fixed effects. In Columns (1) and (2), the coefficients on *Post* are −0.131 (*t*-stat = −2.27) and −0.143 (*t*-stat = −2.23), respectively, showing that the cost of loans originating after rivals' adverse ESG incidents is significantly lower than that of loans originating before the incidents.

In Columns (3) and (4) of Table 4, we show the results of a further test of the dynamic impact of rivals' influential ESG incidents. Specifically, we replace *Post* with five indicators, namely $Before^{-3}$, $Before^{-2}$, $After^{+1}$, $After^{+2}$, and $After^{+3}$. The five indicators are set to one if the loan originated in the third year before rivals' adverse ESG incidents ($Before^{-3}$), the second year before ($Before^{-2}$), the first year after ($After^{+1}$), the second year after ($After^{+2}$), and the third year after ($After^{+3}$), respectively, and zero otherwise. In Columns (3) and (4), the significantly negative coefficients on $After^{+2}$ and $After^{+3}$ suggest that loans that originate in the second and third years after rivals' adverse ESG incidents have lower interest rates. This finding is consistent with the competitive effect hypothesis.

[Insert Table 4 here]

4.4.4 Heterogeneity of Baseline Results: Borrowers' ESG Risk, Industry Competition, and Market Share

In this subsection, we first examine whether the negative association between rivals' adverse ESG incidents and loan spreads varies among borrowers with different ESG risks. We expect firms with better ESG practices to benefit more from industry rivals' adverse ESG incidents. This is because, after the exposure to ESG incidents, lenders tend to be particularly concerned about firms with poor ESG profiles. Those firms are potentially related to higher regulatory and litigation risk, leading to higher credit risk. Moreover, lenders will be faced with litigation and reputational risk directly if they lend to firms with poor ESG profiles. In contrast, borrowers with efficient implementation of ESG practices maintain competitive advantages (Hart 1995; Jones 1995; Aragón-Correa 1998), have better employee engagement (Turban and Greening 1996; Flammer and Luo 2017), and have better financial performance (Flammer 2015a; Ferrell et al. 2016; Lins et al. 2017; Albuquerque et al. 2019). When industry rivals experience adverse ESG incidents, the well-behaved will arguably be more competitive and better positioned to attract more favourable loan contract terms. Therefore, we expect the firms with better ESG performance to have lower interest rates

after industry rivals' adverse ESG incidents. To test this prediction, we construct an indicator variable *Low ESG Risk,* which equals one if a firm's number of ESG incidents during the past three years (weighted by *Severity* and *Reach*) is below or equal to the industry median and zero otherwise. Then, we interact *Ln Rival Incident Count* with *Low ESG Risk*, forming our variable of interest, whose coefficient is expected to be negative. The results are presented in Column (1) of Table 5. In line with our conjecture, *Ln Rival Incident Count* × *Low ESG Risk* has a negatively significant effect at the 5% level, showing firms with better ESG profiles have lower loan spreads if their industry rivals suffer adverse ESG incidents.

We then test how industry competition affects the negative association between rivals' adverse ESG incidents and firms' loan spreads. Prior literature on bankruptcy suggests the competition effect to be greater in more concentrated industries (Lang and Stulz 1992; Jorion and Zhang 2007) because firms in concentrated industries are more likely to benefit from the exit of an industry-dominant competitor through actions such as raising the price of their output (Lang and Stulz 1992). However, ESG practices play a more significant role in whether firms obtain competitive advantages in markets where competition is intense (Flammer 2015b; Fernández-Kranz and Santaló 2010; Dupire and M'Zali 2018). The efficient implementation of ESG practices can differentiate firms from competitors and attract customers in a competitive market where the price competition is strong (Baron 2001; Fisman et al. 2006; Siegel and Vitaliano 2007; Flammer 2015b). Following the exposure of industry rivals' misconduct over ESG issues, a firm can differentiate itself and exploit product-market opportunities as customers are more likely to switch to products offered by firms free from misconduct (Kang et al. 2021). The stronger market power could decrease credit risk (Bonfim 2009) and the cost of capital (Sullivan 1978).

To test the impact of industry competition, we construct an indicator variable *High Competition,* which equals one if an industry's Herfindahl-Hirschman index is below or equal to the sample median and zero otherwise. The Herfindahl-Hirschman index is computed as the sum of the squared fractions of each firm's sales over the total sales of the industry in each year. A lower Herfindahl-Hirschman index indicates a more competitive industry. Then, we interact *Ln Rival Incident Count* with *High Competition*, forming our variable of interest. As presented in Column (2) of Table 5, the coefficient on the interaction term *Ln Rival Incident Count* × *High Competition* is significantly negative at 5%, suggesting the competition effect caused by rivals' ESG concerns is more pronounced in competitive industries. In addition, the coefficient on *High Competition* is positively significant at 1%,

suggesting firms in highly competitive industries have higher loan spreads. This is consistent with Valta (2012), who finds that the cost of bank debt is higher for firms in more competitive product markets.

[Insert Table 5 here]

We further verify whether firms acquire advantages from rivals' adverse ESG incidents by gaining a greater market share. In Table 6, we present the results of regressing firms' market share on *Ln Rival Incident Count*. The dependent variables in Columns (1), (2), and (3) are the ratio of a firm's sales to the total sales of the industry in year *t* (*Market Share*$_t$), the ratio of a firm's sales to the total sales of the industry during years *t-2* to *t* (*Market Share*$_{t-2, t}$), and the average ratio of a firm's sales to the total sales of the industry from years *t-2* to *t* (*Avg Market Share*$_{t-2, t}$). As shown in Table 6, the coefficients on *Ln Rival Incident Count* across all columns are significantly positive at 5%, indicating an increase in firms' market share due to rivals' adverse ESG incidents.

[Insert Table 6 here]

4.4.5 The Effect of Different Types of Incidents

ESG incidents of various natures might have varying value implications for stakeholders and lenders (Bouslah et al. 2013; Sassen et al. 2016). In this section, we examine heterogeneity in the effect of rivals' ESG incidents across different types of incidents. We define *Ln Rival Environmental Incident Count*, *Ln Rival Social Incident Count*, *Ln Rival Governance Incident Count*, and *Ln Rival Cross-cutting Incident Count* as the natural logarithm of one plus the number of environmental, social, governance, and cross-cutting incidents of a firm's industry rivals during the past three years, weighted by the *Severity* and *Reach* of the news respectively. We regress borrowers' loan spreads on *Ln Rival Environmental Incident Count*, *Ln Rival Social Incident Count*, *Ln Rival Governance Incident Count*, and *Ln Rival Cross-cutting Incident Count*, and present the results in Table 7. Across all columns in Table 7, the coefficients on the above four variables are significantly negative. This indicates that all types of industry rivals' incidents significantly decrease borrowing firms' loan spreads.

[Insert Table 7 here]

4.5 Conclusion

In this paper, we find that a firm's loan spreads decrease substantially with its industry rivals' ESG incidents' intensity and influence. This spillover effect holds in a battery of

robustness checks using alternative methods, specifications, and measures, and is present both across all incidents and for incidents falling into four subcategories of ESG issues. Mechanism analysis shows that the competition effect is more relevant than the contagion effect in explaining this result. Our findings show that a borrower's industrial ESG environment is essential to its cost of credit. A firm obtains substantial financial benefit by distinguishing itself from rivals who fail to deliver commendable ESG performance. Our research adds to the growing literature on the consequences of ESG practices and the intra-industry spillover effects on the loan markets. Our findings strongly suggest that peer effects could impact firms' financing costs substantially and motivate firms to enhance their ESG conduct. Such peer effects could form the foundation for government or corporate policies promoting companies' environmental and social responsibilities.

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Table 1
Summary Statistics for Variables Used in the Main Analysis

This table reports the summary statistics for variables used in the main analysis. The sample period is 2010 to 2019 which includes 2,083 loan-facility level observations. Definition and details for the variables can be found in Appendix A. The continuous variables are winsorized at the 1st and 99th percentiles.

	Mean	SD	P25	P50	P75
Panel A: ESG Incidents (Number of observations = 2,083, Number of Borrower Firms = 706)					
Rival Incident Count	98.85	187.86	0.00	10.00	87.00
Ln Rival Incident Count	2.53	2.29	0.00	2.40	4.48
Rival Incident	0.66	0.47	0.00	1.00	1.00
Rival Severe Incident	0.55	0.50	0.00	1.00	1.00
Borrowers Incident Count	4.42	17.29	0.00	0.00	0.00
Ln Borrower Incident Count	0.49	1.08	0.00	0.00	0.00
Borrower Incident	0.22	0.42	0.00	0.00	0.00
Panel B: Firm Attributes (Number of observations = 2,083, Number of Borrower Firms = 706)					
Total Assets (in million $)	3,383.00	10,479.09	172.96	546.77	1,985.79
Ln Total Assets	20.19	1.91	18.97	20.12	21.41
Leverage	0.65	0.25	0.47	0.64	0.81
ROA	0.04	0.10	0.00	0.03	0.07
Loss	0.25	0.43	0.00	0.00	1.00
Tangibility	0.19	0.25	0.01	0.07	0.26
Panel C: Loan Attributes (Number of observations = 2,083, Number of Borrower Firms = 706)					
AISD (in bps)	299.98	150.85	185.00	290.00	400.00
Ln AISD	5.55	0.61	5.22	5.67	5.99
Maturity (in month)	66.05	33.84	50.00	60.00	72.00
Ln Maturity	4.07	0.51	3.91	4.09	4.28
Loan Amount (in million $)	435.04	768.42	50.07	169.85	487.93
Ln Loan Amount	18.80	1.63	17.73	18.95	20.01
Performance Pricing	0.06	0.23	0.00	0.00	0.00
Number of Lenders	7.14	6.11	3.00	5.00	9.00
Syndication	0.93	0.25	1.00	1.00	1.00
Financial Covenant Count	0.17	0.51	0.00	0.00	0.00

Table 2
The Effect of Industry Rivals' ESG Incidents on Firms' Loan Spreads

The table below reports the results of the effect of industry rivals' ESG incidents on firms' loan spreads. The dependent variable is *Ln AISD*, the natural logarithm of all-in-drawn spread. The variable *Ln Rival Incident Count* is the natural logarithm of one plus the number of reported ESG incidents of a firm's industry rivals during the past three years weighted by the *Severity* and *Reach* of the news. All variables are defined in Appendix A. The sample period is 2010-2019. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Ln AISD*						
	Number of Rivals' Incidents		Dummy of Rivals' Incidents		Control for Borrowers' Incidents		Placebo Test
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Ln Rival Incident Count	−0.051***	−0.055***			−0.051***	−0.056***	
	(−2.80)	(−3.15)			(−2.88)	(−3.21)	
Rival Incident			−0.035				
			(−0.66)				
Rival Severe Incident				−0.170**			
				(−2.28)			
Ln Borrower Incident Count					−0.062		
					(−1.00)		
Borrower Incident						0.125**	
						(2.45)	
Ln Non-rival Incident Count							0.004
							(0.72)
Ln Total Assets		0.002	0.003	0.001	−0.002	0.002	0.003
		(0.08)	(0.13)	(0.04)	(−0.06)	(0.09)	(0.10)
Leverage		−0.086	−0.088	−0.091	−0.093	−0.075	−0.083
		(−0.84)	(−0.82)	(−0.85)	(−0.89)	(−0.73)	(−0.77)
ROA		−0.053	−0.010	0.011	−0.059	−0.037	−0.005
		(−0.31)	(−0.06)	(0.07)	(−0.33)	(−0.23)	(−0.03)
Loss		0.120	0.118	0.123	0.115	0.126	0.117
		(1.53)	(1.47)	(1.55)	(1.53)	(1.61)	(1.45)
Tangibility		0.092	0.086	0.098	0.072	0.138	0.097
		(0.41)	(0.37)	(0.43)	(0.31)	(0.63)	(0.42)
Ln Maturity		0.102***	0.098***	0.101***	0.103***	0.103***	0.098***

		(3.90)	(3.73)	(3.88)	(3.89)	(3.96)	(3.75)
Ln Loan Amount		−0.006	−0.007	−0.007	−0.006	−0.005	−0.007
		(−0.68)	(−0.81)	(−0.81)	(−0.71)	(−0.64)	(−0.85)
Performance Pricing		0.000	−0.001	0.001	−0.002	0.001	−0.000
		(0.00)	(−0.02)	(0.01)	(−0.03)	(0.01)	(−0.00)
Number of Lenders		−0.012*	−0.012*	−0.012	−0.012*	−0.012*	−0.012
		(−1.68)	(−1.65)	(−1.60)	(−1.72)	(−1.69)	(−1.62)
Syndication		−0.052	−0.053	−0.055	−0.051	−0.051	−0.054
		(−0.79)	(−0.80)	(−0.84)	(−0.77)	(−0.78)	(−0.82)
Financial Covenant Count		−0.023	−0.028	−0.031	−0.027	−0.019	−0.031
		(−0.49)	(−0.59)	(−0.67)	(−0.56)	(−0.41)	(−0.67)
Loan Type Indicators	No	Yes	Yes	Yes	Yes	Yes	Yes
Loan Purpose Indicators	No	Yes	Yes	Yes	Yes	Yes	Yes
Constant	5.678***	5.482***	5.376***	5.482***	5.593***	5.423***	5.361***
	(124.27)	(8.98)	(8.74)	(8.95)	(8.76)	(9.08)	(8.74)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2,083	2,083	2,083	2,083	2,083	2,083	2,083
R^2	0.850	0.867	0.865	0.867	0.868	0.868	0.865

Table 3
Rival Types and the Effect of Industry Rivals' ESG Incidents on Firms' Loan Spread

The table below reports the results of how rival type alters the effect of industry rivals' ESG incidents on loan spreads. The variable *Ln Public Rival Incident Count* (*Ln Private Rival Incident Count*) is the natural logarithm of one plus the number of reported ESG incidents of a firm's public (private) industry rivals during the past three years weighted by the *Severity* and *Reach* of the news. The variable *Ln Big Rival Incident Count* (*Ln Small Rival Incident Count*) is the natural logarithm of one plus the number of reported ESG incidents of a firm's big (small) rivals during the past three years weighted by the *Severity* and *Reach* of the news. The control variables are the same as Table 2, including *Ln Total Assets*, *Leverage*, *ROA*, *Loss*, *Tangibility*, *Ln Maturity*, *Ln Loan Amount*, *Performance Pricing*, *Number of Lenders*, *Syndication* and *Financial Covenant Count*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The sample period is 2010-2019. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Ln AISD*					
	Incidents of Public vs. Private Rivals			Incidents of Big vs. Small Rivals		
	(1)	(2)	(3)	(4)	(5)	(6)
Ln Public Rival Incident Count	−0.060***		−0.058***			
	(−5.18)		(−4.68)			
Ln Private Rival Incident Count		−0.029	−0.008			
		(−1.45)	(−0.37)			
Ln Big Rival Incident Count				−0.018		−0.022*
				(−1.53)		(−1.76)
Ln Small Rival Incident Count					0.022	0.036
					(0.52)	(0.83)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Loan Type Indicators	Yes	Yes	Yes	Yes	Yes	Yes
Loan Purpose Indicators	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
N	2,083	2,083	2,083	2,083	2,083	2,083
R^2	0.869	0.866	0.869	0.866	0.865	0.866

Table 4

The Effect of Industry Rivals' Influential ESG Incidents on Loan Spreads

The table below reports the results of the analysis of the impact of industry rivals' influential ESG incidents on firms' loan spreads. The variable *Post* is an indicator variable equals to one if a loan is originated within three years after an influential ESG incident reportedly occurring to at least one industry rival and zero if a loan is originated within three years before the ESG incidents. $Before^{-3}$ and $Before^{-2}$ are indicator variables that equal to one if a loan is originated in the third and the second year before an influential ESG incident reportedly occurring to at least one industry rival, and zero otherwise. $After^{+1}$, $After^{+2}$, and $After^{+3}$ are indicator variables that equal to one if a loan is originated in the first, the second, and the third year after an influential ESG incident reportedly occurring to at least one industry rival, and zero otherwise. The control variables are the same as Table 2, including *Ln Total Assets*, *Leverage*, *ROA*, *Loss*, *Tangibility*, *Ln Maturity*, *Ln Loan Amount*, *Performance Pricing*, *Number of Lenders*, *Syndication* and *Financial Covenant Count*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The sample period is 2010-2019. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Ln AISD*			
	(1)	(2)	(3)	(4)
Post	−0.131**	−0.143**		
	(−2.27)	(−2.23)		
$Before^{-3}$			−0.009	−0.035
			(−0.10)	(−0.30)
$Before^{-2}$			−0.068	−0.105
			(−1.00)	(−1.45)
$After^{+1}$			−0.130*	−0.129
			(−1.66)	(−1.48)
$After^{+2}$			−0.142	−0.180**
			(−1.55)	(−2.19)
$After^{+3}$			−0.335***	−0.387***
			(−2.96)	(−3.82)
Controls	No	Yes	No	Yes
Loan-Type Indicators	Yes	Yes	Yes	Yes
Loan-Purpose Indicators	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	587	587	587	587
R^2	0.876	0.890	0.879	0.894

Table 5
The Effect of Borrowers' ESG Risk and Industry Competition

The table below reports the results of the effect of borrower firms' ESG risk and industry competition on firms' loan spread. *Low ESG Risk* is an indicator variable equals one if a firm's ESG incidents is below or equal the median of firms' ESG incident counts in an industry and zero otherwise. *High Competition* is an indicator variable that equal to one if an industry's Herfindahl-Hirschman index is below or equal to the median, and zero otherwise. The control variables are the same as Table 2, including *Ln Total Assets*, *Leverage*, *ROA*, *Loss*, *Tangibility*, *Ln Maturity*, *Ln Loan Amount*, *Performance Pricing*, *Number of Lenders*, *Syndication* and *Financial Covenant Count*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The sample period is 2010-2019. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Ln AISD*	
	(1)	(2)
Ln Rival Incident Count × Low ESG Risk	−0.043**	
	(−2.16)	
Low Borrower ESG Risk	0.112	
	(1.21)	
Ln Rival Incident Count × High Competition		−0.065**
		(−2.05)
High Competition		0.376***
		(2.72)
Ln Rival Incident Count	−0.025	−0.045
	(−1.06)	(−1.41)
Controls	Yes	Yes
Loan Type Indicators	Yes	Yes
Loan Purpose Indicators	Yes	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes
N	2,083	1,201
R^2	0.868	0.849

Table 6
The Effect of Industry Rivals' ESG Incidents on Market Share

The table below reports the results of the effect of industry rivals' ESG incidents on firms' market share. All variables are defined in Appendix A. The sample period is 2010-2019. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Market Share*$_t$	Dependent = *Market Share*$_{t-2,\,t}$	Dependent = *Average Market Share*$_{t-2,\,t}$
	(1)	(2)	(3)
Ln Rival Incident Count	0.077**	0.243**	0.140**
	(2.04)	(2.02)	(2.02)
Ln Total Assets	0.378**	0.476	0.296
	(2.13)	(1.48)	(1.60)
Leverage	0.877	1.496	0.891
	(1.28)	(0.87)	(0.97)
ROA	0.488	0.396	0.723
	(1.43)	(0.42)	(1.14)
Loss	−0.143	−0.369	−0.113
	(−1.33)	(−1.03)	(−0.57)
Ln Firm Age	1.539***	4.242***	2.554***
	(3.48)	(3.06)	(3.24)
Ln Sales Volatility	0.101**	0.370*	0.267**
	(2.02)	(1.76)	(2.00)
Constant	−13.665***	−29.434***	−18.147***
	(−2.85)	(−2.86)	(−3.17)
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
N	1,808	1,808	1,808
R^2	0.939	0.847	0.927

Table 7

Incident Types and the Effect of Industry Rivals' Incidents on Firms' Loan Spread

The table below reports the results of the how incident types alter the effect of industry rivals' incidents on firms' loan spreads. *Ln Rival Environmental Incident Count, Ln Rival Social Incident Count, Ln Rival Governance Incident Count, Ln Rival Cross-cutting Incident Count* are the natural logarithm of one plus the number of environmental, social, governance, and cross-cutting incidents of a firm's industry rivals during the past three years weighted by the *Severity* and *Reach* of the news. The control variables are the same as Table 2, including *Ln Total Assets*, *Leverage*, *ROA*, *Loss*, *Tangibility*, *Ln Maturity*, *Ln Loan Amount*, *Performance Pricing*, *Number of Lenders*, *Syndication* and *Financial Covenant Count*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The sample period is 2010-2019. The t-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Ln AISD*			
	(1)	(2)	(3)	(4)
Ln Rival Environmental Incident Count	−0.067***			
	(−2.83)			
Ln Rival Social Incident Count		−0.058***		
		(−2.99)		
Ln Rival Governance Incident Count			−0.036**	
			(−2.05)	
Ln Rival Cross-cutting Incident Count				−0.051***
				(−3.11)
Controls	Yes	Yes	Yes	Yes
Loan Type Indicators	Yes	Yes	Yes	Yes
Loan Purpose Indicators	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	2,083	2,083	2,083	2,083
R^2	0.867	0.867	0.866	0.867

Appendix A: Variable Definitions

Variable Definitions

Panel A: Variables Appearing in the Main Body

Variable	Definition
After^{+1}	An indicator variable that equals one if a loan originated in the first year after an influential ESG incident reportedly occurred involving at least one industry rival, and zero otherwise. An influential ESG incident has scores of *Severity*≥2, *Reach*≥2, and *Novelty*=2.
After^{+2}	An indicator variable that equals one if a loan originated in the second year after an influential ESG incident reportedly occurred involving at least one industry rival, and zero otherwise.
After^{+3}	An indicator variable that equals one if a loan originated in the third year after an influential ESG incident reportedly occurred involving at least one industry rival, and zero otherwise.
AISD	All-in-spread drawn is defined as the amount the borrower pays in basis points over the London Interbank Borrowing Rate (LIBOR) or LIBOR equivalent for each dollar drawn down.
Before^{-2}	An indicator variable that equals one if a loan originated in the second year before an influential ESG incident reportedly occurred involving at least one industry rival, and zero otherwise.
Before^{-3}	An indicator variable that equals one if a loan originated in the third year before an influential ESG incident reportedly occurred involving at least one industry rival, and zero otherwise.
Borrower Incident Count	The number of reported ESG incidents involving a firm during the past three years, weighted by the *Severity* and *Reach* of the news.
Borrower Incident	An indicator variable that equals one if a borrowing firm has at least one reported ESG incident during the past three years.
Financial Covenant Count	The number of financial covenants for a loan facility.
High Competition	An indicator variable that equals one if an industry's Herfindahl-Hirschman index is below or equal to the sample median, and zero otherwise. The Herfindahl-Hirschman index is computed as the sum of the squared fractions of each individual firm's sales over the total sales of the industry in each year.
Leverage	The sum of total liabilities scaled by total assets.
Ln AISD	The natural logarithm of *AISD*.
Ln Big Rival Incident Count	The natural logarithm of one plus the number of reported ESG incidents involving a firm's big rivals in the past three years, weighted by the *Severity* and *Reach* of the news. A firm is defined as big if its total assets are above the industry median in a year.
Ln Borrower Incident Count	The natural logarithm of one plus the number of reported ESG incidents involving a firm in the past three years, weighted by the *Severity* and *Reach* of the news.
Ln Firm Age	The natural logarithm of firm age.
Ln Loan Amount	The natural logarithm of a loan facility (in USD).
Ln Maturity	The natural logarithm of the number of months to maturity of a loan facility.

Ln Non-rival Incident Count	The natural logarithm of one plus the number of reported ESG incidents involving non-rival firms in the past three years, weighted by the *Severity* and *Reach* of the news.
Ln Private Rival Incident Count	The natural logarithm of one plus the number of reported ESG incidents involving a firm's private industry rivals in the past three years, weighted by the *Severity* and *Reach* of the news.
Ln Public Rival Incident Count	The natural logarithm of one plus the number of reported ESG incidents involving a firm's public industry rivals in the past three years, weighted by the *Severity* and *Reach* of the news.
Ln Rival Cross-cutting Incident Count	The natural logarithm of one plus the number of cross-cutting incidents involving a firm's industry rivals in the past three years, weighted by the *Severity* and *Reach* of the news.
Ln Rival Environmental Incident Count	The natural logarithm of one plus the number of environmental incidents involving a firm's industry rivals in the past three years, weighted by the *Severity* and *Reach* of the news.
Ln Rival Governance Incident Count	The natural logarithm of one plus the number of governance-related incidents involving a firm's industry rivals in the past three years, weighted by the *Severity* and *Reach* of the news.
Ln Rival Incident Count	The natural logarithm of one plus the number of reported ESG incidents involving a firm's industry rivals in the past three years, weighted by the *Severity* and *Reach* of the news.
Ln Rival Social Incident Count	The natural logarithm of one plus the number of social incidents involving a firm's industry rivals in the past three years, weighted by the *Severity* and *Reach* of the news.
Ln Total Assets	The natural logarithm of total assets (in USD).
Ln Small Rival Incident Count	The natural logarithm of one plus the number of reported ESG incidents involving a firm's small rivals in the past three years, weighted by the *Severity* and *Reach* of the news. A firm is defined as small if its total assets are below the industry median.
Loan Purpose Indicators	A series of indicator variables for the purposes of loan facilities, including corporate purpose, working capital, debt repayment, acquisition, and backup line for commercial paper.
Loan Type Indicators	A series of indicator variables for the types of loan facilities, including term loan, revolver greater than one year, revolver less than one year, and 364-day facilities.
Loss	An indicator variable that equals one if a firm's net income is negative and zero otherwise.
Low ESG Risk	An indicator variable that equals one if a firm's number of ESG incidents (weighted by *Severity* and *Reach*) is below or equal the median in an industry and zero otherwise.
Market Share	The ratio of a firm's sales to the total sales of the industry in that year.
Number of Lenders	Number of lender banks in a loan deal.
Performance Pricing	An indicator that equals one if a loan contract includes performance pricing provisions and zero otherwise.
Post	An indicator variable that equals one if a loan originated within the three years after an influential ESG incident reportedly occurred

	involving at least one industry rival and zero if a loan originated within the three years before such an influential ESG incident.
Rival Incident Count	The number of reported ESG incidents involving a firm's industry rivals in the past three years, weighted by the *Severity* and *Reach* of the news.
Rival Incident	An indicator variable that equals one if a firm's industry rivals have reported at least one ESG incident in the past three years, and zero otherwise.
Rival Severe Incident	An indicator variable that equals one if a firm's industry rivals have reported at least one ESG incident with high severity (*Severity*≥2) in the past three years, and zero otherwise.
ROA	Net income scaled by total assets.
Ln Sales Volatility	The natural logarithm of the standard deviation of total sales over the previous three years.
Syndication	An indicator variable that equals one if a loan is syndicated and zero otherwise.
Tangibility	The ratio of fixed assets to total assets.

Panel B: Variables Appearing in the Appendix

Variables	**Definitions**
Cash	Cash and equivalents scaled by total assets.
Lending Relation	An indicator variable that equals one if the lending bank has previously supplied one or more loans to the company in the past three years and zero otherwise.
Ln Ind. Avg. of Rival Incident Count	The natural logarithm of one plus the number of reported ESG incidents involving a firm's industry rivals, averaged across a firm's industry rivals, in the past three years, weighted by the *Severity* and *Reach* of the news.
Ln Monthly Avg. of Rival Incident Count	The natural logarithm of one plus the monthly average number of reported ESG incidents involving a firm's industry rivals in the past three years, weighted by the *Severity* and *Reach* of the news.
Ln Non-weighted Rival Incident Count	The natural logarithm of one plus the number of reported ESG incidents involving a firm's industry rivals in the past three years.
Ln Quarterly Avg. of Rival Incident Count	The natural logarithm of one plus the quarterly average number of reported ESG incidents involving a firm's industry rivals in the past three years, weighted by the *Severity* and *Reach* of the news.
Ln Severity Reach and Novelty Weighted Rivals Incident Count	The natural logarithm of one plus the number of reported ESG incidents involving a firm's industry rivals in the past three years, weighted by the *Severity*, *Reach* and *Novelty* of the news.
Ln Severity weighted Rival Incident Count	The natural logarithm of one plus the number of reported ESG incidents involving a firm's industry rivals in the past three years, weighted by the *Severity* of the news.
Ln Yearly Avg. of Rival Incident Count	The natural logarithm of one plus the yearly average number of reported ESG incidents involving a firm's industry rivals in the past three years, weighted by the *Severity* and *Reach* of the news.
Top 10 Arranger	An indicator variable that equals one if a loan syndication is led by a top 10 arranger and zero otherwise.

Appendix B: ESG Incidents of European Firms

Table B1 ESG Incidents of European Firms

The table below reports the ESG incidents of European firms. We report all the incidents in RepRisk that can be successfully matched to Amadeus from 2007 to 2019. A piece of reported news in RepRisk could be related to multi-types of issues below.

Panel A: Firm Counts		
Number of Firms	Public Firms	Non-public Firms
9,144	1,163	7,981
	12.72%	87.28%
Panel B: Main Types Incident Counts		
Environmental Issues	18,633	
Social Issues	30,067	
Governance Issues	27,051	
Cross-cutting Issues	42,576	
Total Incidents	65,941	
Panel C: 31 Types Incident Counts		
Environmental Issues	Animal mistreatment	916
	Climate change, GHG emissions, and global pollution	5,282
	Impacts on landscapes, ecosystems and biodiversity	13,141
	Local pollution	8,300
	Overuse and wasting of resources	1,399
	Waste issues	3,695
	Other environmental issues	19
Social Issues	Child labour	1,903
	Discrimination in employment	1,559
	Forced labour	2,751
	Freedom of association and collective bargaining	3,027
	Human rights abuses and corporate complicity	10,210
	Impacts on communities	13,250
	Local participation issues	2,938
	Occupational health and safety issues	7,722
	Poor employment conditions	8,648
	Social discrimination	487
	Other social issues	10
Governance Issues	Anti-competitive practices	7,578
	Corruption, bribery, extortion and money laundering	11,145
	Executive compensation issues	1,147
	Fraud	7,507
	Misleading communication	3,647
	Tax evasion	2,110
	Tax optimization	1,140
Cross-cutting Issues	Controversial products and services	5,865
	Products (health and environmental issues)	6,775
	Supply chain issues	8,555
	Violation of international standards	2,693
	Violation of national legislation	28,620
Other		56

Appendix C: Description of Name-Matching between Multiple Databases

We implement a matching procedure following Li and Wu (2020) based on the similarity of firm names to linked firms in RepRisk, Amadeus, and DealScan.

1. To increase the matching quality, we standardize firm names in those databases. Specifically, we standardize common words, characters, prefixes, and suffixes and trim the white spaces and special characters. For example, "*Hill Samuel & Co Ltd*" is standardized into a string "*hillsamuelcompanylimited*".

2. In some cases, DealScan and RepRisk contain additional firm name information. The additional name information is indicated as "*ex*", "*fka*", "*formerly known*", "*also known as*" etc. We generate alternative firm names into separate fields and standardize those alternative firm names. For example, "*Enodis Plc [ex-S&W Berisford Plc]*" is separated into "*Enodis Plc*" and "*S&W Berisford Plc*", then we standardized those two names for matching.

3. We define a match if we obtain any of the following: (1) an exact match of the standardized firm names and headquarter countries; (2) a fuzzy string match of the standardized company names with a matching score greater than or equal to 99, and exact match of headquarter countries.

4. To ensure matching accuracy, for all the potential fuzzy matches, we manually review all those matched pairs with matching scores greater than or equal to 99 and require the differences between the lengths of matched name strings to be less than 15 characters. We only review matched pairs with string length differences of less than 15 characters because a greater difference in string lengths usually indicates a higher likelihood of inappropriate matching. For example, the partial fuzzy-wuzzy matching score between "*Biac Brussels International Airport Co*" and firm "*BIA*" is 100, but they differ greatly in string length and are not an acceptable matched pair. We also randomly select 1% of the matched pairs with matching scores greater than or equal to 99 and require the difference between the lengths of the name strings to be more than 15 characters. Not surprisingly, there are scarcely any acceptable matched pairs.

5. We only keep the best-matched pairs if there are multiple matches for a firm.

Appendix D: Additional Results

Table D1
Sample Distribution by Year

Year	Number of observations	Percentage
2010	93	4.46%
2011	232	11.14%
2012	196	9.41%
2013	219	10.51%
2014	252	12.10%
2015	243	11.67%
2016	187	8.98%
2017	277	13.30%
2018	261	12.53%
2019	123	5.90%
Total	2,083	100%

Table D2
Sample Distribution by Country

Country	Number of observations	Percentage
Austria	3	0.14%
Belgium	50	2.40%
Bulgaria	2	0.10%
Croatia	3	0.14%
Czech Republic	32	1.54%
Denmark	7	0.34%
Finland	7	0.34%
France	306	14.69%
Germany	100	4.80%
Gibraltar	2	0.10%
Greece	4	0.19%
Iceland	3	0.14%
Ireland	18	0.86%
Italy	98	4.70%
Luxembourg	62	2.98%
Malta	5	0.24%
Netherlands	49	2.35%
Norway	69	3.31%
Poland	50	2.40%
Portugal	3	0.14%
Romania	27	1.30%
Russia	9	0.43%
Slovakia	7	0.34%
Spain	399	19.16%
Sweden	24	1.15%
Switzerland	2	0.10%
United Kingdom	742	35.62%
Total	2,083	100.00%

Table D3
Robustness: Different Sample Period

The table below reports the results for the robustness checks using sample period from 2007 to 2019. The control variables are the same as Table 2, including *Ln Total Assets*, *Leverage*, *ROA*, *Loss*, *Tangibility*, *Ln Maturity*, *Ln Loan Amount*, *Performance Pricing*, *Number of Lenders*, *Syndication* and *Financial Covenant Count*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Ln AISD*	
	(1)	(2)
Ln Rival Incident Count	−0.048***	−0.052***
	(−2.63)	(−3.02)
Controls	No	Yes
Loan Type Indicators	Yes	Yes
Loan Purpose Indicators	Yes	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes
N	2,135	2,135
R^2	0.859	0.866

Table D4

Robustness: Standard Errors Clustered at Different Levels

The table below reports the results of the robustness checks using standard errors clustered at different levels. The control variables are the same as Table 2, including *Ln Total Assets*, *Leverage*, *ROA*, *Loss*, *Tangibility*, *Ln Maturity*, *Ln Loan Amount*, *Performance Pricing*, *Number of Lenders*, *Syndication* and *Financial Covenant Count*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The sample period is 2010-2019. The *t*-statistics are in parentheses and clustered at different levels. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Ln AISD*	
	(1)	(2)
Ln Rival Incident Count	−0.055**	−0.055***
	(−2.59)	(−2.66)
Controls	No	Yes
Loan Type Indicators	Yes	Yes
Loan Purpose Indicators	Yes	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes
N	2,083	2,083
R^2	0.867	0.867
Level of Cluster	Industry	Country-Industry

Table D5

Robustness: Industry Rivals' ESG Incidents Measured by Different Weights

The table below reports the results for the robustness checks using industry rivals' incidents with different weights. The control variables are the same as Table 2, including *Ln Total Assets*, *Leverage*, *ROA*, *Loss*, *Tangibility*, *Ln Maturity*, *Ln Loan Amount*, *Performance Pricing*, *Number of Lenders*, *Syndication* and *Financial Covenant Count*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The sample period is 2010-2019. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Ln AISD*		
	(1)	(2)	(3)
Ln Non-weighted Rival Incident Count	−0.073***		
	(−3.53)		
Ln Severity Weighted Rival Incident Count		−0.071***	
		(−3.60)	
Ln Severity Reach and Novelty Weighted Rivals Incident Count			−0.050***
			(−3.04)
Controls	Yes	Yes	Yes
Loan Type Indicators	Yes	Yes	Yes
Loan Purpose Indicators	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
N	2,083	2,083	2,083
R^2	0.868	0.868	0.867

Table D6

Robustness: Average Number of Industry Rivals' ESG Incidents

The table below reports the results for the robustness checks using average number of industry rivals' incidents. The control variables are the same as Table 2, including *Ln Total Assets*, *Leverage*, *ROA*, *Loss*, *Tangibility*, *Ln Maturity*, *Ln Loan Amount*, *Performance Pricing*, *Number of Lenders*, *Syndication* and *Financial Covenant Count*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The sample period is 2010-2019. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Ln AISD*			
	(1)	(2)	(3)	(4)
Ln Yearly Avg. of Rival Incident Count	−0.063***			
	(−3.20)			
Ln Quarterly Avg. of Rival Incident Count		−0.081***		
		(−3.31)		
Ln Monthly Avg. of Rival Incident Count			−0.101***	
			(−3.28)	
Ln Ind. Avg. of Rival Incident Count				−0.065***
				(−2.83)
Controls	Yes	Yes	Yes	Yes
Loan Type Indicators	Yes	Yes	Yes	Yes
Loan Purpose Indicators	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	2,083	2,083	2,083	2,083
R^2	0.867	0.867	0.867	0.867

Table D7

Robustness: Alternative Measure of Loan Spread

The table below reports the results for the robustness checks using the original value of all-in-drawn spread. The dependent variable is *AISD*, i.e., all-in-drawn spread in basis points. The control variables are the same as Table 2, including *Ln Total Assets*, *Leverage*, *ROA*, *Loss*, *Tangibility*, *Ln Maturity*, *Ln Loan Amount*, *Performance Pricing*, *Number of Lenders*, *Syndication* and *Financial Covenant Count*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *AISD*	
	(1)	(2)
Ln Rival Incident Count	−13.658***	−13.707***
	(−2.96)	(−3.07)
Controls	No	Yes
Loan Type Indicators	Yes	Yes
Loan Purpose Indicators	Yes	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes
N	2,083	2,083
R^2	0.764	0.792

Table D8
Robustness: Additional Control Variables

The table below reports the results for the robustness checks including additional control variables. The control variables are the same as Table 2, including *Ln Total Assets*, *Leverage*, *ROA*, *Loss*, *Tangibility*, *Ln Maturity*, *Ln Loan Amount*, *Performance Pricing*, *Number of Lenders*, *Syndication* and *Financial Covenant Count*. All variables are defined in Appendix A. The sample period is 2010-2019. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Ln AISD*		
	(1)	(2)	(3)
Ln Rival Incident Count	−0.050***	−0.055***	−0.055***
	(−2.85)	(−3.25)	(−3.17)
Cash	−0.082		
	(−0.32)		
Top 10 Arranger		−0.121***	
		(−3.89)	
Lender Relation			−0.003
			(−0.09)
Controls	Yes	Yes	Yes
Loan Type Indicators	Yes	Yes	Yes
Loan Purpose Indicators	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
N	2,031	2,083	2,083
R^2	0.866	0.870	0.867

Table D9
Robustness: Different Fixed Effects

The table below reports the results for the robustness checks using different fixed effects. The control variables are the same as Table 2, including *Ln Total Assets*, *Leverage*, *ROA*, *Loss*, *Tangibility*, *Ln Maturity*, *Ln Loan Amount*, *Performance Pricing*, *Number of Lenders*, *Syndication* and *Financial Covenant Count*. The coefficients on all the control variables are omitted for brevity. All variables are defined in Appendix A. The sample period is 2010-2019. The *t*-statistics are in parentheses, computed using robust standard errors clustered at firm level. ***, ** and * indicate statistical significance at 1%, 5% and 10% levels, respectively.

	Dependent = *Ln AISD*	
	(1)	(2)
Ln Rival Incident Count	−0.082***	−0.134**
	(−4.66)	(−2.56)
Controls	Yes	Yes
Loan Type Indicators	Yes	Yes
Loan Purpose Indicators	Yes	Yes
Firm FE	Yes	Yes
Country × Year FE	Yes	No
Country × Year × Industry FE	No	Yes
N	2,067	1,852
R^2	0.897	0.940

Chapter 5

Conclusion

This thesis consists of three empirical essays studying the topics in the field of financial reporting frauds, cost of equity and cost of bank loans in various contexts.

The first essay examines how CEOs' political promotion incentives affect firms' likelihood of financial reporting frauds using Chinese SOEs. Utilizing a unique dataset and hand-collected preceding CEOs' promotions, I use predecessors' promotions as the proxy for the current CEO's political promotion incentives. I find that CEOs' political promotion incentives, proxying by their predecessor' promotions status, significantly decrease the propensity of current CEOs to commit financial reporting misconducts, indicating preceding CEOs' political promotion has a role-model effect on their successors. In addition, the negative association between preceding CEOs' promotion and financial reporting fraud is more pronounced among CEOs with better promotion prospects and firms with a more transparent information environment. This essay contributes to the literature by highlighting the importance of a CEO's non-pecuniary promotion incentive in constraining disclosure fraud in the context of SOEs. The findings also form practical guidance for executives, governments, and SOEs. For executives in Chinese SOEs, this study shows that their predecessors' promotions point the way towards an upward career pathway for successors. From the perspective of governments, this study can help understand the political incentives of top executives in Chinese SOEs. For SOEs, this study highlights that although executives in Chinese SOEs are usually underpaid (Feng and Johansson 2018), the political promotion incentives can serve as substitutes for pecuniary incentives. And executives' political promotion incentives could alleviate agency problems in disclosures.

The second essay investigates the impact of government subsidies on the cost of equity using a sample of U.S. listed firms. I find firms with more government subsidies have significantly lower costs of equity. Channel tests indicate that government subsidies reduce the cost of equity by both decreasing firms' information asymmetry and improving firms' fundamental performance. In addition, both tax-related and non-tax-related subsidies are negatively associated with firms' cost of equity; and state-level government subsidies have a greater impact on the cost of equity than those granted by federal and local governments. The findings have broad implications for academics, governments, regulators, and investors.

From an academic view, although there are some criticisms of government subsidies (Aobdia et al. 2019; Raghunandan 2021; Pappas et al. 2021), this study shows that government interventions lead to positive economic consequences. From the perspective of government, this study provides evidence on the impact of subsidies on various aspects including investors' perception, information environment, and firms' performance. Therefore, this study can help governments better evaluate the cost and benefit of granting firm-specific subsidies. For regulators like GASB and FASB, this study shows that decreasing capital cost is an essential benefit of requiring more transparent disclosure of government subsidies. Lastly, this study is also important for investors as they need to be aware that subsidized firms are less risky, and subsidies substantially impact firms' information environment and performance.

The third essay studies how industry rivals' ESG incidents affect firms' loan spreads using a sample of European firms. Prior literature provides two opposite views of the spillover impact of firm-specific events on industry peers: the contagion effect and the competitive effect (Lang and Stulz 1992). I find that industry rivals' adverse ESG incidents lead to reduce firms' loan spreads, consistent with the competitive effect prevailing over contagion effects. This intra-industry spillover effect pertains to both aggregate ESG records and various subcategories. It is stronger when rivals are public firms or larger firms, when the borrowing firms have better ESG profiles, or when the borrowing firms face greater competition from industry rivals. Notably, I find that firms gain a greater market share after rivals' ESG incidents, confirming the importance of good ESG profiles for the success in product markets. This study has wide implications for firms, banks and policymakers. From the perspective of firms, the evidence suggests that firms' financing costs and competitiveness in the product market rely critically on their ESG profile. For banks, this study shows that it is important to take into account both borrowers' and their industry rivals' ESG practices in their lending decisions. In addition, recently, European policymakers have ramped up their efforts toward sustainability in the financial system. This study contributes to the on-going discussion on the role banks should play in the transition to a more sustainable economy.

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