Doctoral Thesis

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# The Spread of Corporate Misconduct: Determinants, Consequences and Regulatory Responses

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*A thesis submitted in fulfillment of the requirements for the degree of Doctor of Philosophy in the*

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May 28, 2021

# Declaration of Authorship

I, Claudia MARANGONI, declare that, except where specific reference is made to the work of others, the content of this thesis titled "*The Spread of Corporate Misconduct: Determinants, Consequences and Regulatory Responses*" is original and has not been submitted in whole or in part for consideration for any other degree or qualification in this, or any other University.

I further declare that Chapter 3 is co-authored with Prof. Lars Helge Haß, Prof. Paul Hribar, and Dr. Roberto Pinto, whereas Chapter 4 is co-authored with Prof. Igor Goncharov. I finally declare that a version of Chapter 2 has been submitted to a journal.

# *Acknowledgements*

First of all, I would like to thank my supervisor, Professor Igor Goncharov, whose expertise and support have been invaluable throughout this journey. His insightful feedback pushed me to sharpen my thinking and brought my work to a higher level. I would also like to thank Professor Christian Stadler for his help during the first part of my PhD.

I am also extremely grateful to the whole Accounting and Finance faculty at Lancaster University, in particular Shantanu Banerjee, Justin Chircop, Chris Ford, Mahmoud Gad, Lars Helge Haß, Sarah Kroechert, Argyro Panaretou, Ken Peasnell, Dasha Smirnow and Steven Young, for their trust and support. Thanks also to the academic and administrative staff, especially Carole, Joanna, and Julie for assisting me whenever I needed help.

Thanks to my dear friend Bingbing and her incredible daughter Lily for sharing with me these challenging but unforgettable years. Thanks to Giovanna, Roberto, Xiaoxi, and all my wonderful friends in the UK and around the world for supporting me.

Finally, thanks to Dimitris and my family for giving me the strength to make this possible.

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*A te, nonno, perché ovunque tu sia, possa essere orgoglioso di me*
*(To my beloved granddad: May you be proud of me, wherever you are)*

# Chapter 1

# Introduction

The incidents of corporate misconduct (e.g., corruption, fraudulent financial reporting, money laundering) have remarkably increased over the last two decades, causing considerable economic losses and producing long-lasting side effects for the economy worldwide (Conklin, 1977; Nelson, 2014; Soltes, 2016). The spread and severity of these incidents have motivated the literature in accounting, economics, and finance to investigate not only the drivers of corporate misconduct, but also its socio-political and economic consequences (e.g., Palmrose, Richardson, and Scholz, 2004; Kedia, Koh, and Rajgopal, 2015; Parsons, Sulaeman, and Titman, 2018).

Prior literature on corporate misconduct has reached consensus on two fronts. First, corporate misconduct occurs when firms are myopic in their decision-making and overvalue short-term results at the expense of long-term growth and economic value creation (e.g., Jensen, 1986; Hayn, 1995; Burgstahler and Dichev, 1997; Graham, Harvey, and Rajgopal, 2005; Bhojraj et al., 2009). Myopic behavior is triggered by several factors, such as firm characteristics (e.g., leverage, performance, size), corporate culture, capital market pressures, and weak governance and external monitoring mechanisms (e.g., DeFond and Park, 1997; Ndubizu, 2007; Ashbaugh-Skaife et al., 2008; Liu, 2016; Parsons, Sulaeman, and Titman, 2018). Second, corporate misconduct disrupts fair competition among firms and threatens the correct functioning of capital markets (e.g., Stein, 1989; Louis, 2004; Palmrose and Scholz, 2004; Fan, 2007; Myers, Myers, and Skinner, 2007; Giannetti et al., 2021).

The academic community is not the only group that has shown a continuous interest in corporate misconduct. Policymakers and regulators have also consistently treated this

topic as a priority on their agendas. The laws and regulations on oversight and punishment of corporate misconduct have been constantly trying to adapt to its ever-changing forms, both within and across countries (Amiram et al., 2018). For example, in the U.S., the Foreign Corrupt Practices Act (FCPA) of 1977 responded to the worldwide rise in corruption cases by making it illegal for U.S. corporations and foreign corporations with securities listed in the U.S. to bribe foreign government officials in exchange of business and other financial benefits.[1] Moreover, following the egregious financial scandals involving publicly traded firms at the beginning of the twenty-first century, the Sarbanes-Oxley (SOX) Act of 2002 introduced strict criminal punishments and investor protection measures to prevent future corporate frauds.[2] In addition, in the aftermath of the 2007 financial crisis, the Dodd-Frank Act of 2010 tightened investor protection requirements and securities regulation.[3] Outside the U.S., among others, the European Union (EU) established the European Anti-Fraud Office (OLAF) in 1999 to protect the EU institutions against fraud and corruption.[4]

Regulators have also recognized the importance of fighting corporate misconduct in a joint effort by establishing global regulatory frameworks.[5] For example, in 1994 the Organisation for Economic Co-operation and Development (OECD) established the OECD Working Group on Bribery in International Business Transactions to adopt preventive measures against corruption, monitor the application of anti-bribery rules internationally, and enforce legal measures in case of noncompliance.[6] Similarly, the United Nations Convention Against Corruption (UNCAC) has been promoting international cooperation and law enforcement against corruption since 2005.[7]

Despite the substantial regulatory efforts to deter firm wrongdoing, the amount of

[1] Foreign Corrupt Practices Act (FCPA), The U.S. Department of Justice.
[2] Sarbanes-Oxley Act, 107$^{th}$ United States Congress.
[3] Dodd-Frank Act, 111$^{th}$ United States Congress.
[4] European Anti-Fraud Office (OLAF).
[5] The global framework for fighting financial crime, Deloitte: Institute of International Finance, 2019.
[6] In March 2021, the OECD Working Group on Bribery in International Business Transactions included 45 countries worldwide.
[7] The United Nations Convention Against Corruption (UNCAC) was adopted by the UN General Assembly in 2003 and entered into force in 2005. In February 2020, it included 187 parties.

news about corporate misconduct shows no signs of abating.[8] A reason for this evidence is that the environment in which corporate misconduct originates constantly evolves, thus making regulators' responses often untimely (Beale, 2016). The most recent example of a striking change to the environment in which firms operate is the Covid-19 outbreak: The significant declines in liquidity and performance, and the increased bankruptcy risks have exacerbated firms' pressures to beat the competition and avoid losses (e.g., Hitt, Arregle, and Holmes, 2021; Shen et al., 2020; Zhang and Hu, 2021). In line with such pressures, the latest research discusses how this unprecedented crisis has increased firms' incentives to engage in corporate misconduct to gain an edge over the competitors (e.g., Gallego, Prem, and Vargas, 2020; Terziev, Georgiev, and Bankov, 2020). Furthermore, Karpoff (2020) predicts that the disruptions in relative demands and organizational capital caused by the pandemic will increase the likelihood of fraud in the next few years.

Whether and how regulators will respond to the new challenges stemming from the pandemic as well as to other emerging challenges are open questions. However, there is little doubt that corporate misconduct will remain a first-order issue for a considerable time. In light of the ongoing efforts from regulators to fight corporate misconduct and the debate on the still not-fully understood causes of this complex phenomenon (Amiram et al., 2018), corporate misconduct is worthy of further investigation. With my work, I contribute to this debate by extending research knowledge about how corporate misconduct spreads and enhancing the understanding of the effectiveness of different regulatory measures designed to curb misconduct. Overall, my findings are particularly relevant for regulators concerned about how their enforcement actions could reduce the incidents of corporate misconduct to safeguard socio-political and economic stability (Simpson, 2013; Zuber, 2015; Soltes, 2018).

To investigate how corporate misconduct spreads and how firms respond to different regulatory measures intended to curb this spread, I exploit several features of the

[8] Recent examples are Canoo Inc. (Businesswire, 30$^{th}$ March 2021) and Nissan Motor Co. (Bloomberg, 30$^{th}$ March 2021).

U.S. regulation for three reasons. First, the United States represents the largest market economy, and any regulatory change has repercussions globally (Bushee and Leuz, 2005). Second, the continuous debate surrounding regulations in the U.S. signals that their effectiveness remains unclear, thereby creating a suitable setting to examine the implications of these regulations over time and across firms (Hamann, 2019). Third, despite often requiring substantial hand-collection, data on U.S. regulations and public firms are largely available in a systematic way, thus allowing researchers to use such data for empirical analyses (Leuz and Wysocki, 2016).

My doctoral thesis consists of three papers: Each of them relies on theory and anecdotal evidence to form predictions about specific aspects of the spread of corporate misconduct, and provides novel evidence of this complex and multifaceted phenomenon. The first paper, presented in Chapter 2 and titled "*(How) Do firms respond to rivals' corruption? Evidence from financial misstatements*", identifies competitive peer pressures as a determinant of the spread of corporate misconduct. The second paper, presented in Chapter 3 and titled "*Employees' financial wellness, productivity, and firms' myopic behavior*", documents that laws that improve employees' financial status are effective at reducing the spread of corporate misconduct, thereby suggesting that employees' financial incentives are a determinant of misconduct. The third paper, presented in Chapter 4 and titled "*Do compliance monitors help restore trust in firms after bribery scandals? Evidence from the cost of capital*", shows that specific regulatory actions aimed to limit the spread of corporate misconduct are beneficial to firms.

More specifically, in the solo-authored paper "*(How) Do firms respond to rivals' corruption? Evidence from financial misstatements*", I investigate whether the spillover effects have implications for the spread of corporate misconduct. To answer this research question, I examine corruption and financial misstatements, as both are detrimental and widespread forms of corporate misconduct whose scale and underlying dynamics are still not fully understood (e.g., Jain, 2001; Amiram et al., 2018).

Using hand-collected data on violations of the FCPA to identify bribing firms and years and construct a proxy for peers' exposure to rivals' bribery, I predict and find that

firms are more likely to misstate their financial statements and boost their earnings in the years in which corrupt rivals bribe to obtain unfair financial advantages. This prediction relies on the theory of relative performance evaluation, according to which firms often artificially increase their financial statement numbers to report a similar performance to that of their competitors (Holmstrom, 1982; DeFond and Park, 1997; Park and Ro, 2004). Moreover, I find that these income-increasing misstatements are more likely when non-bribing peers report loss of income due to the unfair competitive gains of bribing rivals, and when financial analysts compare the performance of bribing firms to that of their non-bribing peers.

This paper makes three contributions to the literature on corporate misconduct. First, I provide an economic-based explanation—based on relative performance evaluation pressures—for finding spillover effects from corporate misconduct that complements the socio-psychological theories used to explain spillovers from non-corporate offenses (Lanfear, Matsueda, and Beach, 2020). Second, I contribute to the studies on the macroeconomic consequences of corruption and the strategic choices of non-corrupt firms doing business in corrupt environments (Galang, 2012). Third, I provide firm-level measures of corruption that complement the commonly-used (although criticized) country-specific corruption perception indexes (Alexeev and Song, 2013). Overall, by documenting externalities from corporate behavior in general and spillover effects from corporate misconduct in particular, this paper provides new insights into the spread of corporate misconduct.

In the paper "*Employees' financial wellness, productivity, and firms' myopic behavior*" (co-authored with Lars Helge Haß, Paul Hribar, and Roberto Pinto), we investigate whether rank-and-file employees' financial wellness affects firms' myopic behavior. In particular, we predict that improvements in employees' financial wellness reduce firms' need and incentives to make myopic accounting decisions. This prediction relies on the evidence that concerns about the personal financial situation are a primary determinant of workers' productivity (Richardson, Elliott, and Roberts, 2013; Kaur et al., 2021) and on the theory that firms make myopic accounting decisions to boost their reported earnings when they are pressured to achieve specific earnings-based targets (e.g., Roychowdhury,

2006; Bhojraj et al., 2009).

Because employees with fewer financial concerns are less stressed and more productive at work, firms can benefit from this higher productivity and achieve their earnings-based targets while undertaking fewer income-increasing myopic actions. Using hand-collected staggered increases in consumer bankruptcy exemptions across U.S. states to capture increments in employees' financial wellness, we find results consistent with our prediction: Firms engage less in real activities management, financial misstatements, and loss avoidance practices when employees' financial wellness improves.

This paper contributes to four streams of the literature. First, we add to the limited research on the role of rank-and-file employees in firms' myopic accounting decisions (e.g., Dou, Khan, and Zou, 2016) by identifying employees' financial wellness as an understudied, yet relevant determinant. Second, we contribute to the studies on the effects of employees' well-being on employers' financial and real decisions (e.g., Bae, Kang, and Wang, 2011; Pinto, 2021). Third, by showing that debt relief programs, such as consumer bankruptcy exemptions, reduce value-destroying firm myopic decisions, we add to the studies on the positive effects of debt relief programs (Dobbie and Song, 2015). Fourth, we contribute to the literature on the individuals' responses to income risk (Low, Meghir, and Pistaferri, 2010). Overall, our evidence suggests that laws that improve employees' financial status are also effective at curbing the spread of corporate misconduct.

Finally, in the paper "*Do compliance monitors help restore trust in firms after bribery scandals? Evidence from the cost of capital*" (co-authored with Igor Goncharov), we investigate whether regulators' enforcement actions that reduce recidivism in corporate misconduct have implications for firms' cost of financing. Specifically, we predict that the external compliance monitors appointed in connection with the settlements of FCPA violations contribute to reducing firms' cost of capital. This prediction relies on the agency theory, in that the disclosure of firm wrongdoing signals severe agency problems that arise from the failure of corporate compliance systems and cause an increase in cost of capital (Cumming, Dannhauser, and Johan, 2015). By providing external verification and decreasing noncompliance risk, compliance monitors can reduce agency costs, thereby

lowering the cost of capital.

Exploiting hand-collected information from the FCPA enforcement actions to distinguish between bribing firms with and without post-enforcement compliance monitor obligations, we find results consistent with our prediction: Firms that appoint compliance monitors have a lower cost of equity and debt capital after the enforcement actions compared to firms that receive the enforcement actions but do not have post-enforcement compliance obligations. This evidence also suggests that monitors help external stakeholders (re)gain trust in firms involved in wrongdoing, reducing the assessment of such firms' risks. Additional analyses show that the market reaction to the anti-bribery enforcement actions is positive when firms announce the appointment of compliance monitors, whereas it is negative when firms do not have post-enforcement compliance obligations. Moreover, our findings show that compliance monitors act as substitutes for other costly trust repair mechanisms, such as dividend increases.

This paper makes three contributions to the literature. First, we contribute to the emerging literature on the economic role of corporate compliance monitors and the consequences of their appointment and retention (Gallo, Lynch, and Tomy, 2020). Second, we add to the literature on the mechanisms that help firms rebuild their reputation after fraud or other negative corporate events (Farber, 2015). Third, we contribute to the debate about the implications of anti-bribery regulation and the deterrent effects of enforcement (Karpoff, Lee, and Martin, 2017). Overall, we provide novel evidence that specific regulatory actions that deter recidivism in corporate misconduct produce net benefits for firms involved in wrongdoing and contribute to restoring external stakeholders' trust in such firms.

Altogether, by examining how corporate misconduct spreads, what factors contribute to reducing its dissemination, and how the regulatory responses to corporate misconduct produce spillover effects, my research answers important questions that are of interest to academics, policymakers, and practitioners. Only by learning more about the drivers of corporate misconduct and its externalities will societies be able to deal with this complex phenomenon and its detrimental implications that affect a wide range of economic players.

# Chapter 2

# (How) Do firms respond to rivals' corruption? Evidence from financial misstatements

**ABSTRACT**[1]

This study examines whether the actions of corrupt firms affect peer firms' financial misstatements. Using data on violations of the U.S. Foreign Corrupt Practices Act, I find that peer firms misstate their financial statements and increase their earnings during the years in which corrupt rivals bribe foreign officials to gain unfair performance advantages. The likelihood of such income-increasing misstatements is higher when non-bribing peers experience loss of income due to bribing rivals' unfair gains, and when financial analysts compare bribing firms and non-bribing peers' performance. These findings suggest that competitive disadvantage and relative performance evaluation pressure result in spillovers from one type of corporate misconduct (bribery) to another (financial misstatements). By documenting the spillover effects from corporate misconduct, I contribute to the limited evidence of externalities from corporate behavior, and provide new insights into how firms' misconduct spreads.

JEL classification: D73, H23, M41, M48.

Keywords: corruption, financial misstatements, spillover effects.

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## 2.1 Introduction

The past two decades have marked a sharp increase in news about corporate misconduct, which entails a variety of unethical professional actions, including fraudulent financial reporting, money laundering, and corruption (Soltes, 2016). When subject to criminal prosecution, corporate misconduct is known as "corporate crime" or "white-collar crime" (Friedrichs, 2010). Corporate misconduct spreads when more than one subject becomes involved in the same improper activity (contagion effect) or when one type of improper activity leads to other forms of misbehavior (spillover effect). As corporate misconduct causes severe economic losses and produces long-lasting side effects that reverberate throughout the economy, understanding how it spreads is essential for economic and socio-political stability (Zuber, 2015).

While the literature in accounting, economics, and finance both predicts and finds that corporate misconduct is contagious (Kedia, Koh, and Rajgopal, 2015; Parsons, Sulaeman, and Titman, 2018), evidence of its spillover effects is lacking. Moreover, unlike the sociology and psychology literature, which provides some theory and evidence of spillovers from common offenses (Corman and Mocan, 2005), the specificity of the corporate environment prevents researchers from applying the "common crime" theoretical framework to the corporate setting (Benson and Moore, 1992).[2]

Thus whether spillovers from corporate misconduct occur remains unclear. In this paper, to investigate whether and, if so, how the spillover effects influence the spread of corporate misconduct I focus on corruption and financial misstatements.[3] First, I examine whether firms' bribery of foreign officials to gain unfair business advantages influences non-bribing peers' propensity to misstate their financial statements. Second, I investigate whether non-bribing peers' financial misstatements depend on the likelihood of experiencing loss of income due to bribing rivals' unfair gains. Third, I examine

[2] "Common crime" means offenses, either violent or non-violent, that commonly arise in public places, such as crimes against persons and property (Benson and Moore, 1992).

[3] As studies use the terms "corruption" and "bribery" interchangeably (e.g., Shleifer and Vishny, 1993; Ades and Di Tella, 1999), I adopt a similar approach.

whether peer performance pressure is a possible mechanism for the spillover effects of firms' bribery on non-bribing peers' financial misstatements.

Despite the first-order importance of corruption and financial misstatements, research evidence of their emergence and evolution is not conclusive. Corruption is ranked among the main factors hampering economic growth and impairing the legitimacy of the market economy (Healy and Serafeim, 2016). Nevertheless, no consensus exists on the scale of this phenomenon (Jain, 2001). Similarly, despite a broad consensus on the negative consequences of financial misstatements, evidence of its causes is mixed (Amiram et al., 2018). Moreover, corruption and financial misstatements are likely related, because when some firms create wealth unethically, their peers feel pressured to obtain similar results, even though achieving those results might entail engaging in other forms of misconduct (Den Nieuwenboer and Kaptein, 2008).

The considerable efforts of legislators to eradicate corruption notwithstanding, it remains widespread and persistent, possibly because corruption yields significant benefits for corrupt agents (Huntington, 1968; Amore and Bennedsen, 2013). Corrupt firms are often big market players that establish favorable connections with government officials and extract high rents from them (Zhou and Peng, 2012; Giannetti et al., 2021). Through bribery, corrupt firms become more profitable than their fair-trading peers (Williams, Williams, and Kedir, 2016).

Moreover, corruption adversely affects the business activities of non-corrupt firms operating in corrupt industries (Giannetti et al., 2021). Corruption undermines non-bribing firms' efficiency, distorts price formation, and leads to suboptimal resource allocation (Compte, Lambert-Mogiliansky, and Verdier, 2005; Cingano and Pinotti, 2013).

When corrupt firms gain unfair business advantages and improve their performance through illicit payments, their peers have strong incentives to report similar results. Furthermore, the exposure to bribing firms' unethical behaviors through business interactions can alter the understanding of norms related to misbehavior, making engaging in corporate misconduct more acceptable for non-bribing peers (Cialdini and Trost, 1998). Therefore, I hypothesize that non-bribing peers (i.e., non-bribing firms exposed to bribing

rivals) resort to financial misstatements more than firms not exposed to rivals' bribery. To test this hypothesis, I identify non-bribing peers by constructing a proxy for non-bribing firms' exposure to rivals' bribery based on the enforcement actions of the U.S. Department of Justice (DOJ) and the Securities and Exchange Commission (SEC) against firms that violate the U.S. Foreign Corrupt Practices Act (FCPA). Consistent with my hypothesis, I find that non-bribing peers misstate their financial statements more than firms not exposed to bribing rivals.

Moreover, I examine whether non-bribing peers' propensity to misstate their financial statements depends on their likelihood of experiencing loss of income due to rivals' bribery. In this paper, I define "loss of income" as the loss of possible additional income that non-bribing peers would report were competition and trades fair and not distorted by corruption.

First, I hypothesize that non-bribing peers' financial misstatements increase with the magnitude of such loss of income, which in turn depends on the magnitude of bribing rivals' unfair gains. The more that corrupt firms win by bribing, the more their peers are expected to lose out. Using the illicit bribery payments to construct a proxy for bribing firms' benefits, I find that non-bribing peers' performance-enhancing financial misstatements increase with the magnitude of the loss of income.

Second, I hypothesize that non-bribing peers are more likely to be negatively affected by rivals' bribery, in turn having stronger incentives to manipulate their performance, when they lose profits and market share because their bribing rivals steal profitable deals from them (Kaikati et al., 2000). To test this hypothesis, I exploit Hoberg and Phillips (2016)' product market similarity scores to identify non-bribing peers likely to compete directly against their bribing rivals for the same contracts and deals. The results confirm my hypothesis: The greater the product market similarity between non-bribing peers and their bribing rivals, the higher the likelihood that non-bribing peers will misstate their financial statements.

I also explore a possible mechanism through which the performance of bribing firms

affects non-bribing peers' financial misstatements. When bribing firms obtain unfair business advantages from bribing, the firms directly benchmarked against them are likely to underperform and face peer performance pressures. Relative underperformance is costly for these firms, because executives may be fired, markets may react negatively to declines in performance, lenders may be less willing to provide access to credit, and analysts may formulate unfavorable recommendations (e.g., De Franco, Hope, and Larucque, 2015; Jenter and Kanaan, 2015). Therefore, I hypothesize that non-bribing peers benchmarked against bribing rivals have the greatest incentives to manipulate their results and alter the stakeholders' perception of relative underperformance. Consistent with my hypothesis, I find that when peer performance pressures are high because non-bribing peers have the same analysts as their bribing rivals, non-bribing peers are more likely to misstate their financial statements than other firms without (or with fewer) common analysts.

The results are robust to possible alternative explanations. First, if firms engaging in bribery are also more likely to misstate their financial statements, results may reflect contagion in financial misstatements rather than spillover effects (Kedia, Koh, and Rajgopal, 2015). To rule out this possibility, I control for bribing firms' financial manipulation rates in each year and industry. Second, if firms both bribe and misstate their financial statements, and if their bribery remains undetected, my results may reflect contagion in bribery rather than the spillovers of corruption on peers' financial misstatements. To deal with this concern, I test my hypotheses in a setting where firms are unlikely to bribe, making contagion in bribery an unlikely explanation of the results. Specifically, I restrict the sample to non-bribing firms with no significant business in highly corrupt countries. Third, if non-bribing firms exposed to rivals' bribery operate in geographical environments with a high propensity toward misconduct, my results may reflect the effect of location-specific norms rather than spillovers from corporate misconduct (Parsons, Sulaeman, and Titman, 2018). To rule out the possibility that social and cultural factors drive the evidence, I control for state fixed effects. All robustness tests confirm the main results.

Overall, the findings suggest that corruption generates spillover effects on non-bribing

peers' propensity to artificially increase their performance. The frequency of such income-increasing financial misstatements is related to the loss of income that non-bribing peers are likely to experience following bribing rivals' unfair gains. Moreover, among non-bribing peers, those benchmarked against high-performing bribing rivals by financial analysts respond to peer performance pressures by artificially boosting their performance even more.

This paper makes three contributions to the literature on corporate misconduct. First, I provide empirical evidence of the spillover effects from corporate misconduct and offer an economic-based explanation—rooted in relative performance evaluation pressures—for such spillovers. By doing so, I complement the socio-psychological theories explaining the spillover effects from common offenses and show that corporate behavior produces externalities (Leuz and Wysocki, 2016; Lanfear, Matsueda, and Beach, 2020). Second, as my findings suggest that firms' corruption influences non-corrupt peer firms' accounting decisions, I add to the literature on the macroeconomic effects of corruption and the strategic responses of non-corrupt firms operating in corrupt environments (e.g., Mauro, 1995; Galang, 2012). Third, as opposed to studies that rely primarily on country-specific corruption perception indexes, I employ firm-level proxies for corruption and overcome the issues of generalizability associated with corruption heterogeneity across countries (De Maria, 2008; Alexeev and Song, 2013).

The remainder of the paper is organized as follows. Section 2.2 reviews the relevant literature on bribery, summarizes the theory and evidence of the spread and spillover effects from misconduct, and presents the hypothesis development. Section 2.3 describes the data, the sample, and the empirical methodology. Section 2.4 presents the empirical results, and Section 2.5 concludes.

## 2.2 Prior literature and hypothesis development

### 2.2.1 Bribery

Bribery is the offering, receiving, or soliciting of something valuable with the intention to influence the actions of an official in the discharge of his or her public or legal duties (Ades and Di Tella, 1999). Despite bribery being a worldwide phenomenon, regulators are primarily concerned about corruption in developing countries, thus explaining why the FCPA sanctions U.S. corporations for foreign bribery (Weber and Getz, 2004). As the FCPA wrote in 1977, "foreign corporate bribery affects the very stability of overseas business. Foreign corporate bribes also affect our domestic competitive climate when domestic firms engage in such practices as a substitute for healthy competition for foreign business."[4]

In addition to undermining the effectiveness of the business transactions in corrupt environments, corruption impairs domestic operations and weakens peer firms that do not bribe (DOJ and SEC, 2020). By increasing the costs of doing business, corruption leads to price distortion and allocation inefficiencies (e.g., Compte, Lambert-Mogiliansky, and Verdier, 2005; Cingano and Pinotti, 2013).[5] Moreover, bribery undermines the effectiveness of most bid-for-contract mechanisms, so that efficient businesses cannot be certain of selection because "the decisions of the corrupt public agent reflect concealed transactions in the bribery market" (Della Porta and Vannucci, 1997, 522).

A possible explanation for the persistence of widespread corruption is the benefits it yields for unethical agents in general and for large firms in particular (Sahakyan and Stiegert, 2012; Zhou and Peng, 2012). Through bribery, firms obtain permits, licenses, contracts, and the assurance that the markets in which they operate are protected from competitors (Shleifer and Vishny, 1993). Even with all direct and indirect costs of corruption, the net present value of bribery remains positive (Karpoff, Lee, and Martin,

[4] Foreign Corrupt Practices Act (FCPA), The U.S. Department of Justice.

[5] In 2018, a joint publication by the International Chamber of Commerce, Transparency International, the United Nations Global Compact and the World Economic Forum Partnering Against Corruption Initiative (PACI) reports that corruption adds up to 10 percent to the total cost of doing business globally.

2017). Although corruption is overall welfare-reducing, firms generally bribe to obtain unfair performance-enhancing advantages that would not be accessible under fair competition (Dieleman and Sachs, 2008).[6] The benefits of corruption are especially large for big firms, which have few resource constraints and are better able to establish connections with public officials and obtain favorable treatments from them (Fieldhouse, 1986; Zhou and Peng, 2012).[7]

### 2.2.2 Spread and spillover effects from misconduct

Scholars can study the spread of common offenses and corporate misconduct by either investigating whether one type of improper behavior is contagious or whether it leads to other forms of improper behavior through spillover effects. The literature on common offenses provides theory and evidence of both contagion and spillover effects, whereas the corporate misconduct literature has thus far examined only contagion (e.g., Brendgen et al., 2013; Carson, 2013; Kedia, Koh, and Rajgopal, 2015; Parsons, Sulaeman, and Titman, 2018).

The evidence of contagion and spillovers from common offenses relies primarily on sociological and psychological theories, with learning processes and social factors playing the major role in shaping individuals' propensity to misbehave (Gino, 2015). Common offenses spread when individuals learn from other group members that crossing ethical boundaries produces benefits (e.g., Shaw and McKay, 1942; Moore and Gino, 2013). Moreover, Wilson and Kelling (1982) argue that certain crimes cause the local environment to deteriorate (what they call "broken windows") and lead to further major offenses. The empirical findings support researchers' socio-psychological arguments for contagion and spillovers from common offenses (e.g., Jessor and Jessor, 1977; Skogan, 1990; Bingenheimer, Brennan, and Earls, 2005; Ludwig and Kling, 2007; Boman IV et al., 2012).

[6] Firms choose to bribe to maximize their after-bribe profits (Kaufmann and Wei, 1999).

[7] Whether small firms benefit from paying bribes, because resource constraints prevent them from extracting sufficient rents from public officials, is debatable (e.g., Zhou and Peng, 2012; O'Toole and Tarp, 2014). Similarly, whether bribing firms overvalue present profits and discount future ones, thus making corruption value-destroying in the long term, is debatable (e.g., De Rosa, Gooroochurn, and Gorg, 2010; Seker and Yang, 2012; Birhanu, Gambardella, and Valentini, 2016).

The arguments for describing contagion in corporate misconduct rely on rational-economic and sociological explanations, as in Granovetter (1985)' "embeddedness" theory, which states that an economic behavior combines economic and sociological features. Kedia, Koh, and Rajgopal (2015) provide two explanations for interpreting their evidence of contagion in earnings management. The first is the rational crime-based explanation that potential criminals misbehave if they perceive that the benefits of doing so exceed the costs (Becker, 1986). The second is the social norms-based explanation that observing others cheat may modify an individual's understanding of the social norms associated with misbehavior (Cialdini and Trost, 1998). Moreover, although Parsons, Sulaeman, and Titman (2018) consider cultural and social forces the first-order driver of contagion in corporate misconduct, they acknowledge that rational peer pressures can also affect contagion.

### 2.2.3 Hypothesis development

Corruption is performance-enhancing for corrupt firms, particularly in the short term and when firms are big and leading market players (Hellman and Schankerman, 2000; Zhou and Peng, 2012). The benefits of corruption exceed the costs, and the penalties are insufficient for deterring repeat offenses (Lambsdorff and Nell, 2007). Moreover, the increase in market capitalization following the initiation of bribe-related projects is higher than the decrease that follows the disclosure of bribery and the release of enforcement actions (Karpoff, Lee, and Martin, 2017).

Non-corrupt peers operating in corrupt environments suffer from adverse effects, because corruption undermines market efficiency, distorts price formation, and leads to sub-optimal resource allocation (Compte, Lambert-Mogiliansky, and Verdier, 2005; Schoenherr, 2019). Furthermore, only small groups of large corrupt firms generally receive benefits from bribing (e.g., licenses, tenders), whereas the remaining competing firms are cut out of the deals (Fieldhouse, 1986).

When firms produce wealth dishonestly and distort the market competition, their

peers feel pressured to obtain similar results, even though achieving those results entails engaging in other forms of misconduct. In addition, the exposure to competitors' misconduct through common business environments and interactions increases the likelihood that a chain of corporate misbehaviors will follow (Murphy, 2019). For example, the World Bank is concerned that firms have incentives to engage in unethical behaviors when they believe that their competitors are receiving unfair advantages.[8]

A channel that non-bribing peers can exploit to artificially boost their performance and report results similar to the unfair gains of bribing rivals is financial misstatements. Firms often manipulate their financial statement numbers to make their performance comparable to that of their competitors (DeFond and Park, 1997; Park and Ro, 2004). Due to bribing rivals' unfair advantages, I expect non-bribing peers to misstate their financial statements more often than firms not exposed to rivals' corruption. In particular, I expect financial misstatements to enhance non-bribing peers' performance. Using data on foreign bribery to identify non-bribing peers (i.e., non-bribing firms exposed to bribing rivals), I formulate my first hypothesis as follows:

*H1: The bribery of corrupt firms has spillover effects on non-bribing peers' performance-enhancing financial misstatements.*

In response to bribing rivals' unfair gains, non-bribing peers could enhance their strategies to become more efficient and competitive (Galang, 2012). Nevertheless, this response is unlikely because changing corporate strategy is costly and requires time (Wan and Yiu, 2009; Stuebs and Sun, 2010). In contrast, engaging in financial misstatements provides benefits in the short term despite destroying economic value in the long term (e.g., Bhojraj et al., 2009). Moreover, recent evidence shows that non-bribing peers reduce their abnormal accruals when bribing rivals receive the enforcement actions for FCPA violations (Bunkanwanicha and Greusard, 2019). This evidence is consistent with non-bribing peers having fewer incentives to artificially boost their performance when bribery is disclosed and bribing rivals can no longer obtain unfair gains.

[8] Illicit Financial Flows (IFFs), The World Bank (7th July 2017).

Not all firms exposed to corruption are expected to be affected in the same way by rivals' bribery. Non-bribing peers lose possible additional income because competition and trades are distorted by corruption (Giannetti et al., 2021), and this loss of income likely depends on how much corrupt firms unfairly win, and whether bribing firms steal profitable deals from non-bribing peers. I investigate the conditions that exacerbate the competitive disadvantages of corruption, and, in turn, the incentives of non-bribing peers to engage in financial misstatements.

First, I expect non-bribing peers to engage in income-increasing financial misstatements when the magnitude of the loss of income is material. Using the amounts of bribes paid to proxy for both corrupt firms' illicit gains and non-bribing peers' loss of income, I formulate the first specification of my second hypothesis as follows:

*H2a: Non-bribing peers' performance-enhancing financial misstatements increase with the bribes paid by corrupt rivals.*

Second, I expect non-bribing peers to have more incentives to manipulate their performance when they compete directly with bribing rivals for the same deals, because bribery makes non-bribing peers lose profitable deals and market shares, and, in extreme cases, forces them to exit the market (e.g., Bai et al., 2017). Using the Hoberg and Phillips (2016)' product market similarity scores to determine bribers' closest competitors, I formulate the second specification of H2 as follows:

*H2b: Non-bribing peers' performance-enhancing financial misstatements increase with the product market similarity to corrupt rivals.*

I further examine a possible mechanism underlying the spillover effects of corruption on peer firms' financial misstatements. Non-bribing peers likely underperform relative to their bribing rivals that obtain unfair performance-enhancing favors. As both external stakeholders and internal executives are negatively affected when firms' performance is worse than that of their competitors (Du and Shen, 2018), relative underperformance is costly for non-bribing firms.

Corporate stakeholders rely extensively on relative performance evaluation techniques for decision-making. In particular, analysts use relative peer performance as their benchmark for supporting valuation multiples, earnings forecasts, and stock recommendations (e.g., Bradshaw, Miller, and Serafeim, 2009; De Franco, Hope, and Larucque, 2015). Moreover, during the Q&A session of firm conference calls, analysts ask questions about firm results and make comparisons with peers (Brochet, Kolev, and Lerman, 2018).

I therefore expect non-bribing peers to have more incentives to misstate their financial statements upward when financial analysts benchmark them against high-performing corrupt rivals. Using financial analysts common to bribing firms and non-bribing peers to proxy for the intensity of peer performance pressures, I formulate my third hypothesis as follows:

*H3: Among non-bribing peers, those experiencing the strongest peer performance pressures are more likely to misstate their financial statements upward.*

In line with the evidence of contagion in corporate misconduct (e.g., Kedia, Koh, and Rajgopal, 2015), I argue that the spillover effects of corruption on peers' financial misstatements derive primarily from the economic-based mechanism of peer performance pressure. However, my tests do not rule out the possibly contributing role of the sociological and psychological arguments that explain the spread of corporate misconduct (e.g., Den Nieuwenboer and Kaptein, 2008; Parsons, Sulaeman, and Titman, 2018).

## 2.3 Data and empirical methodology

### 2.3.1 Data and sample

To test the hypotheses and construct the variables for the empirical analysis, I examine the FCPA enforcement actions of the U.S. DOJ and the SEC between April 1978 and December 2019.[9] Violations of the FCPA imply that U.S. firms have bribed foreign officials, through foreign operations or foreign subsidiaries, to obtain or retain government contracts and other business in those foreign countries. While firms can bribe both

[9] The enforcement actions are available from the U.S. DOJ and SEC websites.

domestically and internationally, the FCPA punishes firms only for foreign bribery. However, as regulators are mostly concerned about corruption in developing countries (Weber and Getz, 2004; Olken and Pande, 2012), I use a sample of FCPA enforcement actions to investigate the spillover effects from corruption.

Table 2.1 provides the time trend of the enforcement actions release. Between 1978 and 2019, regulators have issued 451 FCPA enforcement actions against 203 different firms in Compustat. Each firm has therefore received, on average, 2.2 enforcement actions, suggesting that bribing firms are repeat offenders and that bribery is profitable (e.g., Sahakyan and Stiegert, 2012; Williams, Williams, and Kedir, 2016). Table 2.1 shows that until the early 2000s, regulator enforcement was relatively moderate, whereas from 2005 the trend has increased, with 51 annual enforcement actions in 2010 and 47 in 2016.[10] This rising trend reflects regulators' industry-wide investigations and international anti-corruption cooperation and enforcement (Koehler, 2013).

Table 2.2 shows where firms have bribed and reveals that China is the country with the highest frequency of illicit payments (12.16 percent), followed by Brazil, Iraq, and Nigeria, whose bribery frequency ranges between 4.12 percent and 4.71 percent.[11]

Table 2.3 reports the descriptive statistics of the enforcement actions and bribing firms. Each bribe is roughly $66.64 million, and the sanctions for violating firms amount on average to $75.56 million. The standard deviation of both variables shows that some firms pay bribes and penalties significantly higher than the sample means. Firms bribe for 5.7 years on average, with 6.5 years the time between the average year in which bribes are paid and the year of the enforcement action. Moreover, almost 28 percent of the enforcement actions are against firm executives, around 21 percent involve M&A deals, more than 63 percent impose compliance obligations as part of case resolutions,

[10] These small figures could raise concerns about selectivity issues, as the enforcement actions for FCPA violations are fewer than the enforcement actions for other accounting and auditing violations. For example, in 2016 the SEC issued 58 enforcement actions for FCPA violations (original figure before data cleaning), but 113 accounting and auditing enforcement actions. However, the observations in my final sample are similar to those used in other studies on the accounting consequences of the SEC enforcement (e.g., Tran and O'Sullivan, 2011; Beatty, Liao, and Yu, 2013; Mehta and Zhao, 2020).

[11] In Table 2.2, the total number of observations (510) is higher than that reported in Table 2.1 (451) because each enforcement action can refer to bribes paid in more than one foreign country.

TABLE 2.1: Enforcement actions after FCPA violations

This table reports the distribution of the enforcement actions for FCPA violations over time. The 451 enforcement actions, issued by the U.S. DOJ and the SEC from April 1978 through December 2019, are against 203 unique firms available in Compustat. Therefore, the average number of enforcement actions per firm is 2.2.

| Year of Enforcement | Observations | Frequency |
|---|---|---|
| 1978 | 2 | 0.44% |
| 1986 | 1 | 0.22% |
| 1988 | 1 | 0.22% |
| 1989 | 4 | 0.89% |
| 1990 | 3 | 0.67% |
| 1991 | 1 | 0.22% |
| 1992 | 1 | 0.22% |
| 1993 | 1 | 0.22% |
| 1994 | 2 | 0.44% |
| 1996 | 1 | 0.22% |
| 1997 | 2 | 0.44% |
| 1999 | 2 | 0.44% |
| 2000 | 2 | 0.44% |
| 2001 | 7 | 1.55% |
| 2002 | 7 | 1.55% |
| 2003 | 2 | 0.44% |
| 2004 | 9 | 2.00% |
| 2005 | 12 | 2.66% |
| 2006 | 14 | 3.10% |
| 2007 | 37 | 8.20% |
| 2008 | 31 | 6.87% |
| 2009 | 22 | 4.88% |
| 2010 | 51 | 11.31% |
| 2011 | 30 | 6.65% |
| 2012 | 28 | 6.21% |
| 2013 | 18 | 3.99% |
| 2014 | 20 | 4.43% |
| 2015 | 12 | 2.66% |
| 2016 | 47 | 10.42% |
| 2017 | 24 | 5.32% |
| 2018 | 25 | 5.54% |
| 2019 | 32 | 7.10% |
| *Total* | *451* | *100.00%* |

and almost 50 percent involve both the U.S. DOJ and the SEC. In addition, bribing firms' performance, measured as sales growth and profitability, improves during bribing years, suggesting that corruption leads to unfair benefits for corrupt firms.

After screening the enforcement actions and examining their characteristics (Tables 2.1—2.3), I identify the years in which firms have bribed and gained business and financial benefits. I focus on bribe payments between 2002 and 2016 for two reasons. First, the database I use to construct the proxies for financial misstatements (Audit Analytics) covers restatements from the early 2000s and is likely incomplete before that date (Gonzales, Schmid, and Yermack, 2013; Karpoff et al., 2017). Second, the observations

TABLE 2.2: Countries where bribes are paid

This table reports the countries where firms bribe, in alphabetical order. As the enforcement actions generally cover more than one country where firms bribe, the total number of observations (510) is higher than that reported in Table 2.1 (451).

| Country of Bribery | Observations | Frequency | Country of Bribery | Observations | Frequency |
|---|---|---|---|---|---|
| Albania | 1 | 0.20% | Laos | 1 | 0.20% |
| Algeria | 1 | 0.20% | Latvia | 1 | 0.20% |
| Angola | 12 | 2.35% | Lebanon | 1 | 0.20% |
| Argentina | 13 | 2.55% | Liberia | 1 | 0.20% |
| Azerbaijan | 4 | 0.78% | Libya | 6 | 1.18% |
| Bahamas | 1 | 0.20% | Lithuania | 1 | 0.20% |
| Bahrain | 2 | 0.39% | Luxembourg | 1 | 0.20% |
| Bangladesh | 6 | 1.18% | Macedonia | 1 | 0.20% |
| Belgium | 1 | 0.20% | Madagascar | 1 | 0.20% |
| Benin | 2 | 0.39% | Malawi | 1 | 0.20% |
| Bolivia | 1 | 0.20% | Malaysia | 3 | 0.59% |
| Bosnia-Herzegovina | 1 | 0.20% | Mali | 2 | 0.39% |
| Brazil | 21 | 4.12% | Mauritania | 3 | 0.59% |
| Bulgaria | 2 | 0.39% | Mexico | 17 | 3.33% |
| Burkina Faso | 2 | 0.39% | Mongolia | 1 | 0.20% |
| Burundi | 1 | 0.20% | Morocco | 2 | 0.39% |
| Cameroon | 1 | 0.20% | Mozambique | 3 | 0.59% |
| Canada | 1 | 0.20% | Myanmar | 1 | 0.20% |
| Chad | 1 | 0.20% | Nepal | 1 | 0.20% |
| Chile | 1 | 0.20% | Netherlands | 1 | 0.20% |
| China | 62 | 12.16% | Nicaragua | 2 | 0.39% |
| Colombia | 5 | 0.98% | Niger | 4 | 0.78% |
| Costa Rica | 3 | 0.59% | Nigeria | 24 | 4.71% |
| Croatia | 3 | 0.59% | North Korea | 2 | 0.39% |
| Cuba | 1 | 0.20% | Oman | 2 | 0.39% |
| Czech Republic | 2 | 0.39% | Pakistan | 4 | 0.78% |
| Democratic Republic of the Congo | 6 | 1.18% | Palestinian Territory | 1 | 0.20% |
| Djibouti | 1 | 0.20% | Panama | 3 | 0.59% |
| Dominican Republic | 2 | 0.39% | Peru | 3 | 0.59% |
| Ecuador | 4 | 0.78% | Philippines | 4 | 0.78% |
| Egypt | 8 | 1.57% | Poland | 6 | 1.18% |
| Equatorial Guinea | 2 | 0.39% | Qatar | 2 | 0.39% |
| France | 2 | 0.39% | Romania | 4 | 0.78% |
| Gabon | 5 | 0.98% | Russia | 16 | 3.14% |
| Georgia | 1 | 0.20% | Rwanda | 1 | 0.20% |
| Germany | 1 | 0.20% | Saudi Arabia | 15 | 2.94% |
| Ghana | 4 | 0.78% | Senegal | 3 | 0.59% |
| Greece | 8 | 1.57% | Serbia and Montenegro | 1 | 0.20% |
| Guatemala | 1 | 0.20% | Slovakia | 1 | 0.20% |
| Guinea | 4 | 0.78% | South Africa | 2 | 0.39% |
| Haiti | 1 | 0.20% | South Korea | 6 | 1.18% |
| Honduras | 1 | 0.20% | Spain | 2 | 0.39% |
| Hungary | 3 | 0.59% | Syria | 2 | 0.39% |
| India | 18 | 3.53% | Taiwan | 4 | 0.78% |
| Indonesia | 19 | 3.73% | Tanzania | 1 | 0.20% |
| Iran | 3 | 0.59% | Thailand | 11 | 2.16% |
| Iraq | 23 | 4.51% | Turkey | 5 | 0.98% |
| Israel | 3 | 0.59% | Turkmenistan | 2 | 0.39% |
| Italy | 3 | 0.59% | Ukraine | 3 | 0.59% |
| Ivory Coast | 2 | 0.39% | Uganda | 1 | 0.20% |
| Jamaica | 1 | 0.20% | United Arab Emirates | 6 | 1.18% |
| Jordan | 1 | 0.20% | Uzbekistan | 6 | 1.18% |
| Kazakhstan | 10 | 1.96% | Venezuela | 8 | 1.57% |
| Kenya | 2 | 0.39% | Vietnam | 7 | 1.37% |
| Kuwait | 4 | 0.78% | Yemen | 1 | 0.20% |
| Kyrgyzstan | 1 | 0.20% | Zimbabwe | 1 | 0.20% |
| *Total* | *295* | | *Total* | *215* | |

TABLE 2.3: Bribing firms and enforcement actions—An overview

This table reports the descriptive statistics of the variables associated with bribery cases and enforcement actions. *Bribes Paid* measures the amount of the bribes that bribing firms pay to obtain or retain business, in millions USD. *Sanctions* is the amounts of the penalties charged against bribing firms by the U.S. DOJ and the SEC as part of the case resolutions, in millions USD. *Bribing Years* is the average number of bribing years, by enforcement action. *Period Bribery-Enforcement* measures the number of years between the average bribing year and the year of the enforcement action. *Executives Involved* is an indicator equal to 1 if the enforcement actions are against executives, and 0 otherwise. *M&A Involved* is an indicator equal to 1 if an M&A operation takes place during the bribing period, and 0 otherwise. *Compliance Obligation* is an indicator equal to 1 if the resolutions of the bribery cases require compliance obligations, and 0 otherwise. *DOJ & SEC Involved* is an indicator equal to 1 if the case resolutions involve both the U.S. DOJ and the SEC, and 0 otherwise. *Growth Before Bribery* is the annual percentage change in sales in the years preceding each bribery case. *Growth During Bribery* is the annual percentage change in sales during bribing years. *ROA Before Bribery* is the return on assets in the years preceding each bribery case. *ROA During Bribery* is the return on assets during bribing years.

| | $25^{th}$ | Median | Mean | $75^{th}$ | S.D. |
|---|---|---|---|---|---|
| Bribes Paid ($ mln) | 0.086 | 1.440 | 66.644 | 13.440 | 270.339 |
| Sanctions ($ mln) | 1.878 | 13.024 | 75.558 | 44.092 | 176.703 |
| Bribing Years | 3.000 | 5.000 | 5.688 | 8.000 | 4.142 |
| Period Bribery-Enforcement | 4.500 | 6.500 | 6.533 | 8.500 | 2.917 |
| Executives Involved | 0.000 | 0.000 | 0.276 | 1.000 | 0.448 |
| M&A Involved | 0.000 | 0.000 | 0.207 | 0.000 | 0.406 |
| Compliance Obligation | 0.000 | 1.000 | 0.631 | 1.000 | 0.484 |
| DOJ & SEC Involved | 0.000 | 0.000 | 0.488 | 1.000 | 0.501 |
| Growth Before Bribery | -0.046 | 0.045 | 0.090 | 0.153 | 0.503 |
| Growth During Bribery | -0.012 | 0.089 | 0.133 | 0.196 | 0.430 |
| ROA Before Bribery | 0.073 | 0.119 | 0.118 | 0.180 | 0.217 |
| ROA During Bribery | 0.079 | 0.124 | 0.139 | 0.190 | 0.117 |

involving bribe payments both before the early 2000s and after 2016 are limited, and are thus not representative of the population of corrupt firms. The lack of data in recent years is likely due to the time that regulators need to identify a bribery event, conduct the investigations, and release the enforcement actions.

Table 2.4 presents the sample selection procedure. The final sample of corrupt firm years consists of firms with securities listed in the U.S., with operations in non-financial industries, and with data available for variable construction. Between 2002 and 2016, Panel A, Table 2.4 reports 684 bribing firm year observations (i.e., 134 unique firms) that meet these requirements.[12]

Using bribing firms and years, I construct the sample of non-bribing firms to test my hypotheses. The sample includes all remaining firms publicly listed in the U.S., with

[12] In contrast to the sample in Tables 2.1—2.3 that focuses on the release of the enforcement actions, the sample of bribing firms in Table 2.4 identifies the years of the illicit payments for each enforcement action.

the same two-digit SIC code as the bribing firms and with data available in Compustat, CRSP, and Thomson Reuters for variable construction. To mitigate the heterogeneity in industry characteristics and to follow previous studies, I limit the sample to firms with the same two-digit SIC code as the bribing firms (e.g., Beatty, Liao, and Yu, 2013). Moreover, this sample selection procedure allows me to exploit the time-series variation in my data and control for firm fixed characteristics in the event study analysis that I report in Section 2.4.

I split the sample of non-bribing firms into non-bribing peers and other non-bribing firms according to the indicator variable *Bribe Exposure*. This indicator equals 1 if firms are exposed to at least one bribing competitor in the same fiscal year $t$ and industry $j$, and 0 if firms are not exposed to bribing rivals. Non-bribing firms exposed to bribing rivals (i.e., non-bribing peers) have the same year and three-digit SIC code as the bribing firms. Non-exposed firm year observations include firm years with the same two-digit SIC code as the bribing firms but a different three-digit SIC code, and firms with the same three-digit SIC code as the bribing firms in non-bribing years.[13]

Panel B, Table 2.4, shows that the initial sample of U.S. publicly listed non-bribing firms between 2002 and 2016 has 166,452 observations. From this sample, I exclude 47,521 observations with no bribing rival in the same two-digit SIC group. I also remove 65,784 observations because of missing data for variable construction and 8,433 financial firm years because the highly-regulated financial industry differs from less-regulated non-financial industries. From 2002 to 2016, the final sample consists of 44,714 non-bribing firm year observations, 23,760 of which were exposed and 20,954 not exposed to rivals'

[13] This division differs from that of a difference-in-differences research design, because the existence of multiple bribery cases in the same year and three-digit SIC group prevents me from identifying time indicators. Suppose that two firms with three-digit SIC code equal to 100 bribe, and the first does so in 2005 and 2006, whereas the second does so in 2008 and 2009. Assuming that treated (peer) firms are non-bribing firms with the same three-digit SIC code as the bribers (i.e., 100), whereas control firms are non-bribing firms with the same two-digit SIC code as the bribers (i.e., 10) but a different three-digit SIC code, I would have the same control firm for multiple treated firms and would be unable to identify a unique time indicator for the control group. In contrast, *Bribe Exposure* equals 1 in 2005, 2006, 2008, and 2009 (i.e., all bribing years, regardless of the bribing rival), and 0 in all other years when firms' three-digit SIC code is 100. Moreover, *Bribe Exposure* equals 0 for all the remaining firms with two-digit SIC code equal to 10 in all years.

bribery.[14,15]

TABLE 2.4: Sample selection

| | |
|---|---|
| **Panel A: Bribing firms** | |
| Public firm year observations in Compustat with anti-FCPA enforcement actions between 1978 and 2019 | 1,336 |
| - Firm year observations with bribe payments both before and after 2002-2016 | (379) |
| - Firm year observations with missing data for variable construction | (204) |
| - Firm year observations within financial industry | (69) |
| *= Final sample of bribing firm year observations (2002-2016)* | *684* |
| **Panel B: Non-bribing firms** (i.e., remaining firms listed in the U.S. without any enforcement action for FCPA violations) | |
| Public firm year observations between 2002 and 2016 | 166,452 |
| - Firm year observations with two-digit SIC codes other than those of bribing firms | (47,521) |
| - Firm year observations with missing data for variable construction | (65,784) |
| - Firm year observations within financial industry | (8,433) |
| *= Final sample of non-bribing firm year observations (2002-2016)* | *44,714* |
| Non-bribing firm year observations with bribe exposure (*Bribe Exposure* = 1) | 23,760 |
| Non-bribing firm year observations without bribe exposure (*Bribe Exposure* = 0) | 20,954 |

### 2.3.2 Empirical methodology

With the first hypothesis (H1), I investigate whether firms' involvement in bribing activities affects the frequency of non-bribing peers' income-increasing financial misstatements. To test this hypothesis, I perform the following multivariate regression analysis,

$$Misstatement(Up)(Down)_{i,t} = \alpha + \beta Bribe\ Exposure_{j,t} + \delta \boldsymbol{Z}_{i,t} + a_j + b_t + \epsilon_{i,t} \tag{2.1}$$

The dependent variable proxies for financial misstatements. I measure these misstatements in three different ways. The first dependent variable captures both income-increasing and income-decreasing misstatements, and identifies all non-bribing firms (both exposed and non-exposed to bribing rivals) that restate their financial statements due

[14] On average, I find 35 peers for 1 bribing firm. Nevertheless, I do not expect all peers to be equally and directly affected by rivals' bribery, as only few have highly overlapping product lines or common analysts. The tests of H2 and H3 exploit these cross-sectional differences within the sample of non-bribing peers.

[15] The unreported sample split by year reveals that the number of bribing firm year observations increases from 2002 to 2006 and decreases afterwards, similarly to the sample of non-bribing peers. In contrast, firm year observations not exposed to rivals' corruption increase over the most recent years (2007-2016). The decrease in bribing firm years is likely due to the enforcement actions being issued, on average, 6.5 years after firms bribe. Moreover, the unreported sample split by industry reveals that bribery occurs mainly in the industries of agriculture, mining, manufacturing, and wholesale trade of nondurable goods.

to previous financial misstatements. I compute this variable from the Non-reliance Restatement database of Audit Analytics, which allows me to detect the fiscal year(s) in which firms have misstated their accounting numbers.[16] Specifically, *Misstatement* is an indicator equal to 1 if firm $i$ engages in financial misstatement activities in fiscal year $t$ and is sanctioned by a subsequent restatement, and 0 otherwise.

Although the majority of financial misstatements are performance-enhancing (Kothari, Mizik, and Roychowdhury, 2016), I construct the second dependent variable to test more directly whether firms misstate their financial statements upward. Specifically, *Misstatement Up* is an indicator that equals 1 if firm $i$ engages in income-increasing misstatement activities in fiscal year $t$ and is sanctioned by a subsequent restatement, and 0 otherwise. The third dependent variable, which I include for completeness, proxies for income-decreasing financial misstatements. This variable is *Misstatement Down*, an indicator that equals 1 if firm $i$ engages in income-decreasing misstatement activities in fiscal year $t$ and is sanctioned by a subsequent restatement, and 0 otherwise.[17]

The independent variable used for testing H1 is *Bribe Exposure*, an indicator that equals 1 if firm $i$ is exposed to at least one bribing competitor in the same fiscal year $t$ and industry $j$, and 0 otherwise.[18] Non-bribing firms exposed to bribing rivals (i.e., non-bribing peers) have the same year and industry as the bribing firms, with industry determined at the three-digit SIC level. Non-exposed firm years include firm years with the same two-digit SIC code as the bribing firms but a different three-digit SIC code, and firms with the same three-digit SIC code as the bribing firms in non-bribing years.

If non-bribing peers misstate their financial statements to report results comparable to the unfair gains of corrupt rivals, I expect $\beta$ to be positive when the dependent

[16] The proxies for financial misstatements rely on the assumption that restatements represent corrections to financial statement misstatements that negligent and opportunistic managers previously made (Baber et al., 2015).

[17] In unreported alternative specifications, I adjust the dependent variables as follows: When I focus on income-increasing (income-decreasing) misstatements, I exclude income-decreasing (income-increasing) misstatements from the sample. Testing my hypotheses with these dependent variables confirms the main evidence.

[18] Although the enforcement actions show when firms start bribing, the benefits of bribery may fully materialize in later periods, thus shifting the effective exposure to rivals' bribery later. In unreported tests, I confirm all the results by excluding the first year in which corrupt firms bribe.

variable captures performance-enhancing misstatements (i.e., *Misstatement Up*). If non-bribing peers misstate their financial statements because rivals' corruption pervades the business environment (e.g., Den Nieuwenboer and Kaptein, 2008), any type of financial misstatements could occur, and $\beta$ would be positive when the dependent variable captures income-increasing financial misstatements, income-decreasing financial misstatements, or both. If non-bribing peers respond to rivals' bribery by becoming more efficient and competitive through strategic decisions (Galang, 2012), or do not react to rivals' bribery, I would not observe any spillover effects from corruption, and $\beta$ would be statistically insignificant.

I do not expect all non-bribing peers to be equally affected by rivals' bribery. With the second hypothesis, I investigate which conditions exacerbate the competitive disadvantages and loss of income of non-bribing peers. In line with the assumption that non-bribing peers' loss of income is proportional to bribing rivals' unfair gains, I first test whether the spillover effects are more severe when bribing firms earn high unfair gains (H2a). To test this hypothesis, I perform the following multivariate regression analysis,

$$Misstatement\ Up_{i,t} = \alpha + \beta Rival\ Bribes_{j,t} + \delta \boldsymbol{Z}_{i,t} + a_j + b_t + \epsilon_{i,t} \tag{2.2}$$

The dependent variable is *Misstatement Up*, an indicator equaling 1 when firms engage in income-increasing financial misstatements and are sanctioned by subsequent restatements, and 0 otherwise. The independent test variable is *Rival Bribes*, which proxies for the magnitude of the loss of income that non-bribing peers are likely to experience because of bribing rivals' unfair gains. I obtain this variable as follows. First, I compute the natural logarithm of the amounts of the bribes from bribing firms in each bribing year and industry (three-digit SIC). Second, I assign this variable to non-bribing peers according to their year and industry group (three-digit SIC). I consider the bribe amounts a good proxy for corrupt firms' unfair gains because the illicit payments are proportional to the benefits earned. These benefits, in turn, constitute an important determinant of unethical actions (e.g., Cheung, Rau, and Stouraitis, 2012; Draca, Koutmeridis, and

Machin, 2019). Because I examine whether non-bribing peers respond to bribing rivals' gains, H2a focuses on performance-enhancing financial misstatements.

Moreover, I test whether bribing firms' closest competitors are more likely to misstate their financial statements than other non-bribing firms that are exposed to bribing rivals but that are less competitively close. This test relies on the assumption that bribing firms' illicit payments cause non-bribing peers' loss of profitable deals and additional income. To test H2b, I perform the following multivariate regression analysis,

$$Misstatement\ Up_{i,t} = \alpha + \beta Rival\ Product\ Similarity_{j,t} + \delta \boldsymbol{Z}_{i,t} + a_j + b_t + \epsilon_{i,t} \tag{2.3}$$

In line with H2a, in H2b I focus on income-increasing financial misstatements and use *Misstatement Up* as the dependent variable. To compute the independent test variable, I use the firm pairwise similarity scores that Hoberg and Phillips (2016) obtain by analyzing the product descriptions contained in firms' 10-K filings. Specifically, I construct *Rival Product Similarity* as an indicator that equals 1 if firm $i$ has at least one bribing competitor in the same fiscal year $t$ and industry $j$ with a pairwise similarity score above the median, and 0 otherwise. I expect that non-bribing peers with the strongest incentives to misstate their financial statements upward are bribing firms' closest competitors.[19]

With the third hypothesis, I investigate whether a possible mechanism underlying the spillover effects of corruption on non-bribing peers' financial misstatements is relative performance pressure. To test H3, I perform the following multivariate regression analysis,

$$\begin{aligned} Misstatement\ Up_{i,t} = \alpha + \beta Common\ Analyst(Percentage\ Common\ Analyst)_{j,t} + \\ + \delta \boldsymbol{Z}_{i,t} + a_j + b_t + \epsilon_{i,t} \end{aligned} \tag{2.4}$$

The dependent variable, *Misstatement Up*, is the same as in equations (2.2) and (2.3).

[19] Previous studies argue that U.S. non-bribing firms under FCPA regulation face competitive disadvantages particularly when they do business abroad, because their European and Asian competitors beat them out of profitable deals by bribing (e.g., Kurer, 1993; Brown et al., 2021). In unreported analyses, I confirm all the main findings by using the sample of non-bribing firms that do business abroad (i.e., firms with non-zero income taxes from foreign operations reported in Compustat).

The first independent test variable is *Common Analyst*, an indicator that equals 1 if firm $i$ has at least one financial analyst in common with bribing firms in fiscal year $t$ and industry $j$, and 0 otherwise. The second independent test variable is *Percentage Common Analyst*, which measures the percentage of common financial analysts between bribing firms and their non-bribing peers in a given fiscal year. I expect that having the same financial analyst(s) as bribing firms increases peer performance pressures and the likelihood that non-bribing peers will misstate their financial performance more often than other firms that are exposed to corruption but that have few or no analysts in common.

The models in equations (2.1)—(2.4) include additional control variables, $\boldsymbol{Z}$, (which the literature associates with corporate misconduct), particularly financial manipulation incentives and ability, industry structure, firm characteristics, and firm performance (e.g., Beatty, Liao, and Yu, 2013; Kedia, Koh, and Rajgopal, 2015; Liu, 2016). To proxy for manipulation incentives, I use leverage, reported losses, and market-to-book ratio. Firms with high leverage have more incentives to misstate their financial statements to avoid debt covenant violations (Richardson, Tuna, and Wu, 2003). Moreover, firms misstate their financial statements to window-dress losses (Healy and Wahlen, 1999) or signal high future growth opportunities (Povel, Singh, and Winton, 2007). I measure *Leverage* as the ratio of short- and long-term debt to lagged total assets; *Loss* as an indicator equaling 1 if firms report losses in the current fiscal year, and 0 otherwise; and *Market-to-Book* as the market value of assets scaled by the book value of assets.

As Big Four auditors scrutinize firms' financial statements to prevent misstatements (DeFond and Zhang, 2014), to proxy for manipulation ability I use *Big4*, an indicator equaling 1 if firms are audited by Big Four audit firms, and 0 otherwise. In addition, to proxy for industry concentration, I use *Normalized HHI*, which is the normalized value of the Herfindahl-Hirschman Index (e.g., Datta, Iskandar-Datta, and Singh, 2013; Balakrishnan and Cohen, 2014).

I also include the following firm characteristics and performance measures: *Age* is the natural logarithm of one plus firm age, which is the number of years since the inclusion of the firms in Compustat. *Buy-and-Hold Return* measures firms' stock performance as

the contemporaneous twelve-month buy-and-hold return. *Capex* is the ratio of capital expenditure to lagged property, plant, and equipment, and proxies for the barriers to market entry (Porter, 1979). *CFO* is cash flows from operations scaled by lagged total assets. *Earnings Volatility* is the standard deviation of earnings per share for the twelve quarters ending with the year of observation. *Growth* accounts for firms' growth and is the annual percentage change in sales for firm $i$ in fiscal year $t$. *Institutional Investor* measures the percentage of institutional ownership—an important corporate governance mechanism that reduces agency conflicts and limits managers' accounting discretion (Jiambalvo, Rajgopal, and Venkatachalam, 2010). *ROA* measures firms' profitability and is the ratio of operating income before depreciation to lagged total assets, and *Size* proxies for firms' size and is the natural logarithm of lagged total assets.

I include industry (three-digit SIC) and year fixed effects ($a_j$ and $b_t$, respectively) in the empirical models in equations (2.1)—(2.4). To estimate the coefficients, I use logistic regressions, and I cluster the standard errors at the firm level.

### 2.3.3 Descriptive statistics

Table 2.5 provides the descriptive statistics for the main variables, which I present separately for bribing firm year observations, non-bribing peers (i.e., non-bribing firms exposed to bribing rivals), and firm years not-exposed to bribing rivals.

Section 1 of Table 2.5 shows that bribing firms in bribing years are on average bigger and more profitable than non-bribing firms, consistent with the evidence that corruption is performance-enhancing, particularly when bribing firms are leading market players (e.g., Zhou and Peng, 2012). The analysis of non-bribing firms in sections 2 and 3 of Table 2.5 shows that the average frequency of misstatements detected by subsequent restatements (*Misstatement*) is 13.2 percent for firms exposed to bribing rivals, and 12.7 percent for non-exposed firms.[20] Moreover, the frequency of income-increasing financial

[20] Bribing firm years have higher misstatement rates, on average, than the rest of the sample. I assume that a likely reason for this evidence is that, during bribing years, corrupt firms hide the illicit payments through accounting misstatements. However, I argue that such misstatements do not affect peers' propensity to misbehave.

TABLE 2.5: Summary statistics

This table provides descriptive statistics of the variables used for testing H1, H2, and H3, by type of firm. The section *Bribing Firm Years* presents descriptive statistics for the sample of bribing firms. The section *Exposed Firm Years* reports descriptive statistics for the sample of firms exposed to bribing rivals (i.e., non-bribing peers), and the section *Non-exposed Firm Years* provides descriptive statistics for the sample of firms not exposed to bribing rivals. All variables are defined in Appendix 2.A.

| | (1) Bribing Firm Years *Obs. 684* | | | (2) Exposed Firm Years *Obs. 23,760* | | | (3) Non-exposed Firm Years *Obs. 20,954* | | |
|---|---|---|---|---|---|---|---|---|---|
| | Mean | Median | S.D. | Mean | Median | S.D. | Mean | Median | S.D. |
| **Panel A: Dependent variables** | | | | | | | | | |
| Misstatement | 0.151 | 0.000 | 0.358 | 0.132 | 0.000 | 0.339 | 0.127 | 0.000 | 0.333 |
| Misstatement Up | 0.127 | 0.000 | 0.333 | 0.113 | 0.000 | 0.316 | 0.105 | 0.000 | 0.307 |
| Misstatement Down | 0.035 | 0.000 | 0.184 | 0.027 | 0.000 | 0.162 | 0.028 | 0.000 | 0.164 |
| Misstatement Revenue Up | 0.066 | 0.000 | 0.248 | 0.031 | 0.000 | 0.172 | 0.023 | 0.000 | 0.151 |
| Misstatement Revenue Down | 0.003 | 0.000 | 0.054 | 0.005 | 0.000 | 0.068 | 0.004 | 0.000 | 0.061 |
| **Panel B: Independent test variables** | | | | | | | | | |
| Bribe Exposure | N/A | N/A | N/A | 1.000 | 1.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Rival Bribes | N/A | N/A | N/A | 2.880 | 2.231 | 1.868 | N/A | N/A | N/A |
| Rival Product Similarity | N/A | N/A | N/A | 0.207 | 0.000 | 0.405 | N/A | N/A | N/A |
| Common Analyst | N/A | N/A | N/A | 0.312 | 0.000 | 0.463 | N/A | N/A | N/A |
| Percentage Common Analyst | N/A | N/A | N/A | 0.099 | 0.000 | 0.199 | N/A | N/A | N/A |
| **Panel C: Other independent variables** | | | | | | | | | |
| *Manipulation incentives* | | | | | | | | | |
| Leverage | 0.189 | 0.173 | 0.140 | 0.141 | 0.043 | 0.203 | 0.218 | 0.184 | 0.217 |
| Loss | 0.045 | 0.000 | 0.208 | 0.309 | 0.000 | 0.462 | 0.142 | 0.000 | 0.349 |
| Market-to-Book | 1.621 | 1.295 | 1.177 | 2.092 | 1.413 | 2.871 | 1.458 | 1.091 | 1.419 |
| *Manipulation ability* | | | | | | | | | |
| Big4 | 0.940 | 1.000 | 0.238 | 0.745 | 1.000 | 0.436 | 0.753 | 1.000 | 0.431 |
| *Industry structure* | | | | | | | | | |
| Normalized HHI | 0.050 | 0.038 | 0.034 | 0.039 | 0.032 | 0.026 | 0.052 | 0.041 | 0.041 |
| *Firm characteristics and performance* | | | | | | | | | |
| Age | 3.124 | 3.091 | 0.704 | 2.660 | 2.639 | 0.660 | 2.899 | 2.890 | 0.741 |
| Buy-and-Hold Return | 0.146 | 0.106 | 0.473 | 0.123 | 0.012 | 0.675 | 0.142 | 0.071 | 0.593 |
| Capex | 0.244 | 0.217 | 0.132 | 0.301 | 0.245 | 0.219 | 0.219 | 0.172 | 0.173 |
| CFO | 0.097 | 0.093 | 0.085 | -0.017 | 0.068 | 0.345 | 0.054 | 0.077 | 0.190 |
| Earnings Volatility | 0.435 | 0.318 | 0.348 | 0.315 | 0.190 | 0.323 | 0.409 | 0.266 | 0.380 |
| Growth | 0.122 | 0.099 | 0.211 | 0.118 | 0.084 | 0.352 | 0.083 | 0.058 | 0.297 |
| Institutional Investor | 0.395 | 0.345 | 0.370 | 0.383 | 0.310 | 0.360 | 0.437 | 0.440 | 0.360 |
| ROA | 0.133 | 0.125 | 0.106 | -0.021 | 0.081 | 0.424 | 0.070 | 0.105 | 0.246 |
| Size | 8.873 | 8.886 | 2.022 | 5.724 | 5.524 | 2.175 | 6.539 | 6.575 | 2.092 |

misstatements (*Misstatement Up*) is 11.3 percent for firm years exposed to corruption and 10.5 percent for firm years not exposed to corruption, whereas income-decreasing financial misstatements (*Misstatement Down*) are less than 3 percent in both subsamples. This descriptive evidence confirms that income-increasing misstatements are more common

than income-decreasing ones (e.g., Kothari, Mizik, and Roychowdhury, 2016).

For the independent variables, the natural logarithm of the bribe amounts from corrupt firms and aggregated by year and industry (*Rival Bribes*) is on average 2.88.[21] Around 21 percent of the sample of the non-bribing peers have at least one bribing rival with a product market similarity score above the median (*Rival Product Similarity*). Moreover, 31.2 percent of non-bribing peers share with their bribing rivals on average at least one financial analyst (*Common Analyst*), and the percentage of common analysts (*Percentage Common Analyst*) is around 10 percent. The remaining descriptive statistics are similar to those of studies on the determinants of corporate misconduct (e.g., Beatty, Liao, and Yu, 2013; Kedia, Koh, and Rajgopal, 2015; Liu, 2016; Du and Shen, 2018).

## 2.4 Empirical results

### 2.4.1 Main results—Exposure to bribery and financial misstatements

Table 2.6, Panel A, presents the results of testing H1 from equation (2.1). With *Misstatement* as the dependent variable, column 1 shows that firms exposed to bribing rivals have a higher likelihood of engaging in financial misstatements than firms not exposed to bribery. The coefficient of *Bribe Exposure* is positive (0.141) and statistically significant at the 5 percent level. Column 2 reports the results when the dependent variable captures income-increasing financial misstatements (*Misstatement Up*). In line with H1, I find that corruption positively affects non-bribing peers' upward financial misstatements. Specifically, the coefficient of *Bribe Exposure* is positive and statistically significant (coef. 0.196, z-stat 2.65). In terms of economic impact, non-bribing peers have a predicted probability of misstating their financial statements upward by 1.9 more percentage points than

[21] The dollar value of the natural logarithm of the bribes differs from the amount reported in Table 2.3 for the following three reasons. First, Table 2.3 and Table 2.5 use different samples. Second, the natural logarithm reported in Table 2.5 aggregates bribe payments by year and industry. Third, as the logarithmic function is nonlinear, the mean of the logarithm of the bribes is not equal to the logarithm of the mean of the bribes.

non-exposed firms. In contrast, the exposure to rivals' bribery does not affect income-decreasing financial misstatements: The coefficient of *Bribe Exposure* in column 3 is statistically insignificant.

For the control variables, the results show that firms with high *Leverage* misstate their financial statements more often, possibly to avoid debt covenant violations (Richardson, Tuna, and Wu, 2003). Moreover, similar to previous studies, the results indicate that firms with high *Earnings Volatility* and *Growth* engage more in financial misstatement activities (e.g., Povel, Singh, and Winton, 2007; Johnson, Ryan, and Tian, 2009).

Although non-bribing peers could misstate any accounting number to report a higher performance, I expect them to engage primarily in revenue misstatements, because bribery allows corrupt firms to obtain new contracts and businesses that generate additional revenues (Ryan, 2000). After replacing the dependent variables of equation (2.1) with proxies for upward and downward revenue misstatements, I examine whether non-bribing firms exposed to bribing rivals engage in revenue misstatements more than non-exposed firms. Panel B, Table 2.6, reports the results of this analysis. The dependent variable in column 4 (5) is *Misstatement Revenue Up* (*Down*), an indicator that equals 1 if firms misstate their revenues upward (downward) in a given fiscal year and are sanctioned by subsequent restatements, and 0 otherwise. Consistent with my expectation, the results suggest that non-bribing peers are more likely to misstate their revenues upward than firms not exposed to rivals' corruption (coef. 0.263, z-stat. 1.92 in column 4).

Overall, the empirical findings of H1 suggest that the corrupt actions of some industry members affect non-bribing peers' upward financial misstatements, and non-bribing peers misstate their financial statements to make their performance comparable to the unfair gains of corrupt rivals. Such manipulations primarily affect revenues.

The research design employed thus far assumes no unobservable differences in the propensity toward financial misstatements between firms exposed and non-exposed to rivals' bribery. To control for unobservable firm fixed characteristics, I exploit the time-series variation of my data. I perform an event study analysis comparing financial misstatement frequencies of non-bribing firms before and during the bribing years. As every

TABLE 2.6: Hypothesis 1—Financial misstatements and bribe exposure

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively. This table presents the results of estimating equation (2.1) to test the association between firms' exposure to bribery (i.e., the existence of at least one bribing industry rival) and their likelihood of engaging in financial misstatement activities. Two-tailed z-statistics, based on standard errors adjusted for firm-level clustering, are presented in parentheses. Marginal effects, where reported, are in brackets below the z-statistics. Continuous variables are winsorized at the 1 percent and 99 percent levels. All variables are defined in Appendix 2.A.

| | **Panel A: All Manipulations** | | | **Panel B: Revenue Manipulations** | |
|---|---|---|---|---|---|
| | (1) Misstatement | (2) Misstatement Up | (3) Misstatement Down | (4) Misstatement Revenue Up | (5) Misstatement Revenue Down |
| *Test variable* | | | | | |
| **Bribe Exposure** | **0.141**** | **0.196***** | **-0.065** | **0.263*** | **-0.007** |
| | **(2.05)** | **(2.65)** | **(-0.49)** | **(1.92)** | **(-0.02)** |
| | **[0.015]** | **[0.019]** | **[-0.002]** | **[0.007]** | **[-0.000]** |
| *Manipulation incentives* | | | | | |
| Leverage | 0.681*** | 0.726*** | 0.437** | 0.369 | 0.039 |
| | (6.34) | (6.24) | (2.05) | (1.55) | (0.07) |
| Loss | -0.075 | -0.119* | 0.125 | -0.081 | -0.306 |
| | (-1.22) | (-1.81) | (1.06) | (-0.65) | (-0.97) |
| Market-to-Book | -0.034** | -0.046** | -0.021 | -0.119** | -0.039 |
| | (-2.11) | (-2.45) | (-1.13) | (-2.58) | (-0.53) |
| *Manipulation ability* | | | | | |
| Big4 | -0.135** | -0.123* | -0.151 | 0.022 | -0.349 |
| | (-2.05) | (-1.74) | (-1.15) | (0.17) | (-1.01) |
| *Industry structure* | | | | | |
| Normalized HHI | 1.867 | 2.259 | 1.511 | -1.309 | -0.114 |
| | (1.32) | (1.32) | (0.47) | (-0.32) | (-0.01) |
| *Firm characteristics and performance* | | | | | |
| Age | 0.008 | 0.005 | 0.085 | -0.052 | -0.176 |
| | (0.19) | (0.11) | (1.01) | (-0.61) | (-0.85) |
| Buy-and-Hold Return | -0.059** | -0.068** | -0.049 | -0.089 | 0.153 |
| | (-2.20) | (-2.30) | (-0.99) | (-1.46) | (1.43) |
| Capex | 0.096 | 0.162 | -0.133 | 0.608*** | 0.923* |
| | (0.89) | (1.41) | (-0.59) | (3.09) | (1.78) |
| CFO | 0.351*** | 0.238* | 0.315* | 0.038 | 0.936 |
| | (2.76) | (1.67) | (1.75) | (0.12) | (1.60) |
| Earnings Volatility | 0.370*** | 0.383*** | 0.326*** | 0.415*** | -0.122 |
| | (5.58) | (5.36) | (2.66) | (3.17) | (-0.39) |
| Growth | 0.138*** | 0.165*** | 0.077 | 0.245** | 0.387* |
| | (2.66) | (2.87) | (0.76) | (2.34) | (1.69) |
| Institutional Investor | 0.265*** | 0.329*** | -0.017 | 0.247 | 0.323 |
| | (3.18) | (3.68) | (-0.10) | (1.38) | (0.83) |
| ROA | -0.370*** | -0.191* | -0.489*** | 0.077 | -0.679*** |
| | (-3.63) | (-1.70) | (-3.88) | (0.27) | (-2.92) |
| Size | -0.014 | -0.017 | 0.006 | 0.029 | -0.005 |
| | (-0.80) | (-0.90) | (0.19) | (0.80) | (-0.07) |
| Intercept | -2.080*** | -2.566*** | -3.764** | -3.429*** | -3.564 |
| | (-3.17) | (-3.55) | (-2.28) | (-3.51) | (-1.32) |
| Industry FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| Pseudo $R^2$ | 0.0395 | 0.0452 | 0.0443 | 0.0590 | 0.0729 |
| Obs. | 44,714 | 44,714 | 44,714 | 44,714 | 44,714 |

FCPA enforcement action represents an event, the bribing years represent that event duration. Following Beatty, Liao, and Yu (2013), I classify non-bribing firms as either exposed (treated) or non-exposed (control) to a specific bribery event.[22] For each bribery case at the three-digit SIC level, I classify treated firms as non-bribing firms with the same three-digit SIC code as the bribing firms, and classify control firms as non-bribing firms with the same two-digit SIC code as the bribing firms but with a different three-digit SIC code. Moreover, for each event I identify the pre-bribery period (the five years before the bribery occurs) and the during-bribery period (the first five years in which the bribery occurs).

To compare financial misstatement frequencies of treated and control firms before vis-à-vis during bribing years, I cannot associate control firms with multiple treatments. Therefore, to have a distinct time indicator for each control group, I select only the first bribery event in each two-digit SIC group and construct the time variables accordingly. Given that the results of H1 suggest that non-bribing peers engage in income-increasing financial misstatements when their rivals bribe, the next set of analyses focuses on upward financial misstatements.

The results of this event study appear in Panel A, Table 2.7. For parsimony, I display only the coefficient estimates of the test variables. However, the regressions include the control variables from equation (2.1), and firm and year fixed effects. The independent test variable is *Treat Bribing Years*, which represents the interaction between the treatment indicator and the time indicator.[23] The dependent variable is *Misstatement Up*, an indicator that equals 1 when firms misstate their financial statements upward, and 0 otherwise. Column 1 reports the results of the regression without the control variables, and column 2 reports the results of the regression with controls. In line with my expectation that the frequency of income-increasing financial misstatements is higher for firms

[22] As in the results of the main tests, I consider firms exposed to rivals' bribery to be non-bribing peers (treated firms).

[23] The treatment indicator equals 1 if non-bribing firms have the same three-digit SIC code as the bribers, and 0 if they have the same two-digit SIC code as the bribers but a different three-digit SIC code. The time indicator equals 1 in the first five bribing years in a given two-digit SIC group, and 0 in the five years immediately before.

TABLE 2.7: Identification strategy—Controlling for firm fixed characteristics

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively. Panel A reports the results of the event study analysis that compares the financial misstatement rates of firms exposed (treated) and non-exposed (control) to bribing rivals in the five years before the beginning of the first bribery event in each industry (two-digit SIC code) vis-à-vis during the first five bribing years in the same industry. Panel B reports the results of a placebo event study analysis performed over two pre-bribery periods. The multivariate linear regressions in columns 1—4 include control variables (where indicated), and firm and year fixed effects. Two-tailed t-statistics, based on standard errors adjusted for industry-level clustering, are presented in parentheses. Continuous variables are winsorized at the 1 percent and 99 percent levels. All variables are defined in Appendix 2.A.

| | **Panel A: Event Study** | | **Panel B: Placebo Event Study** | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | Misstatement Up | Misstatement Up | Misstatement Up | Misstatement Up |
| **Treat Bribing Years** | **0.031**** | **0.036**** | | |
| | **(2.69)** | **(2.21)** | | |
| **Treat Pre-Bribing Years** | | | **-0.005** | **-0.015** |
| | | | **(-0.42)** | **(-0.48)** |
| Intercept | 0.067*** | -0.305** | 0.074*** | 0.011 |
| | (26.05) | (-3.48) | (18.94) | (0.18) |
| Other Controls | No | Yes | No | Yes |
| Firm FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Adj. $R^2$ | 0.3215 | 0.3305 | 0.4397 | 0.4542 |
| Obs. | 5,696 | 3,341 | 3,735 | 1,988 |

exposed to corruption in bribing years, the coefficient of the variable *Treat Bribing Years* is positive and statistically significant, ranging from 0.031 in column 1 to 0.036 in column 2.[24]

To further ensure that the treated and control firms do not inherently differ in a persistent way, I perform a placebo event study by comparing firms' misstatement frequencies in two pre-bribery periods. In this test, I expect to find no differences between treated and control firms during these pre-bribery years. The results appear in Panel B, Table 2.7. The treatment and control groups are the same as in Panel A. However, the time indicator equals 1 in the two years before bribing firms start their illicit payments (*t-1* and *t-2*), and 0 in the years *t-3* and *t-4* from the bribery onset in each industry. The coefficient of the test variable, *Treat Pre-Bribing Years*, which captures the change in financial misstatement rates between the treated and control groups in the two pre-bribery periods, is no longer positive and significant.

[24] To preserve the sample size, I exclude the variable *Buy-and-Hold Return* from the analysis in column 2. When I include this variable, the coefficient estimate of *Treat Bribing Years* is slightly insignificant, although statistically indistinguishable from the coefficient estimated without *Buy-and-Hold Return* (the $Chi^2$ test for the difference in coefficients equals 0.37).

The findings in Table 2.7 provide some confidence that unobservable firm fixed characteristics do not drive the results of the main analysis. Moreover, that treated firms show evidence of spillover effects but control firms do not supports using the three-digit SIC industry definition for identifying non-bribing peers and performing the H2a, H2b, and H3 tests.

### 2.4.2 Main results—Bribing firms' unfair gains and peers' financial misstatements

Panel A, Table 2.8, reports the results of testing H2a and H2b through equations (2.2) and (2.3) with the sample of non-bribing peers (i.e., when *Bribe Exposure* equals 1). The results of H2a (column 1) suggest that non-bribing peers are more likely to misstate their financial statements when they experience high loss of income because of bribing rivals' unfair gains. The coefficient of *Rival Bribes* (0.495) is positive and statistically significant at the 5 percent level.

Moreover, the evidence in column 2 (H2b) shows that when bribing firms and their non-bribing peers have a high product market similarity (i.e., when *Rival Product Similarity* equals 1), non-bribing peers are more severely affected by rivals' corruption, in turn engaging in financial misstatements more often. From an economic standpoint, non-bribing peers with high product market similarity to their bribing rivals have a 2-percentage point higher predicted probability of engaging in income-increasing financial misstatements than less competitively close peers.

Panel B, Table 2.8, reports the results of testing the same hypotheses when revenue misstatements are the expected response to corruption. Columns 3 and 4 show that the peers of bribing firms manipulate their revenues upward more often when the bribes paid are high (H2a) and when the competitive similarity to corrupt rivals is significant (H2b). Overall, the findings in Table 2.8 suggest that the spillovers of corruption on peers' financial misstatements are severe when non-bribing peers are more likely to face loss of income due to bribing rivals' unfair benefits.

TABLE 2.8: Hypothesis 2—Financial misstatements and loss of income

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively. This table reports the results of estimating equations (2.2) and (2.3) to test the association between the loss of income experienced by non-bribing peers and their likelihood of engaging in financial misstatement activities. I perform this analysis using the sample of firms exposed to rivals' bribery (i.e., when *Bribe Exposure* is equal to 1). Two-tailed z-statistics, based on standard errors adjusted for firm-level clustering, are presented in parentheses. Marginal effects, where reported, are in brackets below the z-statistics. Continuous variables are winsorized at the 1 percent and 99 percent levels. All variables are defined in Appendix 2.A.

| | **Panel A: All Manipulations** | | **Panel B: Revenue Manipulations** | |
|---|---|---|---|---|
| | (1) Misstatement Up | (2) Misstatement Up | (3) Misstatement Revenue Up | (4) Misstatement Revenue Up |
| *Test variables* | | | | |
| **Rival Bribes** | **0.495**** | | **0.781**** | |
| | **(2.27)** | | **(2.09)** | |
| | **[0.048]** | | **[0.025]** | |
| **Rival Product Similarity** | | **0.210***** | | **0.218** |
| | | **(2.70)** | | **(1.62)** |
| | | **[0.020]** | | **[0.007]** |
| *Manipulation incentives* | | | | |
| Leverage | 0.761*** | 0.663*** | 0.558* | 0.502* |
| | (4.49) | (4.12) | (1.75) | (1.65) |
| Loss | -0.149* | -0.123 | -0.107 | -0.088 |
| | (-1.71) | (-1.53) | (-0.69) | (-0.59) |
| Market-to-Book | -0.041* | -0.032 | -0.081 | -0.076 |
| | (-1.71) | (-1.57) | (-1.57) | (-1.56) |
| *Manipulation ability* | | | | |
| Big4 | -0.153 | -0.185** | 0.055 | 0.049 |
| | (-1.60) | (-2.10) | (0.33) | (0.30) |
| *Industry structure* | | | | |
| Normalized HHI | 9.062 | 9.430* | 11.758 | 13.880 |
| | (1.61) | (1.77) | (0.97) | (1.18) |
| *Firm characteristics and performance* | | | | |
| Age | -0.011 | 0.023 | 0.021 | 0.068 |
| | (-0.17) | (0.38) | (0.18) | (0.61) |
| Buy-and-Hold Return | -0.036 | -0.044 | -0.033 | -0.035 |
| | (-0.91) | (-1.21) | (-0.44) | (-0.49) |
| Capex | 0.159 | 0.154 | 0.630** | 0.612** |
| | (1.05) | (1.09) | (2.57) | (2.57) |
| CFO | 0.618** | 0.533*** | 0.604 | 0.535 |
| | (2.54) | (2.63) | (1.34) | (1.26) |
| Earnings Volatility | 0.375*** | 0.390*** | 0.386** | 0.417** |
| | (3.48) | (3.92) | (2.05) | (2.38) |
| Growth | 0.142* | 0.157** | 0.232* | 0.192 |
| | (1.82) | (2.18) | (1.81) | (1.54) |
| Institutional Investor | 0.174 | 0.207* | 0.176 | 0.170 |
| | (1.37) | (1.74) | (0.73) | (0.73) |
| ROA | -0.327 | -0.314** | -0.247 | -0.156 |
| | (-1.59) | (-2.01) | (-0.68) | (-0.44) |
| Size | 0.005 | 0.004 | 0.044 | 0.029 |
| | (0.19) | (0.14) | (0.94) | (0.66) |
| Intercept | -4.145*** | -3.996*** | -5.036** | -6.381*** |
| | (-2.94) | (-2.89) | (-1.98) | (-2.65) |
| Industry FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Pseudo $R^2$ | 0.0430 | 0.0436 | 0.0490 | 0.0504 |
| Obs. | 23,760 | 23,760 | 23,760 | 23,760 |

### 2.4.3 Main results—Relative performance pressure and peers' financial misstatements

With H3, I investigate whether a possible mechanism for the spillover effects from corruption is peer performance pressure. Table 2.9 reports the results of this test performed on the sample of non-bribing peers (i.e., when *Bribe Exposure* equals 1). Panel A focuses on all income-increasing financial misstatements. The coefficient of *Common Analyst*, which is positive and statistically significant in column 1, shows that the predicted probability of income-increasing manipulations is 1.6 percentage points higher for non-bribing peers with at least one financial analyst in common with bribing rivals than for other non-bribing peers.

This finding is similar to that in column 2: The higher the percentage of common analysts between bribing firms and their non-bribing peers (*Percentage Common Analyst*), the higher the likelihood of non-bribing peers' performance-enhancing financial misstatements. In terms of economic impact, the marginal effect of moving from the 25$^{th}$ to the 75$^{th}$ percentile of *Percentage Common Analyst* is associated with a 2.95 percent change in the probability that non-bribing peers will misstate their financial statements upward.[25]

Analysts often compare firms' performance by revenue growth (e.g., Turner et al., 2001), thus motivating non-bribing peers to manipulate their revenues when dealing with the relative underperformance caused by bribing rivals' unfair revenues. In line with the summary statistics in Table 2.5, such an assumption is plausible because non-bribing peers have growth rates roughly comparable to those of bribing rivals (11.8 percent and 12.2 percent, respectively).[26] In Panel B, Table 2.9, I test H3 by focusing on upward

[25] For example, I multiply the marginal effect for *Percentage Common Analyst* of 0.030 reported in column 2 of Table 2.9, Panel A, by the inter-quartile range for *Percentage Common Analyst* of 0.111. When I then divide the result of this calculation (0.0033) by the mean value of *Misstatement Up* (0.113), I obtain 2.95 percent, which is the change in the probability of upward financial misstatements by the non-bribing peers that have analysts in common with bribing rivals.

[26] The t-test of the difference in *Growth* means between firms exposed to corruption and their bribing rivals equals -0.327 and is not statistically significant. The sales growth rate of bribing firms in bribing years is lower than that reported in Table 2.3, because the final sample excludes (a) firms in the financial industry and (b) firms without available data for variable construction. Compared to the comprehensive sample in Table 2.3, the statistics in Table 2.5 underestimate the sales increase of corrupt firms in bribing years.

TABLE 2.9: Hypothesis 3—Financial misstatements and peer performance pressure

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively. This table reports the results of estimating equation (2.4) to test whether peer performance pressure is the channel for the spillover effects of corruption on peers' financial misstatements. I perform this analysis using the sample of firms exposed to rivals' bribery (i.e., when *Bribe Exposure* is equal to 1). Two-tailed z-statistics, based on standard errors adjusted for firm-level clustering, are presented in parentheses. Marginal effects, where reported, are in brackets below the z-statistics. Continuous variables are winsorized at the 1 percent and 99 percent levels. All variables are defined in Appendix 2.A.

| | **Panel A: All Manipulations** | | **Panel B: Revenue Manipulations** | |
|---|---|---|---|---|
| | (1) Misstatement Up | (2) Misstatement Up | (3) Misstatement Revenue Up | (4) Misstatement Revenue Up |
| *Test variables* | | | | |
| **Common Analyst** | **0.166**** | | **0.183** | |
| | **(2.06)** | | **(1.38)** | |
| | [**0.016**] | | [**0.006**] | |
| **Percentage Common Analyst** | | **0.307*** | | **0.946***** |
| | | **(1.72)** | | **(3.53)** |
| | | [**0.030**] | | [**0.029**] |
| *Manipulation incentives* | | | | |
| Leverage | 0.673*** | 0.672*** | 0.519* | 0.529* |
| | (4.16) | (4.16) | (1.70) | (1.74) |
| Loss | -0.105 | -0.104 | -0.069 | -0.068 |
| | (-1.31) | (-1.30) | (-0.47) | (-0.47) |
| Market-to-Book | -0.035* | -0.033 | -0.080 | -0.080 |
| | (-1.68) | (-1.61) | (-1.64) | (-1.63) |
| *Manipulation ability* | | | | |
| Big4 | -0.181** | -0.178** | 0.053 | 0.055 |
| | (-2.06) | (-2.02) | (0.33) | (0.34) |
| *Industry structure* | | | | |
| Normalized HHI | 9.084* | 8.894* | 13.459 | 13.273 |
| | (1.71) | (1.66) | (1.15) | (1.08) |
| *Firm characteristics and performance* | | | | |
| Age | 0.024 | 0.021 | 0.069 | 0.073 |
| | (0.39) | (0.34) | (0.62) | (0.66) |
| Buy-and-Hold Return | -0.041 | -0.044 | -0.030 | -0.033 |
| | (-1.10) | (-1.18) | (-0.43) | (-0.47) |
| Capex | 0.160 | 0.169 | 0.618*** | 0.631*** |
| | (1.13) | (1.20) | (2.58) | (2.62) |
| CFO | 0.529*** | 0.538*** | 0.531 | 0.558 |
| | (2.60) | (2.66) | (1.26) | (1.32) |
| Earnings Volatility | 0.397*** | 0.392*** | 0.426** | 0.431** |
| | (3.99) | (3.93) | (2.42) | (2.46) |
| Growth | 0.159** | 0.159** | 0.195 | 0.203 |
| | (2.21) | (2.21) | (1.56) | (1.62) |
| Institutional Investor | 0.201* | 0.221* | 0.160 | 0.161 |
| | (1.68) | (1.86) | (0.70) | (0.70) |
| ROA | -0.315** | -0.324** | -0.161 | -0.194 |
| | (-1.99) | (-2.07) | (-0.46) | (-0.56) |
| Size | -0.007 | 0.000 | 0.017 | 0.006 |
| | (-0.27) | (0.00) | (0.36) | (0.13) |
| Intercept | -3.910*** | -3.900*** | -6.208*** | -6.136** |
| | (-2.82) | (-2.81) | (-2.59) | (-2.45) |
| Industry FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Pseudo $R^2$ | 0.0433 | 0.0431 | 0.0502 | 0.0532 |
| Obs. | 23,760 | 23,760 | 23,760 | 23,760 |

revenue misstatements and find results similar to those in Panel A: The higher the overlap in analyst coverage between bribing firms and their non-bribing peers, the higher the frequency of non-bribing peers' upward revenue misstatements.

### 2.4.4 Additional analyses to rule out alternative explanations

This section reports the results of additional analyses for ruling out the possibility of alternative explanations. First, if firms engaging in bribery are also more likely to misstate their accounting numbers, my results may reflect contagion in financial misstatements (as in Kedia, Koh, and Rajgopal, 2015) rather than the spillovers of corruption on peers' financial misstatements. Therefore, I test the hypotheses controlling for bribing firms' financial misstatement rates.[27] I report the results of this test in Table 2.10. For parsimony, I only display the coefficient estimates of the test variables, but logistic regressions include control variables and fixed effects, as reported in equations (2.1)—(2.4). Table 2.10, which reports the test results, shows no evidence of contagion in financial misstatements.

Second, as I classify only firms receiving the FCPA enforcement actions as corrupt firms, my research design is subject to the possible misclassification error arising from undetected bribery. If undetected bribing firms misstate their financial statements along with bribing, disentangling the spillover effects of corruption on peers' financial misstatements from contagion in bribery becomes a challenge. To deal with this possibility, I identify a setting where firms labeled "non-bribing" are unlikely to bribe, making bribery contagion an unlikely explanation of the results. Specifically, I consider non-bribing firms that do not have significant business in highly corrupt countries as unlikely to bribe. One reason for firms possibly not having business in highly corrupt countries is that their bribing rivals have already gained a monopoly position there (e.g., Zhu and Deng, 2018).

[27] Consistent with the dependent variables, I construct three proxies for bribers' financial misstatements, i.e., all financial misstatements, income-increasing financial misstatements, and income-decreasing financial misstatements. For simplicity, I label all of them *Misstatement Bribers*.

TABLE 2.10: Alternative explanation—Contagion in financial misstatements

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively. This table reports the results of the tests of H1, H2, and H3 when I control for bribing firms' financial misstatements, proxied by *Misstatement Bribers*, which is the average financial misstatement rate of bribing firms in each bribing year and industry (three-digit SIC code). The analysis in Panel A is performed using the sample of non-bribing firms exposed and non-exposed to bribing rivals, whereas the analysis in Panels B and C is performed using the sample of exposed firms only (i.e., when *Bribe Exposure* is equal to 1). Two-tailed z-statistics, based on standard errors adjusted for firm-level clustering, are presented in parentheses. Marginal effects, where reported, are in brackets below the z-statistics. Continuous variables are winsorized at the 1 percent and 99 percent levels. All variables are defined in Appendix 2.A.

| | **Panel A: H1—Bribe Exposure** | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | Misstatement | Misstatement Up | Misstatement Down |
| **Bribe Exposure** | **0.141**** | **0.198***** | **-0.056** |
| | **(2.05)** | **(2.67)** | **(-0.43)** |
| | **[0.015]** | **[0.019]** | **[0.002]** |
| Misstatement Bribers | 0.002 | 0.099 | -0.250 |
| | (0.02) | (1.01) | (-0.73) |
| Other Controls and Intercept | Yes | Yes | Yes |
| Industry & Year FE | Yes | Yes | Yes |
| Pseudo $R^2$ | 0.0395 | 0.0452 | 0.0444 |
| Obs. | 44,714 | 44,714 | 44,714 |

| | **Panel B: H2—Loss of Income** | |
|---|---|---|
| | (1) | (2) |
| | Misstatement Up | Misstatement Up |
| **Rival Bribes** | **0.497**** | |
| | **(2.28)** | |
| | **[0.048]** | |
| **Rival Product Similarity** | | **0.211***** |
| | | **(2.71)** |
| | | **[0.021]** |
| Misstatement Bribers | 0.172 | 0.177 |
| | (1.31) | (1.34) |
| Other Controls and Intercept | Yes | Yes |
| Industry & Year FE | Yes | Yes |
| Pseudo $R^2$ | 0.0431 | 0.0438 |
| Obs. | 23,760 | 23,760 |

| | **Panel C: H3—Peer Performance Pressure** | |
|---|---|---|
| | (1) | (2) |
| | Misstatement Up | Misstatement Up |
| **Common Analyst** | **0.167**** | |
| | **(2.07)** | |
| | **[0.016]** | |
| **Percentage Common Analyst** | | **0.306*** |
| | | **(1.72)** |
| | | **[0.030]** |
| Misstatement Bribers | 0.176 | 0.172 |
| | (1.33) | (1.30) |
| Other Controls and Intercept | Yes | Yes |
| Industry & Year FE | Yes | Yes |
| Pseudo $R^2$ | 0.0435 | 0.0433 |
| Obs. | 23,760 | 23,760 |

TABLE 2.11: Alternative explanation—Contagion in bribery

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively. This table reports the results of the tests of H1, H2, and H3 when I restrict the sample to firms with no business in highly corrupt countries. Highly corrupt countries are those ranked below the bottom quartile of the Corruption Perceptions Index (CPI) distribution in each year. This restriction aims at mitigating the confounding effects of potential contagion in bribery. The analysis in Panel A is performed using the sample of non-bribing firms exposed and non-exposed to bribing rivals, whereas the analysis in Panels B and C is performed using the sample of exposed firms only (i.e., when *Bribe Exposure* is equal to 1). Two-tailed z-statistics, based on standard errors adjusted for firm-level clustering, are presented in parentheses. Marginal effects, where reported, are in brackets below the z-statistics. Continuous variables are winsorized at the 1 percent and 99 percent levels. All variables are defined in Appendix 2.A.

| | **Panel A: H1—Bribe Exposure** | | |
|---|---|---|---|
| | (1) Misstatement | (2) Misstatement Up | (3) Misstatement Down |
| **Bribe Exposure** | **0.180**** | **0.225***** | **-0.012** |
| | **(2.35)** | **(2.72)** | **(-0.08)** |
| | [**0.020**] | [**0.022**] | [**-0.000**] |
| Other Controls and Intercept | Yes | Yes | Yes |
| Industry & Year FE | Yes | Yes | Yes |
| Pseudo $R^2$ | 0.0420 | 0.0480 | 0.0489 |
| Obs. | 36,062 | 36,062 | 36,062 |

| | **Panel B: H2—Loss of Income** | |
|---|---|---|
| | (1) Misstatement Up | (2) Misstatement Up |
| **Rival Bribes** | **0.542**** | |
| | **(2.28)** | |
| | [**0.055**] | |
| **Rival Product Similarity** | | **0.220***** |
| | | **(2.59)** |
| | | [**0.022**] |
| Other Controls and Intercept | Yes | Yes |
| Industry & Year FE | Yes | Yes |
| Pseudo $R^2$ | 0.0433 | 0.0432 |
| Obs. | 19,115 | 19,115 |

| | **Panel C: H3—Peer Performance Pressure** | |
|---|---|---|
| | (1) Misstatement Up | (2) Misstatement Up |
| **Common Analyst** | **0.179**** | |
| | **(2.00)** | |
| | [**0.018**] | |
| **Percentage Common Analyst** | | **0.285** |
| | | **(1.47)** |
| | | [**0.029**] |
| Other Controls and Intercept | Yes | Yes |
| Industry & Year FE | Yes | Yes |
| Pseudo $R^2$ | 0.0429 | 0.0426 |
| Obs. | 19,115 | 19,115 |

I define the countries ranked below the bottom quartile of the Corruption Perceptions Index (CPI) distribution in a given year as "highly corrupt,"[28] and I exploit the information about corporate geographical business segments reported in Compustat Geographical Segments to identify the countries where firms do business and to classify them according to their level of corruption. Table 2.11 reports the results restricting the sample to firm years showing no business in highly corrupt countries. The findings show that contagion in bribery is an unlikely explanation of my results.

Third, Parsons, Sulaeman, and Titman (2018) show that social and cultural forces—proxied by the geographical location of corporations—are a first-order determinant of corporate misconduct. To ensure that my results do not reflect the effect of city-level culture on financial misstatements, I test my hypotheses controlling for state fixed effects. I report the results of this test in Table 2.12, which shows that the magnitude of the variable coefficients and their statistical significance are similar to those of the main tests. Therefore, my results are unaffected by the role of social and cultural forces. Only the coefficients of *Common Analyst* and *Percentage Common Analyst* (H3) are slightly insignificant.

## 2.5 Conclusion

This study provides empirical evidence of the existence of spillover effects from corporate corruption. Using the enforcement actions for FCPA violations to identify corrupt firms, I find that when corrupt firms bribe to gain unfair business advantages, their non-bribing peers respond by misstating their financial statements upward. Moreover, the likelihood of non-bribing peers reporting losses when rivals bribe affects non-bribing firms' propensity to misstate their financial statements. These losses depend on how much bribing rivals unfairly gain through bribery and on whether non-bribing peers are likely to lose profitable deals because of that bribery. The results also suggest that peer performance

[28] The CPI ranks countries on a scale of 0-100 from highly corrupt (low index) to very clean (high index). For example, in 2013 the U.S. has a CPI of 73 (comparable to UK and Canada, with CPIs of 76 and 81, respectively). Among the most corrupt countries are Afghanistan, North Korea, and Somalia (with CPIs of 8).

TABLE 2.12: Alternative explanation—Social and cultural factors

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively. This table reports the results of the tests of H1, H2, and H3 when I control for the social and cultural factors that affect firms' attitude towards financial misconduct (Parsons, Sulaeman, and Titman, 2018). Specifically, all regressions include state fixed effects. The analysis in Panel A is performed using the sample of non-bribing firms exposed and non-exposed to bribing rivals, whereas the analysis in Panels B and C is performed using the sample of exposed firms only (i.e., when *Bribe Exposure* is equal to 1). Two-tailed z-statistics, based on standard errors adjusted for firm-level clustering, are presented in parentheses. Marginal effects, where reported, are in brackets below the z-statistics. Continuous variables are winsorized at the 1 percent and 99 percent levels. All variables are defined in Appendix 2.A.

| | **Panel A: H1—Bribe Exposure** | | |
|---|---|---|---|
| | (1) Misstatement | (2) Misstatement Up | (3) Misstatement Down |
| **Bribe Exposure** | **0.137*** | **0.196**** | **-0.065** |
| | **(1.90)** | **(2.54)** | **(-0.46)** |
| | **[0.016]** | **[0.019]** | **[-0.002]** |
| Other Controls and Intercept | Yes | Yes | Yes |
| Industry, State & Year FE | Yes | Yes | Yes |
| Pseudo $R^2$ | 0.0494 | 0.0564 | 0.0608 |
| Obs. | 44,714 | 44,714 | 44,714 |

| | **Panel B: H2—Loss of Income** | |
|---|---|---|
| | (1) Misstatement Up | (2) Misstatement Up |
| **Rival Bribes** | **0.498**** | |
| | **(2.17)** | |
| | **[0.050]** | |
| **Rival Product Similarity** | | **0.086** |
| | | **(1.08)** |
| | | **[0.009]** |
| Other Controls and Intercept | Yes | Yes |
| Industry, State & Year FE | Yes | Yes |
| Pseudo $R^2$ | 0.0602 | 0.0589 |
| Obs. | 23,760 | 23,760 |

| | **Panel C: H3—Peer Performance Pressure** | |
|---|---|---|
| | (1) Misstatement Up | (2) Misstatement Up |
| **Common Analyst** | **0.077** | |
| | **(0.89)** | |
| | **[0.008]** | |
| **Percentage Common Analyst** | | **0.151** |
| | | **(0.75)** |
| | | **[0.015]** |
| Other Controls and Intercept | Yes | Yes |
| Industry, State & Year FE | Yes | Yes |
| Pseudo $R^2$ | 0.0589 | 0.0589 |
| Obs. | 23,760 | 23,760 |

pressure is a possible mechanism for such spillover effects.

My work underscores the detrimental role of corruption, the side effects of which overstep the well-known impairment of economic growth and development. Specifically, I find that bribing firms' corruption leads other industry members to engage in alternative forms of corporate misconduct, such as financial misstatements. This finding is not only important for researchers, because it reveals an overlooked determinant of financial misstatements, but also valuable for regulators concerned about how their enforcement actions could more effectively deter corporate misconduct in general and corruption in particular. Future researchers can collect more detailed information about corporate misconduct in different institutional settings, and investigate whether the spillover effects occur in those other settings, thereby extending research knowledge about how corporate misconduct spreads.

# 2.A Appendix—Variable description

| Variable Label | Description |
|---|---|
| **Panel A**: Dependent variables | |
| Misstatement | Indicator equal to 1 if firm $i$ engages in financial misstatement activities in fiscal year $t$, and 0 otherwise. I identify misstating firms via subsequent restatements using the Non-reliance Restatement database of Audit Analytics. |
| Misstatement Up | Indicator equal to 1 if firm $i$ engages in income-increasing financial misstatement activities in fiscal year $t$, and 0 otherwise. I identify misstating firms via subsequent restatements using the Non-reliance Restatement database of Audit Analytics. |
| Misstatement Down | Indicator equal to 1 if firm $i$ engages in income-decreasing financial misstatement activities in fiscal year $t$, and 0 otherwise. I identify misstating firms via subsequent restatements using the Non-reliance Restatement database of Audit Analytics. |
| Misstatement Revenue Up | Indicator equal to 1 if firm $i$ engages in income-increasing revenue misstatement activities in fiscal year $t$, and 0 otherwise. I identify misstating firms via subsequent restatements using the Non-reliance Restatement database of Audit Analytics. |
| Misstatement Revenue Down | Indicator equal to 1 if firm $i$ engages in income-decreasing revenue misstatement activities in fiscal year $t$, and 0 otherwise. I identify misstating firms via subsequent restatements using the Non-reliance Restatement database of Audit Analytics. |

*(Continued)*

| Variable Label | Description |
|---|---|
| **Panel B**: Independent test variables | |
| Bribe Exposure | Indicator equal to 1 if firm $i$ is exposed to at least one bribing competitor in the same fiscal year $t$ and industry $j$, and 0 otherwise. Non-bribing firms exposed to bribing rivals (i.e., non-bribing peers) have the same year and industry as the bribing firms, with industry determined at the three-digit SIC level. Non-exposed firm years include (a) firm years with the same two-digit SIC code as the bribing firms but a different three-digit SIC code and (b) firms with the same three-digit SIC code as the bribing firms in non-bribing years. |
| Rival Bribes | Proxy for the magnitude of the loss of income that non-bribing peers are likely to experience because of bribing rivals' unfair gains: I obtain this variable in two steps. First, I compute the natural logarithm of the amounts of the bribes (from bribing firms) in each bribing year and industry (three-digit SIC code). Second, I assign this variable to non-bribing peers according to their year and industry group (three-digit SIC code). |
| Rival Product Similarity | Proxy for the product market similarity between bribing firms and non-bribing peers. Using Hoberg and Phillips (2016)' pairwise similarity scores, I construct an indicator equal to 1 if firm $i$ has at least one bribing competitor in the same fiscal year $t$ and industry $j$ with a pairwise similarity score above the median, and 0 otherwise. |
| Common Analyst | Indicator equal to 1 if firm $i$ has at least one financial analyst in common with its bribing rival(s) in fiscal year $t$ and industry $j$, and 0 otherwise. |
| Percentage Common Analyst | Percentage of common financial analysts between firm $i$ and its bribing rival(s) in fiscal year $t$. |
| Treat Bribing Years | Interaction term between the following treatment and time indicators: The treatment indicator equals 1 for firms with the same three-digit SIC code as the bribing firms, and 0 for firms with the same two-digit SIC code as the bribing firms, but a different three-digit SIC code (as in Beatty, Liao, and Yu, 2013). The time indicator, specific for each two-digit SIC code, equals 1 in the first five bribing years, and 0 in the five years before the bribery beginning. |
| Treat Pre-Bribing Years | Interaction term between the following treatment and time indicators: The treatment indicator equals 1 for firms with the same three-digit SIC code as the bribing firms, and 0 for firms with the same two-digit SIC code as the bribing firms, but a different three-digit SIC code (as in Beatty, Liao, and Yu, 2013). The time indicator, specific for each two-digit SIC code, equals 1 in the two years (*t–2* and *t–1*) before firms bribe, and 0 in the years *t–4* and *t–3* from the first bribing year. |

*(Continued)*

| Variable Label | Description |
|---|---|
| **Panel C**: Control variables | |
| Age | Natural logarithm of one plus firm age, i.e., the number of years since the inclusion of firm $i$ in Compustat. |
| Big4 | Indicator equal to 1 if firm $i$ is audited by a Big Four audit firm in fiscal year $t$, and 0 otherwise. |
| Buy-and-Hold-Return | Contemporaneous twelve-month buy-and-hold return of firm $i$ in fiscal year $t$. |
| Capex | Ratio of capital expenditure to lagged property, plant, and equipment of firm $i$ in fiscal year $t$. |
| CFO | Ratio of cash flow from operations to lagged total assets of firm $i$ in fiscal year $t$. |
| Earnings Volatility | Standard deviation of earnings per share of firm $i$ for the twelve quarters ending with the year of observation. |
| Growth | Annual percentage change in sales for firm $i$ in fiscal year $t$. |
| Institutional Investor | Percentage of common shares that institutional investors own in firm $i$ in fiscal year $t$. |
| Leverage | Ratio of short- and long-term debt to lagged total assets of firm $i$ in fiscal year $t$. |
| Loss | Indicator equal to 1 if firm $i$ reports a loss in fiscal year $t$, and 0 otherwise. |
| Market-to-Book | Ratio of market value of assets to book value of assets of firm $i$ in fiscal year $t$. |
| Normalized HHI | Normalized Herfindahl-Hirschman Index, defined as $$\textit{Normalized HHI} = (n \cdot H - 1)/(n - 1), \quad (2.5)$$ where $H$ is the sum of the squared market shares (based on sales) of all firms available in Compustat in each year and industry (three-digit SIC code), and $n$ is the total number of firms in each year and industry (three-digit SIC code). |
| ROA | Ratio of operating income before depreciation to lagged total assets of firm $i$ in fiscal year $t$. |
| Size | Natural logarithm of lagged total assets of firm $i$ in fiscal year $t$. |

# Chapter 3

# Employees' financial wellness, productivity, and firms' myopic behavior


## ABSTRACT[1]

We study how rank-and-file employees' financial wellness affects employers' myopic accounting decisions. Using staggered increases in consumer bankruptcy exemptions across U.S. states to capture increments in employees' financial wellness, we find that firms reduce their real activities management, have fewer misstatements, and decrease their loss avoidance behaviors in response to higher consumer bankruptcy protection. We identify two channels that might explain these findings. First, we show that more consumer bankruptcy protection reduces employees' absenteeism from the workplace, and second, we show that firm productivity improves when consumer bankruptcy protection increases. Thus, higher productivity facilitates firms' achievement of their earnings-based targets, reducing the need for myopic performance-enhancing behaviors. Our evidence suggests that improving employees' financial wellness benefits firms through higher productivity and fewer myopic decisions.



JEL classification: K35, J24, M41.

Keywords: accounting myopia, consumer bankruptcy, employees, productivity.



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## 3.1 Introduction

Productive employees are increasingly seen as an enduring source of competitive advantage (Pfeffer and Veiga, 1999; Bae, Kang, and Wang, 2011). Employees' productivity largely depends on their financial wellness, in that concerns about the inability to pay off personal debt and uncertainty about the financial future undermine individuals' psychological condition and performance at work (Richardson, Elliott, and Roberts, 2013; Kaur et al., 2021). While employees (hereafter also workers or rank-and-file employees) represent a key asset for corporate success (Pfeffer, 1996), little is known as to whether their financial wellness produces spillover effects on employers' decisions.

To contribute to this limited evidence, we investigate whether employees' financial wellness affects employers' myopic accounting decisions, and we do so for two reasons. First, although myopia in financial reporting choices represents a substantial threat to the existence and efficiency of capital markets, no consensus exists on the determinants of this phenomenon (Amiram et al., 2018). Second, although researchers have recently enhanced understanding of the interaction between rank-and-file employees and firms' myopia (e.g., Dou, Khan, and Zou, 2016; Haß, Hribar, and Kalogirou, 2019), the role of financial wellness, which is the primary determinant of employees' well-being and behaviors, remains largely unexplored.[2]

Consistent with employees being less stressed and worried when they have fewer financial concerns (Kaur et al., 2021), we predict that improvements in employees' financial wellness produce positive spillover effects on firms, and find results consistent with our prediction: Firms reduce their real activities management, have fewer financial misstatements, and engage less in loss avoidance practices when their employees have fewer financial concerns. We further investigate the possible mechanism underlying these results and find that better employees' financial wellness reduces workers' absenteeism while it improves firm productivity. Altogether, our findings suggest that the benefits of better

[2] CareerBuilder, in its 2017 survey, finds that 78% of U.S. workers are under financial pressure and live paycheck to paycheck (Source: Forbes). Furthermore, in 2018, Fidelity Research finds that the top two sources of stress for American workers are their job and finances.

workers' financial wellness reduce managers' need and incentives to undertake myopic accounting actions to meet short-term earnings objectives at the expense of long-term economic value creation (Jensen, 2005; Bhojraj et al., 2009).

We exploit the features of the U.S. consumer bankruptcy laws and use the staggered increments in consumer bankruptcy exemptions across U.S. states up to 2005 to identify improvements in individuals' financial wellness (Pinto, 2021). Exemptions refer to the amount (dollar value) of debtors' assets that creditors cannot seize during personal bankruptcy proceedings. Individuals with debt outstanding and residing in states that increase exemptions can retain more of their assets when they file for bankruptcy.[3] We focus on the period up to 2005 because in 2005 the U.S. Congress passed the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA), which issued more stringent requirements to become eligible to file for bankruptcy, thereby undermining the role of the exemptions.

We compare firms in states that increase exemptions to firms in states without such changes in a difference-in-differences framework, and find that firms reduce myopic accounting behaviors when consumer bankruptcy exemptions increase. We measure firms' myopic behaviors along three dimensions and find consistent results across them. First, we exploit residual-based measures to examine real activities management and accrual-based earnings management (e.g., Dechow and Dichev, 2002; Roychowdhury, 2006). Second, we exploit restatements to examine detected cases of financial misstatements (e.g., Kedia, Koh, and Rajgopal, 2015). Third, we exploit the discontinuity in the earnings distribution around zero to examine loss avoidance practices (e.g., Burgstahler and Dichev, 1997). Using the same research design, we further investigate the possible mechanism underlying these results, and find that workers' absenteeism decreases and firm productivity increases in response to higher consumer bankruptcy exemptions. Altogether, our evidence suggests that improving employees' financial wellness benefits firms through higher productivity and fewer myopic decisions.

[3] "Bankruptcy is roughly the size of two of the highest profile government redistribution programs, TANF and UI, combined. Not only is bankruptcy an enormous transfer program, it has grown quickly." (Lefgren and Mcintyre, 2009, 368).

In terms of economic impact, a 10% increase in house-related (i.e., homestead) exemptions leads to a reduction in real activities management of 0.0018 (around 2.5% of corporate earnings). In contrast, we fail to find that firms adjust their discretionary accruals when the burden of employees' debt alleviates. Moreover, the evidence from actual misreporting cases and discontinuity in the earnings distribution around zero indicates that firms reduce financial misstatements and loss avoidance practices in response to better employees' financial wellness.

We perform a set of cross-sectional tests to investigate whether different incentives to manage earnings affect the relation between firms' real activities management and the improvements in their rank-and-file employees' financial wellness. First, we examine the role of seasoned equity offerings (SEOs), as previous studies show that managers have more incentives to manage earnings around these events (Cohen and Zarowin, 2010; Kothari, Mizik, and Roychowdhury, 2016). However, in the sample of firms issuing SEOs, we fail to find higher real activities management in response to better employees' financial wellness. This finding suggests that the incentives to increase real activities management when firms issue SEOs and the incentives to reduce real activities management when employees' financial wellness increases compensate each other.

Second, we examine the role of firms' incentives to repeatedly meet or beat analysts' forecasts, as previous literature documents that firms manage earnings more when they are "repeat forecast beaters" to avoid adverse stock price consequences (Bartov, Givoly, and Hayn, 2002). However, we do not find that "repeat beaters" engage in real activities management more when employees' financial wellness improves. This result suggests that the incentives to increase real activities management when firms keep beating analysts' forecasts compensate the incentives to reduce real activities management when employees' financial wellness improves.

Third, we examine the role of CEOs and CFOs' equity incentives to manage earnings (e.g., Cheng and Warfield, 2006; Bergstresser and Philippon, 2006; Jiang, Petroni, and Wang, 2010), and show that such incentives do not affect the relation between firms' real activities management and the improvements in their employees' financial wellness.

Finally, we test the robustness of the results to alternative proxies for real activities management and find evidence consistent with our main findings: Firms reduce their real activities management when consumer bankruptcy exemptions increase.

Increases in bankruptcy exemptions are a good proxy for improvements in employees' financial wellness if employees do not increase their borrowings following an increase in bankruptcy exemptions. In contrast, if employees borrow more as a result of more generous bankruptcy exemptions, the reduction in the cost of personal bankruptcy could be offset by the increase in financial risk stemming from more personal debt. However, we are confident that this concern does not affect our inferences because Severino and Brown (2020) show that, despite changes in the composition of borrowings, the aggregate level of household debt does not change following the increases in bankruptcy exemptions. Overall, this evidence suggests that employees can withhold the benefits of more generous bankruptcy exemptions, and, in turn, contribute to producing positive spillover effects on firms.

This paper contributes to four streams of the literature. First, by showing that employees' financial wellness affects firms' accounting myopia, we add to the limited research on the role of employees in firms' myopic accounting decisions. Among the few studies, Dou, Khan, and Zou (2016) examine firms' response to enhanced unemployment benefits and find that firms in states with more generous unemployment benefits reduce their abnormal accruals and increase the recognition of special items and write-downs. Our paper differs from theirs, in that the economic mechanism underlying our results is the enhanced workplace productivity resulting from better employees' financial wellness, whereas Dou, Khan, and Zou (2016) ascribe their findings to the employees' perceptions of employment security. In the same stream of literature, Gao, Zhang, and Zhang (2018) investigate and find that managers consider employees' turnover likelihood while making their earnings management choices, whereas Haß, Hribar, and Kalogirou (2019) identify labor mobility as a disciplining mechanism able to reduce firms' myopic accounting activities.

Second, and more broadly, our work relates to the studies investigating the effects of employees' well-being on employers' financial and real decisions. Among these studies,

Pinto (2021) examines how improvements in employees' well-being affect firm profitability, and finds that firms become more profitable after personal bankruptcy protection increases. Moreover, Bae, Kang, and Wang (2011) show that a firm's provision of fair employment treatments to the employees influences corporate financing decisions.

Third, we add to the literature examining the positive effects of debt relief programs on individuals. Among others, Dobbie and Song (2015) show that Chapter 13 bankruptcy protection increases individuals' annual earnings by $5,562, decreases five-year mortality by 1.2 basis points, and decreases five-year foreclosure rates by 19.1 basis points. Related to this, a complementary stream of literature documents the adverse effects of indebtedness on individuals' physical and mental health conditions (e.g., Richardson, Elliott, and Roberts, 2013).

Fourth, our paper contributes to the literature that analyzes the individuals' precautionary responses to income risk. In this stream of literature, Low, Meghir, and Pistaferri (2010) show that changes in employee-specific risk (i.e., employee-specific uncertainty that exists independently of employers' characteristics) have substantial welfare implications and call for more research on the consequences of changes in individuals' risk.

The rest of the paper is organized as follows. Section 3.2 reviews prior literature on firms' myopic behavior and discusses the hypothesis development. Section 3.3 provides the institutional background on consumer bankruptcy laws in the United States and describes our empirical methodology. Section 3.4 describes the data and the main variables. Section 3.5 presents the empirical findings, and Section 3.6 concludes.

## 3.2 Prior literature and hypothesis development

### 3.2.1 Firms' myopic accounting behavior

Prior research has ascribed myopic behavior to the combination of strong distaste for losses and focus on short horizons (Stein, 1989; Thaler et al., 1997). Firms engage in myopic behaviors by sacrificing the long-term growth for the purpose of meeting short-term performance goals (Porter, 1992; Bhojraj et al., 2009).

To meet short-term objectives at the expense of long-term value creation, firms can make financial reporting choices, such as changing accrual accounting methods or estimates, that obscure the true economic corporate performance (e.g., Dechow, Sloan, and Sweeney, 1995; Kothari, Leone, and Wesley, 2005; Dechow, Ge, and Schrand, 2010; Peasnell, Pope, and Young, 2012). Specifically, managers exercise discretion and judgment regarding accounting choices (Cohen, Dey, and Lys, 2008). Alternatively, firms can engage in real activities management by altering the execution of real business transactions to mislead the stakeholders and make them believe that the reported financial performance results from the normal course of operations (e.g., Graham, Harvey, and Rajgopal, 2005; Roychowdhury, 2006; Kothari, Mizik, and Roychowdhury, 2016).

Overall, through accrual-based earnings management, firms shift earnings across periods to meet their short-term earnings targets (Cohen, Dey, and Lys, 2008), whereas through real activities management firms forego positive net present value projects with long return horizons (Kothari, Mizik, and Roychowdhury, 2016). Importantly, both approaches sacrifice future earnings to boost current earnings.[4]

When firms' financial statements are not GAAP compliant and performance manipulations lead to fraudulent misconduct or other severe irregularities, regulators can force firms to restate their financial statements (Hennes, Leone, and Miller, 2008; Amiram et al., 2018). Moreover, firms' preference for "consistent profitability" and incentives to avoid earnings decreases and losses explain why myopic decisions are largely taken to cross the zero earnings "red line" (Hayn, 1995; Burgstahler and Dichev, 1997).

As myopic behaviors produce negative side effects for firms (e.g., Hribar and Jenkins, 2004; McNichols and Stubben, 2008), the accounting literature has widely investigated the factors that exacerbate or alleviate firms' engagement in such behaviors. Among the factors exacerbating firm accounting manipulations, previous studies have identified firm operating characteristics, such as performance, leverage, growth and investment, and size (Dechow, Ge, and Schrand, 2010). For example, firms that are highly levered, thus

[4] This assumes income-increasing earnings management. There are also reasons why firms might shift earnings to the future, but we are not concerned with those in this paper.

closer to debt covenant violations, or that are performing poorly, have more incentives to misreport their performance by managing earnings (e.g., Petroni, 1992; Skinner, 1993; Balsam, Haw, and Lilien, 1995; DeFond and Park, 1997).

In addition, considerable evidence shows that weak governance and monitoring mechanisms, such as internal controls, boards of directors, shareholders, and executive and non-executive committees facilitate firms' undertaking of myopic accounting actions (e.g., Collins and DeAngelo, 1990; Dechow and Sloan, 1991; Abbott, Parker, and Peters, 2004). Similarly, managerial incentives, especially those driven by compensation, increase the likelihood and the magnitude of accrual-based earnings management and real activities management (e.g., Efendi, Srivatsava, and Swanson, 2007). In contrast, several studies document that auditors reduce firms' myopic accounting decisions by exercising monitoring and scrutiny (e.g., Caramanis and Lennox, 2008; DeFond and Zhang, 2014).

Only few studies have investigated whether rank-and-file employees' characteristics affect firms' myopia. Among them, Dou, Khan, and Zou (2016) show a decrease in firms' accrual-based earnings management and an increase in recognition of special items and write-downs in states with more generous unemployment benefits. Moreover, Gao, Zhang, and Zhang (2018) document that managers consider employees' turnover likelihood in their myopic accounting choices, whereas Haß, Hribar, and Kalogirou (2019) identify labor mobility as a disciplining mechanism able to reduce firms' engagement in myopic accounting decisions. While these studies enhance understanding of the interaction between employees and firms' myopia, they overlook the role of workers' financial wellness. This lack of evidence is surprising, given that happier and less stressed employees are more productive at work, and can influence substantially firms' competitive success and corporate decisions (Bae, Kang, and Wang, 2011; Kaur et al., 2021; Pinto, 2021).[5] By investigating how rank-and-file employees' financial wellness affects firms' engagement in myopic accounting behaviors, our study adds to this limited evidence.

[5] Recent evidence from Oxford University's Saïd Business School shows that happy workers are more productive (October 2019).

### 3.2.2 Hypothesis development

Workers' financial wellness affects several corporate decisions and outcomes (Bae, Kang, and Wang, 2011; Pinto, 2021). When employees are less stressed and worried about their financial situation, and in turn have higher levels of wellness, they are more attentive and productive at work (Kaur et al., 2021). In line with this evidence, survey results show that workers with financial problems are 5.8 times more likely not to complete their daily tasks on time and 4.9 times more likely to work below their quality standards. Combined, these results lead to a total loss of 25-31 workdays annually.[6] Moreover, the financial burden of personal debt is associated with a higher likelihood of suffering from physical and mental health conditions (Richardson, Elliott, and Roberts, 2013).

Since productive workers are a key determinant of firms' success (Pfeffer, 1996), we hypothesize that improvements in rank-and-file employees' wellness arising from fewer financial concerns produce positive spillover effects on firms. Specifically, we expect firms' myopic accounting behaviors to decrease as a result of better employees' financial wellness. This prediction is consistent with the assumption that more productive workers increase firm productivity and allow managers to meet their earnings targets while engaging less in myopic performance-enhancing activities.

We investigate firms' myopic behavior in response to enhanced workers' financial wellness along three dimensions. First, we examine real activities management and accrual-based earnings management. Second, we examine financial misstatements detected by regulators and sanctioned by subsequent restatements. Third, we examine loss avoidance behaviors through the discontinuity in the earnings distribution around the zero threshold. We formulate three separate hypotheses—one for each myopic behavior—about firms' myopia as a result of better rank-and-file employees' financial wellness. These hypotheses are as follows:

*H1: Firms' real activities management and accrual-based earnings management decrease when workers' financial wellness increases.*

*H2: Firms' financial misstatements decrease when workers' financial wellness increases.*

[6] SalaryFinance, Employer's Guide to Financial Wellbeing 2018-19.

*H3: Firms' loss avoidance practices decrease when workers' financial wellness increases.*

This threefold strategy allows us to address our research question using different samples and alternative definitions of myopic behavior, thus mitigating the limitations of each proxy (Chen, Hribar, and Melessa, 2018). Through real activities management and accrual-based earnings management, we exploit a panel dataset and construct residual measures to obtain large sample evidence. Through the restatement analysis, we focus on a small sample of detected cases of performance misreporting, whereas through the loss avoidance analysis, we examine the existence of a discontinuity in the earnings distribution around a specific performance threshold characterized by high incentives for performance manipulation.

## 3.3 Institutional setting and empirical methodology

### 3.3.1 The U.S. consumer bankruptcy framework

In the U.S., two filing procedures form the consumer bankruptcy code. The first is Chapter 7 bankruptcy filing (i.e., "total liquidation"). Under this procedure, debtors repay creditors by transferring all their unprotected assets to a bankruptcy trustee, who is in charge of their liquidation. Eligible unsecured debt above the value of available assets is discharged, and therefore filers do not use their future income to repay debt. The second is Chapter 13 bankruptcy filing (i.e., "re-scheduling"). Under this procedure, debtors must propose a repayment plan to the judge responsible for the bankruptcy filing, and this judge will decide on whether to accept or reject it. Moreover, Chapter 13 filers keep their assets and use their future income to settle the repayment plan.

Under Chapter 7, debtors are entitled to retain the value of their assets that falls below state-specific exemption limits. For simplicity, exemptions are classified in two categories—homestead and non-homestead (personal) exemptions—based on the type of asset they refer to. Homestead exemptions refer to the equity value of the debtors' principal residence that is secured in case of bankruptcy, whereas non-homestead exemptions refer to the amount of any other pre-specified asset exempted from bankruptcy. Common

examples of assets that qualify for non-homestead exemptions are jewelry and motor vehicles. In contrast, under Chapter 13, filers do not transfer their assets but propose a repayment plan to the court. Strategically, the amount outlined in the repayment plan is not larger than the one debtors would have lost under Chapter 7 filing (otherwise, it would be strictly preferable to default and file for Chapter 7). Therefore, exemptions also affect the generosity of the Chapter 13 bankruptcy procedure.

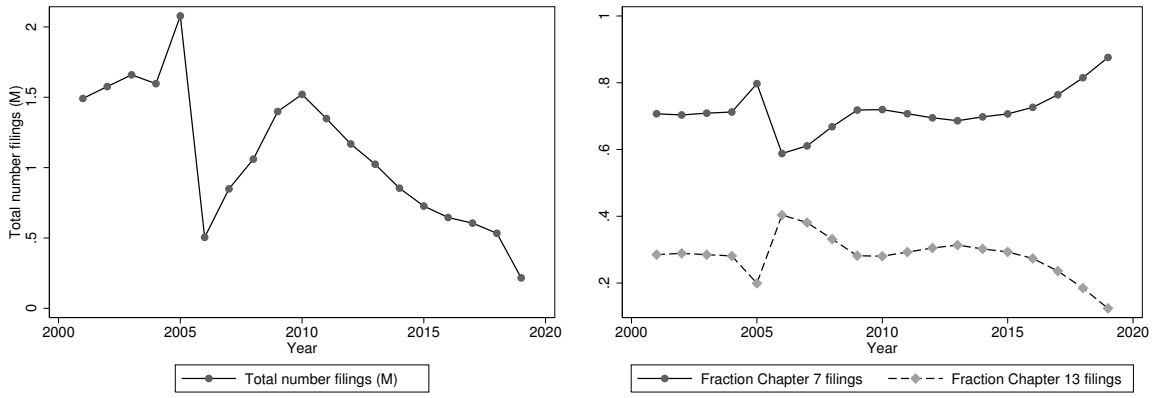
In 2005, the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) changed the consumer bankruptcy code significantly, making filing for Chapter 7 more complex and less attractive for debtors.[7] Since this reform, Chapter 13 bankruptcy has become the default option and debtors who choose to file for Chapter 7 have to pass a two-step test. First, a formula determines if the filer is able to pay 25% of her non-priority unsecured debt (e.g., credit card bills). Second, the debtor's income is compared to the median income of her state of residency (Pinto, 2021). As a result, debtors that have income above the state median and are able to afford 25% of the non-priority unsecured debt cannot file for Chapter 7 bankruptcy. Debtors can file for Chapter 7 only if their income is lower than the state median even if they can afford 25% of the unsecured debt (Li and Sarte, 2002). However, this latter option is subject to the court's approval. Overall, the BAPCPA is more creditor-friendly than the preceding consumer bankruptcy regulation (Coelho, 2021).

The evidence of the filings distribution presented in Figure 3.1 suggests that debtors have perceived this reform as impairing their positions. Before the BAPCPA became effective, the individuals with a positive option value of filing for bankruptcy exploited the features of Chapter 7 (Pinto, 2021). As shown in Figure 3.1, in 2005 over 2 million Chapter 7 bankruptcy filings occurred. This strong reaction also suggests that individuals

[7] As Pinto (2021) reports, during the Opening Statement at the Bankruptcy Reform Hearing (Senate Committee on the Judiciary), Sen. Chuck Grassley said *"[T]his legislation eliminates some of the opportunities for abuse that exist under the current system. Our current system allows wealthy people to continue to abuse the system at the expense of everyone else. People with good incomes can run up massive debts and then use bankruptcy to get out of honoring them. Omissis... it has been estimated that every American family pays as much as $550 a year in a hidden tax as a result of the actions from these abuses. My bankruptcy reform legislation will help eliminate this hidden tax by implementing a means test to make wealthy people who can repay their debts actually honor them."*

FIGURE 3.1: Chapter 7 and Chapter 13 consumer bankruptcy filings

This figure shows the annual Chapter 7 and Chapter 13 consumer bankruptcy filings. The 2005 is a key year because of the enactment of the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA). Data are from the Statistics Division of the Administrative Office of the United States Courts.




respond to changes in consumer bankruptcy generosity and value the protection provided by debt relief programs. Overall, bankruptcy exemptions provide natural leverage (i.e., more bargaining power) that debtors can use to renegotiate their claims with creditors.

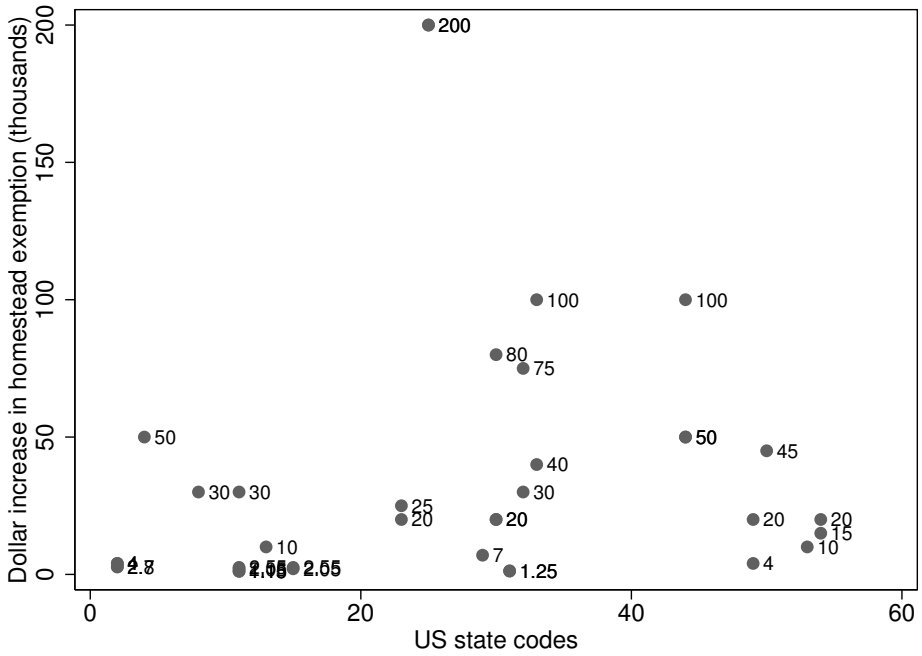
Although consumer bankruptcy is regulated at the federal level, states can choose their own exemption levels, and filers are subject to the exemptions of the state in which they reside. In our empirical strategy, we use state-specific increases in exemptions to capture gains in rank-and-file employees' financial wellness. By doing so, we exploit the time-series variation in the generosity of the bankruptcy exemptions across states. This variation is particularly relevant for the homestead exemptions, which represent the largest and most important exemption category. Figure 3.2 shows the dollar value increase for each homestead exemption change in our dataset, suggesting that the variation across states and the economic magnitude of the policies associated with consumer bankruptcy are substantial.

### 3.3.2 Empirical methodology

Using a difference-in-differences framework, our empirical model exploits staggered changes in consumer bankruptcy exemptions across U.S. states and over time to account for the variation in employees' financial wellness. An important feature of this framework is

FIGURE 3.2: Homestead exemptions increments

This figure shows the dollar value increments in homestead exemptions in our dataset. More details on the definition of homestead exemptions can be found in Appendix 3.A.




that more generous consumer bankruptcy exemptions affect firms only through the impact on their employees' financial wellness, as in the United States the bankruptcy code distinguishes between corporate and consumer bankruptcy. Provisions contained in the consumer bankruptcy code do not apply to corporate bankruptcy proceedings. This feature helps us argue in favor of complying with the exclusion restriction of our empirical model, which can be seen as the second stage regression of a two-stage least square model in which we use bankruptcy exemptions to instrument the unobservable changes in rank-and-file employees' financial wellness.

In the main empirical analysis, we test the effects of higher bankruptcy exemptions on firms' myopic accounting decisions by comparing the outcome variables in states that increase exemptions to the outcome variables in states without such changes. To estimate

these effects, we use the following firm-level linear model,

$$y_{i,j,s,t} = \alpha + \beta_1 Log\ Homestead_{s,t-1} + \beta_2 Log\ Non\text{-}Homestead_{s,t-1} + \\ + \delta \boldsymbol{Z}_{s,t-1} + \gamma \boldsymbol{X}_{i,j,s,t-1} + a_t + b_i + \epsilon_{i,j,s,t} \tag{3.1}$$

where the dependent variable, $y$, is an accounting myopia proxy of firm $i$ operating in industry $j$, and headquartered in state $s$ in fiscal year $t$. The independent test variables are *Log Homestead* and *Log Non-Homestead*, which measure the natural logarithms of state-level homestead and non-homestead consumer bankruptcy exemptions, respectively. The model includes time-varying state controls ($\boldsymbol{Z}$) to account for heterogeneity in local macroeconomic and labor conditions, and time-varying firm controls ($\boldsymbol{X}$) to account for observable firm-specific characteristics. In addition, the model includes time fixed effects, $a_t$, to account for unobservable time-trends, and firm fixed effects, $b_i$, which ensure that the estimates reflect actual changes in the outcome variables and exemptions generosity over time, and not simple cross-sectional correlations.[8] We present the detailed description of all the variables in Appendix 3.A.

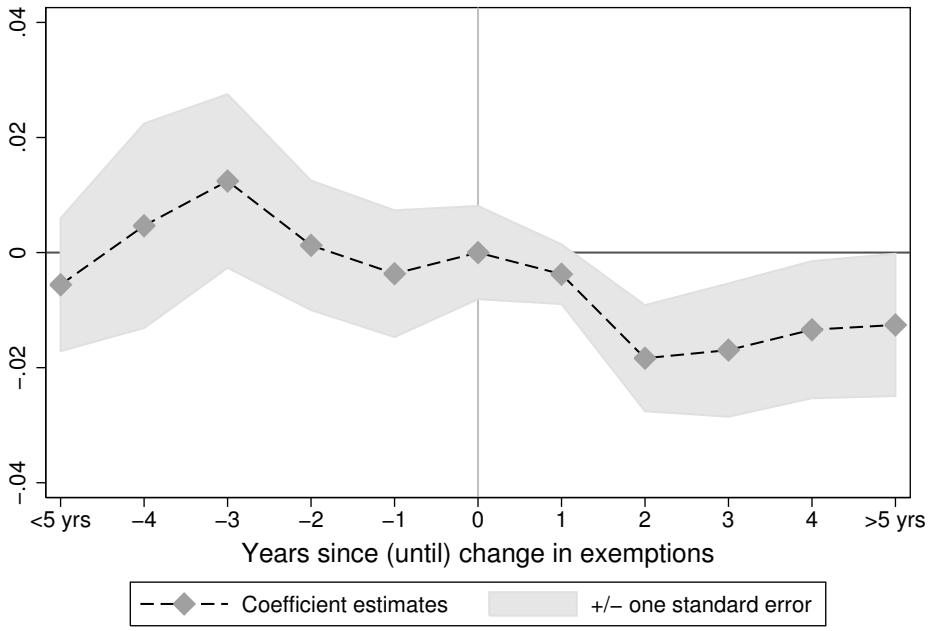
The identification strategy underlying the empirical model reported in equation (3.1) relies on within states and across time variation in bankruptcy exemptions, under the assumption that changes in consumer bankruptcy exemptions are quasi-random with respect to firms' myopic decisions. Importantly, bankruptcy exemptions affect firms only through their effects on employees' financial wellness because they do not apply to publicly traded firms, which follow instead corporate bankruptcy laws. Nevertheless, some time-varying unobservable variables correlating with both firms' myopic accounting decisions and shifts in bankruptcy exemptions could affect our results. To assess this possibility, we look at parallel trends. In the absence of treatment, the average response of firms in both treated and control groups should be the same. We use pre-treatment data to show that trends in one of our main measures of myopic behavior (i.e., real activities management)

[8] Our baseline empirical model does not include industry-by-year fixed effects because the dependent variables are measured as residuals from regressions estimated by year and industry groups. However, when the dependent variables are not measured as residuals from industry-year regressions, we include industry-by-year fixed effects in the empirical model.

follow a similar path in both groups of firms. Specifically, Figure 3.3 indicates an absence of systematic differences in firms' real activities management between treated and control groups before the increases in consumer bankruptcy exemptions.

FIGURE 3.3: Dynamic effect of homestead exemptions on real activities management

This figure shows the dynamic effect of homestead exemptions on real activities management. The coefficients plotted are obtained from the model in equation (3.1) and represent the relative effect of homestead exemptions on real activities management with respect to the year in which homestead exemptions increase.




In subsequent analysis, we use the same identification strategy to examine the mechanism underlying the relation between improvements in employees' financial wellness and reductions in employers' myopic accounting choices. Specifically, we test whether bankruptcy exemptions affect rank-and-file employees' absenteeism from the workplace and firm productivity, consistent with the evidence that alleviating financial concerns allows workers to become more attentive and productive at work (Kaur et al., 2021).

First, we estimate the effect of higher bankruptcy exemptions on rank-and-file employees' absenteeism from the workplace. To do so, we exploit establishment-level injury data from the U.S. Bureau of Labor Statistics' (BLS) annual Survey of Occupational Injuries and Illnesses (SOII). As the firm's plants can be located in different states from

the firm's headquarter, and can be therefore subject to different bankruptcy regimes, establishment-level data allow us to measure the bankruptcy regime workers are subject to more accurately. Our baseline establishment-level linear model is as follows,

$$\begin{aligned} y_{e,s,t} = \alpha + \beta_1 Log\ Homestead_{s,t-1} + \beta_2 Log\ Non\text{-}Homestead_{s,t-1} + \\ + \delta \boldsymbol{Z}_{s,t-1} + a_t + b_s + \epsilon_{e,s,t} \end{aligned} \tag{3.2}$$

where the dependent variable, $y$, is one of our two injury- and illness-related measures of absenteeism for rank-and-file employees that work in establishment $e$, located in state $s$ during the fiscal year $t$. The independent test variables, *Log Homestead* and *Log Non-Homestead*, are as in equation (3.1), and the model includes time-varying state controls ($\boldsymbol{Z}$), time fixed effects, $a_t$, and state fixed effects, $b_s$. Moreover, we investigate the effects of better employees' financial wellness on firm productivity by estimating a firm-level linear model similar to the one reported in equation (3.1), where the dependent variable is a proxy for total firm productivity.

In all our empirical models, we compute robust state-level clustered standard errors to control for the within-group correlation structure. When the number of clusters is large, clustering alleviates the problems arising from serially correlated outcome variables. Consistent with the rule of thumb of 50 groups suggested by Bertrand, Duflo, and Mullainathan (2004), our estimations include 51 clusters (i.e., 50 U.S. states plus the District of Columbia).

## 3.4 Data and variable description

We obtain our dataset using several sources. To construct the independent variables, we proceed as follows. First, we hand-collect state-level data on consumer bankruptcy exemptions—homestead and non-homestead—as defined in Chapter 7 of the United States Bankruptcy Code, from the state legal documents of all U.S. states from 1995 to 2005. Our dataset includes 36 homestead and 73 non-homestead exemption increases

for the sample period 1995-2005.[9] The sample ends in 2005 because the enactment of the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) that occurred in 2005 made it more complex and less relevant to file for bankruptcy. Using data on bankruptcy exemptions, we compute *Log Homestead* and *Log Non-Homestead*, which are the natural logarithms of the dollar value of homestead and non-homestead exemptions, respectively.

Second, we collect financial data from Compustat to construct firm-specific control variables. *Big 4* is an indicator equaling 1 if firm $i$ is audited by a Big Four audit firm, and 0 otherwise; *Book Leverage* is the ratio between short- and long-term debt and lagged total assets; *Dividend* is the ratio between dividends and lagged total assets; *Market-to-Book* is the ratio between the market value of assets and the book value of assets; *ROA* is the ratio between operating income before depreciation and lagged total assets; *Size* is the natural logarithm of total assets; and *Total Accruals* is the ratio between total accruals and lagged total assets.

Third, we collect macroeconomic and labor data to construct state-specific control variables for every state from 1995 to 2005. Data on GDP growth rate (variable *GDP Growth*), disposable income (variable *Per-capita Income*), and unemployment rate (variable *Unemployment Rate*) are from the U.S. Bureau of Economic Analysis (BEA), whereas unemployment insurance data (variable *UI*) are from the U.S. Department of Labor website, which has information from 1977.[10]

Furthermore, we collect data from Compustat and Audit Analytics and construct our dependent variables capturing accounting myopia. To measure real activities management and accrual-based earnings management, we apply the commonly used two-step procedure that consists of using the residuals from ordinary least squares as the dependent variable in a second regression (Dechow, Ge, and Schrand, 2010). As previous

[9] We use the same exemptions dataset as Pinto (2021). Consistent with what he reports on page 12, we focus on increases in exemptions because of the limited number of decreases. In our sample, we have only 1 case of homestead and 14 cases of non-homestead exemption decreases, respectively. The disproportion in the number of events reflects the fact that non-homestead exemptions are defined as the sum of exempted values of several personal assets. Even if the value of non-homestead exemptions changes just for one asset, this counts as a shock to non-homestead exemptions.

[10] Significant Provisions of State Unemployment Insurance Laws, the U.S. Department of Labor.

studies show that this procedure could lead to biased estimates and standard errors, and thus incorrect inferences (Chen, Hribar, and Melessa, 2018), we later supplement these measures with alternative proxies for myopic accounting decisions.

We follow Roychowdhury (2006) to measure real activities management (i.e., *RAM*). Specifically, *RAM* is the combination of three proxies, i.e., abnormal discretionary expenses, abnormal cash flow from operations, and abnormal production costs. These measures capture the reduction in discretionary expenses, such as advertising, R&D, and SG&A expenses, the acceleration of sales via more lenient credit terms or higher price discounts, and the determination of a lower cost of goods sold via increased production.

To construct our real activities management proxy, we start by estimating the following models for each year-industry (two-digit SIC) group,

$$\frac{Disx_{i,t}}{Assets_{i,t-1}} = \kappa_{1t}\frac{1}{Assets_{i,t-1}} + \kappa_{2t}\frac{Sales_{i,t-1}}{Assets_{i,t-1}} + \epsilon_{i,t} \tag{3.3}$$

$$\frac{CFO_{i,t}}{Assets_{i,t-1}} = \kappa_{1t}\frac{1}{Assets_{i,t-1}} + \kappa_{2t}\frac{Sales_{i,t}}{Assets_{i,t-1}} + \kappa_{3t}\frac{\Delta Sales_{i,t}}{Assets_{i,t-1}} + \epsilon_{i,t} \tag{3.4}$$

$$\frac{Prod_{i,t}}{Assets_{i,t-1}} = \kappa_{1t}\frac{1}{Assets_{i,t-1}} + \kappa_{2t}\frac{Sales_{i,t}}{Assets_{i,t-1}} + \kappa_{3t}\frac{\Delta Sales_{i,t}}{Assets_{i,t-1}} + \kappa_{4t}\frac{\Delta Sales_{i,t-1}}{Assets_{i,t-1}} + \epsilon_{i,t} \tag{3.5}$$

where $Disx_{i,t}$ represents discretionary expenditures defined as the sum of advertising, R&D, and SG&A expenses for firm $i$ in year $t$; $CFO_{i,t}$ represents cash flow from operations for firm $i$ in year $t$; and $Prod_{i,t}$ represents production costs defined as the sum of costs of goods sold and the change in inventories for firm $i$ in year $t$. $Sales_{i,t}$ measures total sales for firm $i$ in year $t$, $Sales_{i,t\text{-}1}$ measures total sales for firm $i$ in year *t-1*, and $\Delta$ indicates the annual change in *Sales* (from *t-1* to $t$ or from *t-2* to *t-1*). Finally, $Assets_{i,t\text{-}1}$ represents total assets for firm $i$ in year *t-1*. We measure abnormal discretionary expenses, abnormal cash flow form operations, and abnormal production costs as the difference between the actual values and the values predicted from equations (3.3), (3.4), and (3.5), respectively.

Our main measure of real activities management, *RAM*, is the combination of abnormal discretionary expenses, abnormal cash flow from operations, and abnormal production costs. Specifically, following Cohen and Zarowin (2010), we compute this metric (i.e., *RAM*) as the sum of abnormal discretionary expenses, abnormal cash flow from operations (both multiplied by negative one), and abnormal production costs (*Abnormal Disx.*, *Abnormal CFO*, and *Abnormal Prod.*, respectively). Multiplying by negative ones ensures that higher (lower) values correspond to more (less) real activities management for all metrics. As the proxies for real activities management are residuals that we use as dependent variables, in robustness (unreported) tests we remove the bias identified in Chen, Hribar, and Melessa (2018) by including all first stage regressors in our second stage regressions.

We further measure accrual-based earnings management in two ways following Dechow, Sloan, and Sweeney (1995) and Dechow and Dichev (2002), respectively. To construct the first proxy, we start by estimating the following model for each year-industry (two-digit SIC) group,

$$\frac{Total\ Accruals_{i,t}}{Assets_{i,t-1}} = \kappa_{1t}\frac{1}{Assets_{i,t-1}} + \kappa_{2t}\frac{\Delta Sales_{i,t} - \Delta Rec_{i,t}}{Assets_{i,t-1}} + \kappa_{3t}\frac{PPE_{i,t}}{Assets_{i,t-1}} + \epsilon_{i,t} \quad (3.6)$$

where $Total\ Accruals_{i,t}$ represents total accruals for firm $i$ in year $t$ measured as per Hribar and Collins (2002), $\Delta Sales_{i,t}$ measures the annual change in total sales for firm $i$ in year $t$, $\Delta Rec_{i,t}$ measures the annual change in accounts receivables for firm $i$ in year $t$, $PPE_{i,t}$ measures gross property, plant, and equipment for firm $i$ in year $t$, and $Assets_{i,t\text{-}1}$ represents total assets for firm $i$ in year *t-1*. We measure discretionary accruals (variable *Discretionary Accruals Modified-Jones*) as the difference between the actual values and the values predicted from equation (3.6).

To construct the second proxy for accrual-based earnings management, we start by estimating the following model for each year-industry (two-digit SIC) group,

$$\frac{Total\ Accruals_{i,t}}{Assets_{i,t-1}} = \kappa_{1t}\frac{CFO_{i,t}}{Assets_{i,t}} + \kappa_{2t}\frac{CFO_{i,t-1}}{Assets_{i,t-1}} + \kappa_{3t}\frac{CFO_{i,t+1}}{Assets_{i,t+1}} + \\ + \kappa_{4t}\frac{\Delta Sales_{i,t}}{Assets_{i,t-1}} + \kappa_{5t}\frac{PPE_{i,t}}{Assets_{i,t-1}} + \epsilon_{i,t} \tag{3.7}$$

where $Total\ Accruals_{i,t}$ represents total accruals for firm $i$ in year $t$ measured as per Hribar and Collins (2002), *CFO* represents cash flow from operations for firm $i$ in years $t$, *t-1*, or *t+1*, $\Delta Sales_{i,t}$ measures the annual change in total sales for firm $i$ in year $t$, $PPE_{i,t}$ measures gross property, plant, and equipment for firm $i$ in year $t$, and *Assets* represents total assets for firm $i$ in years $t$, *t-1*, or *t+1*. We measure discretionary accruals (variable *Discretionary Accruals Dechow-Dichev*) as the difference between the actual values and the values predicted from equation (3.7).

To test whether firms have fewer misstatements when consumer bankruptcy exemptions increase, we construct a sample of firms that have misstated their financial statements at least once during the sample period 1995-2005. We collect misstatement information from the Non-Reliance Restatement database of Audit Analytics, which allows to detect not only the date of restatements, but more importantly, the fiscal year(s) when firms have misstated their accounting numbers.[11] Furthermore, detected cases of financial misstatements provide an ex-post measure of severe accrual-based earnings manipulations, thereby allowing us to overcome some limitations associated with the discretionary accruals measures (Chen, Hribar, and Melessa, 2018).

Using restatement data, we construct the following dependent variables: *Restatement*, an indicator equaling 1 if firms have misstated their financial statements in fiscal year $t$ and are sanctioned by an ex-post restatement, and 0 otherwise; *Restatement Up*, an indicator equaling 1 if firms have misstated their financial statements upward in fiscal year $t$ and are sanctioned by an ex-post restatement, and 0 otherwise; *Num. Restatement*, the number of financial misstatements that firms have committed in fiscal year $t$; and

[11] Because Audit Analytics is incomplete before 2000 (e.g., Gonzales, Schmid, and Yermack, 2013), in unreported tests we confirm our results by limiting the sample to the 2000-2005 period.

*Num. Restatement Up*, the number of upward financial misstatements that firms have committed in fiscal year *t*.

To test firms' loss avoidance myopic behaviors, we follow Burgstahler and Dichev (1997) and examine whether the distribution of changes in earnings (measured as change in net income scaled by lagged total assets) before vis-à-vis after bankruptcy exemptions increase is discontinuous at zero and abnormally high above such threshold. Moreover, we perform manipulation tests based on the local-polynomial density discontinuity around zero.

Finally, consistent with Cohn and Wardlaw (2016) and İmrohoroğlu and Tüzel (2014), we collect data from the U.S. Bureau of Labor Statistics website and Compustat, and construct our proxies for workers' absenteeism and firm productivity. Specifically, using data from the U.S. Bureau of Labor Statistics' (BLS) annual Survey of Occupational Injuries and Illnesses (SOII), we obtain two proxies: Rank-and-file employees' absenteeism from the workplace as the incidence rate of total injury and illness cases (variable *TCR*), and rank-and-file employees' absenteeism from the workplace as the incidence rate of injury and illness cases leading to days away from work (variable *DAFWII*). Using Compustat data, we measure total firm productivity (variable *Total Productivity*) as per İmrohoroğlu and Tüzel (2014). Table 3.1 summarizes the descriptive statistics of all variables.

## 3.5 Empirical results

### 3.5.1 Impact of bankruptcy exemptions on firm myopic decisions

In the first part of our empirical tests, we use three empirical strategies to investigate whether firms make fewer myopic accounting choices in response to increases in consumer bankruptcy exemptions (H1, H2, and H3). First, we analyze firms' myopic decisions by looking at real activities management and accrual-based earnings management (H1). Table 3.2 reports results from estimating the model in equation (3.1) when the dependent variable measures real activities management (i.e., *RAM*). The findings document that more generous consumer bankruptcy protection leads to a decrease in firms' real activities

TABLE 3.1: Summary statistics

This table reports the summary statistics of the variables used in our main tests. Panel A shows the summary statistics of the main dependent variables. Panel B reports the statistics of homestead and non-homestead exemptions, which are the independent test variables capturing the generosity of the consumer bankruptcy system in the United States. Panel C reports the statistics of firm-level controls. Panel D shows the summary statistics of state-level variables capturing macroeconomic conditions, and Panel E reports the descriptive statistics of additional variables used for robustness tests. Details on the definition, source, and computation of the variables can be found in Appendix 3.A. All financial variables are winsorized at 1% tails.

| | Mean | S.D. | $25^{th}$ | Median | $75^{th}$ |
|---|---|---|---|---|---|
| | **Panel A: Dependent variables** | | | | |
| TCR | 10.434 | 2.195 | 8.854 | 10.263 | 11.929 |
| DAFWII | 3.364 | 1.121 | 2.670 | 3.150 | 3.696 |
| Total Productivity | -0.320 | 0.445 | -0.525 | -0.312 | -0.089 |
| RAM | -0.074 | 0.480 | -0.322 | -0.059 | 0.180 |
| Discretionary Accruals Modified-Jones | -0.045 | 0.158 | -0.114 | -0.034 | 0.032 |
| Discretionary Accruals Dechow-Dichev | -0.002 | 0.094 | -0.032 | 0.005 | 0.040 |
| Restatement | 0.353 | 0.478 | 0.000 | 0.000 | 1.000 |
| Restatement Up | 0.318 | 0.466 | 0.000 | 0.000 | 1.000 |
| Num. Restatement | 0.423 | 0.644 | 0.000 | 0.000 | 1.000 |
| Num. Restatement Up | 0.371 | 0.597 | 0.000 | 0.000 | 1.000 |
| | **Panel B: Independent test variables** | | | | |
| Homestead ($) | 60,704 | 75,526 | 15,000 | 34,850 | 75,000 |
| Non-Homestead ($) | 7,661 | 8,610 | 4,250 | 5,706 | 8,500 |
| | **Panel C: Firm-level controls** | | | | |
| Big 4 | 0.687 | 0.464 | 0.000 | 1.000 | 1.000 |
| Book Leverage | 0.193 | 0.186 | 0.014 | 0.154 | 0.321 |
| Dividend | 0.007 | 0.017 | 0.000 | 0.000 | 0.003 |
| Market-to-Book | 1.952 | 2.378 | 0.824 | 1.244 | 2.149 |
| ROA | 0.078 | 0.177 | 0.039 | 0.114 | 0.174 |
| Size | 5.120 | 1.923 | 3.759 | 4.966 | 6.333 |
| Total Accruals | -0.061 | 0.166 | -0.108 | -0.050 | 0.002 |
| | **Panel D: State-level controls** | | | | |
| GDP Growth (%) | 5.774 | 2.197 | 4.300 | 5.800 | 7.100 |
| Per-capita Income ($) | 26,681 | 3,405 | 24,482 | 26,258 | 29,106 |
| UI ($) | 7,872 | 1,781 | 6,724 | 7,553 | 8,877 |
| Unemployment Rate (%) | 5.227 | 1.234 | 4.367 | 5.225 | 6.050 |
| | **Panel E: Additional variables** | | | | |
| Abnormal Disx. | -0.052 | 0.271 | -0.171 | -0.016 | 0.091 |
| Abnormal CFO | 0.002 | 0.136 | -0.072 | -0.009 | 0.061 |
| Abnormal Prod. | -0.024 | 0.236 | -0.150 | -0.031 | 0.089 |
| RAM1 | -0.077 | 0.448 | -0.292 | -0.045 | 0.158 |
| RAM2 | -0.050 | 0.276 | -0.185 | -0.030 | 0.098 |

management. Column 1 of Table 3.2 shows the coefficient estimates of the test independent variables (*Log Homestead* and *Log Non-Homestead*) without any control variable, whereas column 2 summarizes the results of our baseline specification when we control for time-varying firm characteristics, state macroeconomic factors, time-invariant firm

unobservable characteristics, and time-varying unobservables.

The estimated effects are both statistically and economically meaningful. For example, in column 2 we find that a 10% increase in homestead exemptions (around $6,070) leads to a real activities management reduction of 0.0018, which is around 2.5% of corporate earnings, based on a *ROA* mean for treated firms of approximately 7.5%. This increment in exemptions represents a sizeable fraction (around 23%) of the individuals' average disposable income. In contrast, we find that non-homestead exemptions do not affect corporate real activities management. The estimates across the specifications are neither statistically nor economically significant. This result is consistent with non-homestead exemptions being smaller in dollar value than homestead exemptions ($7,661 versus $60,704, on average, respectively), thereby affecting a limited portion of individuals' wealth.

Firms are assigned to a state based on their headquarters' location. Workers of firms' plants located in states different from the headquarter are subject to the bankruptcy laws of their state of location. Therefore, if the workforce is geographically dispersed, we might be capturing the benefits of bankruptcy exemptions with error. To address this concern, we split the sample between firms in industries with less dispersed and more dispersed workforce (as in Agrawal and Matsa, 2013). Columns 3 and 4 of Table 3.2 report the results of this subsample analysis. We find that increases in bankruptcy exemptions lead to a decrease in firms' myopic real activities management when the workforce is less dispersed, that is when rank-and-file employees are more likely to work in the same state as the headquarter state.

In Table 3.3, we investigate whether firms reduce their discretionary accruals in response to more generous consumer bankruptcy exemptions, but fail to find evidence in support of this hypothesis. Specifically, in both columns 1 and 2 of Table 3.3, results show that homestead and non-homestead exemptions do not affect accrual-based earnings management, which is measured by the modified Jones' model (Dechow, Sloan, and Sweeney, 1995) and the Dechow and Dichev's model (Dechow and Dichev, 2002). Altogether, the evidence provided by our panel dataset analysis (Tables 3.2 and 3.3) suggests

TABLE 3.2: Effect of exemptions on real activities management

This table reports the coefficients from estimating the model in equation (3.1) when the dependent variable proxies for firms' real activities management (*RAM*), which is measured as the sum of abnormal discretionary expenses, abnormal cash flow from operations (both multiplied by negative one), and abnormal production costs. The independent variables of interest are *Log Homestead* and *Log Non-Homestead*, which measure the natural logarithms of lagged homestead and non-homestead exemptions, respectively. Firm-level controls include the following lagged variables: *Big 4*, *Book Leverage*, *Dividend*, *Market-to-Book*, *ROA*, *Size*, and *Total Accruals*. State-level controls include lagged state *GDP Growth*, lagged unemployment insurance (*UI*), and lagged *Unemployment Rate*. To split the sample between firms with less and more dispersed employees, we use industry classifications and define Transportation (two-digit SIC: 40-47), Wholesale (two-digit SIC: 50-51), and Retail (two-digit SIC: 52-59) as industries that are more likely to have geographically dispersed employees. All firms' financial controls are winsorized at 1% tails. Details on the definition, source, and computation of the variables can be found in Appendix 3.A. Standard errors in parentheses are robust and clustered at the state level. $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% level, respectively.

| | Full Sample | | Less Dispersed Employees | More Dispersed Employees |
|---|---|---|---|---|
| | (1) RAM | (2) RAM | (3) RAM | (4) RAM |
| Log Homestead$_{t-1}$ | -0.020*** | -0.018*** | -0.019** | -0.017 |
| | (0.007) | (0.006) | (0.008) | (0.027) |
| Log Non-Homestead$_{t-1}$ | 0.005 | 0.000 | -0.000 | 0.005 |
| | (0.010) | (0.007) | (0.008) | (0.023) |
| Big 4 | | -0.023*** | -0.022*** | -0.029 |
| | | (0.006) | (0.007) | (0.022) |
| Book Leverage | | -0.064*** | -0.061** | -0.066 |
| | | (0.023) | (0.025) | (0.061) |
| Dividend | | -0.027 | 0.035 | -0.415 |
| | | (0.170) | (0.161) | (0.384) |
| Market-to-Book | | -0.016*** | -0.016*** | -0.008 |
| | | (0.002) | (0.002) | (0.015) |
| ROA | | -0.211*** | -0.214*** | -0.181* |
| | | (0.026) | (0.029) | (0.096) |
| Size | | 0.043** | 0.043** | 0.040** |
| | | (0.018) | (0.020) | (0.017) |
| Total Accruals | | -0.005 | 0.007 | -0.093** |
| | | (0.028) | (0.031) | (0.037) |
| Intercept | Yes | Yes | Yes | Yes |
| State Controls | No | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Adj. $R^2$ | 0.7502 | 0.7567 | 0.7516 | 0.7865 |
| Obs. | 30,982 | 30,982 | 26,049 | 4,933 |

that firms reduce their real activities management in response to better workers' financial wellness and that real activities management and accrual-based earnings management act as substitutes (Zang, 2011).

Second, we focus on firms that misstate their financial statements during our sample period to test H2. Detected cases of misstatements leading to subsequent restatements provide an ex-post measure of accrual-based earnings manipulations that are not GAAP compliant, allowing us to overcome some limitations associated with discretionary accruals measures (Chen, Hribar, and Melessa, 2018). In Table 3.4, the coefficient estimate of *Log Homestead* is negative and statistically significant across all specifications (with the only exception of column 4), suggesting that firms reduce the likelihood and the number of financial misstatements in general and of income-increasing financial misstatements in particular after homestead bankruptcy exemptions increase.

Finally, we examine firms' loss avoidance behaviors around zero in the earnings distribution to test H3. As profitability improves in response to enhanced employees' financial wellness (Pinto, 2021) and workplace productivity increases when employees have fewer financial concerns (Kaur et al., 2021), we expect firms to have less need and incentives to manage their earnings to obtain positive results and maintain "consistent profitability" (Hayn, 1995). To test firms' loss avoidance behaviors, we examine whether the distributions of changes in earnings before vis-à-vis after bankruptcy exemptions increases are discontinuous at zero and abnormally high above such threshold. Consistent with less need to engage in loss avoidance practices when employees' financial wellness improves, Figure 3.4 shows that during the three years before exemptions increase, earnings changes are discontinuous around zero, whereas during the three years following bankruptcy exemptions increases, the discontinuity at zero is less pronounced. Moreover, the histograms reported at the top of Figure 3.4 highlight that the distribution of the changes in earnings shifts to the positive region after bankruptcy exemptions increase compared to before such changes. Overall, these results suggest that firms benefiting from enhanced employees' financial wellness have less need to manage their financial performance to meet (short-term) earnings-based targets.

TABLE 3.3: Effect of exemptions on accrual-based earnings management

This table reports the coefficients from estimating the model in equation (3.1) when the dependent variable proxies for accrual-based earnings management. Column 1 dependent variable is discretionary accruals measured as per Dechow, Sloan, and Sweeney (1995), i.e., *Discretionary Accruals Modified-Jones*. Column 2 dependent variable is discretionary accruals measured as per Dechow and Dichev (2002), i.e., *Discretionary Accruals Dechow-Dichev*. The independent variables of interest are *Log Homestead* and *Log Non-Homestead*, which are the natural logarithms of lagged homestead and non-homestead exemptions, respectively. Firm-level controls include the following lagged variables: *Big 4*, *Book Leverage*, *Dividend*, *Market-to-Book*, *ROA*, and *Size*. State-level controls include lagged state *GDP Growth*, lagged unemployment insurance (*UI*), and lagged *Unemployment Rate*. All firms' financial controls are winsorized at 1% tails. Details on the definition, source, and computation of the variables can be found in Appendix 3.A. Standard errors in parentheses are robust and clustered at the state level. $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% level, respectively.

| | (1) Discretionary Accruals Modified-Jones | (2) Discretionary Accruals Dechow-Dichev |
|---|---|---|
| Log Homestead$_{t-1}$ | 0.001 | 0.004 |
| | (0.004) | (0.004) |
| Log Non-Homestead$_{t-1}$ | -0.010** | -0.003 |
| | (0.004) | (0.002) |
| Big 4 | -0.001 | -0.001 |
| | (0.003) | (0.002) |
| Book Leverage | -0.039*** | 0.017** |
| | (0.008) | (0.007) |
| Dividend | 0.110 | 0.082 |
| | (0.094) | (0.061) |
| Market-to-Book | -0.001 | -0.002* |
| | (0.001) | (0.001) |
| ROA | 0.096*** | 0.070*** |
| | (0.012) | (0.007) |
| Size | -0.029*** | -0.019*** |
| | (0.003) | (0.003) |
| Intercept | Yes | Yes |
| State Controls | Yes | Yes |
| Firm FE | Yes | Yes |
| Year FE | Yes | Yes |
| Adj. $R^2$ | 0.2420 | 0.1973 |
| Obs. | 33,121 | 29,017 |

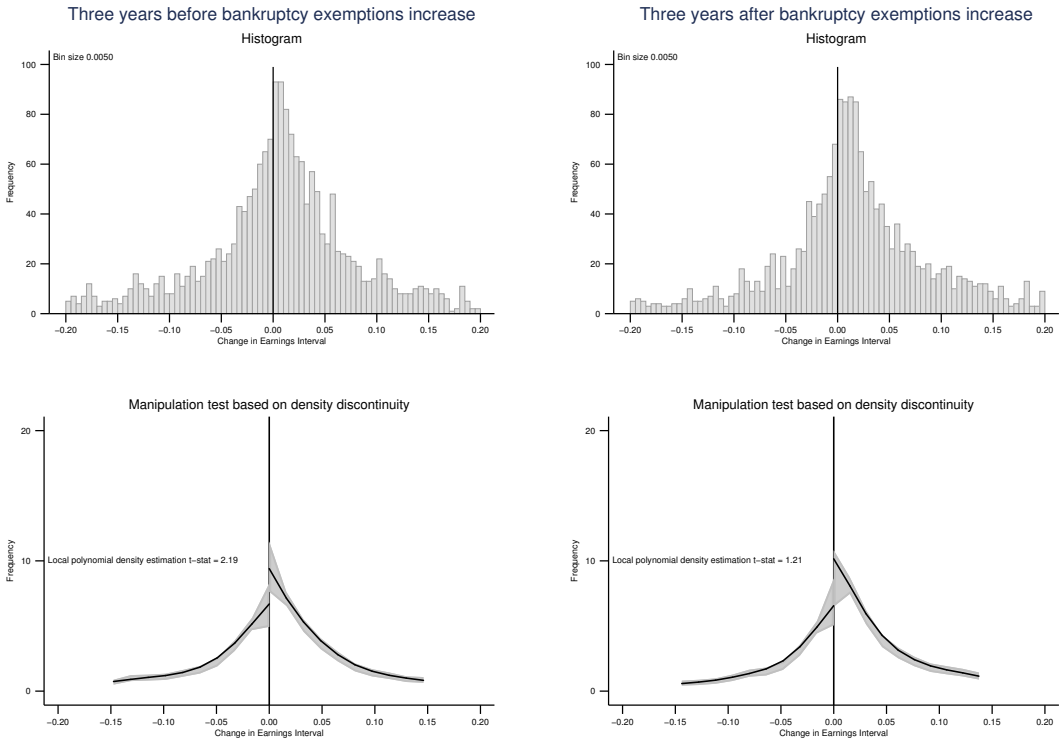
TABLE 3.4: Effect of exemptions on financial misstatements

This table shows the results from investigating the effects of consumer bankruptcy exemptions on firms' misstatement activities. The sample includes only firms that have misstated their financial statements at least once during the sample period (based on the Non-Reliance Restatement database of Audit Analytics), and it excludes firms whose misstatements are due to clerical errors. Results are from estimating the model in equation (3.1), with the following dependent variables. Column 1 dependent variable is an indicator equal to 1 if firms have misstated their financial statements in year $t$ and are sanctioned by an ex-post restatement, and 0 otherwise (*Restatement*). Column 2 dependent variable is an indicator equal to 1 if firms have misstated their financial statements upward in year $t$ and are sanctioned by an ex-post restatement, and 0 otherwise (*Restatement Up*). Column 3 dependent variable measures the number of misstatements that firms have committed during year $t$ (*Num. Restatement*). Column 4 dependent variable measures the number of upward misstatements that firms have committed during year $t$ (*Num. Restatement Up*). The independent variables of interest are *Log Homestead* and *Log Non-Homestead*, which measure the natural logarithms of lagged homestead and non-homestead exemptions, respectively. Firm-level controls include the following lagged variables: *Big 4*, *Book Leverage*, *Dividend*, *Market-to-Book*, *ROA*, and *Size*. State-level controls include lagged state *GDP Growth*, lagged unemployment insurance (*UI*), and lagged *Unemployment Rate*. All firms' financial controls are winsorized at 1% tails. Details on the definition, source, and computation of the variables can be found in Appendix 3.A. Standard errors in parentheses are robust and clustered at the state level. $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% level, respectively.

| | (1) Restatement | (2) Restatement Up | (3) Num. Restatement | (4) Num. Restatement Up |
|---|---|---|---|---|
| Log Homestead$_{t-1}$ | -0.046** | -0.044* | -0.057** | -0.051 |
| | (0.019) | (0.023) | (0.026) | (0.033) |
| Log Non-Homestead$_{t-1}$ | -0.044** | -0.042* | -0.067* | -0.048 |
| | (0.022) | (0.021) | (0.034) | (0.032) |
| Big 4 | -0.024 | -0.021 | -0.003 | 0.002 |
| | (0.027) | (0.025) | (0.036) | (0.033) |
| Book Leverage | 0.050 | 0.053 | 0.087 | 0.089 |
| | (0.053) | (0.050) | (0.070) | (0.058) |
| Dividend | 0.556 | 0.883** | 0.985** | 1.231*** |
| | (0.366) | (0.362) | (0.488) | (0.425) |
| Market-to-Book | 0.006*** | 0.007*** | 0.011*** | 0.011*** |
| | (0.002) | (0.002) | (0.003) | (0.003) |
| ROA | 0.179*** | 0.183*** | 0.135** | 0.137** |
| | (0.043) | (0.054) | (0.059) | (0.068) |
| Size | 0.047*** | 0.049*** | 0.062*** | 0.058*** |
| | (0.011) | (0.012) | (0.014) | (0.014) |
| Intercept | Yes | Yes | Yes | Yes |
| State Controls | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes |
| Industry-by-Year FE | Yes | Yes | Yes | Yes |
| Adj. $R^2$ | 0.3397 | 0.3579 | 0.3801 | 0.3862 |
| Obs. | 11,810 | 11,810 | 11,810 | 11,810 |

FIGURE 3.4: Loss avoidance before and after bankruptcy exemptions increase

This figure shows the empirical distributions of changes in annual net income scaled by total assets as of the beginning of the first year *(Net Income$_t$ - Net Income$_{t-1}$)/Total Assets$_{t-2}$* distinguishing between the three years before bankruptcy exemptions increase and the three years after bankruptcy exemptions increase. In both histograms reported at the top of the figure, the distribution interval widths are 0.0050 and the location of zero on the horizontal axis is marked by the vertical line. For example, the first interval to the right of zero contains all scaled changes in earnings in the interval [0.0000, 0.0050), the second interval contains [0.0050, 0.0100), and so on. The vertical axis labeled "Frequency" represents the number of observations in each earnings change interval. The two graphs at the bottom of the figure report the results of the manipulation tests based on the local-polynomial density discontinuity at the zero earnings threshold.




### 3.5.2 Impact of bankruptcy exemptions on employees' absenteeism and firm productivity

In the second part of our empirical tests, we analyze the possible mechanism underlying the relation between the improvements in employees' financial wellness and the reduction in employers' myopic behaviors. Specifically, we test the impact of bankruptcy exemptions on (i) rank-and-file employees' absenteeism from the workplace due to injury and illness cases, and (ii) firm productivity. Consistent with the fact that workers lose

several workdays annually and are less productive when they have financial problems (Kaur et al., 2021),[12] Table 3.5 shows that more generous exemptions decrease rank-and-file employees' absenteeism, while increasing firm productivity. The first two columns of Table 3.5 present the results of the establishment-level analysis of increases in consumer bankruptcy exemptions on employees' absenteeism. The dependent variables are *TCR* in column 1, and *DAFWII* in column 2, respectively. *TCR* measures the incidence rate of total injury and illness cases, whereas *DAFWII* measures the incidence rate of injury and illness cases leading to days away from work. In terms of economic impact, for example, column 2 of Table 3.5 shows that a 10% increase in homestead exemptions leads to a 2.2% decrease in injury and illness cases with days away from work.

In column 3 of Table 3.5, we show that increased bankruptcy exemptions improve total firm productivity. We label firm productivity as *Total Productivity* and compute it following İmrohoroğlu and Tüzel (2014). In terms of economic impact, a 10% increase in homestead exemptions leads to an increase in total firm productivity of 16 basis points. Overall, the findings in Table 3.5 suggest that policies that improve the financial wellness of rank-and-file employees have positive spillover effects on labor supply and firm productivity. These findings, combined with the evidence in Pinto (2021) of higher profitability as a result of higher consumer bankruptcy exemptions, support our argument that firms have less need and incentives to engage in performance-enhancing myopic decisions when they can rely on higher productivity and profitability generated by better workers' financial wellness.

### 3.5.3 Cross-sectional and robustness tests

Restatements are likely to capture detected cases of severe accrual-based earnings manipulations, thus addressing some concerns that our discretionary accruals proxies are biased (Amiram et al., 2018; Chen, Hribar, and Melessa, 2018). Similarly, loss avoidance tests provide indirect evidence that either real activities management or accrual-based earnings management has occurred. In contrast, our real activities management proxies

[12] SalaryFinance, Employer's Guide to Financial Wellbeing 2018-19.

TABLE 3.5: Effect of exemptions on workers' absenteeism and firm productivity

This table reports the establishment- and firm-level regression results capturing the effects of debt relief programs on proxies for workers' absenteeism and firm productivity. In particular, the variables capturing workers' absenteeism are Total Case Rate (*TCR*) and the case rate of Days Away From Work for Injuries and Illnesses (*DAFWII*) in columns 1 and 2, respectively. Detailed definitions of these variables can be found on the U.S. Bureau of Labor Statistics website. The dependent variable in column 3 proxies for total firm productivity (*Total Productivity*) and is measured as per İmrohoroğlu and Tüzel (2014). The independent variables of interest are *Log Homestead* and *Log Non-Homestead*, which measure the natural logarithms of lagged homestead and non-homestead exemptions, respectively. State-level controls are lagged state *GDP Growth*, lagged *Per-capita Income*, and lagged *Unemployment Rate*. Firm-level controls in column 3 include the following lagged variables: *Big 4*, *Book Leverage*, *Dividend*, *Market-to-Book*, *ROA*, *Size*, and *Total Accruals*. All firms' financial controls are winsorized at 1% tails. Details on the definition, source, and computation of the variables can be found in Appendix 3.A. Standard errors in parentheses are robust and clustered at the state level. $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% level, respectively.

| | Establishment-level | | Firm-level |
|---|---|---|---|
| | (1) TCR | (2) DAFWII | (3) Total Productivity |
| Log Homestead$_{t-1}$ | -0.547** | -0.222** | 0.016** |
| | (0.215) | (0.106) | (0.008) |
| Log Non-Homestead$_{t-1}$ | 0.351 | 0.036 | -0.022* |
| | (0.617) | (0.197) | (0.012) |
| Intercept | Yes | Yes | Yes |
| Firm Controls | No | No | Yes |
| State Controls | Yes | Yes | Yes |
| State FE | Yes | Yes | No |
| Year FE | Yes | Yes | No |
| Firm FE | No | No | Yes |
| Industry-by-Year FE | No | No | Yes |
| Adj. $R^2$ | 0.0403 | 0.0387 | 0.5685 |
| Obs. | 255,730 | 255,730 | 23,054 |

do not have an ex-post measure of detected cases of accounting manipulations. Therefore, to alleviate concerns that coefficients and standard errors are biased, we investigate cross-sectional differences in firms' real activities management in response to better rank-and-file employees' financial wellness.

Table 3.6 presents the results of our cross-sectional tests, where we analyze the effect of homestead exemptions on firms with different incentives to engage in real activities management. In columns 1 and 2 of Table 3.6, we focus on firms that issue SEOs during

the period 1995-2005, and split the sample into *SEO Years* and *Non-SEO Years*. The former includes firms in the SEO year and in the two subsequent years, whereas the latter includes firms in all remaining years. Although previous literature shows that firms have strong incentives to engage in real activities management during SEOs (Cohen and Zarowin, 2010; Kothari, Mizik, and Roychowdhury, 2016), the evidence in column 1 suggests that such incentives are offset by the lower incentives to manage earnings following the improvements in employees' financial wellness. The coefficient of *Log Homestead* is indeed statistically insignificant in column 1. In contrast, during non-SEO years, the benefits arising from better employees' financial wellness prevail, thereby motivating firms to reduce their real activities management.

In columns 3 and 4 of Table 3.6, we focus on firms that repeatedly meet or beat analysts' forecasts. Specifically, the sample in column 3 consists of firms whose frequency of meeting or beating analysts' forecasts over the previous four quarters is above 50% (*Repeat Beaters*), whereas the sample in column 4 includes firms whose frequency is below 50% (*Non-Repeat Beaters*). Consistent with the evidence in column 1, the insignificant results in column 3 suggest that, for "repeat beaters", the incentives to engage in real activities management to avoid the negative stock market consequences in case of failure to meet the targets (Bartov, Givoly, and Hayn, 2002) compensate the incentives to reduce the engagement in real activities management in response to better employees' financial wellness.

Finally, in columns 5 and 6 of Table 3.6, we focus on CEOs and CFOs' incentives to manage earnings (Jiang, Petroni, and Wang, 2010) by splitting the sample into *High CEO-CFO Options* and *Low CEO-CFO Options*. The former includes firms for which the Black-Scholes value of option compensation as a proportion of total compensation received by the CEO and the CFO is above the median, whereas the latter includes firms whose CEO and CFO option compensation is below the median. The results in columns 5 and 6 show no evidence that firms decrease their real activities management practices in response to greater consumer bankruptcy protection when CEOs and CFOs have stronger equity incentives. Taken together, the results of our cross-sectional tests

TABLE 3.6: Cross-sectional tests

This table reports the results of the cross-sectional tests performed estimating the model in equation (3.1) when the dependent variable proxies for firms' real activities management (*RAM*). This analysis is performed on subsamples constructed according to firms' incentives to manage earnings. Columns 1 and 2 report the results based on the role of SEOs (Cohen and Zarowin, 2010). *SEO Years* includes firms in the SEO year and in the two subsequent years, whereas *Non-SEO Years* includes firms in all remaining years. Columns 3 and 4 report results based on whether firms repeatedly beat/meet analysts' forecasts (Bartov, Givoly, and Hayn, 2002). *Repeat Beaters* (*Non-Repeat Beaters*) includes firms whose frequency of meeting/beating analysts' earnings forecasts in the past four quarters is above (below) 50%. Columns 5 and 6 report results according to managerial equity incentives, measured following Cohen and Zarowin (2010). *High CEO-CFO Options* (*Low CEO-CFO Options*) identifies when the the Black-Scholes value of option compensation as a proportion of total compensation received by the CEO and the CFO of a firm is above (below) the median in a given fiscal year. The dependent variable across all specifications is *RAM*, and is measured as the sum of abnormal discretionary expenses, abnormal cash flow from operations (both multiplied by negative one), and abnormal production costs. The independent variables of interest are *Log Homestead* and *Log Non-Homestead*, which measure the natural logarithms of lagged homestead and non-homestead exemptions, respectively. Firm-level controls include the following lagged variables: *Big 4*, *Book Leverage*, *Dividend*, *Market-to-Book*, *ROA*, *Size*, and *Total Accruals*. State-level controls include lagged state *GDP Growth*, lagged unemployment insurance (*UI*), and lagged *Unemployment Rate*. All firms' financial controls are winsorized at 1% tails. Details on the definition, source, and computation of the variables can be found in Appendix 3.A. Standard errors in parentheses are robust and clustered at the state level. $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% level, respectively.

| | Dependent Variable RAM | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | SEO Years | Non-SEO Years | Repeat Beaters | Non-Repeat Beaters | High CEO-CFO Options | Low CEO-CFO Options |
| Log Homestead$_{t-1}$ | -0.043 | -0.046** | -0.012 | -0.058** | -0.004 | 0.012 |
| | (0.038) | (0.020) | (0.021) | (0.027) | (0.011) | (0.016) |
| Log Non-Homestead$_{t-1}$ | 0.017 | 0.002 | 0.016 | -0.006 | 0.001 | -0.004 |
| | (0.025) | (0.017) | (0.014) | (0.020) | (0.024) | (0.012) |
| Intercept | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| State Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. $R^2$ | 0.7717 | 0.7597 | 0.8291 | 0.7722 | 0.8461 | 0.8254 |
| Obs. | 2,459 | 5,688 | 8,019 | 2,673 | 3,706 | 3,286 |

suggest that managerial incentives to manage earnings when firms issue SEOs and when firms are "repeat forecast beaters" are compensated by the lower incentives arising from the improvements in employees' financial wellness.

In Table 3.7, we examine the robustness of our findings to alternative proxies for real activities management and confirm our main results. Specifically, column 1 dependent variable is abnormal discretionary expenses measured as per equation (3.3) and multiplied by negative one (*Abnormal Disx.*). Column 2 dependent variable is abnormal discretionary cash flow from operations measured as per equation (3.4) and multiplied by negative one (*Abnormal CFO*), whereas column 3 dependent variable is abnormal production costs measured as per equation (3.5) (*Abnormal Prod.*). Column 4 dependent variable is the sum of abnormal discretionary expenses (multiplied by negative one) and abnormal production costs, whereas column 5 dependent variable is the sum of abnormal discretionary expenses and abnormal cash flow from operations (both multiplied by negative one).[13] Results are robust across all specifications and confirm our main findings that real activities management decreases when consumer bankruptcy exemptions increase.

Finally, we investigate whether unemployment insurance and per-capita disposable income have a mediating effect on the relation between enhanced consumer bankruptcy protection and real activities management. The results of this analysis, reported in Table 3.8, show that unemployment insurance and individuals' wealth do not affect our main findings. In columns 2 and 3 of Table 3.8, we test our real activities management hypothesis (H1) by splitting the sample based on the generosity of state unemployment insurance

[13] In Table 3.5, we find that rank-and-file employees are less absent from the workplace and become more productive after bankruptcy exemptions increase, thereby suggesting that employees positively contribute to firms' cash flows and production. Although the empirical model we use to estimate our main proxy for real activities management, *RAM*, includes abnormal cash flows and abnormal production, it does not control for workers' normal contribution to cash flows and productivity. Therefore, abnormal cash flows could be mechanically higher because of workers' enhanced performance, rather than because of firms' myopic decisions. Similarly, when workers become more productive, firms' production is likely to increase. The residuals from equation (3.5)—the real activities management proxy capturing abnormal productivity and measured by cost of goods sold plus changes in inventory—could be mechanically higher than in the absence of shocks to employees' financial wellness. However, both mis-measurements should work against us finding evidence of reduced myopia. Moreover, our results are robust when using alternative real activities management proxies that do not include abnormal cash flows and abnormal production, as reported in Table 3.7.

TABLE 3.7: Alternative measures of real activities management

This table shows the results from investigating the effects of consumer bankruptcy exemptions on alternative proxies for real activities management. Results are from estimating the model in equation (3.1), with alternative dependent variables. Column 1 dependent variable is abnormal discretionary expenses multiplied by negative one (*Abnormal Disx.*). Column 2 dependent variable is abnormal cash flow from operations multiplied by negative one (*Abnormal CFO*). Column 3 dependent variable is abnormal production costs (*Abnormal Prod.*). Column 4 dependent variable is *RAM1*, which is the sum of *Abnormal Disx.* and *Abnormal Prod.*, whereas Column 5 dependent variable is *RAM2*, which is the sum of *Abnormal Disx.* and *Abnormal CFO*. The independent variables of interest are *Log Homestead* and *Log Non-Homestead*, which measure the natural logarithms of lagged homestead and non-homestead exemptions, respectively. Firm-level controls include the following lagged variables: *Big 4*, *Book Leverage*, *Dividend*, *Market-to-Book*, *ROA*, *Size*, and *Total Accruals*. State-level controls include lagged state *GDP Growth*, lagged unemployment insurance (*UI*), and lagged *Unemployment Rate*. All firms' financial controls are winsorized at 1% tails. Details on the definition, source, and computation of the variables can be found in Appendix 3.A. Standard errors in parentheses are robust and clustered at the state level. $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% level, respectively.

| | (1) Abnormal Disx. | (2) Abnormal CFO | (3) Abnormal Prod. | (4) RAM1 | (5) RAM2 |
|---|---|---|---|---|---|
| Log Homestead$_{t-1}$ | -0.009** | -0.004** | -0.006** | -0.015** | -0.012*** |
| | (0.004) | (0.002) | (0.003) | (0.006) | (0.004) |
| Log Non-Homestead$_{t-1}$ | -0.004 | 0.002 | 0.001 | -0.003 | -0.003 |
| | (0.005) | (0.003) | (0.003) | (0.007) | (0.005) |
| Intercept | Yes | Yes | Yes | Yes | Yes |
| Firm Controls | Yes | Yes | Yes | Yes | Yes |
| State Controls | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| Adj. $R^2$ | 0.7784 | 0.5375 | 0.7190 | 0.7836 | 0.7246 |
| Obs. | 30,982 | 30,982 | 30,982 | 30,982 | 30,982 |

TABLE 3.8: Effect of unemployment insurance and per-capita disposable income

This table shows the results from investigating the heterogeneous effects of homestead exemptions increases on states with different levels of unemployment insurance and per-capita disposable income. Results are from estimating the model in equation (3.1) in different subsamples. Columns 2 and 3 report the results when splitting the sample between high and low unemployment insurance (*High* and *Low UI*, respectively). Columns 4 and 5 report the results when splitting the sample between high and low per-capita disposable income (*High* and *Low Per-capita Income*, respectively). The dependent variable across all specifications is *RAM*, and is measured as the sum of abnormal discretionary expenses, abnormal cash flow from operations (both multiplied by negative one), and abnormal production costs. The independent variables of interest are *Log Homestead* and *Log Non-Homestead*, which measure the natural logarithms of lagged homestead and non-homestead exemptions, respectively. Firm-level controls include the following lagged variables: *Big 4*, *Book Leverage*, *Dividend*, *Market-to-Book*, *ROA*, *Size*, and *Total Accruals*. State-level controls include lagged state *GDP Growth*, lagged unemployment insurance (*UI*)—not in columns 2 and 3, and lagged *Unemployment Rate*. All firms' financial controls are winsorized at 1% tails. Details on the definition, source, and computation of the variables can be found in Appendix 3.A. Standard errors in parentheses are robust and clustered at the state level. $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% level, respectively.

| | Dependent Variable RAM | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| | Full Sample | High UI | Low UI | High Per-capita Income | Low Per-capita Income |
| Log Homestead$_{t-1}$ | -0.018*** | -0.016*** | -0.050* | -0.018** | -0.023*** |
| | (0.006) | (0.005) | (0.027) | (0.008) | (0.007) |
| Log Non-Homestead$_{t-1}$ | 0.000 | -0.002 | 0.002 | -0.017* | 0.006 |
| | (0.007) | (0.011) | (0.013) | (0.008) | (0.010) |
| Intercept | Yes | Yes | Yes | Yes | Yes |
| Firm Controls | Yes | Yes | Yes | Yes | Yes |
| State Controls | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| Adj. $R^2$ | 0.7567 | 0.7567 | 0.7620 | 0.7635 | 0.7575 |
| Obs. | 30,982 | 13,080 | 17,254 | 11,450 | 19,234 |

(*High* and *Low UI*), and find no differences in the relation between homestead exemptions and firms' real activities management across the different levels of unemployment insurance. This evidence suggests that subsidies to unemployment do not substitute consumer bankruptcy social programs. Similarly, in columns 4 and 5, we test our real activities management hypothesis by splitting the sample between high and low per-capita disposable income (variable *Per-capita Income*), and find that consumer bankruptcy protection reduces real activities management regardless of individuals' wealth levels.

## 3.6 Conclusion

In this paper, we investigate the effect of rank-and-file employees' financial wellness on firms' myopic behavior. By exploiting staggered increments in consumer bankruptcy generosity across U.S. states from 1995 to 2005, we show that firms reduce their real activities management, have fewer misstatements, and engage less in loss avoidance practices in response to higher consumer bankruptcy protection. Two findings help explain these results. First, we show that more generous consumer bankruptcy protection reduces workers' absenteeism, and second, we show that firm productivity improves when consumer bankruptcy protection increases. Taken together, our evidence indicates that improvements in rank-and-file employees' financial wellness affect their performance at work, thus reducing firms' need and incentives to take myopic accounting decisions.

# 3.A Appendix—Variable description

| Variable Label | Description |
|---|---|
| **Panel A**: Dependent variables | |
| TCR | Incidence rate of total injuries and illnesses. The incidence rate of injuries and illnesses is computed from the following formula: (Number of injury and illness cases x 200,000) / Employee hours worked. The TCR includes all cases recorded on the OSHA Form 300 (Column G + Column H + Column I + Column J). Further information on injury and illness incidence rates is available on the U.S. Bureau of Labor Statistics website. |
| DAFWII | Incidence rate of injuries and illnesses leading to days away from work. The incidence rate of injuries and illnesses is computed from the following formula: (Number of injury and illness cases x 200,000) / Employee hours worked. The DAFWII includes cases recorded in Column H on the OSHA Form 300. Further information on injury and illness incidence rates is available on the U.S. Bureau of Labor Statistics website. |
| Total Productivity | Total factor productivity measured by İmrohoroğlu and Tüzel (2014). The authors estimate the production function given in $$y_{i,t} = \beta_0 + \beta_k k_{i,t} + \beta_l l_{i,t} + \omega_{i,t} + \epsilon_{i,t},$$ where $y$ is the log of the value added for firm $i$ in fiscal year $t$; $l$ and $k$ are log values of labor and capital of the firm, respectively; $\omega$ is the productivity, and $\epsilon$ is the error term. The semiparametric procedure suggested by Olley and Pakes (1996) is used to estimate the parameters of this production function, and once the production function parameters are estimated, the firm-level (log) total factor productivity is obtained as follows, $$\omega_{i,t} = y_{i,t} - \hat{\beta}_0 - \hat{\beta}_l l_{i,t} - \hat{\beta}_k k_{i,t}.$$ We label $\omega_{i,t}$ as *Total Productivity*. |
| RAM | Real activities management computed as the sum of abnormal discretionary expenses multiplied by negative one (*Abnormal Disx.*), abnormal cash flow from operations multiplied by negative one (*Abnormal CFO*), and abnormal production costs (*Abnormal Prod.*). Abnormal discretionary expenses, abnormal cash flow from operations, and abnormal production costs are computed as per equations (3.3), (3.4), and (3.5), respectively, and following Roychowdhury (2006). |
| Discretionary Accruals Modified-Jones | Discretionary accruals measured as per equation (3.6) and following Dechow, Sloan, and Sweeney (1995). |

*(Continued)*

| Variable Label | Description |
|---|---|
| Discretionary Accruals Dechow-Dichev | Discretionary accruals measured as per equation (3.7) and following Dechow and Dichev (2002). |
| Restatement | Indicator equal to 1 if firms have misstated their financial statements in fiscal year $t$ and are sanctioned by ex-post restatements, and 0 otherwise. |
| Restatement Up | Indicator equal to 1 if firms have misstated their financial statements upward in fiscal year $t$ and are sanctioned by ex-post restatements, and 0 otherwise. |
| Num. Restatement | Number of financial misstatements that firms have committed during fiscal year $t$ and sanctioned by ex-post restatements. |
| Num. Restatement Up | Number of upward financial misstatements that firms have committed during fiscal year $t$ and sanctioned by ex-post restatements. |
| **Panel B**: Independent test variables | |
| Log Homestead | Natural logarithm of the dollar value of homestead exemptions. Homestead exemptions refer to the equity value of the house which cannot be seized by creditors during the foreclosure following the Chapter 7 filing of an individual. For ease of understanding, the summary statistics report the dollar value of homestead exemptions. |
| Log Non-Homestead | Natural logarithm of the dollar value of non-homestead exemptions. Non-Homestead exemptions refer to the maximum dollar value of personal items that cannot be seized by creditors during the liquidation process following the Chapter 7 filing. This variable refers to the sum of the protected dollar value of motor vehicles, jewelry, tools of the trade including implements and books, and the wild card (special protection that can be applied to any other non-specified personal items). For ease of understanding, the summary statistics report the dollar value of non-homestead exemptions. |
| **Panel C**: Firm and state control variables | |
| Big 4 | Indicator equal to 1 if firm $i$ is audited by a Big Four audit firm, and 0 otherwise (Compustat variable $au$). |
| Book Leverage | Book leverage computed as the ratio between the total book value of debt (Compustat variables $dltt + dlc$) and lagged total assets (Compustat variable $at$). |
| Dividend | Ratio between dividends (Compustat variable $dvc$) and lagged total assets (Compustat variable $at$). |
| Market-to-Book | Ratio between market value of assets (Compustat variables $at + csho{*}prcc_f - ceq$) and book value of assets (Compustat variable $at$). |

*(Continued)*

| Variable Label | Description |
|---|---|
| ROA | Return on assets computed as the ratio between operating income before depreciation (Compustat variable *oibdp*) and lagged total assets (Compustat variable *at*). |
| Size | Natural logarithm of firms' total assets (Compustat variable *at*). |
| Total Accruals | Ratio between total accruals (Compustat variables $ib - (oancf - xidoc)$) and lagged total assets (Compustat variable *at*). |
| GDP Growth (%) | State-level gross domestic product (GDP) growth rate. Data are from the U.S. Bureau of Economic Analysis. |
| Per-capita Income ($) | State-level per-capita disposable income, computed as the total personal income net of personal current taxes. Data are from the U.S. Bureau of Economic Analysis. |
| UI | Unemployment insurance (or unemployment benefits) measured as the natural logarithm of the maximum weekly allowance that an unemployed individual is entitled to receive given her state of residency, multiplied by the maximum number of weeks for which an individual in entitled to receive the benefits. For ease of understanding, the summary statistics report the dollar value of unemployment insurance. Data are from the U.S. Department of Labor. |
| Unemployment Rate (%) | State-level unemployment rate. Data are from the U.S. Bureau of Labor Statistics. |
| **Panel D**: Additional variables | |
| Abnormal Disx. | Abnormal discretionary expenses computed as per equation (3.3) following Roychowdhury (2006), and multiplied by negative one. |
| Abnormal CFO | Abnormal cash flow from operations computed as per equation (3.4) following Roychowdhury (2006), and multiplied by negative one. |
| Abnormal Prod. | Abnormal production costs computed as per equation (3.5) following Roychowdhury (2006). |
| RAM1 | Real activities management computed as the sum of abnormal discretionary expenses multiplied by negative one (*Abnormal Disx.*) and abnormal production costs (*Abnormal Prod.*). Abnormal discretionary expenses and abnormal production costs are computed as per equations (3.3) and (3.5), respectively, and following Roychowdhury (2006). |
| RAM2 | Real activities management computed as the sum of abnormal discretionary expenses and abnormal cash flow from operations, both multiplied by negative one (*Abnormal Disx.* and *Abnormal CFO*, respectively). Abnormal discretionary expenses and abnormal cash flow form operations are computed as per equation (3.3) and (3.4), respectively, and following Roychowdhury (2006). |

# Chapter 4

# Do compliance monitors help restore trust in firms after bribery scandals? Evidence from the cost of capital


## ABSTRACT[1]

We examine whether the compliance monitors appointed in connection with anti-bribery enforcement actions influence firms' cost of capital. We find that firms appointing compliance monitors have a lower cost of equity and debt capital in the post-enforcement period compared to firms with no such obligations. This evidence suggests that capital providers (re)gain trust in firms with compliance obligations, and thus attribute a lower risk to such firms, because monitors reduce firms' agency costs by providing external verification and by supervising the actions that led to noncompliance or represent a risk for future noncompliance. We further find that the market reaction to the anti-bribery enforcement actions is positive for firms that announce the appointment of compliance monitors, while it is negative for firms with no compliance obligations. Additional analyses show that compliance monitors act as substitutes for other trust repair mechanisms, such as dividend increases. Overall, our evidence suggests that compliance monitors help external stakeholders regain trust in the firms involved in wrongdoing.



JEL classification: D73, K42, G38, M41.

Keywords: agency costs, corporate compliance monitor, cost of capital, trust.



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## 4.1 Introduction

Since the early 2000s, the scope of corporate compliance programs has grown substantially (Soltes, 2018). Nevertheless, noncompliance continues, and regulators respond by imposing increasingly more stringent compliance requirements on noncomplying firms (Hamann, 2019). One of the most debated regulatory requirements is the appointment and retention of external compliance monitors (Khanna and Dickinson, 2007; Yockey, 2012).[2] The mandate of these monitors is to investigate why the corporate compliance system has failed, provide recommendations on how to improve its deficiencies, and help firms avoid future wrongdoing (Root, 2014). However, monitors often exert influence outside the scope of their mandate, impose decisions on the board of directors and executive management, and interfere with business operations (Nelson, 2014). Moreover, the costs of monitors' appointment and retention can be prohibitively high for firms (O'Hare, 2008).[3] Therefore, while recent evidence suggests that compliance monitors reduce recidivism in corporate wrongdoing (Gallo, Lynch, and Tomy, 2020), whether they produce net benefits for firms is an empirical question.

To provide evidence on the net benefits—if any—of compliance monitors, we investigate whether their appointment reduces firms' cost of capital. Answering this research question is relevant because it allows to shed new light on the economic role of these monitors. We examine the relation between compliance monitors and cost of capital by exploiting the features of the U.S. anti-bribery regulation, as it provides a powerful setting for two reasons. First, firms' violations of the Foreign Corrupt Practices Act (FCPA) allow the U.S. Department of Justice (DOJ) and the Securities and Exchange Commission (SEC) to mandate that firms appoint and retain compliance monitors in connection with the anti-bribery case settlements.[4] Second, previous studies show that

[2] "[A]s experience with monitorships has grown, it has become increasingly clear that they can sometimes create serious problems for the companies they are intended to help." White Collar – Corporate Monitors: Peace, at What Cost?, Crowell & Moring LLP, January 2018.

[3] "The average cost for organizations that experience non-compliance related problems is nearly $9.4 million." The True Cost of Compliance: A Benchmark Study of Multinational Organizations, Ponemon Institute LLC, January 2011.

[4] As studies use the terms "corruption" and "bribery" interchangeably (e.g., Shleifer and Vishny, 1993; Ades and Di Tella, 1999), we adopt a similar approach.

the appointment and retention of compliance monitors are particularly relevant in the anti-bribery setting (Files, Martin, and Sun, 2018; Gallo, Lynch, and Tomy, 2020), likely because corruption is a serious firm wrongdoing whose deterrence requires strict legal measures (Healy and Serafeim, 2016).

The disclosure of firm wrongdoing uncovers the failure of the corporate monitoring and compliance mechanisms and signals severe agency problems accompanied by high agency costs (e.g., Alexander and Cohen, 1999; Arnold and Lange, 2004; Cumming, Dannhauser, and Johan, 2015; Farber, 2015). Because agency costs consist, to a large extent, of monitoring expenditures (Jensen and Meckling, 1976), compliance monitors can reduce such agency costs by providing external verification. Moreover, as external compliance monitors are usually appointed when trust is damaged by serious corporate law violations (e.g., O'Hare, 2008; Barkow and Barkow, 2011; Ford and Hess, 2011), their role could involve restoring trust by monitoring aspects of corporate behavior that led to noncompliance and other aspects of business activities with significant agency cost exposure and high risk of future noncompliance.[5] As a result, if monitors are effective in reducing agency costs, we expect capital providers to (re)gain trust in firm operations and to reduce their assessment of firm risks, leading to a lower cost of capital.

With specific reference to equity capital, research evidence shows that investors impound firm noncompliance risk into the cost of equity (Corvino and Breugem, 2021). In particular, investors attribute a high risk to the firms involved in bribery scandals and require to be compensated with high returns, thus leading to an increase in the cost of equity relative to the pre-wrongdoing period (e.g., Gray and Kaufmann, 1998; Velikonja, 2013). However, the compliance monitors appointed in connection with anti-bribery case settlements can reduce noncompliance risk and, more broadly, agency costs by overseeing the corporate actions that led to noncompliance or represent an area of risk for future noncompliance (Khanna and Dickinson, 2007; Root, 2014; Haugh, 2018). If monitors

[5] Restoring Trust is the report that Richard C. Breeden, former Chairman of the SEC appointed to act as WorldCom's corporate compliance monitor, presented in August 2003 to the Hon. Jed S. Rakoff (The United States District Court for the Southern District of New York) to discuss the role of trust following firm wrongdoing.

succeed at reducing agency costs, thus helping investors (re)gain trust in firms, we expect the cost of equity to decrease to its level before the specific noncompliance became an issue. This prediction is consistent with prior evidence that firms with better reputation enjoy a lower cost of equity financing (Cao et al., 2015; Dupond and Karpoff, 2019; Karpoff, 2020).

To empirically test this prediction, we first classify the firms that receive enforcement actions for FCPA violations in two groups: Firms that are required to appoint a compliance monitor in connection with the anti-bribery case settlements and firms that do not receive any post-enforcement compliance obligation. Second, following Lee, Myers, and Swaminathan (1999) and Veenman (2013), we compute the implied cost of equity and examine how it changes as a result of the FCPA enforcement actions for firms with compliance monitor obligations and firms with no such obligations. The results are consistent with our prediction: The implied cost of equity decreases by approximately 1.8 percentage points after the anti-bribery enforcement actions for firms required to appoint and retain a compliance monitor, whereas it increases by 0.9 percentage points for firms with no post-enforcement compliance obligations. Overall, these findings suggest that compliance monitors are effective at reducing the agency costs triggered by firm noncompliance.

As the measurement of the implied cost of equity is often criticized for having little predictive power for future realized returns and for relying on strong assumptions (Gebhardt, Lee, and Swaminathan, 2001; Easton and Monahan, 2005), we supplement our main analysis with additional tests. Building on the previous evidence that disclosure of firm wrongdoing leads to negative market reactions (e.g., Palmrose, Richardson, and Scholz, 2004; Karpoff, Lee, and Martin, 2017), we investigate whether the announcement of compliance monitors' appointment mitigates such negative reactions by decreasing future agency risks. Specifically, we examine event-window market returns for anti-bribery enforcement actions and compare the after-announcement stock returns of firms that appoint compliance monitors to the returns of firms without such obligations. The results

support the findings of our cost of equity analysis: Firms that appoint compliance monitors report positive cumulative abnormal returns over a three-day window around the enforcement actions (approximately 1.5%), whereas firms without compliance obligations have negative cumulative abnormal returns around the enforcement actions (approximately -1%). This difference is both economically and statistically significant. Because market reactions likely incorporate direct and indirect costs of using compliance monitors, the results are consistent with net benefits of appointing compliance monitors.[6]

We further examine whether the appointment of compliance monitors has implications for debt financing costs, which may respond to reductions in agency costs and can be unambiguously determined. In particular, we investigate the cost of bank loans. Because banks assess borrowers' creditworthiness and face legal and reputational risks if they are held liable for the corrupt actions of their clients (Graham, Li, and Qiu, 2008; Meloni and Ereira, 2016; Files and Gurun, 2018), they are likely to incorporate the information about the bribery disclosure in their risk assessment, tightening loan contractual terms. However, if firms appoint compliance monitors and their monitoring is expected to be effective, lenders can rely on the information produced by such external monitors and decrease their monitoring efforts and costs relative to the pre-appointment period, leading to less tightened loan terms. This mechanism relies on the cross-monitoring hypothesis (Booth, 1992), and assumes that the degree of monitoring and control that regulators maintain over bribing firms through compliance monitors affects lenders' residual monitoring efforts, and ultimately loan terms (Levmore, 1982).

Nevertheless, as lenders have access to borrowers' private information to perform loan contract monitoring (Bharath et al., 2011; Minnis and Sutherland, 2017), the disclosure of the bribery scandal and the appointment of external compliance monitors may not influence lenders' behavior. This alternative prediction is consistent with lenders using their private information to adjust loan terms already at the time of firm wrongdoing,

[6] Market returns do not only depend on differences in the discount rate (cost of capital), but also on forecasts of cash flows (e.g., Fama, 1990; Hecht and Vuolteenaho, 2006), which likely include the direct and indirect costs of compliance monitors. Therefore, our market reaction analysis can estimate the net benefits of appointing compliance monitors more directly than the cost of equity analysis.

and thus before the monitors' appointment (Chen, 2016).

The results of this analysis show that loan prices (i.e., interest spreads on loans) decrease when firms appoint a compliance monitor in connection with the FCPA case settlement, whereas they increase after firms receive an anti-bribery enforcement action but are not required to appoint compliance monitors. The decrease in loan prices for firms appointing compliance monitors is not compensated by the tightening of non-pricing contractual terms (i.e., loan covenants and loans secured by a collateral), as we find no evidence of tightening of non-pricing contractual terms when firms appoint compliance monitors as part of the anti-bribery case settlements. These findings are consistent with the cross-monitoring hypothesis (Booth, 1992; Black et al., 2004) and suggest that lenders and compliance monitors act as substitutes.

Overall, we provide evidence that compliance monitors mitigate the negative consequences of a bribery scandal disclosure, and can serve as trust agents to help companies repair their reputation. Specifically, by reducing noncompliance risks and supervising other business areas exposed to high agency risks, compliance monitors signal managers' commitment to ethical and value-creating decisions. This commitment contributes to reducing agency costs, and ultimately firm cost of financing. Altogether, although monitors are costly and interfere beyond their mandate with business operations, our results suggest that their appointment and retention are perceived favorably by firms' capital providers.

An interesting follow-up question is whether compliance monitors act as substitutes or complements for other trust repair mechanisms. For example, the agency cost literature predicts and finds that firms use dividend increases to signal to external stakeholders reductions in agency costs, managers' commitment to use firm resources diligently, and a lower likelihood of suboptimal decisions, including costly firm wrongdoing (e.g., Jensen, 1986; La Porta, Shleifer, and Vishny, 2000; Hail, Tahoun, and Clare, 2014). Analyzing dividend policy changes for firms that are required to appoint compliance monitors in connection with anti-bribery case settlements relative to firms that bribe but do not have to appoint compliance monitors, we find evidence consistent with compliance monitors

acting as substitutes for other (costly) mechanisms of trust repair.

This paper makes three contributions to the literature. First, we add to the emerging literature on the consequences of using corporate compliance monitors. While recent research shows that monitors reduce recidivism in firm wrongdoing (Gallo, Lynch, and Tomy, 2020), we provide evidence of broader implications for firms and capital providers. Specifically, we show that compliance monitors contribute to reducing agency risks and firm cost of capital. Second, our work contributes to the literature on the mechanisms that assist firms in restoring trust after fraud or other adverse corporate events (e.g., Farber, 2015), as we interpret the reduction in firm cost of capital following the appointment of compliance monitors as an indicator that firms' capital providers have (re)gained trust in such firms. Third, by investigating the effectiveness of different post-enforcement remedies, this study informs the debate about the consequences of anti-bribery regulation and the deterrent effects of enforcement (e.g., Graham and Stroup, 2016; Karpoff, Lee, and Martin, 2017). Furthermore, because corporate financing allows firms to make long-term investments (e.g., Myers and Majluf, 1984; Fan, Titman, and Twite, 2012), this work ultimately contributes to our knowledge of how regulatory actions can assist or undermine the firms' capability of long-term growth.

The remainder of the paper is organized as follows. Section 4.2 provides an overview of the role of compliance monitors, the institutional background on anti-bribery regulation, and the relation between corruption and capital providers' behavior. Section 4.3 presents the data, the sample, and the empirical methodology. Section 4.4 discusses the empirical results, and Section 4.5 concludes.

## 4.2 Institutional setting and prior literature

### 4.2.1 The role of compliance monitors

The use of compliance monitors to resolve cases of firm wrongdoing has increased significantly over the last two decades (Hamann, 2019). The appointment of compliance monitors is often part of Deferred Prosecution Agreements (DPAs) or Non-Prosecution

Agreements (NPAs). Both DPAs and NPAs are pretrial settlement agreements between the government and the corporation that allow firms to avoid the collateral damage of a criminal prosecution (Gallo, Lynch, and Tomy, 2020). A company agreeing to a DPA or an NPA generally recognizes wrongdoing, accepts to cooperate with the government to resolve the case, pays a fine, and agrees to improve its compliance system and to face prosecution if it does not comply with the terms of the agreement.[7]

The terms of the monitorship are defined by the government in the settlement agreements and are tailored to the specific corporate misconduct (Nelson, 2014). While this lack of a "one-size-fits-all" approach allows regulators to impose firm-specific legal measures, predicting when the government will require that firms appoint a monitor is challenging (Warin, Diamant, and Root, 1999). The monitors' tasks are to investigate why firm wrongdoing occurred, determine what firms can do to prevent recidivism, and provide recommendations (to share with both the corporation and the government) on how to improve the corporate legal and regulatory compliance.[8] Monitors are generally retained for three years, but most of them obtain an extension (Nelson, 2014). Moreover, recent evidence shows that their effects extend beyond the tenure period (Gallo, Lynch, and Tomy, 2020), consistent with companies maintaining frequent interactions and reporting duties with their monitors even after the monitorship has concluded (Root, 2014).

Before providing their recommendations, compliance monitors perform a review of the business activities to learn about the corporation and its culture. However, as monitors are generally lawyers and former prosecutors who do not have technical knowledge of business, they only achieve a limited understanding of the company's operations and internal rules (Khanna and Dickinson, 2007; Nelson, 2014). Given this surface understanding, the company's employees perceive the monitors as inappropriately intrusive, especially when they supervise business operations unrelated to compliance (Yockey, 2012). This perception results in "adversarial relationships" between monitors and employees that

[7] Speech by the Head of the U.S. DOJ's Criminal Division, Assistant Attorney General Lanny Breuer to the New York City Bar Association on 13th September 2012. 2012 Year-End Update on Corporate DPAs and NPAs, Gibson Dunn.

[8] Morford Memorandum, U.S. DOJ, Office of Deputy Attorney General, 7th March 2008.

decrease employees' motivation and productivity, and lead to lower rates of compliance relative to those that would exist without external compliance monitors (Khanna and Dickinson, 2007; Regan Jr., 2007; Root, 2014).

### 4.2.2 Anti-bribery enforcement and compliance obligations

When firms violate the FCPA by bribing foreign government officials to obtain or retain their business, the U.S. DOJ and the SEC can resolve the cases in different ways.[9] Both companies and regulators want to avoid that the FCPA case is taken to trial, because the consequences of a corporation under federal prosecution are detrimental for the corporation, and the costs of prosecution are prohibitively high for prosecutors (Nelson, 2014).[10] Therefore, the most common (pretrial) corporate resolutions are Deferred Prosecution Agreements (DPAs), Non-Prosecution Agreements (NPAs), and administrative proceedings.[11]

DPAs and NPAs, generally issued by the U.S. DOJ, impose that the defendant pays a monetary penalty, recognizes the wrongdoing, agrees to cooperate with the government to resolve the case, and accepts certain compliance and remediation commitments, which often include the appointment and retention of a corporate compliance monitor (Nelson, 2014). Administrative proceedings, issued by the SEC, are non-judicial determinations of fault that entail a variety of legal measures, such as monetary penalties, limitation on or suspension of activities, censure, and revocation of registration. Moreover, in administrative proceedings, the SEC can ask an administrative law judge to require the respondent to cease and desist from any current or future violations of the securities laws (DOJ and SEC, 2020). That FCPA investigations and case settlements involve both the U.S. DOJ and the SEC highlights that corruption is a first-order issue that produces

[9] More information is provided in the FCPA section of the U.S. DOJ and SEC websites.
[10] Less than 1.5% of the FCPA cases are taken to trial, and these are excluded from our analysis.
[11] Plea Agreement is an additional form of FCPA case settlement that generally applies to individual defendants, such as executives or rank-and-file employees (Yockey, 2012). In Plea Agreements, the defendant admits to the facts supporting the charges, admits guilt, and is convicted of the charged crimes when the Plea Agreement is accepted by the court (DOJ and SEC, 2020).

long-lasting side effects and undermines the stability and trust in the economic and socio-political system (e.g., Clinard and Yeager, 1980; Kurer, 1993; Mauro, 1995; Croall, 2001; Simpson, 2011; Simpson, 2013; Zuber, 2015; Healy and Serafeim, 2016).

In addition to monetary penalties and other charges, the U.S. DOJ and the SEC can impose reporting and compliance obligations in connection with FCPA case settlements (usually DPAs and NPAs). Among the factors that contribute to the final decision, the most relevant ones are the nature and seriousness of the offense and the pervasiveness of wrongdoing within the corporation (Krever, 2007). Regulators also evaluate the corporation's history of misconduct and the effectiveness of the pre-existent compliance programs (Yockey, 2013). Finally, to determine the appropriate settlement of the FCPA violations, regulators consider whether corporations have voluntarily disclosed their misconduct, have cooperated with regulators, and have implemented remedial measures timely and appropriately (Scholz, 1984; Yockey, 2012).

Based on the combination of all the elements described above, regulators can impose one of the following compliance and reporting obligations on noncomplying firms. If regulators do not envision the risk of repeat offense and believe that the firms' existing compliance programs are not flawed, they do not impose any post-enforcement compliance obligations. The entity is therefore not required to provide the government with any status reports about its ongoing compliance programs (DOJ and SEC, 2020). In contrast, if regulators believe that firms need to revise their current compliance system or implement new programs to deter future wrongdoing, they impose one of the following compliance and reporting obligations. On the one hand, the entity may be required to provide intermittent reports to the government regarding its efforts to develop new compliance policies and the progress of such implementation. On the other hand, the entity may have to appoint an independent compliance monitor to evaluate the entity's efforts to implement new compliance policies, support the company during this implementation phase, and report the progress and the outcomes of this process to the regulators (Khanna

and Dickinson, 2007; O'Hare, 2008; Nelson, 2014).[12]

Clearly, among the alternative compliance and reporting obligations, appointing and retaining a compliance monitor imposes the highest direct and indirect costs on corporations (Ford and Hess, 2009; Nelson, 2014; Files, Martin, and Sun, 2018). Direct costs include compliance officers' salary and compensation, while indirect costs refer to the possible disruptions to firm operations due to monitors' ongoing supervision and control. Moreover, as regulators must approve the appointment of the monitors and interact with them on a regular basis, this obligation allows authorities to maintain substantial control over noncomplying firms (O'Hare, 2008). In contrast, self-reporting the compliance efforts is less strict than outside monitoring because it provides firms with discretion about the timing and the content of compliance-related disclosures to regulators.

As the purpose of this paper is to investigate the role of compliance monitors, we focus our discussion on the comparison between firms with no reporting and compliance obligations and firms required to appoint an independent compliance monitor. We therefore exclude from our theoretical discussion and empirical analyses the firms with self-reporting obligations.[13] By doing so, we avoid the potential confounding effects of a regulatory outcome that contains mixed features and is subject to substantial discretion. Because firms can disclose their compliance improvements to regulators in several ways, the external stakeholders' reaction to self-reporting obligations is difficult to predict theoretically. Moreover, the high within-group variation due to reporting discretion complicates the empirical comparisons.

### 4.2.3 Firms' corruption and capital providers' behavior

Corruption is among the most detrimental forms of corporate misconduct and carries serious reputational risks in addition to the financial implications of heavy monetary

[12] Alternatively, the entity may be asked to establish a hybrid system, consisting of compliance monitors followed by self-reporting. However, this solution is very rare in practice (less than 10% of the sample of firms receiving post-enforcement obligations) and we do not investigate it in this paper.

[13] The proportion of enforcement actions with self-reporting obligations is similar to that of the enforcement actions with compliance monitor obligations (approximately 25%).

penalties and compliance costs (Sampath, Gardberg, and Rahman, 2018).[14] For example, Walmart's compliance, investigatory, and remediation efforts to resolve the FCPA violations have cost the retailing giant more than $900 million in 2019, without considering the reputational damage.[15] The financial costs and reputation losses related to the FCPA enforcement actions can undermine the firms' future prospects (Kaikati et al., 2000). Furthermore, the disclosure of a bribery scandal reveals that the firms' internal control system is flawed, as it allowed to hide the illicit payments through financial misreporting. Overall, the corporate monitoring and compliance mechanisms prove unsuccessful and signal severe agency problems (Cumming, Dannhauser, and Johan, 2015; Karpoff, 2020).

Following the announcement of a bribery case, external stakeholders expect a decline in corporate performance, question the firms' integrity and reporting quality, and become concerned about possible repeat offense (e.g., Ashford, Wong, and Sternbach, 2008; Karpoff, Lee, and Martin, 2017; Bunkanwanicha and Greusard, 2019). Moreover, the severe agency problems uncovered by the disclosure of corporate wrongdoing impairs trust in firms (Farber, 2015). As a result, investors attribute a higher risk to noncomplying firms and require to be compensated with higher returns, leading to an increase in the cost of equity (e.g., Long and Rao, 1995; Gray and Kaufmann, 1998; Easley and O'Hara, 2004; Lambert, Leuz, and Verrecchia, 2007; Costello and Wittenberg-Moerman, 2011; Karpoff, 2020). In addition, the announcement of corporate wrongdoing leads to negative market reactions, further indicating that investors adjust their investment decisions to account for the news that includes information about firm future risk and cash flows (e.g., Palmrose, Richardson, and Scholz, 2004; Graham, Li, and Qiu, 2008; Hanlon and Slemrod, 2009; Menon and Williams, 2010; Karpoff, Lee, and Martin, 2017).

Similarly, lenders attribute a higher risk to noncomplying firms and set tighter loan contractual terms when firm wrongdoing is disclosed (Costello and Wittenberg-Moerman,

[14] In the UK, the 2014 report of the National Strategic Assessment of Serious and Organized Crime asserts that "the impact of corruption is disproportionate to the level and frequency at which it occurs."

[15] "*Analysis: Walmart's Spend-and-Tell Strategy Paid Off in Bribery Settlement*," The Wall Street Journal, 26th June 2019.

2011; Chen, 2016; Files and Gurun, 2018). This effect is particularly prominent in case of corruption, because lenders can be held liable for the corrupt actions of their clients (Meloni and Ereira, 2016).[16]

While the announcement of an FCPA enforcement action signals severe agency problems and leads to an increase in the cost of capital (e.g., Farber, 2015; Karpoff, 2020), this increase may be fully or partially mitigated for firms with compliance monitors. First, by keeping managers under close supervision (Nelson, 2014), compliance monitors can mitigate the perception of the firm risk and reduce agency costs, thus leading investors to demand lower rates of return that translate into lower cost of equity capital. Second, if compliance monitors act as substitutes for debt capital providers, the cost of debt financing could also decrease, because lenders would rely on the information produced by the monitors and, in turn, reduce their monitoring efforts and costs (Booth, 1992). Therefore, to the extent that compliance monitors are incrementally useful for the type of monitoring that debt providers already exercise, the cost of borrowing is expected to decrease.

However, whether enforcers have the skills to establish proper compliance obligations is highly debated (Langevoort, 2017). Moreover, as monitors often act beyond their mandate and interfere with business operations, they can trigger adversarial attitudes from the board of directors, executive management, and low-level employees (Yockey, 2012; Root, 2014), leading external stakeholders to question monitors' ability to produce net benefits to firms. If equity and debt capital providers consider the anti-bribery enforcement outcomes inadequate or potentially detrimental for corporations, the monitors' appointment should not lower the cost of capital. Furthermore, although the cost of capital decreases when firms appoint compliance monitors, this decrease may not be sufficient to cover the high direct costs of using compliance monitors (O'Hare, 2008). Finally, because capital providers in general and debt providers in particular are corporate monitors themselves, the cost of capital would decrease only if they considered

[16] A report by Norton Rose Fulbright discusses the civil claims against banks arising from bribery and corruption issues (November 2017).

compliance monitors more effective than themselves in monitoring firms and mitigating the agency problems uncovered by the disclosure of wrongdoing. Thus whether the cost of capital substantially decreases as a result of the appointment of monitors as part of the anti-bribery case settlements is an empirical question whose answer can shed new light on the economic role of compliance monitors.

## 4.3 Data and empirical methodology

### 4.3.1 Data and sample

To answer our research question, we collect the enforcement actions released by the U.S. DOJ and the SEC from April 1978 to January 2020 following firms' violations of the FCPA.[17] From these documents, we manually extract the information about the post-enforcement compliance obligations, and classify them into two categories: Compliance monitor obligation and no compliance obligation.[18] Figure 4.1 reports the extract of an enforcement action addressed to a firm required to appoint and retain a compliance monitor.

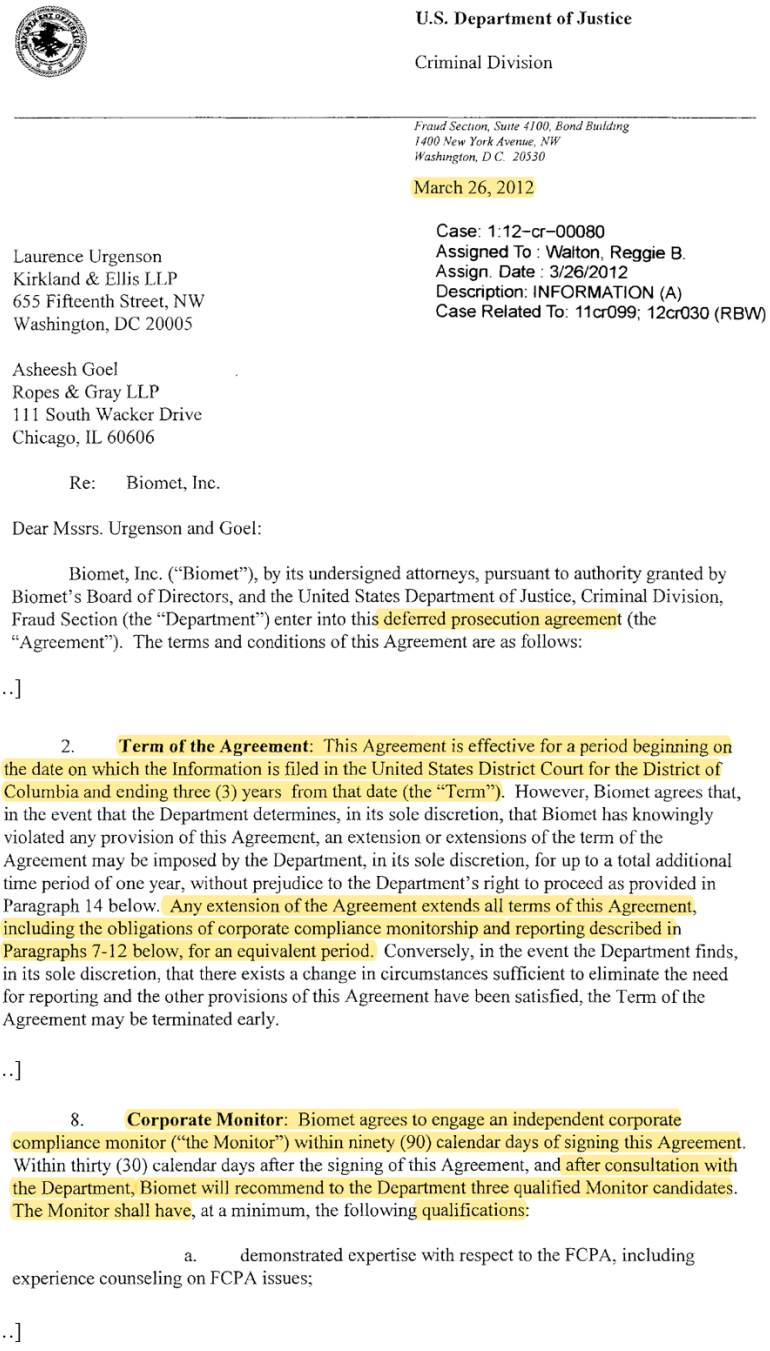
We further collect monthly analysts' earnings consensus forecasts and prices from IBES to compute firms' implied cost of equity capital. Moreover, we collect bank loan data from the Loan Pricing Corporation's (LPC) DealScan database, which includes the details about commercial loans issued to U.S. and foreign corporations. Specifically, DealScan aggregates loan data by deal (also referred to as package), which is a contract between a borrower and lender(s) at a certain date. Each package contains either one facility (the unit of a loan) or multiple facilities. Following prior studies (e.g., Chen, 2016), we consider each facility as a separate unit of loan observation because loan spreads and other loan characteristics differ across facilities.

[17] The enforcement actions are accessible through the U.S. DOJ and the SEC websites.

[18] In addition to compliance monitors, the entity may receive a self-reporting obligation or may be asked to establish a hybrid system—consisting of a compliance monitor followed by a self-reporting period. However, as we are interested in the role of compliance monitors, we exclude the self-reporting and the hybrid post-enforcement obligations from our analyses.

FIGURE 4.1: Anti-bribery enforcement actions and compliance monitors

This figure reports an extract of the enforcement action released against Biomet, Inc., which was required to hire an outside compliance monitor as part of the anti-bribery case settlement. The full text of the enforcement action is available on the U.S. DOJ website.



**U.S. Department of Justice**

Criminal Division

*Fraud Section, Suite 4100, Bond Building*
*1400 New York Avenue, NW*
*Washington, D.C. 20530*

March 26, 2012

Case: 1:12-cr-00080
Assigned To : Walton, Reggie B.
Assign. Date : 3/26/2012
Description: INFORMATION (A)
Case Related To: 11cr099; 12cr030 (RBW)

Laurence Urgenson
Kirkland & Ellis LLP
655 Fifteenth Street, NW
Washington, DC 20005

Asheesh Goel
Ropes & Gray LLP
111 South Wacker Drive
Chicago, IL 60606

Re: Biomet, Inc.

Dear Mssrs. Urgenson and Goel:

Biomet, Inc. ("Biomet"), by its undersigned attorneys, pursuant to authority granted by Biomet's Board of Directors, and the United States Department of Justice, Criminal Division, Fraud Section (the "Department") enter into this deferred prosecution agreement (the "Agreement"). The terms and conditions of this Agreement are as follows:

[…]

2. **Term of the Agreement**: This Agreement is effective for a period beginning on the date on which the Information is filed in the United States District Court for the District of Columbia and ending three (3) years from that date (the "Term"). However, Biomet agrees that, in the event that the Department determines, in its sole discretion, that Biomet has knowingly violated any provision of this Agreement, an extension or extensions of the term of the Agreement may be imposed by the Department, in its sole discretion, for up to a total additional time period of one year, without prejudice to the Department's right to proceed as provided in Paragraph 14 below. Any extension of the Agreement extends all terms of this Agreement, including the obligations of corporate compliance monitorship and reporting described in Paragraphs 7-12 below, for an equivalent period. Conversely, in the event the Department finds, in its sole discretion, that there exists a change in circumstances sufficient to eliminate the need for reporting and the other provisions of this Agreement have been satisfied, the Term of the Agreement may be terminated early.

[…]

8. **Corporate Monitor**: Biomet agrees to engage an independent corporate compliance monitor ("the Monitor") within ninety (90) calendar days of signing this Agreement. Within thirty (30) calendar days after the signing of this Agreement, and after consultation with the Department, Biomet will recommend to the Department three qualified Monitor candidates. The Monitor shall have, at a minimum, the following qualifications:

a. demonstrated expertise with respect to the FCPA, including experience counseling on FCPA issues;

[…]

In Table 4.1, column 1, we present the sample selection procedure to construct the cost of equity sample, whereas in column 2, we present the sample selection procedure to construct the cost of debt sample. Column 1 in Panel A of Table 4.1 shows that between

TABLE 4.1: Sample selection

| **Panel A: Anti-bribery enforcement actions** | (1) Cost of equity sample | (2) Cost of debt sample |
|---|---|---|
| Enforcement actions between April 1978 and January 2020 with available firm identifier | 452 | 452 |
| - Enforcement actions to firms without IBES earnings forecasts or prices | (157) | |
| - Enforcement actions with IBES earnings forecasts or prices outside the period 2006-2016 | (125) | |
| - Enforcement actions to firms without DealScan loan facility initiations | | (34) |
| - Enforcement actions with DealScan loan initiations outside the period 2006-2016 | | (179) |
| = Final sample of anti-bribery enforcement actions | 170 | 239 |
| *Unique bribing firms* | *79* | *105* |
| **Panel B: Firm-level data** | | |
| IBES earnings forecasts and prices for *79 unique bribing firms* between 2006 and 2016 | 535 | |
| - Observations with missing data for control variable construction | (149) | |
| DealScan loan facilities initiated by *105 unique bribing firms* between 2006 and 2016 | | 777 |
| - Observations with missing data for control variable construction | | (300) |
| = Final sample with non-missing controls | 386 | 477 |
| *Unique bribing firms* | *59* | *68* |

April 1978 and January 2020, the U.S. DOJ and the SEC have issued 452 enforcement actions addressed to firms with available Compustat identifiers (i.e., *gvkey*). Because we need analysts' earnings forecasts and prices to compute the implied cost of equity, we exclude from this sample 157 enforcement actions addressed to firms with no analysts' earnings consensus forecasts and prices in IBES. We further remove 125 enforcement actions with earnings consensus forecasts or prices outside the time period 2006-2016. We exclude the pre-2006 years because regulators have imposed compliance and reporting obligations in connection with the FCPA enforcement actions since mid-2000s. Similarly, we exclude the post-2016 years to have sufficient post-enforcement observations for our empirical analyses.

This sample selection procedure leads to 170 enforcement actions addressed to 79 unique bribing firms. Therefore, each bribing firm receives, on average, 2.2 enforcement actions.[19] Column 1 in Panel B of Table 4.1 shows that these 79 unique bribing firms are associated with 535 monthly analysts' earnings forecasts and prices between 2006 and

[19] Enforcement actions can be addressed to companies and/or their executives. When both companies and executives receive enforcement actions around the same dates, the enforcement actions are grouped as a single FCPA enforcement matter. The same applies when a single bribery case leads to multiple releases around the same dates (e.g., separate U.S. DOJ and SEC enforcement actions). More details are available in the "Definitions" section of the FCPA Clearinghouse website.

TABLE 4.2: Sample distribution by year

This table reports the sample split by year. Details on definition, source, and computation of the variables can be found in Appendix 4.A.

| | (1) Cost of equity sample | | | | (2) Cost of debt sample | | | |
|---|---|---|---|---|---|---|---|---|
| Year | Obs. | % of sample | Monitor | % by year | Obs. | % of sample | Monitor | % by year |
| 2006 | 66 | 12.34% | 17 | 25.76% | 109 | 14.03% | 22 | 20.18% |
| 2007 | 63 | 11.78% | 15 | 23.81% | 122 | 15.70% | 43 | 35.25% |
| 2008 | 62 | 11.59% | 14 | 22.58% | 70 | 9.01% | 13 | 18.57% |
| 2009 | 46 | 8.60% | 11 | 23.91% | 24 | 3.09% | 8 | 33.33% |
| 2010 | 53 | 9.91% | 14 | 26.42% | 79 | 9.79% | 20 | 25.32% |
| 2011 | 43 | 8.04% | 11 | 25.58% | 83 | 10.17% | 17 | 20.48% |
| 2012 | 46 | 8.60% | 11 | 23.91% | 65 | 8.37% | 10 | 15.38% |
| 2013 | 40 | 7.48% | 10 | 25.00% | 57 | 7.34% | 18 | 31.58% |
| 2014 | 40 | 7.48% | 10 | 25.00% | 67 | 8.62% | 12 | 17.91% |
| 2015 | 41 | 7.66% | 10 | 24.39% | 61 | 7.85% | 9 | 14.75% |
| 2016 | 35 | 6.54% | 11 | 31.43% | 40 | 5.15% | 7 | 17.50% |
| Total | 535 | 100.00 % | 134 | 25.05 % | 777 | 100.00% | 179 | 23.04% |

2016. After removing all the observations with missing data for variable construction, we obtain 386 analysts' earnings forecasts and prices for 59 unique bribing firms.

We apply a similar sample selection procedure to construct the cost of debt sample. As shown in column 2, Panel A of Table 4.1, we remove 34 enforcement actions from the initial sample because these are not associated with any DealScan loan initiations. We further exclude 179 enforcement actions because they are associated with firms with loans initiated outside the time period 2006-2016. This procedure leads to a final sample of 239 enforcement actions and 105 unique bribing firms. As shown in column 2, Panel B of Table 4.1, these 105 unique bribing firms correspond to 777 loan-facility initiations between 2006 and 2016. After removing all the observations with missing data for variable construction, we obtain 477 loan facilities initiated by 68 unique bribing firms over the sample period 2006-2016.

Table 4.2 displays the sample distribution over time. Section 1 reports the observations of the cost of equity sample, whereas section 2 reports those of the cost of debt sample. Overall, the firms are homogeneously distributed in both samples over the period 2006-2016, although the years 2012-2016 show a reduction in the total number of observations. With reference to the cost of equity sample, the compliance monitors are

TABLE 4.3: Sample distribution by industry

This table reports the sample split by industry, based on one-digit SIC codes. Details on definition, source, and computation of the variables can be found in Appendix 4.A.

| | (1) Cost of equity sample | | | | (2) Cost of debt sample | | | |
|---|---|---|---|---|---|---|---|---|
| Industry | Obs. | % of sample | Monitor | % by industry | Obs. | % of sample | Monitor | % by industry |
| Agriculture, forestry and fishing | 19 | 3.55% | 11 | 57.89% | 11 | 1.42% | 3 | 27.27% |
| Mining and construction | 31 | 5.79% | 13 | 41.94% | 36 | 4.63% | 11 | 30.56% |
| Manufacturing | 322 | 60.19% | 66 | 20.50% | 406 | 52.25% | 74 | 18.23% |
| Transportation and communications | 34 | 6.36% | 17 | 50.00% | 101 | 13.00% | 23 | 22.77% |
| Wholesale and retail trade | 18 | 3.36% | 11 | 61.11% | 27 | 3.47% | 15 | 55.56% |
| Finance, insurance and real estate | 40 | 7.48% | 0 | 0.00% | 96 | 12.36% | 0 | 0.00% |
| Services | 66 | 12.34% | 11 | 16.67% | 95 | 12.22% | 48 | 50.53% |
| Public administration and other | 5 | 0.93% | 5 | 100.00% | 5 | 0.64% | 5 | 100.00% |
| Total | 535 | 100.00 % | 134 | 25.05% | 777 | 100.00% | 179 | 23.04% |

appointed in approximately 25% of the cases, peaking in 2016, when 11 out of 35 firm year observations (approximately 31%) are associated with monitors' appointment. With reference to the cost of debt sample, the monitors are present, on average, in 23% of the sample, ranging from 15% in 2015 to more than 35% in 2007.

Table 4.3 reports the sample distribution by industry.[20] Section 1 focuses on the cost of equity sample, whereas section 2 focuses on the cost of debt sample. In the cost of equity sample, the industries with the highest frequency of compliance monitors are agriculture, transportation, and wholesale and retail trade, whereas no firms in the finance, insurance, and real estate sector have received enforcement actions with compliance monitor obligations. In the cost of debt sample, the industries of wholesale and retail trade and services have the highest frequency of compliance monitors, whereas no firms in the finance sector have received anti-bribery enforcement actions.[21]

### 4.3.2 Empirical methodology

To perform our analysis, we exploit the staggered appointments of compliance monitors in connection with anti-bribery case settlements. Our primary empirical test examines

[20] In the descriptive analysis of Table 4.3, we aggregate the industry observations by one-digit SIC code for ease of presentation, although in the empirical analyses we include two-digit SIC codes as industry fixed effects.

[21] In unreported tests, we examine the robustness of all our findings by excluding firms in the financial industry, and find similar results, both economically and statistically.

how the implied cost of equity changes as a result of the FCPA enforcement actions for firms with compliance monitor obligations and firms with no such obligations. We estimate this effect using the following firm-level linear model,

$$\begin{aligned} \textit{Implied cost of capital}_{i,t} = \alpha + \beta_1 \textit{Post enforcement}_t + \beta_2 \textit{Compliance monitor}_i + \\ + \beta_3 \textit{Post enforcement}_t \times \textit{Compliance monitor}_i + \\ + \gamma \boldsymbol{Z}_{i,t} + \delta \boldsymbol{X}_i + c_j + \epsilon_{i,t} \end{aligned} \tag{4.1}$$

The dependent variable (*Implied cost of capital*) proxies for the cost of equity capital. Following Lee, Myers, and Swaminathan (1999) and Veenman (2013), we compute *Implied cost of capital* using the Residual Income Valuation model with a three-year forecast horizon as presented below,

$$V_0 = bv_0 + \frac{ri_1}{(1+r_e)} + \frac{ri_2}{(1+r_e)^2} + \frac{ri_3}{(1+r_e)^3} + \frac{ri_3(1+g)}{(r_e-g)(1+r_e)^3} \tag{4.2}$$

where $bv_0$ is the current book value of equity per share, $r_e$ is the cost of equity capital, $ri_t$ is the residual income in period $t$ (computed as the difference between expected earnings per share (EPS) in period $t$ and $r_e$ times the beginning book value of equity per share $bv_{t\text{-}1}$), and $g$ is the assumed rate of growth in residual income beyond the three-year forecast horizon (i.e., 1%). The implied cost of equity capital is the rate of $r_e$ that results in the valuation ($V_0$) being equal to the current market price ($P_0$). In our analyses, we label $r_e$ as *Implied cost of capital*.

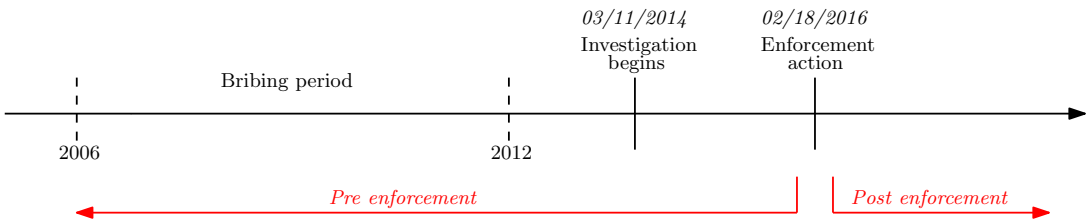
The independent test variables are *Post enforcement*, which is an indicator equal to 1 if prices and analysts' earnings consensus forecasts used to compute the implied cost of equity are issued after the anti-bribery enforcement actions, and 0 otherwise,[22] and *Compliance monitor*, which is an indicator equal to 1 for firms required to appoint a compliance monitor as part of the anti-bribery case settlement, and 0 for firms with no

[22] The *Post enforcement* indicator variable is based on the first FCPA enforcement action that a firm receives.

post-enforcement compliance and reporting obligations.[23] The variable *Post enforcement* $\times$ *Compliance monitor* is the interaction term between the time variable and the variable indicating firms with compliance monitor obligations. It measures whether the cost of equity changes as a result of the FCPA enforcement actions for firms with compliance monitor obligations and firms with no post-enforcement compliance obligations. Figure 4.2 reports the time frame of the anti-bribery enforcement process, and describes how we construct the time variable *Post enforcement* for a sample firm.

FIGURE 4.2: Anti-bribery enforcement process

This figure reports the enforcement process of a sample firm: VimpelCom Ltd. The information is collected from the enforcement action released by the SEC on 18th February 2016. VimpelCom Ltd. is a corporation headquartered in Amsterdam (Netherlands) but organized under the laws of Bermuda. The Dutch-based company issues and maintains a class of publicly traded securities registered pursuant to Section 12(b) of the Security and Exchange Act of 1934. Such securities were traded on the New York Stock Exchange until 2013, and on the NASDAQ after 2013. From 2006 to at least 2012, VimpelCom offered and paid bribes to a government official in Uzbekistan in connection with its Uzbek operations. During the course of the bribery scheme, VimpelCom made or caused to be made at least $114 million in improper payments in order to obtain and retain business that generated more than $2.5 billion in revenues for VimpelCom. The investigation date is provided by the FCPA Clearinghouse and is the earliest of: The latest date on which any defendant to an enforcement action is formally charged with FCPA-related offenses, the investigation closure/declination date, or disclosure of the investigation closure/declination. *Post enforcement* captures prices and analysts' earnings forecasts (or loans) issued in the period after VimpelCom Ltd. receives the anti-bribery enforcement action, whereas *Pre enforcement* captures prices and analysts' earnings forecasts (or loans) issued in the period before the enforcement action.




The model in equation (4.1) also includes time-varying control variables ($\boldsymbol{Z}$), time-invariant control variables ($\boldsymbol{X}$), and industry fixed effects (based on the two-digit SIC

[23] When firms receive more than one enforcement action, and the first requires no compliance obligation, whereas the subsequent actions have at least one compliance monitor obligation, we end the post-enforcement period before the release of the enforcement action(s) with monitor obligations. In this way, we avoid the overlap of time periods characterized by different compliance obligations. We do not impose any further time limits to the post-enforcement period because Gallo, Lynch, and Tomy (2020) show that the effects of compliance monitors extend beyond their appointment period, which ranges between 3 and 5 years. However, in our samples, the post-enforcement period is, on average, 5 years.

classification, $c_j$). We select the time-varying control variables from prior cost of capital literature. In particular, we control for *Buy-and-hold return*, the contemporaneous 12-month buy-and-hold return of firm $i$ in fiscal year $t$, because the return performance influences the investors' assessment of firm risk (Kedia, Koh, and Rajgopal, 2015). We further include *Leverage*, the ratio between total liabilities and lagged total assets, because highly levered firms are more risky (Gebhardt, Lee, and Swaminathan, 2001). We control for firm future growth, performance, and size, as they all affect the perception of the company's risk, and ultimately the cost of equity (Cao et al., 2015). Specifically, we include *Market-to-book*, which is the ratio between market value of assets and book value of assets; *Cash flow*, which is the ratio between cash flow from operations and lagged total assets; *ROA*, which is the ratio between income before extraordinary items and lagged total assets, and *Size*, which is the natural logarithm of firm total assets.

We further control for the quality of firm internal control systems and the existence of ongoing lawsuits, because internal control weaknesses and lawsuits reduce the credibility of corporate financial reporting and increase the firm risk (Beneish, Billings, and Hodder, 2008). Specifically, *Internal control weakness* is an indicator equal to 1 if, according to the assessment of disclosure controls under SOX 302, the internal control system is found to have material weaknesses, and 0 otherwise, whereas *Lawsuit* is an indicator equal to 1 if firms have an ongoing lawsuit in fiscal year $t$, and 0 otherwise. Finally, we include *Crisis*, an indicator equal to 1 for observations in fiscal years 2007, 2008, and 2009, and 0 otherwise, to account for the potential effect of the financial crisis on investors' behavior.

We also control for time-invariant enforcement-specific characteristics that proxy for the severity of the bribery scandals and are likely to influence the investors' assessment of firm risk of recidivism and seriousness of the agency problems (Kaufmann, 1997). Specifically, *Bribing years* measures the number of years during which a firm has bribed a foreign official to gain business or other benefits; *DOJ&SEC involved* is an indicator equal to 1 if both the U.S. DOJ and the SEC are involved in the FCPA case settlement,

and 0 otherwise;[24] *Mitigate* is an indicator equal to 1 if the case settlement is mitigated by at least one mitigating factor, and 0 otherwise;[25] and *Penalty* measures the natural logarithm of the sanctions imposed on bribing firms. A detailed description of all the variables is reported in Appendix 4.A.

As the appointment of compliance monitors is not random, we also use a within-firm design and study how the cost of equity capital changes over time for each firm. This design allows us to control for unobservable time-invariant factors that affect the investors' behavior. A further reason why we use a fixed effects model is that Gallo, Lynch, and Tomy (2020) show that the factors driving regulators' requirement to appoint a compliance monitor are time-invariant.[26] The fixed effects design is as follows,

$$\begin{aligned} \textit{Implied cost of capital}_{i,t} = \alpha + \beta \textit{Post enforcement}_t \times \textit{Compliance monitor}_i + \\ + \gamma \boldsymbol{Z}_{i,t} + b_t + a_i + \epsilon_{i,t} \end{aligned} \tag{4.3}$$

The dependent variable is *Implied cost of capital*, as in equation (4.1), whereas the only independent test variable is the interaction term *Post enforcement* $\times$ *Compliance monitor*, because the effect of *Post enforcement* and *Compliance monitor* is subsumed by year and firm fixed effects.[27] Year fixed effects ($b_t$) control for aggregate shocks and common trends in regulatory enforcement activities, whereas firm fixed effects ($a_i$) account for time-invariant firm characteristics. The model in equation (4.3) also includes the time-varying control variables reported in equation (4.1). All regressions include robust standard errors clustered at the firm-level, and continuous variables are winsorized at the 1% and 99% level, respectively.

[24] Because the main anti-bribery case settlements are DPAs, NPAs (both usually issued by the U.S. DOJ), and administrative proceedings (issued by the SEC), *DOJ&SEC involved* also controls for the type of anti-bribery resolution.

[25] Mitigating factors include voluntary reporting of the misconduct, cooperation with regulators, voluntary implementation of remedial measures, engagement in the misconduct only from low-level employees, or other measures.

[26] Several factors, such as distance to regulators, innate risks of operations, type of business, and firm propensity to misbehave, that prior literature uses to explain enforcement actions are predicted to remain relatively stable over time (e.g., Dechow et al., 2011; Kedia and Rajgopal, 2011).

[27] Some specifications, detailed below, include instead month-year and firm fixed effects.

To test the effects of the compliance monitors' appointment on the cost of debt financing, we examine how the cost of debt changes as a result of the FCPA enforcement actions for firms with compliance monitor obligations and firms with no such obligations. To estimate these effects, we use the following firm-level linear model,

$$\begin{aligned} \textit{Log spread}_{i,t} = \alpha + \beta_1 \textit{Post enforcement}_t + \beta_2 \textit{Compliance monitor}_i + \\ + \beta_3 \textit{Post enforcement}_t \times \textit{Compliance monitor}_i + \\ + \gamma \boldsymbol{Z}_{i,t} + \delta \boldsymbol{X}_i + c_j + p_k + t_p + \epsilon_{i,t} \end{aligned} \tag{4.4}$$

The dependent variable is *Log spread* and measures the natural logarithm of the loan facility spread, computed as the all-in-spread drawn in basis points over LIBOR. The independent test variables are as follows. *Post enforcement* is an indicator equal to 1 for loans initiated after firms receive the anti-bribery enforcement actions, and 0 otherwise; *Compliance monitor* is an indicator equal to 1 if firms are required to appoint and retain a compliance monitor as part of an anti-bribery case settlement, and 0 if no obligation is imposed. *Post enforcement* $\times$*Compliance monitor* is the interaction term between the time variable and the variable indicating the compliance monitor obligation, and measures whether the cost of bank loans changes after the anti-bribery enforcement actions for firms with compliance monitor obligations and firms with no post-enforcement compliance obligations.

The model in equation (4.4) includes the time-varying control variables reported in equation (4.1) and additional control variables capturing loan-specific and credit market characteristics. Loan characteristics are *Log facility amount*, which is the natural logarithm of the loan facility amount (in millions USD), and *Log facility maturity*, which is the natural logarithm of the loan facility maturity (in months). Credit-market characteristics are *Credit spread*, which is the difference between the yields of BAA- and AAA-rated corporate bonds obtained from the Federal Reserve Board of Governors, and *Rated*, which is an indicator equal to 1 if a firm's debt securities are rated, and 0 otherwise. In addition to industry fixed effects ($c_j$), the regression model reported in equation (4.4) includes

deal purpose ($p_k$) and facility-type ($t_p$) fixed effects.

Consistent with the cost of equity analysis, we also test our prediction about the effect of compliance monitors on the cost of debt by using a fixed effects model as reported below,

$$\begin{aligned} \textit{Log spread}_{i,t} = \alpha + \beta \textit{Post enforcement}_t \times \textit{Compliance monitor}_i + \\ + \gamma \boldsymbol{Z}_{i,t} + b_t + a_i + \epsilon_{i,t} \end{aligned} \tag{4.5}$$

The dependent variable is the same as per equation (4.4) and the independent test variable is the interaction term *Post enforcement* ×*Compliance monitor*. The model also includes year and firm fixed effects ($b_t$ and $a_i$, respectively), and the time-varying control variables included in equation (4.4).

### 4.3.3 Sample overview and descriptive statistics

Table 4.4 provides the descriptive statistics of the main variables. The sample is split between firms with post-enforcement compliance monitor obligations in section 1 (Compliance monitor), and firms without any obligation to inform regulators about their compliance system in section 2 (No compliance obligation).

Regarding the main dependent variables (Panel A), the implied cost of equity (*Implied cost of capital*) is approximately 8% for both firms with compliance obligations and firms without compliance monitor obligations, whereas the cost of bank debt (*Spread*) is approximately 207 basis points in both samples. The other dependent variables, reported in Panel A and used in additional analyses, show that the abnormal returns around the enforcement action release are positive for firms that appoint compliance monitors (approximately 2%) and negative for firms with no compliance obligations (approximately

TABLE 4.4: Summary statistics

This table reports the summary statistics of the main variables of interest by type of compliance obligation (i.e., compliance monitor obligation and no compliance obligation, respectively). Panel A shows the summary statistics of the main dependent variables. Panel B reports the statistics of firm characteristics, Panel C reports the summary statistics of enforcement action characteristics, and Panel D shows the summary statistics of the loan and credit market characteristics. Details on definition, source, and computation of the variables can be found in Appendix 4.A.

| | (1) Compliance monitor | | | (2) No compliance obligation | | |
|---|---|---|---|---|---|---|
| | Mean | Median | S.D. | Mean | Median | S.D. |
| **Panel A: Dependent variables** | | | | | | |
| Implied cost of capital | 0.08 | 0.08 | 0.03 | 0.08 | 0.08 | 0.03 |
| Spread (bps) | 206.65 | 185.00 | 136.16 | 207.30 | 175.00 | 165.96 |
| CAR market model | 0.02 | 0.01 | 0.04 | -0.00 | 0.00 | 0.03 |
| CAR FF3 model | 0.02 | 0.01 | 0.04 | -0.01 | 0.00 | 0.03 |
| Covenant | 0.67 | 0.00 | 0.97 | 0.75 | 0.00 | 0.99 |
| Secured | 0.40 | 0.00 | 0.49 | 0.42 | 0.00 | 0.49 |
| Dividend increase | 0.57 | 1.00 | 0.50 | 0.50 | 0.00 | 0.50 |
| **Panel B: Firm characteristics** | | | | | | |
| Buy-and-hold return | 0.31 | 0.22 | 0.66 | 0.12 | 0.09 | 0.47 |
| Cash flow | 0.05 | 0.03 | 0.08 | 0.04 | 0.03 | 0.05 |
| Crisis | 0.35 | 0.00 | 0.48 | 0.23 | 0.00 | 0.42 |
| Internal control weakness | 0.01 | 0.00 | 0.09 | 0.03 | 0.00 | 0.16 |
| Lawsuit | 0.11 | 0.00 | 0.32 | 0.07 | 0.00 | 0.25 |
| Leverage | 0.62 | 0.61 | 0.16 | 0.66 | 0.66 | 0.20 |
| Market-to-book | 2.37 | 1.99 | 1.32 | 1.59 | 1.39 | 0.68 |
| ROA | 0.02 | 0.01 | 0.02 | 0.01 | 0.01 | 0.02 |
| Size | 9.47 | 9.66 | 1.23 | 9.81 | 9.49 | 2.02 |
| **Panel C: Enforcement action characteristics** | | | | | | |
| Bribing years | 5.85 | 5.00 | 3.61 | 3.51 | 3.00 | 3.76 |
| DOJ&SEC involved | 0.06 | 0.00 | 0.24 | 0.62 | 1.00 | 0.49 |
| Mitigate | 0.92 | 1.00 | 0.27 | 0.70 | 1.00 | 0.46 |
| Penalty | 3.20 | 3.52 | 2.01 | 0.61 | -0.30 | 2.71 |
| **Panel D: Loan and credit market characteristics** | | | | | | |
| Facility amount ($ mln) | 1,144.26 | 551.93 | 1,549.98 | 1,306.28 | 400.00 | 2,815.19 |
| Facility maturity (months) | 58.93 | 60.00 | 22.66 | 49.01 | 60.00 | 22.05 |
| Credit spread | 1.05 | 0.93 | 0.40 | 1.13 | 0.95 | 0.52 |
| Rated | 0.90 | 1.00 | 0.30 | 0.84 | 1.00 | 0.37 |

-1%).[28] Moreover, the number of loan covenants and the frequency of loans secured by a

[28] Karpoff, Lee, and Martin (2017) perform an event study analysis around bribery disclosures and show that the cumulative abnormal return is -1.72% for bribing firms without additional fraud charges, and -3.07% for the whole sample of bribing firms. Their results differ from ours for the following reasons. First, we compute the market reaction to the enforcement action release, whereas Karpoff, Lee, and Martin (2017) measure the stock returns upon the initial news that the firm engaged in bribery. Second, Karpoff, Lee, and Martin (2017) compute compound cumulative abnormal returns to account for all discrete and incremental announcements that pertain to each enforcement action, whereas we measure the market reaction only to the enforcement action release, as we are interested in the announcement of the corporate monitors' appointment.

collateral are 0.67 and 0.40, respectively, for firms with compliance monitors, whereas 0.75 and 0.42, respectively, for firms with no such compliance obligations. Finally, dividend increases occur in 57% of the cases for firms with compliance monitors, whereas in 50% of the cases for firms with no compliance obligations.

With regards to firm characteristics (Panel B), firms with compliance monitors have higher buy-and-hold returns, cash flow, market-to-book ratio, lawsuits, and ROA than firms with no compliance obligations. With reference to the enforcement action characteristics (Panel C), only 6% of the sample of firms required to appoint a compliance monitor involve both the U.S. DOJ and the SEC, whereas in the sample of firms with no post-enforcement obligations, both regulators are involved in 62% of the cases. Moreover, the mitigating factors are included in the pretrial settlement of 92% of the bribery cases requiring the appointment of a compliance monitor, and 70% of the cases in which the enforcement does not require any post-enforcement obligation. This descriptive evidence suggests that mitigating factors do not exempt firms from receiving compliance monitor obligations. In addition, firms with compliance monitor obligations bribe for a higher number of years and receive higher sanctions, suggesting that the most severe bribery cases lead to post-enforcement compliance monitor obligations. Finally, Panel D shows that loans initiated by firms with post-enforcement obligations have lower (higher) amounts (maturity) than those of firms without compliance requirements, whereas the credit market characteristics highlight similar credit spreads and proportion of rated debt securities in the two samples.

Although the evidence in Panel A of Table 4.4 provides a preliminary overview of the descriptive statistics of our main dependent variables, Table 4.5 provides more details about whether and, if so, how the cost of equity and debt capital differs before and after the anti-bribery enforcement actions for firms with compliance monitor obligations and firms with no such obligations.

Column 1 in Panel A of Table 4.5 shows that in the pre-enforcement period the implied cost of equity capital is, on average, higher for firms that will appoint a compliance monitor after the enforcement actions compared to firms with no compliance obligations

TABLE 4.5: Cost of capital, market reaction, and dividend payments before and after anti-bribery enforcement actions

This table reports the t-tests of the difference in the means of the main dependent variables by type of compliance obligation (i.e., compliance monitor obligation and no compliance obligation, respectively) and time with respect to the enforcement action (i.e., pre versus post enforcement). The two main outcome variables in Panel A are *Implied cost of capital*, which proxies for the cost of equity capital and is computed based on the Residual Income Valuation model following Lee, Myers, and Swaminathan (1999) and Veenman (2013), and *Spread*, which is the loan facility spread, measured by the all-in-spread drawn in basis points over LIBOR. The other dependent variables in Panel B are as follows: *CAR market model* measures the cumulative abnormal returns obtained using the CAPM over the three-day window surrounding each anti-bribery enforcement action; *CAR FF3 model* measures the cumulative abnormal returns obtained following Fama and French (1992) 3-factor model over the three-day window surrounding each anti-bribery enforcement action; *Covenant* measures the number of covenants associated with loan initiations; *Secured* is an indicator equal to 1 if loan initiations are secured by a collateral, and 0 otherwise; and *Dividend increase* is an indicator equal to 1 when firm $i$ initiates or increases its dividends in fiscal year $t$, and 0 otherwise. Details on definition, source, and computation of the variables can be found in Appendix 4.A.

| | **Panel A** Main dependent variables | | **Panel B** Other dependent variables | | | | |
|---|---|---|---|---|---|---|---|
| | (1) Implied cost of capital | (2) Spread | (3) CAR market model | (4) CAR FF3 model | (5) Covenant | (6) Secured | (7) Dividend increase |
| **Pre enforcement** | | | | | | | |
| Compliance monitor (T) | 0.089 | 219.608 | | | 0.724 | 0.463 | 0.603 |
| No compliance obligation (C) | 0.077 | 198.327 | | | 0.645 | 0.445 | 0.433 |
| *Diff. (T-C)* | *0.012**** | *21.282* | | | *0.079* | *0.018* | *0.170**** |
| **Post enforcement** | | | | | | | |
| Compliance monitor (T) | 0.078 | 168.067 | 0.018 | 0.015 | 0.511 | 0.222 | 0.532 |
| No compliance obligation (C) | 0.086 | 219.617 | -0.003 | -0.006 | 0.897 | 0.397 | 0.575 |
| *Diff. (T-C)* | *-0.009** | *-51.550*** | *0.021*** | *0.021** | *-0.386**** | *-0.175*** | *-0.043* |

(i.e., 8.9% vis-à-vis 7.7%, respectively). The evidence is reversed in the post-enforcement period: The firms with compliance monitors have a lower cost of equity capital, on average, compared to firms with no compliance obligations (i.e., 7.8% vis-à-vis 8.6%, respectively).

With regards to the cost of debt, the evidence in column 2 shows that loan spreads are higher for firms that subsequently receive a compliance monitor obligation compared to firms without such obligations (i.e., 219.61 bps vis-à-vis 198.33 bps, respectively). In contrast, in the post-enforcement period, firms with compliance monitors have lower loan spreads than firms without compliance obligations (i.e., 168.07 bps vis-à-vis 219.62 bps, respectively), and this difference is statistically significant at the 5% level.

Altogether, the preliminary evidence of Panel A, Table 4.5 suggests that firms that ex-post are required to appoint and retain a compliance monitor are ex-ante perceived as more risky by equity and debt capital providers, while the appointment of compliance monitors mitigates such risk perception, as the cost of capital decreases post-enforcement actions for firms with compliance monitor obligations. In contrast, firms with no compliance monitor obligations are perceived as more risky after the anti-bribery enforcement actions, as their cost of capital increases in the post-enforcement period.

## 4.4 Empirical results

### 4.4.1 Compliance monitors and cost of capital

Table 4.6 reports the results of testing our prediction that firms' cost of equity decreases after the anti-bribery enforcement actions when firms appoint and retain compliance monitors, as these monitors signal the firm's commitment to avoid future wrongdoing, mitigate the perception of the company's risk, and help restore investors' trust in such firms. The dependent variable is the same in all columns and represents the implied cost of equity capital (*Implied cost of capital*) computed following Lee, Myers, and Swaminathan (1999), and Veenman (2013). Columns 1 and 2 use equation (4.1) to estimate the effect of compliance monitors' appointment on firms' cost of equity with one observation per fiscal year. Specifically, we keep only the implied cost of equity capital computed with the most recent analysts' earnings consensus forecast for a given fiscal year-end. However, because changes in the cost of capital occur gradually over time as markets learn about the effectiveness of the monitors, we also use within-year variation in the cost of capital based on updated monthly analysts' earnings forecasts and prices.[29]

The results in columns 1—4 of Table 4.6 show that after receiving the anti-bribery enforcement actions, firms' cost of equity capital increases. The coefficient estimate of *Post enforcement* is indeed positive and statistically significant at the 1% or 5% level

[29] Due to the panel structure of the data and the autocorrelation in monthly earnings forecasts and other variables, in unreported tests, we adjust the standard errors for two-way clustering by month and firm (e.g., Petersen, 2009; Veenman, 2013) and confirm all our results.

TABLE 4.6: Compliance monitors and implied cost of capital

This table reports the results of the multivariate regressions investigating the relationship between the implied cost of equity capital and the compliance obligations imposed on firms in connection with anti-bribery case settlements. Details on definition, source, and computation of all the variables can be found in Appendix 4.A. Standard errors in parentheses are robust and clustered at the firm level. $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% level, respectively.

| | (1) Implied cost of capital | (2) Implied cost of capital | (3) Implied cost of capital | (4) Implied cost of capital | (5) Implied cost of capital | (6) Implied cost of capital |
|---|---|---|---|---|---|---|
| **Post enforcement** | **0.009**** | **0.008**** | **0.010***** | **0.009***** | | |
| | **(0.003)** | **(0.003)** | **(0.003)** | **(0.003)** | | |
| Compliance monitor | 0.007 | 0.007 | 0.011** | 0.008** | | |
| | (0.005) | (0.005) | (0.005) | (0.004) | | |
| **Post enforcement x Compliance monitor** | **-0.027***** | **-0.023***** | **-0.030***** | **-0.022***** | **-0.015***** | **-0.014**** |
| | **(0.008)** | **(0.008)** | **(0.008)** | **(0.006)** | **(0.006)** | **(0.005)** |
| Buy-and-hold return | | 0.006** | | -0.002 | | -0.004** |
| | | (0.003) | | (0.002) | | (0.001) |
| Cash flow | | -0.052 | | -0.041 | | 0.007 |
| | | (0.033) | | (0.029) | | (0.021) |
| Internal control weakness | | -0.002 | | -0.002 | | 0.003 |
| | | (0.006) | | (0.005) | | (0.004) |
| Lawsuit | | -0.009 | | -0.006 | | -0.001 |
| | | (0.007) | | (0.006) | | (0.004) |
| Leverage | | 0.014* | | 0.016** | | 0.008 |
| | | (0.008) | | (0.006) | | (0.005) |
| Market-to-book | | -0.003 | | -0.003* | | 0.003* |
| | | (0.002) | | (0.002) | | (0.001) |
| ROA | | 0.080** | | 0.040 | | 0.023 |
| | | (0.039) | | (0.029) | | (0.022) |
| Size | | 0.001 | | -0.001 | | 0.015*** |
| | | (0.001) | | (0.002) | | (0.003) |
| Bribing years | | -0.000 | | -0.001 | | |
| | | (0.001) | | (0.000) | | |
| Crisis | | 0.017*** | | 0.016*** | | |
| | | (0.003) | | (0.002) | | |
| DOJ&SEC involved | | 0.003 | | 0.006 | | |
| | | (0.005) | | (0.004) | | |
| Mitigate | | 0.005 | | 0.003 | | |
| | | (0.005) | | (0.004) | | |
| Penalty | | -0.001 | | 0.001 | | |
| | | (0.001) | | (0.001) | | |
| Intercept | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes | No | No |
| Firm FE | No | No | No | No | Yes | Yes |
| Month-Year FE | No | No | No | No | Yes | Yes |
| Adj. $R^2$ | 0.2714 | 0.4034 | 0.3024 | 0.4280 | 0.6083 | 0.6323 |
| Unique firms | 79 | 59 | 79 | 60 | 77 | 77 |
| Obs. | 535 | 386 | 5,605 | 4,049 | 5,605 | 5,558 |
| **$H_0$: Post enforcement + (Post enforcement x Compliance monitor) = 0** | | | | | | |
| Sum of coefficients | -0.018*** | -0.015** | -0.020*** | -0.013** | | |
| F-test | 7.44 | 4.62 | 9.21 | 5.38 | | |
| P-value | 0.008 | 0.036 | 0.003 | 0.024 | | |

across all specifications, suggesting that investors perceive the enforcement actions as a negative shock and demand higher returns as a compensation for the higher company's risk. In terms of economic impact, for example, the evidence in column 1 shows that, after the anti-bribery enforcement actions, firms with no compliance obligations experience an average increase in their cost of equity capital of 0.9 percentage points, which corresponds to an increase of approximately 13% over their pre-enforcement cost of equity capital.

In contrast, as opposed to firms with no compliance obligations, firms required to appoint compliance monitors as part of the anti-bribery case settlements experience a decrease in their cost of equity capital: The coefficient estimate of the interaction term *Post enforcement* × *Compliance monitor* is indeed negative and statistically significant at the 1% level in columns 1—4. Moreover, the test on the sum of the coefficients *Post enforcement* and *Post enforcement* × *Compliance monitor* is negative and statistically significant at the 1% or 5% level across all specifications, further highlighting that firms with compliance monitors report a reduction in their cost of equity capital post-enforcement actions. In terms of economic impact, for example, column 1 shows that firms with monitors experience, on average, a reduction in their cost of equity capital of 1.8 percentage points, which corresponds to a decrease of approximately 20% over their pre-enforcement cost of equity capital.

In columns 5 and 6, we report the results of the same analysis when the multivariate linear model includes firm and month-year fixed effects, as reported in equation (4.3). This research design choice accounts for unobservable heterogeneity and controls for the time-invariant determinants of the compliance monitors' appointment (Gallo, Lynch, and Tomy, 2020). Specifically, columns 5 and 6 report the results of the fixed effects model without and with controls, respectively, when using month-year data.[30] The evidence in columns 5 and 6 is consistent with that in columns 1—4. After the enforcement action, the implied cost of equity capital of firms with monitors decreases as opposed to firms with no compliance obligations, suggesting that compliance monitors contribute to reducing

[30] The model in column 6 of Table 4.6 excludes the time-invariant control variables, which are *Bribing years*, *DOJ&SEC involved*, *Mitigate*, and *Penalty*. Furthermore, the model excludes the variable *Crisis*, because its effect is absorbed by the time fixed effects.

the agency costs and help restore investors' trust in firms after serious law violations.[31]

The calculation of the implied cost of equity capital relies on several assumptions, such as the earnings forecast horizon and the growth rate in the terminal value. Therefore, to ensure that our results are not purely driven by the way the dependent variable is computed, we perform an event study analysis and compare the market reaction to the anti-bribery enforcement actions of firms with and without the requirement to appoint a compliance monitor. To do so, we apply the following research design,

$$CAR_{i,t} = \alpha + \beta Compliance\ monitor_i + \gamma \boldsymbol{Z}_{i,t} + b_t + \epsilon_{i,t} \tag{4.6}$$

where the dependent variable is one of the following alternative proxies for cumulative abnormal returns: *CAR market model* and *CAR FF3 model*. The former refers to the cumulative abnormal returns computed according to the CAPM over the three-day window around each anti-bribery enforcement action, whereas the latter refers to the cumulative abnormal returns computed according to the Fama and French 3-factor model (Fama and French, 1992) over the same event window. The independent variable is *Compliance monitor*, an indicator equal to 1 if firms are required to appoint an independent compliance monitor as part of the anti-bribery case settlement, and 0 if no compliance obligation is imposed. The model includes time-varying controls ($\boldsymbol{Z}$), year fixed effects, and robust standard errors.

Starting with the univariate results, columns 3 and 4 in Panel B of Table 4.5 report the market reactions to the anti-bribery enforcement actions for firms that announce the appointment of compliance monitors and firms with no compliance obligations. Column 3 reports the results with *CAR market model*, whereas column 4 reports the results with *CAR FF3 model*. In both columns, the market reaction to the anti-bribery enforcement actions is positive for firms that announce the appointment of compliance monitors

[31] In unreported tests, we perform the same analysis while using only the most recent earnings forecast for each fiscal year-end and controlling for year fixed effects instead of month-year fixed effects. The magnitude of the results is almost unaffected. The coefficients (standard errors) of *Post enforcement* × *Compliance monitor* in the year fixed effects model without and with control variables are -0.016 (0.009) and -0.014 (0.009), respectively, although the latter is not statistically significant at the conventional level.

(approximately 1.5%) and negative for firms with no compliance obligations (between -0.3% and -0.6%), thus suggesting that investors perceive the appointment of compliance monitors favorably.

TABLE 4.7: Compliance monitors and market reaction

This table reports the results of the multivariate regressions investigating the relationship between the market reaction to each anti-bribery enforcement action and the compliance obligations imposed on firms in connection with anti-bribery case settlements. Details on definition, source, and computation of all the variables can be found in Appendix 4.A. Robust standard errors are reported in parentheses. $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% level, respectively.

| | (1) CAR market model | (2) CAR market model | (3) CAR FF3 model | (4) CAR FF3 model |
|---|---|---|---|---|
| **Compliance monitor** | **0.024*** | **0.027**** | **0.026*** | **0.022**** |
| | **(0.012)** | **(0.010)** | **(0.013)** | **(0.010)** |
| Bribing years | | -0.002** | | -0.002* |
| | | (0.001) | | (0.001) |
| Cash flow | | 0.188* | | 0.189** |
| | | (0.093) | | (0.084) |
| DOJ&SEC involved | | -0.018* | | -0.014 |
| | | (0.010) | | (0.009) |
| Leverage | | -0.016 | | -0.015 |
| | | (0.025) | | (0.029) |
| Market-to-book | | -0.012** | | -0.011** |
| | | (0.005) | | (0.004) |
| Mitigate | | 0.002 | | -0.002 |
| | | (0.012) | | (0.013) |
| Penalty | | 0.000 | | 0.000** |
| | | (0.000) | | (0.000) |
| ROA | | -0.302 | | -0.235 |
| | | (0.270) | | (0.301) |
| Size | | -0.001 | | -0.002 |
| | | (0.002) | | (0.003) |
| Intercept | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Adj. $R^2$ | 0.1276 | 0.2744 | 0.1565 | 0.2672 |
| Obs. | 64 | 59 | 64 | 59 |

Table 4.7 shows the results of the multivariate analysis after controlling for time-trends and other determinants of the market reaction to the disclosure of firm wrongdoing and compliance obligations. The results in columns 1 and 3 are estimated without controls, whereas those in columns 2 and 4 include time-varying control variables. The findings, in terms of both statistical and economic significance, are consistent across all the specifications. In terms of economic impact, for example, the results in column 1 suggest that abnormal market returns are 2.4 percentage points higher when bribing firms are required to appoint a compliance monitor, as opposed to when they are not. This result

is consistent with abnormal returns being approximately 1.5% for firms with compliance monitors and -1% for firms with no obligations, as reported in columns 3 and 4 in Panel B of Table 4.5.

The evidence in Table 4.7, combined with that in Table 4.6, suggests that investors consider the compliance monitors' appointment favorably when they assess the risk of the company and demand the returns on their investments. Moreover, because market returns also depend on forecasted cash flows, which are likely to include the direct and indirect costs of compliance monitors, the analysis reported in Table 4.7 provides more direct evidence of the net benefits of monitors' appointment than that in Table 4.6.

We further examine whether the cost of debt decreases when firms appoint and retain compliance monitors as part of the anti-bribery case settlements. Specifically, we expect the cost of borrowing to decrease for firms with compliance monitors if compliance monitors are incrementally useful for the type of monitoring that banks already exercise (Bharath et al., 2011; Carrizosa and Ryan, 2017), thus leading banks to decrease their monitoring efforts and costs and rely on monitors' effective supervision. We present the results of this analysis in Table 4.8.

The dependent variable (*Log spread*) is the same in all columns and represents the natural logarithm of the loan facility spread, measured by the all-in-spread drawn in basis points over LIBOR. Column 1 presents the results when the baseline model reported in equation (4.4) only controls for deal purpose, facility-type, and industry fixed effects, whereas the results in column 2 consider the multivariate regression model with a full set of control variables and fixed effects. The evidence in both columns suggests that when firms initiate a loan after receiving an enforcement action for FCPA violations, loan prices are, on average, higher, although the coefficient of *Post enforcement* is not statistically significant at the conventional level.

In contrast, and consistent with the evidence in column 2 of Table 4.5, when firms are required to appoint a compliance monitor, banks relax their contractual terms. The interaction term *Post enforcement* $\times$ *Compliance obligation* is indeed negative and statistically significant in columns 1 and 2, indicating that loan prices decrease for firms

TABLE 4.8: Compliance monitors and loan facility spreads

This table reports the results of the multivariate regressions investigating the relationship between the loan spreads and the compliance obligations imposed on firms in connection with anti-bribery case settlements. Details on definition, source, and computation of all the variables can be found in Appendix 4.A. Standard errors in parentheses are robust and clustered at the firm level. $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% level, respectively.

| | (1) Log spread | (2) Log spread | (3) Log spread | (4) Log spread |
|---|---|---|---|---|
| **Post enforcement** | **0.178** | **0.166** | | |
| | **(0.123)** | **(0.109)** | | |
| Compliance monitor | 0.164 | 0.543*** | | |
| | (0.177) | (0.162) | | |
| **Post enforcement x Compliance monitor** | **-0.404*** | **-0.413**** | **-0.487**** | **-0.271**** |
| | **(0.215)** | **(0.203)** | **(0.185)** | **(0.119)** |
| Buy-and-hold return | | 0.017 | | -0.007 |
| | | (0.047) | | (0.075) |
| Cash flow | | -0.700 | | -0.872 |
| | | (0.714) | | (0.825) |
| Credit spread | | 0.451*** | | 0.356* |
| | | (0.130) | | (0.184) |
| Internal control weakness | | 0.457 | | 0.141 |
| | | (0.299) | | (0.208) |
| Lawsuit | | -0.123 | | -0.140 |
| | | (0.246) | | (0.153) |
| Leverage | | 0.003 | | 0.133 |
| | | (0.220) | | (0.269) |
| Log facility amount | | -0.069** | | -0.132*** |
| | | (0.026) | | (0.041) |
| Log facility maturity | | -0.011 | | 0.112* |
| | | (0.068) | | (0.063) |
| Market-to-book | | -0.126** | | -0.058 |
| | | (0.050) | | (0.065) |
| Rated | | 0.024 | | 0.054 |
| | | (0.204) | | (0.115) |
| ROA | | -1.761 | | 0.576 |
| | | (1.913) | | (2.372) |
| Size | | -0.079 | | -0.348** |
| | | (0.055) | | (0.144) |
| Bribing years | | 0.036*** | | |
| | | (0.013) | | |
| Crisis | | -0.372*** | | |
| | | (0.099) | | |
| DOJ&SEC involved | | 0.216** | | |
| | | (0.093) | | |
| Mitigate | | -0.378** | | |
| | | (0.146) | | |
| Penalty | | -0.010 | | |
| | | (0.030) | | |
| Intercept | Yes | Yes | Yes | Yes |
| Deal purpose FE | Yes | Yes | No | No |
| Facility-type FE | Yes | Yes | No | No |
| Industry FE | Yes | Yes | No | No |
| Firm FE | No | No | Yes | Yes |
| Year FE | No | No | Yes | Yes |
| Adj. $R^2$ | 0.4344 | 0.6163 | 0.4813 | 0.5719 |
| Unique firms | 105 | 68 | 88 | 74 |
| Obs. | 777 | 477 | 777 | 620 |
| **$H_0$: Post enforcement + (Post enforcement x Compliance monitor) = 0** | | | | |
| Sum of coefficients | -0.226 | -0.247 | | |
| F-test | 1.63 | 1.97 | | |
| P-value | 0.204 | 0.165 | | |

with post-enforcement compliance monitor obligations compared to firms with no such obligations. The sum of the coefficients *Post enforcement* and *Post enforcement* × *Compliance monitor*, despite being statistically insignificant at the conventional level, shows a negative sign, further highlighting that the loan prices of firms that appoint compliance monitors are lower in the post-enforcement period compared to the pre-enforcement period. Specifically, in terms of economic impact, after the enforcement actions, the firms appointing compliance monitors experience a reduction in their loan spreads of approximately 23%, or about 50 basis points.

Columns 3 and 4 report the results of the same analysis when the multivariate model includes firm and year fixed effects, as per equation (4.5). In particular, column 3 reports the results of the fixed effects model with no controls, whereas column 4 reports the results with control variables. The evidence in both columns is consistent with that in columns 1 and 2. After the enforcement actions, loans initiated by firms with compliance monitor obligations obtain lower loan spreads compared to firms without such obligations. Altogether, this evidence suggests that compliance monitors are considered trust agents not only by investors, but also by banks. Banks rely on compliance monitors' scrutiny and control to prevent future firm wrongdoing, and if companies do not engage in repeat offense, lenders' risk of being liable for their clients' corrupt actions also decreases. Therefore, when compliance monitors are appointed, banks can reduce their monitoring efforts and costs, and in turn loan prices.

New loans are generally negotiated as packages that combine pricing and non-pricing contractual terms. Although the evidence in Table 4.8 suggests that banks reduce loan prices when firms appoint compliance monitors, this effect could be offset by tighter non-pricing contractual terms. To assess this possibility, we test whether non-pricing loan terms change after the anti-bribery enforcement actions, and in particular after the appointment of corporate compliance monitors. Following prior literature (e.g., Chen, 2016), we examine lenders' use of loan covenants and collateral to secure the loans when firms receive compliance monitor obligations in connection with anti-bribery case settlements.

Columns 5 and 6 in Panel B of Table 4.5 show the univariate tests of the changes in non-pricing loan contractual terms before and after the enforcement actions for firms with post-enforcement compliance obligations and firms with no such obligations. The non-pricing contractual term in column 5 is *Covenant*, and measures the number of covenants associated with loan initiations, whereas that in column 6 is *Secured*, an indicator equal to 1 if loans are secured by a collateral, and 0 otherwise. As shown in columns 5 and 6 of Table 4.5, the pre-enforcement period does not highlight statistical differences between the two samples in the number of covenants and in the frequency of loans secured by a collateral. In contrast, in the post-enforcement period, firms with compliance monitors have fewer covenants associated with loan initiations and a lower frequency of loans secured by a collateral compared to firms with no obligations. Moreover, firms with monitors decrease both loan covenants and loans secured by a collateral after receiving the anti-bribery enforcement actions, whereas firms with no obligations show mixed results, in that loan covenants increase and loans secured by a collateral decrease post-enforcement.

Using equation (4.4) with *Covenant* and *Secured* as alternative dependent variables, Table 4.9 reports the results of the multivariate analysis after controlling for fixed effects and other determinants of loan contractual terms. The dependent variable in columns 1—4 is *Covenant*, whereas that in columns 5—8 is *Secured*. Columns 1—2 and 5—6 provide the results of this analysis using the baseline model in equation (4.4) without and with control variables, respectively. Columns 3—4 and 7—8 perform the same analysis with the fixed effects model as per equation (4.5) without and with control variables, respectively. In all columns, we fail to find evidence that the use of covenants or a collateral to secure the loans changes in response to the appointment of compliance monitors, although the univariate results show fewer covenants and a lower likelihood of loans secured by a collateral in the post-enforcement period for firms with compliance monitors.

Altogether, the results in Tables 4.8 and 4.9 indicate that the requirement to appoint independent compliance monitors decreases loan prices for firms with such obligations, suggesting that compliance monitors act as trust agents and allow lenders to reduce their

TABLE 4.9: Compliance monitors and non-pricing loan terms

This table reports the results of the multivariate regressions investigating the relationship between the loan covenants and collateral and the compliance obligations imposed on firms in connection with anti-bribery case settlements. Details on definition, source, and computation of all the variables can be found in Appendix 4.A. Standard errors in parentheses are robust and clustered at the firm level. ∗ ∗ ∗, ∗∗, and ∗ indicate significance at the 1%, 5%, and 10% level, respectively.

| | (1) Covenant | (2) Covenant | (3) Covenant | (4) Covenant | (5) Secured | (6) Secured | (7) Secured | (8) Secured |
|---|---|---|---|---|---|---|---|---|
| **Post enforcement** | **0.058** | **0.140** | | | **-0.123*** | **-0.203***** | | |
| | **(0.137)** | **(0.163)** | | | **(0.067)** | **(0.067)** | | |
| Compliance monitor | 0.197 | -0.231 | | | -0.113 | 0.013 | | |
| | (0.212) | (0.340) | | | (0.086) | (0.147) | | |
| **Post enforcement x Compliance monitor** | **-0.562**** | **-0.436** | **-0.159** | **-0.446** | **0.007** | **0.121** | **0.153** | **0.271** |
| | **(0.265)** | **(0.294)** | **(0.367)** | **(0.596)** | **(0.151)** | **(0.233)** | **(0.191)** | **(0.271)** |
| Buy-and-hold return | | -0.059 | | -0.051 | | -0.033 | | -0.070 |
| | | (0.099) | | (0.072) | | (0.044) | | (0.059) |
| Cash flow | | -0.741 | | -2.013* | | 0.480 | | -0.204 |
| | | (1.100) | | (1.016) | | (0.328) | | (0.320) |
| Credit spread | | 0.278 | | 0.424 | | 0.095 | | 0.013 |
| | | (0.195) | | (0.292) | | (0.062) | | (0.106) |
| Internal control weakness | | 0.326 | | 0.235 | | -0.058 | | 0.068 |
| | | (0.295) | | (0.250) | | (0.199) | | (0.149) |
| Lawsuit | | -0.216 | | 0.034 | | -0.184 | | -0.165 |
| | | (0.423) | | (0.345) | | (0.138) | | (0.107) |
| Leverage | | -0.837* | | -1.127 | | -0.016 | | -0.018 |
| | | (0.488) | | (0.686) | | (0.160) | | (0.186) |
| Log facility amount | | -0.053 | | 0.008 | | -0.035* | | -0.058*** |
| | | (0.045) | | (0.038) | | (0.018) | | (0.018) |
| Log facility maturity | | -0.137 | | 0.066 | | 0.008 | | 0.132*** |
| | | (0.103) | | (0.063) | | (0.042) | | (0.031) |
| Market-to-book | | -0.032 | | 0.197 | | -0.015 | | 0.099* |
| | | (0.095) | | (0.131) | | (0.043) | | (0.055) |
| Rated | | 0.332 | | -0.019 | | 0.102 | | 0.033 |
| | | (0.359) | | (0.479) | | (0.104) | | (0.093) |
| ROA | | 3.818 | | 1.032 | | -2.906** | | -2.607* |
| | | (3.554) | | (4.207) | | (1.325) | | (1.318) |
| Size | | -0.169* | | 0.203 | | -0.078** | | 0.010 |
| | | (0.092) | | (0.223) | | (0.031) | | (0.082) |
| Bribing years | | -0.076*** | | | | -0.019** | | |
| | | (0.022) | | | | (0.009) | | |
| Crisis | | 0.094 | | | | -0.022 | | |
| | | (0.142) | | | | (0.055) | | |
| DOJ&SEC involved | | -0.198 | | | | 0.039 | | |
| | | (0.190) | | | | (0.073) | | |
| Mitigate | | 0.237 | | | | 0.083 | | |
| | | (0.250) | | | | (0.089) | | |
| Penalty | | 0.128*** | | | | 0.032* | | |
| | | (0.044) | | | | (0.018) | | |
| Intercept | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Deal purpose FE | Yes | Yes | No | No | Yes | Yes | No | No |
| Facility-type FE | Yes | Yes | No | No | Yes | Yes | No | No |
| Industry FE | Yes | Yes | No | No | Yes | Yes | No | No |
| Firm FE | No | No | Yes | Yes | No | No | Yes | Yes |
| Year FE | No | No | Yes | Yes | No | No | Yes | Yes |
| Adj. $R^2$ | 0.3393 | 0.4739 | 0.5471 | 0.5302 | 0.4230 | 0.5309 | 0.4606 | 0.5344 |
| Unique firms | 104 | 68 | 88 | 74 | 104 | 68 | 88 | 74 |
| Obs. | 777 | 477 | 777 | 620 | 777 | 477 | 777 | 620 |
| **$H_0$: Post enforcement + (Post enforcement x Compliance monitor) = 0** | | | | | | | | |
| Sum of coefficients | -0.504** | -0.296 | | | -0.116 | -0.082 | | |
| F-test | 4.63 | 1.04 | | | 0.65 | 0.12 | | |
| P-value | 0.034 | 0.311 | | | 0.423 | 0.730 | | |

monitoring costs and efforts. In contrast, non-pricing contractual terms do not seem to be affected by the release of anti-bribery enforcement actions.

### 4.4.2 Substitution and complementarity of monitoring mechanisms

We further investigate whether compliance monitors act as substitutes or complements for other trust repair mechanisms. Specifically, in line with the agency theory predicting that dividend increases are less frequent when managers are committed to use the economic resources diligently and avoid wrongdoing (e.g., La Porta, Shleifer, and Vishny, 2000; Hail, Tahoun, and Clare, 2014), we examine whether firms increase their dividend payments less when they can rely on compliance monitors to signal such commitment. We therefore compare the dividend payout policies of firms with and without compliance monitor obligations before and after the anti-bribery enforcement actions.

Column 7 in Panel B of Table 4.5 reports the results of the univariate analysis that compares the difference in means of *Dividend increase*—an indicator equal to 1 if firms increase or initiate a dividend payment in a given fiscal year, and 0 otherwise—for firms with compliance monitor obligations and firms with no such obligations before and after the anti-bribery enforcement actions. The pre-enforcement period highlights a statistically significant difference in the frequency of dividend increases, with firms appointing compliance monitors increasing or initiating dividend payments more often than firms with no compliance obligations (i.e., 60.3% vis-à-vis 43.3%, respectively). In contrast, the difference is reversed in the post-enforcement period, although it is not statistically significant at the conventional level: Approximately 53% of the sample of firms with compliance monitor obligations increase their dividend payments post-enforcement, as opposed to the 58% of the sample of firms without compliance monitors.

Table 4.10 reports the results of the multivariate analysis with fixed effects and control variables. The dependent variable, *Dividend increase*, is the same in all columns.[32] The independent variables are the same as those reported in equation (4.1), with the addition of *Repurchase*, which is an indicator equal to 1 for share repurchases, and 0 otherwise.[33] The evidence in columns 1 and 2, without and with control variables, respectively, shows

[32] Despite *Dividend increase* being binary, the results presented in Table 4.10 are obtained from OLS regressions because logit regressions perform poorly with large sets of fixed effects (Woolridge, 2010). However, we perform logit regressions in robustness tests and find similar results.

[33] In this test, the independent test variable *Post enforcement* is an indicator equal to 1 when dividend increases or initiations occur after firms receive the anti-bribery enforcement actions, and 0 otherwise.

TABLE 4.10: Compliance monitors and dividend increases

This table reports the results of the multivariate regressions investigating the relationship between the dividend payout policies and the compliance obligations imposed on firms in connection with anti-bribery case settlements. Details on definition, source, and computation of all the variables can be found in Appendix 4.A. Standard errors in parentheses are robust and clustered at the firm level. $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% level, respectively.

| | (1) Dividend increase | (2) Dividend increase | (3) Dividend increase | (4) Dividend increase |
|---|---|---|---|---|
| **Post enforcement** | **0.165*** ** | **0.139** ** | | |
| | **(0.062)** | **(0.063)** | | |
| Compliance monitor | 0.164 | 0.137 | | |
| | (0.100) | (0.099) | | |
| **Post enforcement x Compliance monitor** | **-0.238*** | **-0.238** ** | **-0.201*** ** | **-0.263** ** |
| | **(0.126)** | **(0.111)** | **(0.066)** | **(0.101)** |
| Buy-and-hold return | | 0.019 | | 0.006 |
| | | (0.032) | | (0.034) |
| Cash flow | | 0.200 | | 0.426 |
| | | (0.393) | | (0.283) |
| Internal control weakness | | -0.043 | | 0.023 |
| | | (0.084) | | (0.075) |
| Lawsuit | | 0.004 | | -0.027 |
| | | (0.067) | | (0.057) |
| Leverage | | 0.080 | | 0.004 |
| | | (0.099) | | (0.071) |
| Market-to-book | | 0.001 | | 0.002 |
| | | (0.036) | | (0.029) |
| Repurchase | | 0.276*** | | 0.170*** |
| | | (0.053) | | (0.045) |
| ROA | | 0.760* | | 0.004 |
| | | (0.397) | | (0.160) |
| Size | | 0.109*** | | 0.052 |
| | | (0.025) | | (0.051) |
| Bribing years | | -0.005 | | |
| | | (0.009) | | |
| Crisis | | -0.039 | | |
| | | (0.046) | | |
| DOJ&SEC involved | | -0.024 | | |
| | | (0.077) | | |
| Mitigate | | -0.010 | | |
| | | (0.079) | | |
| Penalty | | 0.000 | | |
| | | (0.016) | | |
| Intercept | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | No | No |
| Firm FE | No | No | Yes | Yes |
| Year FE | No | No | Yes | Yes |
| Adj. $R^2$ | 0.0672 | 0.2587 | 0.3435 | 0.3923 |
| Unique firms | 120 | 87 | 114 | 101 |
| Obs. | 1,102 | 686 | 1,102 | 919 |
| **$H_0$: Post enforcement + (Post enforcement x Compliance monitor) = 0** | | | | |
| Sum of coefficients | -0.073 | -0.099 | | |
| F-test | 0.48 | 0.97 | | |
| P-value | 0.490 | 0.327 | | |

that firms with no compliance obligations increase their dividends more often after the enforcement actions. The coefficient of *Post enforcement* is indeed positive and statistically significant at the conventional level.[34] This finding is consistent with dividends being distributed to deal with investors' concerns after an adverse corporate event.

While firms with no compliance monitor obligations respond to the enforcement actions by increasing their dividend payments, firms with compliance monitor obligations decrease such payments (column 7 in Table 4.5 shows that *Dividens increase* is 60.3% in the pre-enforcement period and 53.2% in the post-enforcement period). Moreover, the negative and statistically significant coefficients on the interaction term *Post enforcement* × *Compliance monitor* reported in columns 1 and 2 of Table 4.10 further highlight that dividend increases occur less often for firms with post-enforcement compliance monitors compared to firms with no such monitors. These findings are confirmed in columns 3—4 when we apply the fixed effects model as per equation (4.3). Altogether, the findings in Table 4.10 suggest that compliance monitors act as substitutes for dividend increases to reduce agency costs and help restore investors' trust in firms after the bribery scandals.

## 4.5 Conclusion

In this study, we examine whether the compliance monitors appointed in connection with the settlement of FCPA violations decrease firms' cost of capital. Our prediction relies on the agency theory, in that we expect compliance monitors to mitigate the agency problems of noncomplying firms by providing external verification and supervising the corporate actions that led to noncompliance or represent a risk for future noncompliance. While reducing noncompliance risk, monitors can help external stakeholders (re)gain trust in noncomplying firms (Farber, 2015). The results support our prediction: Firms required to appoint a compliance monitor after an FCPA enforcement action experience a reduction in their cost of equity and debt capital.

[34] In terms of economic impact, the marginal effects obtained from the unreported logit regression results show that the predicted probability of increasing dividends for firms with no compliance obligations is approximately 15 percentage points higher in the post-enforcement period compared to the pre-enforcement period.

Additional analyses show that the market reaction to the anti-bribery enforcement actions is positive when firms announce the appointment of compliance monitors, while it is negative when firms do not appoint compliance monitors. Moreover, we show that compliance monitors act as substitutes for other trust repair mechanisms. Specifically, firms that appoint compliance monitors do not increase their dividend payments to signal commitment to ethical and value-creating decisions when they can rely on compliance monitors to achieve the same results.

Altogether, in this paper we show that the role of compliance monitors exceeds their mandate of preventing future rule violations (Gallo, Lynch, and Tomy, 2020), in that these external officers mitigate the severe agency problems uncovered by the disclosure of bribery scandals, leading to lower cost of capital. Overall, compliance monitors contribute to restoring external stakeholders' trust and credibility in the firms involved in wrongdoing.

# 4.A Appendix—Variable description

| Variable Label | Description |
|---|---|
| **Panel A**: Dependent variables | |
| Implied cost of capital | Implied cost of capital (expected rate of return) based on the Residual Income Valuation model, and computed following Lee, Myers, and Swaminathan (1999) and Veenman (2013). The valuation is based on a three-year forecast horizon: $$V_0 = bv_0 + \frac{ri_1}{(1+r_e)} + \frac{ri_2}{(1+r_e)^2} + \frac{ri_3}{(1+r_e)^3} + \frac{ri_3(1+g)}{(r_e-g)(1+r_e)^3} \quad (4.7)$$ where $bv_0$ is the current book value of equity per share, $r_e$ is the cost of equity capital, $ri_t$ is the residual income in period $t$ (that is the difference between expected earnings per share (EPS) in period $t$ and $r_e$ times the beginning book value of equity per share $bv_{t\text{-}1}$), and $g$ is the assumed rate of growth in residual income beyond the three-year forecast horizon (i.e., 1%). The implied cost of equity capital is the rate of $r_e$ that results in the valuation ($V_0$) being equal to the current market price ($P_0$) (source: *IBES*). |
| Log spread | Natural logarithm of the loan facility spread, measured by the all-in-spread drawn in basis points over LIBOR. For ease of understanding, the descriptive statistics report loan spreads in basis points (source: *DealScan*). |
| CAR market model | Cumulative abnormal returns over the three-day window around each anti-bribery enforcement action and defined according to the CAPM. Abnormal returns are computed as follows: $$AR = R - E(R) = R - (r_f + \alpha + \beta(r_m - r_f)) \quad (4.8)$$ (source: *WRDS*). |
| CAR FF3 model | Cumulative abnormal returns over the three-day window around each anti-bribery enforcement action and defined according to the Fama-French 3-factor model (Fama and French, 1992). Abnormal returns are computed as follows: $$AR = R - E(R) = R - (r_f + \alpha + \beta_1(r_m - r_f) + \beta_2 SMB + \beta_3 HML) \quad (4.9)$$ (source: *WRDS*). |
| Covenant | Number of covenants associated with loan initiations (source: *DealScan*). |
| Secured | Indicator equal to 1 if loan facilities are secured by a collateral, and 0 otherwise (source: *DealScan*). |

*(Continued)*

| Variable Label | Description |
|---|---|
| Dividend increase | Indicator equal to 1 if firm $i$ initiates or increases the dividends paid between fiscal years *t-1* and $t$, and 0 otherwise (source: *Compustat*). |
| **Panel B**: Independent test variables | |
| Compliance monitor | Indicator equal to 1 if the enforcement actions require that firms appoint and retain a compliance monitor to help implement new compliance systems and report the efforts and outcomes of such implementation to regulators, and 0 if they do not require any reporting and compliance obligations (sources: *U.S. DOJ* and *SEC*). |
| Post enforcement | Indicator equal to 1 when prices and analysts' earnings consensus forecasts are issued (Table 4.6) or loans are initiated (Table 4.8) after the anti-bribery enforcement actions, and 0 otherwise. When firms receive more than one enforcement action and the first is with no compliance obligation, whereas the subsequent ones have at least one compliance obligation, the post enforcement period ends before the issuance of the subsequent enforcement actions with monitor obligations (sources: *U.S. DOJ* and *SEC*). |
| **Panel C**: Firm control variables | |
| Buy-and-hold-return | Contemporaneous 12-month buy-and-hold return of firm $i$ in fiscal year $t$ (source: *CRSP*). |
| Cash flow | Cash flow from operations scaled by lagged total assets (source: *Compustat*). |
| Crisis | Indicator equal to 1 during fiscal years from 2007 to 2009, and 0 otherwise (source: *Compustat*). |
| Internal control weakness | Indicator equal to 1 if, according to the assessment of disclosure controls under SOX 302, the internal control system is found to have material weaknesses, and 0 otherwise (source: *Audit Analytics*). |
| Lawsuit | Indicator equal to 1 if firms have an ongoing lawsuit in fiscal year $t$, and 0 otherwise (source: *Stanford Law School's Securities Class Action Clearinghouse*). |
| Leverage | Ratio between total liabilities and lagged total assets (source: *Compustat*). |
| Market-to-book | Ratio between market value of assets and book value of assets (source: *Compustat*). |
| Repurchase | Indicator equal to 1 when firm $i$ repurchases its shares in fiscal year $t$, and 0 otherwise. Repurchases are measured as the purchase of common and preferred shares less any decrease in the value of preferred stock (source: *Compustat*). |
| ROA | Return on assets, measured as the ratio between income before extraordinary items and lagged total assets (source: *Compustat*). |

*(Continued)*

| Variable Label | Description |
|---|---|
| Size | Natural logarithm of total assets (source: *Compustat*). |
| **Panel D**: Enforcement action control variables | |
| Bribing years | Number of bribing years (sources: *U.S. DOJ* and *SEC*). |
| DOJ&SEC involved | Indicator equal to 1 if both the U.S. DOJ and the SEC are involved in the bribery case settlement, and 0 otherwise (sources: *U.S. DOJ* and *SEC*). |
| Mitigate | Indicator equal to 1 if the case settlement is influenced by at least one mitigating factor, and 0 otherwise. Mitigating factors include voluntary reporting of the misconduct, cooperation with regulators, voluntary implementation of remedial measures, engagement in the misconduct only from low-level employees, or other measures (sources: *U.S. DOJ* and *SEC*). |
| Penalty | Natural logarithm of the sanctions charged for anti-bribery violations (sources: *U.S. DOJ* and *SEC*). |
| **Panel E**: Loan and credit market control variables | |
| Log facility amount | Natural logarithm of loan facility amount. For ease of understanding, the descriptive statistics report loan facility amounts in millions USD (source: *DealScan*). |
| Log facility maturity | Natural logarithm of loan facility maturity. For ease of understanding, the descriptive statistics report loan maturities in months (source: *DealScan*). |
| Credit spread | Credit spread measured as the difference between the yields of BAA- and AAA-rated corporate bonds (source: *Federal Reserve Board of Governors*). |
| Rated | Indicator equal to 1 if firms' debt securities are rated, and 0 otherwise (source: *Compustat Capital IQ*). |

# Chapter 5

# Conclusion

The past two decades have marked a significant increase in news about corporate misconduct, which refers to a variety of unethical professional actions, such as corruption, fraudulent financial reporting, and money laundering (Soltes, 2016). As corporate misconduct generates severe economic losses and has long-lasting side effects for the entire economy, understanding how it spreads is essential to safeguard economic and socio-political stability (Zuber, 2015).

Despite the extensive academic research and the continuous regulatory debate about corporate misconduct and its spread, this complex and multifaceted phenomenon is not fully understood yet, thus requiring further investigation (e.g., Jain, 2001; Amiram et al., 2018). My work contributes to this debate by extending research knowledge on how corporate misconduct spreads and by enhancing the understanding of the effectiveness of different regulatory measures designed to curb misconduct. In particular, my doctoral thesis provides novel evidence of different aspects of the spread of corporate misconduct. In Chapter 2, I identify competitive peer pressures as a determinant of the spread of corporate misconduct. In Chapter 3, my co-authors and I document that laws that improve employees' financial status are effective at limiting the spread of corporate misconduct, whereas in Chapter 4, my co-author and I show that specific regulatory actions targeted to curb the spread of corporate misconduct have broader implications for firms and their external stakeholders.

More specifically, in Chapter 2, I exploit the enforcement actions against firms that violate the U.S. FCPA to show that corruption produces spillover effects on peer firms'

financial misstatements. My empirical analyses reveal that firms are more likely to misstate their financial statements to artificially boost their performance when their corrupt rivals bribe and obtain unfair financial advantages. These findings rely on the theory of relative performance evaluation, according to which firms often manipulate their financial statements to report a similar performance to that of their competitors (DeFond and Park, 1997; Park and Ro, 2004). Overall, this study underscores the detrimental role of corruption, which has side effects beyond the well-established impairment of economic growth and development (Mauro, 1995; Healy and Serafeim, 2016).

In Chapter 3, my co-authors and I show that improvements in employees' financial status—proxied by increases in consumer bankruptcy exemptions—reduce firms' myopic income-increasing actions to meet earnings-based targets, thus suggesting that employees' financial incentives are a determinant of the spread of corporate misconduct. These results rely on prior evidence that financial concerns cause lack of productivity in the workplace (Richardson, Elliott, and Roberts, 2013; Kaur et al., 2021) and on the theory that firms' myopic actions are driven by the pressure to achieve earnings-based targets (Roychowdhury, 2006). As less financially concerned employees are more productive at work, firms can benefit from this enhanced productivity and achieve their performance targets while taking fewer myopic income-increasing decisions. Overall, our evidence suggests that laws that improve employees' financial status have positive spillover effects on firms and help reduce the spread of corporate misconduct.

In Chapter 4, my co-author and I document that specific regulatory measures designed to reduce recidivism in corporate misconduct (Gallo, Lynch, and Tomy, 2020) have implications for firms' cost of financing. Specifically, by exploiting the compliance obligations that regulators impose on firms that violate the FCPA, our results show that firms required to appoint compliance monitors in connection with anti-bribery case settlements have a lower cost of equity and debt capital compared to firms with no such obligations. We link these results to the agency theory, because the disclosure of firm wrongdoing signals severe agency problems, which compliance monitors help reduce by providing external verification and by monitoring business areas exposed to high risks of future

noncompliance. By reducing firms' agency risks, compliance monitors also help capital providers (re)gain trust in the firms involved in wrongdoing (Cumming, Dannhauser, and Johan, 2015; Farber, 2015).

Overall, the findings of my research are valuable for regulators and policymakers interested in how their enforcement actions could more effectively deter the spread of corporate misconduct to safeguard economic and socio-political stability (Zuber, 2015). At the same time, my results provide insights into unintended consequences of regulation. On the one hand, I show that regulations addressed to individuals, such as consumer bankruptcy exemptions, can influence firms' decision-making in general and the spread of corporate misconduct in particular. On the other hand, I show that regulations designed to curb the spread of corporate misconduct can have broader implications for firms and their stakeholders.

My results should be considered in light of two caveats. First, my research focuses on U.S. regulations: Although the U.S. is the biggest market economy and any regulatory change has repercussions globally (Bushee and Leuz, 2005; Leuz and Wysocki, 2016), the findings of my research may not generalize to other country-settings. However, it should be noted that under the FCPA regulation, which I exploit in Chapter 2 and Chapter 4, U.S. corporations are accused of misconduct committed abroad, thereby extending the setting beyond the boundary of the United States and mitigating generalizability concerns. Second, given that I classify only firms receiving enforcement actions as misbehaving firms, my results are likely to capture the lower bound of the spread of corporate misconduct. Future studies can build on these limitations and extend research knowledge about the spread of corporate misconduct by using more granular data or by studying this phenomenon in different international jurisdictions.

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