REX WANG

Those Who Move Stock Prices

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Marktspelers die aandelenkoersen beïnvloeden

Thesis

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Preface

The journey is more important than the destination. I cannot agree with this saying more when it comes to doing a PhD. As exciting as finishing the PhD is, what matters to me the most is the person I have become, the skills acquired, and the fantastic people I got to know along the way. Let me take this opportunity to express my deep appreciation to a few of them in particular.

A PhD gives you a license to pursue an academic career, which, however, would never cross my mind if I had not met Jan Brinkhuis, Adriana Gabor, and Alex Koning in my first undergraduate year. When I took their courses, they recognized my potential to be a researcher and encouraged me to do a PhD. Back then, I had absolutely no clue what to expect from a PhD or how academia looks like, and they had gone great lengths to help me find out. Adriana introduced me to Saskia Krijger, who was organizing the ESE Research traineeship for Dutch students with foreign background. This traineeship provided opportunities to work as research assistant and to hear firsthand stories shared by PhD candidates and senior faculty members. Meanwhile, Jan introduced me to Paul de Boer, who employed me to assist teaching in four economics and econometrics courses for two consecutive years. Alex hired me to assist him completing some lecture notes for an ERIM course, and later also supervised my bachelor thesis of which the advanced version is published in the European Journal of Operational Research. All of these valuable experiences acquainted me with different aspects of academia and eventually led me to apply for a PhD-position. I am grateful to the kindness of Adriana, Alex, Jan, Paul, and Saskia.

As much as academia already appealed to me after the traineeship, I could

not stop wondering how industry jobs would look like. To minimize any counterfactual regrets in the future, I applied to Robeco for some industry experience and got a part-time internship at the Quant Strategy department, where I had learned a lot from Martin Martens. Martin had not only showed me how to make sense of econometric models from a practitioner's view, but also helped me think about the trade-offs between academia and industry. As someone who had worked full-time in both, he knew exactly my considerations and how to deal with them. I thank Martin for sharing his knowledge and also for telling me that Prof. Patrick Verwijmeren was looking for a PhD student.

I am fortunate to have Patrick as my advisor, whose extensive guidance and invaluable mentorship have made me the researcher I am. Being the head of department and working on multiple top publications at the same time, Patrick remains remarkably accessible to his PhD-students: his door is always open for discussions and questions, and he always provides constructive and thoughtful feedbacks on very short notices. Patrick is always positive and optimistic, which is particularly helpful in times when I get stuck in research. He teaches me how to deal with rejections and quickly bounce back from such setbacks. He also stays open-minded, and encourages me to explore my own research interests. Even when my new ideas seem to be immature and rash, he has never dismissed any of them, but patiently helped me improve the ideas and develop them into papers. One good example is Chapter 4 of this dissertation, which is forthcoming in Financial Management. The list could go on and on, and words are just never enough to express my appreciation and thankfulness. So I would like to conclude this paragraph by emphasizing my gratitude to him once again: Thank you, Patrick, it was a privilege to be your PhD-student, and I hope to continue working with you and learning from you in the future.

During my PhD, another great source of inspiration came from Shuo Xia, who is my friend, a former PhD colleague, and also the co-author of Chapter 5 of this thesis. His broad interests and contagious enthusiasm about research help me stay motivated and keep me up to date with the literature. My days at the office often started with him knocking on the door and asking, "Have you seen this paper?” And the follow-up discussion would give me some food for thought that last for days. Shuo is also one of the most pleasant person I have ever met. Whenever I stopped by to share my frustrations or my “Eureka!” moments, he always knew the way to cheer me up or calm me down. Thank you, Shuo, for being such a nice and helpful colleague, and a best friend for life.

I would also like to thank my fellow PhD colleagues, especially my kind and supportive roommates. In office H8-15, Ning helped me start my PhD smoothly in the first year, while Dyaran had to put up with me for three more years. When we moved from the Tinbergen building to the E-building, it was Antti’s turn to share the office E2-40 with me. Your presence made my days at work joyful. I also thank Amy, Bo, Chen, Hao, Jingni, José, Lingtian, Omar, Nishad, Sha, Simon, Xiao, and Yuhao for all the great time we spent together.

Throughout my PhD life, I had benefited a lot from talks with many present and past Erasmus faculty members, especially Sjoerd van Bekkum, Mike Mao, Francisco Urúza, and Vadym Volosovych. I thank Sjoerd for providing many valuable comments on my job market paper and writing a reference letter for me; Mike for giving advices about work-life balance; Francisco for his Chilean, Chinese, and Dutch mixed jokes and wisdom; and Vadym for coordinating the PhD events and organizing mock interviews. Furthermore, I am indebted to TI staff Carolien and Judith, ERIM staff Miho, Myra, and Tineke, and ESE-BE staff Cia, Linda, Shirley, Suzanne, and Tulay for offering such an excellent working environment.

In the fourth year of my PhD, I had the opportunity to visit the Marshall School of Business at University of Southern California, hosted by Prof. Arthur Korteweg. Arthur’s comments and ideas helped me make significant progress in research, and his warm hospitality made my first trip to Los Angeles memorable. At Marshall, I met Prof. Jerry Hoberg, whose inspiring insights had largely improved my job market paper. I am also grateful to both Arthur and Jerry for writing reference letters to support my job applications.

My final word of gratitude goes to my friends and family. I am indebted to my best friend Elsa, who believes in me more than I do. Her million dollar question of “So what?” on each of my research ideas keeps motivating me to study more important issues. My deepest thanks go to my family, without whom I would not achieve anything in life.

谢谢你，妈妈！感谢你赐予我生命，以及在生活中给予我的支持，包容和理解。

Dank je wel, Jenny, mijn lieve, getalenteerde zusje, jij maakt onze familie compleet!

感谢舅舅，在我人生成长道路上在各方面给予的启蒙和指引。

最后，感谢外公外婆对我的养育之恩。谢谢你们给了我一个幸福温馨的家，让我能够无忧无虑地长。希望你们能够一直健康快乐长寿。我很荣幸能把这本书献给你们。

Rex Wang Renjie 王人杰

Rotterdam, June 2019

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Chapter 1

Introduction

Financial agents such as equity analysts and board of directors play important roles in stock markets. Their actions influence investors' trading strategies, managerial decisions, and corporate outcomes, and thereby affect capital allocations, market efficiency, and stock prices. This thesis consists of four empirical essays on analysts and directors, focusing on the causes and consequences of their behavior and labor markets.

In Chapter 2, we study how sell-side equity analysts form earnings expectations. Many investors rely on analyst forecasts to evaluate companies' future prospects and make trading decisions. However, different analysts who cover the same company at the same time often disagree with each other and issue divergent forecasts. Such disagreement might not only increase information uncertainty but also lead investors to form opposing opinions about the company, which could result in capital misallocations and mispricings. To the best of our knowledge, despite the large body of literature on analysts, which mostly focuses on what makes a good analyst and what improves forecast accuracy, there is little empirical evidence on the source of analyst disagreement. This chapter contributes to the literature by documenting a channel that systematically drives the disagreement among analysts.

More specifically, we exploit the fact that the majority of U.S. sell-side analysts cover two or more different industries at the same time, and test the hypothesis that analysts' expectations vary with the performance of their other coverage industries. Using a large panel of earnings forecasts for the period 1993-2016, we find strong evidence that negative shocks to other coverage industries make analysts more pessimistic about the focal firms. Our identification approach compares different analysts making forecast for the same firm at the same time, ensuring that differences in firms' fundamentals do not confound the estimation results. The effect is more pronounced when the focal firm is subject to ex-ante higher information asymmetries.

Moreover, we find that those pessimistic forecasts are less accurate and significantly lower than the realized earnings, and that analysts become more pessimistic even if the focal firms have no relationships with the shocked industry. These findings cannot be fully explained by information spillover effects, i.e., analysts covering shocked industries acquire valuable information about the focal firms that are not accessible to other analysts. Instead, the evidence is consistent with the idea that analysts heuristically overgeneralize bad news from other coverage industries and become overly pessimistic about the focal firms.

To obtain insights into why financial economists should care about this heuristic expectation-formation, Chapter 3 builds on the finding of analyst overgeneralization and examines its impacts on the financial market. We develop a simple trading model to derive two testable predictions. First, because overgeneralization induces analyst disagreement, it leads to higher trading volumes and larger return volatilities. Second, the model predicts that bad news in other coverage industries lead analysts to make excessively pessimistic forecasts, which exert downward pressure and induce underpricing.

Taking these theoretical predictions to the data, we find strong supporting evidence. Considering the performance of other coverage industries as belief shocks that affect analyst expectations, we show that a one-standard-deviation increase in belief shock dispersion translates to 6.4%-8.1% more analyst disagreement and is associated with up to 13.7% higher daily trading volume and 5.9% larger stock return volatility. In other words, analyst overgeneralization significantly aggravates information asymmetries and increases information uncertainty about firms' fundamentals.

Moreover, firms with more analysts affected by negative belief shocks experience a significant decline in stock price prior to earnings announcements. Consistent with the underpricing prediction that the price will reverse when the true information is revealed, affected firms experience higher positive price-reversal upon earnings announcements, conditional on the direction and magnitude of earnings surprises. In sum, the empirical evidence confirms the theoretical predictions that analyst overgeneralization significantly affects trading activities, volatility, and pricing.

These two chapters also provide useful insights for other strands of literature. As overgeneralization affects analyst expectations and consequently moves the stock prices for reasons not related to firms' fundamentals, it can be used to construct instrumental variables for analyst or investor disagreement, as well as for trading volumes, volatilities, and temporary mispricing. Given the scarcity of exogenous variations of these variables, my results could be helpful for future empirical research in related areas.

Findings about analyst overgeneralization not only help explain analyst disagreement, but also shed light on how sophisticated financial agents form expectations, especially those who are multi-tasking. The evidence from equity analysts suggests that multi-tasking agents might overgeneralize outcomes from one task when forming beliefs and making decisions for other tasks. The question then arises: Does multi-tasking also affect agents' actions in other ways?

Chapter 4 turns to the board of directors, another type of multi-tasking agent in the financial market. They have the critical task of actively monitoring and advising top management, to ensure that managers act in the best interest of shareholders. However, a directorship is rarely a full-time job. Most directors have other occupations, and many directors serve on multiple boards. Given that attention is not unlimited for directors, we study the effect of director distraction on corporate decision making and valuation.

We rely on a sample of directors with multiple directorships for the period 1996-2017. These directors need to distribute attention among their directorships, which provides a useful setting to study the effect of director attention.

Although we cannot observe exactly how much time or energy directors spend on each of their directorships, we conjecture that directors may be distracted when attention-grabbing events occur to the other directorships they have, in particular, industry-specific shocks. We follow this idea to construct a firm-quarter-level distraction measure by exploiting shocks to unrelated industries in which directors hold additional positions. To validate whether this measure really captures director attention, we examine board meeting attendance and show that directors that our measure identifies as distracted indeed attend fewer board meetings.

By examining Tobin's Q and stock performance, we find that firm value drops significantly when board members are distracted. A deviation from no distraction to the average distraction level is associated with a 3.3% discount in quarterly Tobin's Q, and a stock market underperformance of about 72 basis points per quarter. This effect is particularly strong when the distracted directors are independent and/or sit on an important committee of the board. Firms with more director distraction are less active, as they invest significantly less and are less likely to announce takeovers. The evidence is consistent with the idea that board monitoring intensity declines with director distraction, which gives managers the freedom to shirk at the expense of shareholder value.

Our results contribute to the important debate on the busyness of corporate boards. Directors with multiple directorships may be too busy to effectively monitor management, but the busyness also reflects the quality of directors, which could provide advantages for firms. This study disentangles busyness from director ability and provides evidence on the costs of having busy directors. Thus, our findings support policies restricting the number of directorships that an individual is allowed to have.

In Chapter 5, we shift focus from the behavior of financial agents to their labor markets frictions. In particular, we study how sorting in the labor market explains the performance differences across sell-side equity analysts. Workers at more prestigious companies tend to have better performance. For example, academic researchers at higher ranked schools have better publication records; and attorneys at larger law firms win more court cases. In the case of equity analysts, those employed by more reputable brokerage houses produce on average more accurate earnings forecasts. An analyst employed by the most reputable brokerage is about 6% more accurate than an analyst employed by a minor brokerage, which is equivalent to an advantage of 17.5 years of more experience.

This performance premium is driven by two distinct effects: more reputable brokerage firms have more resources that improve analysts' forecast accuracy; and the sorting in the labor market, which allows more reputable brokerage houses to hire more talented analysts in the first place. Distinguishing these two effects is however challenging, as the sorting mechanism creates an endogeneity problem. The relation between firm reputation and analyst performance is endogenous, because more talented analysts work for more reputable firms and analysts' talent is not observable.

We disentangle these two effects and quantify their relative importance, by estimating a two-sided matching model for the labor market of analysts. The matching model allows for a one-to-many assortative matching process between firms and analysts, which helps control for the selection effect. Our estimation results suggest that both effects are important: the influence effect accounts for 73% of the total effect of brokerage firms' reputation on analyst forecast accuracy, while the sorting effect accounts for the remaining 27%.

To summarize, this dissertation provides new insights into the behavior and labor markets of important financial agents. Psychological and cognitive factors significantly influence the decision-making of analysts and directors, and consequently affect stock trading activities and firm valuations. On the other hand, the evidence from analysts' labor markets shows to what extent employers could help improve the judgment of individual agents. Our findings not only contribute to the academic literature, but also have relevant implications for practitioners and policy makers.

Declaration of contribution

In this section, I declare my contribution to each chapter of this dissertation and acknowledge the contribution of others.

Chapter 1&6: I have written this chapter independently.

Chapter 2&3: These two chapters are based on my single-authored job market paper, Renjie (2019). I am grateful to Patrick Verwijmeren (promotor) for his invaluable guidance and advice. I thank Aleksandar Andonov, Sjoerd van Bekkum, Francesco D'Acunto, Ingolf Dittman, Bruce Grundy, Rob Hansen (discussant), Jerry Hoberg, Dušan Isakov (discussant), Arthur Korteweg, Yaron Levi, Anastasios Maligkris (discussant), Stefan Obernberger, Geoffrey Tate, Vadym Volosovych, Ben Zhang, Remco Zwinkels, and conference/seminar participants at Amsterdam Business School, Erasmus University Rotterdam, NOVA SBE, Université Paris-Dauphine, VU SBE, AFBC 2017 (PhD Forum), FMA 2017 (Doctoral Consortium), Paris December Meeting 2018, and RBFC 2018 for insightful comments. Part of the paper was written while I was visiting the USC Marshall School of Business.

Chapter 4: This chapter is based on the paper Renjie and Verwijmeren (2019), which is forthcoming in the *Financial Management*. It has benefited from comments by Marc Gabarro, Iftekhar Hasan, Mike Qinghao Mao, Francisco Urzúa, David Yermack, David S. Thomas, and seminar participants at Erasmus Research Institute of Management, Tinbergen Institute, Research in Behavioral Finance Conference (2016), and Paris Financial Management Conference (2016). The writing was a joint work with my co-author, and I did the majority of data work and empirical analysis.

Chapter 5: This chapter is based on the working paper Renjie and Xia (2019). We thank seminar participants at VU SBE for insightful comments. We had jointly formulated the research question and worked out the empirical strategy. The contribution and workload is about equally distributed between us.

Chapter 2

Overgeneralization and Analyst Beliefs[1]

2.1 Introduction

Equity analysts are key information agents in financial markets. They process information about coverage companies and produce earnings forecasts that help investors evaluate firms' future prospects and make trading decisions. However, analysts often disagree with each other and issue divergent forecasts for the same firm at the same time. Such disagreements lead investors to form different expectations about firms' future cash flows, which could result in capital misallocations and mispricings. Despite the large body of literature on analysts, empirical evidence on the source of their disagreement remains limited. In this paper, I exploit the diversity of analysts' coverage industries to study whether their earnings expectations vary with the performance of other industries that they cover. Comparing earnings forecasts made by *different* analysts for the *same* firm in the *same* quarter, I find that analysts become more pessimistic following negative shocks to their other coverage industries.

There are two potential channels through which shocks to other coverage industries can affect analysts' expectations about the focal firms. First, these industry shocks may contain valuable information about the focal firms that is

[1] This chapter is based on Renjie (2019).

only learned by analysts covering those industries and is not accessible to the other analysts. Industry coverage facilitates information acquisition, especially soft information obtained through their social networks (Cohen, Frazzini, and Malloy, 2010). Accumulated industry expertise also allows analysts to better assess the effects of the industry shocks (Bradley, Gokkaya, and Liu, 2017). Moreover, analysts who do not cover the shocked industries may have limited attention and may therefore overlook the impact of related news events on the focal firm (e.g., Hirshleifer and Teoh, 2003; Cohen and Frazzini, 2008). Consequently, analysts covering shocked industries can obtain a comparative information advantage and incorporate superior information in their earnings forecasts.

The second channel is implied by a common behavioral phenomenon known as overgeneralization, which is the process of overly extending evidence from an unrepresentative sample to reach broad and inaccurate conclusions (e.g. Beck, 1979; Clark, Beck, and Alford, 1999; Walton, 1999). This mechanism can lead analysts to overgeneralize an industry-specific shock to form expectations about the state of the world (e.g., economic conditions and business cycles). As a result, even though the industry shocks do not encompass any useful information about the focal firms, analysts would still adjust their forecasts accordingly as if the shocks were informative. Because the affected forecasts are essentially based on noise rather than information, I refer to this channel as the noise channel. I conduct a number of tests to distinguish between the *information* channel and the *noise* channel, and the evidence supports the second mechanism.

My main findings can be illustrated with the following example. Consider two equity analysts covering a coal mining company, COAL Corp, in 2011.[2] Analyst A additionally covers two firms in the transportation industry, while analyst B covers a gold mining company. Forecasts made by both analysts are usually close to the consensus and to the actual earnings. However, when forecasting COAL's earnings for fiscal quarter 2011Q3, their opinions diverge

[2]This example comes directly from my sample, but for courtesy I change the name of the company and analysts and adjust the exact calendar dates.

significantly. While analyst B issues an EPS forecast of \$0.32 on September 21, which is near the consensus of \$0.34, analyst A holds an exceptionally negative view about COAL and issues a forecast of \$-0.28 on September 22. My hypothesis suggests that analyst A's distinct pessimism is due to the recent performance of the transportation industry, which indeed fell by 19% over the period from June 22 to September 21. Because the gold mining industry has not experienced such negative shocks, analyst B's forecast does not deviate from the consensus or from his historical standards. This difference in opinions has significant effects on COAL: between September 20 and 22, the option-implied volatility increases by 43% and the daily trading volume increases by about 150%; and the stock closes down 3.1% on September 22. Analyst A's pessimism eventually turns out to be mistaken, as COAL announces its earnings of 2011Q3 to be \$0.35 per share later in October.

This example represents a systematic pattern across the universe of equity analysts in the I/B/E/S database over my sample period from 1993 to 2016. To capture analysts' belief shocks resulting from other industries' performance, I define industries based on the 49 Fama-French industry classifications and use the corresponding portfolio returns to measure industry shocks. Consistent with the notion that other coverage industries' performance affects analysts' expectations, I find that analysts produce significantly more pessimistic earnings forecasts following negative shocks to the other industries they cover. Specifically, suppose a given industry experiences a cumulative return of -10% in a quarter, analysts who cover this industry will issue on average 2.7% more pessimistic earnings forecasts for the firms operating in another industry relative to their peers who cover the same firm at the same time but do not cover the shocked industry. This effect is more pronounced (up to 4.3%) when analysts are forecasting for firms with more information asymmetry.

I further investigate why industry shocks turn into belief shocks that influence analysts' expectations. Do analysts acquire more information by learning from negative shocks to the other industries and foresee companies' unfavorable earnings (*information*)? Or do they just heuristically overgeneralize other industries' performance and become overly pessimistic (*noise*)? First, I test the effect of belief shocks on analyst forecast accuracy. The information hypothesis predicts that analysts acquire superior information and therefore produce more accurate forecasts, whereas the noise hypothesis predicts that analysts incorrectly adjust their expectations and therefore produce less accurate forecasts. Second, I estimate the effect of belief shocks from related and unrelated industries separately. Two industries are considered unrelated if they do not have the same three-digit NAICS code, have no industry-level or firm-level supplier-customer relationships, and do not belong to the same product market. Because news in unrelated industries are unlikely to encompass useful information about the focal firms, the information hypothesis predicts an insignificant effect of those belief shocks, while the noise hypothesis predicts a significant effect because overgeneralization also applies to unrelated industries.

The results of these two tests provide strong evidence in support of the noise channel. Following a belief shock of -10%, the affected analysts are about 2.1% less accurate because their forecasts are much lower than the realized earnings. This effect is economically sizable, as analysts need about 7 years more of firm-specific experience to offset this inaccuracy. Negative shocks to *both* related and unrelated industries significantly lower analysts' expectations and mislead them to make inaccurate forecasts. These findings are difficult to reconcile with any information stories, but they conform to the idea that analysts heuristically overgeneralize other industries' performance and mistakenly lower their expectations.

To identify the effects of belief shocks, I control for stock × fiscal year-quarter fixed effects in all specifications to exploit variation *within* firm-quarters by comparing earnings forecasts made by analysts with *different* belief shocks for the *same* firm at the *same* time. These fixed effects capture firm-quarter variation resulting from factors that make a particular company's earnings easier (or harder) to predict for *all* analysts in some quarters than in others, or from events that make *all* analysts more pessimistic (or optimistic) in some quarters than in others. Examples of such factors are voluntary management disclosures, merger rumors, and worker strikes. My results remain virtually the same when I control for time-varying observable analyst characteristics such as their experience and workload. Moreover, I use calendar quarter fixed effects to control for time trends (e.g., business cycles) that affect all analysts issuing forecasts around the same time for different firms, and analyst $\times$ stock fixed effects to control for all unobserved but time-invariant analyst characteristics, such as talent, education, industry expertise, and firm-specific preferences.

Using other industries' stock market performance to identify analyst belief shocks has a number of advantages. First, industry performance is arguably exogenous to analysts' personal characteristics. Second, reverse causality is implausible because it is unlikely that any single analyst can influence the performance of an entire industry. Third, it is much more difficult to come up with any confounding factors that would drive industry performance and analysts' earnings forecasts for firms in a different industry simultaneously. In contrast, firm-level performance is more ambiguous because of the potential correlation between stocks covered by the same analyst. Studies such as Israelsen (2016) document excess comovement among stocks covered by the same analyst. Finally, industry returns capture industry-wide shocks such as (de)regulations and technology innovations, which, compared to firm-level idiosyncratic shocks, are more likely to influence analysts' expectations about the state of the world.

Nevertheless, one may still argue that analysts' forecasts may affect firm policies of industry leaders and thereby influence the industry performance (*reverse causality*), or that the belief shock variable does not adequately capture industry shocks (*measurement errors*). To rule out these confounding stories, I exploit the oil price crash in 2014-15 as an exogenous negative shock to the oil industry. The price plunge is mostly due to the excess supply and weakening global demand, which is totally orthogonal to analysts' opinions. In a difference-in-differences framework, I find that analysts who cover the oil industry become about 7.6% more pessimistic and 4.4% less accurate about *non-oil* firms after the shock, relative to those who cover the same firm at the same time but do not cover the oil industry. This provides another piece of evidence suggesting that the effect of industry shocks on analyst expectation is causal.

Throughout this paper, I take analyst coverage as given and remain agnostic about why analysts cover particular companies or industries. This implicit assumption is unlikely to contaminate my results for two reasons. First, because analysts' industry coverage remains mostly time-invariant in my sample, it has already been absorbed by the analyst × stock fixed effects. Second, analysts are more likely to initiate coverage for firms about which they have favorable expectations (e.g., McNichols and O'Brien, 1997; Tehranian et al., 2013). Therefore, analysts' endogenous coverage choices would prevent me from finding any effects of the negative belief shocks.

It is noteworthy that I do not find a similar effect from positive belief shocks. Analysts mainly respond to negative shocks. This asymmetry is likely due to the negativity bias—that is, events of a more negative nature have a greater impact on one's behavior and cognition than those with equal intensity but of a more positive nature (e.g., Baumeister et al., 2001). It is also consistent with the findings in the psychology literature that individuals tend to overgeneralize negative news much more than positive ones (e.g., Walton, 1999).

A remaining concern is whether my results merely capture analyst distraction instead of changes in analyst expectations. One may argue that analysts issue relatively lower earnings forecasts for focal firms because they are distracted by other coverage industries with salient negative performance. I test this possibility by examining analyst forecast revisions. I find no evidence of distraction because analysts revise their forecasts with the same frequency when other industries perform extremely well or poorly. On the contrary, shocks to other industries lead analysts to revise forecasts in the same direction and magnitude, reinforcing the view that shocks to other industries influence analyst beliefs.

Further robustness tests show that my baseline results are persistent in different subperiods of my sample and are robust to alternative industry classifications based on the Fama-French 12-industry classification, the three-digit

GICS industries, industries based on two-digit SICH codes, and the Hoberg and Phillips (2016) 10-K text-based 50-industry classification (FIC-50). My findings are not merely driven by specific crisis episodes or any particular industry (mis)classifications.

When using earnings surprises to decompose belief shocks into expected and unexpected components, I find that analysts respond to both expected and unexpected shocks. Additionally controlling for the relative performance of coverage firms shows that a negative industry shock lowers analysts' beliefs more if their coverage firms in that industry are substantially affected by the shock. Analysts are also more likely to overgeneralize industry-wide shocks than firm-level idiosyncratic shocks. However, while more experienced analysts working for bigger brokerage houses are on average more accurate, these factors do not mitigate the impact of overgeneralization.

This chapter contributes to several strands of literature. First, my findings speaks to the large body of literature on the determinants of analysts' forecast accuracy and bias (see Kothari, So, and Verdi (2016a) for a recent literature review). In particular, my findings add to the literature on how psychological biases induce analysts' forecast errors (e.g., Ramnath, Rock, and Shane, 2008). I show that overgeneralization leads analysts to incorrectly adjust expectations and to consequently make inaccurate forecasts. Unlike most prior studies, this heuristic can also explain the sign of forecast errors.

Second, I contribute to the more general literature that studies the impact of experience on decision making in financial markets (e.g., Vissing-Jorgenson, 2003; Kaustia and Knüpfer, 2008; Greenwood and Nagel, 2009)). Murfin (2012) shows that banks impose stricter loan covenants when they suffer losses on their loan portfolios. In the same spirit, Koudijs and Voth (2016) demonstrate that personal experience can affect individual risk-taking in margin lending. Gurun et al. (2015) and Giannetti and Wang (2016) document that corporate scandals and Madoff-Ponzi schemes reduce households' trust and confidence in the financial market. My paper is closely related to Malmendier and Nagel (2011, 2016), who establish that personal lifetime experiences shape individuals' expectations. My findings are similar to theirs to the extent that analysts' expectations are influenced by their "recent experience", that is, recent performance of other coverage industries. However, the implication of overgeneralization is different in the sense that it can lead a multi-tasking agent to weight information from one task too heavily when making decisions for other tasks. To the best of my knowledge, this paper is the first to link overgeneralization to the belief-forming process of financial agents. Because many financial agents multitask (e.g., portfolio managers with multiple funds), this heuristic can be useful for modeling their expectations.

My findings are also relevant from practitioners' perspective. Due to a lack of supply of industry-experienced analysts, brokerage houses face the trade-off between the costs and benefits of allocating non-industry experts (Bradley et al., 2017). I show that all analysts covering multiple industries diminish expectations and produce less accurate forecasts once they are influenced by belief shocks, even those who cover only two industries. Overgeneralization could thus be considered a potential cost of assigning multiple industries to analysts.

The remainder of this chapter is organized as follows. Section 2.2 discusses the data and presents descriptive statistics. Section 2.3 outlines the conceptual framework and discusses the empirical methodology. Section 2.4 presents the main findings of this paper, and section 2.5 further strengthens the identification by exploiting an exogenous industry shock. Section 2.6 explores the heterogeneity of analysts. Section 2.7 concludes the paper.

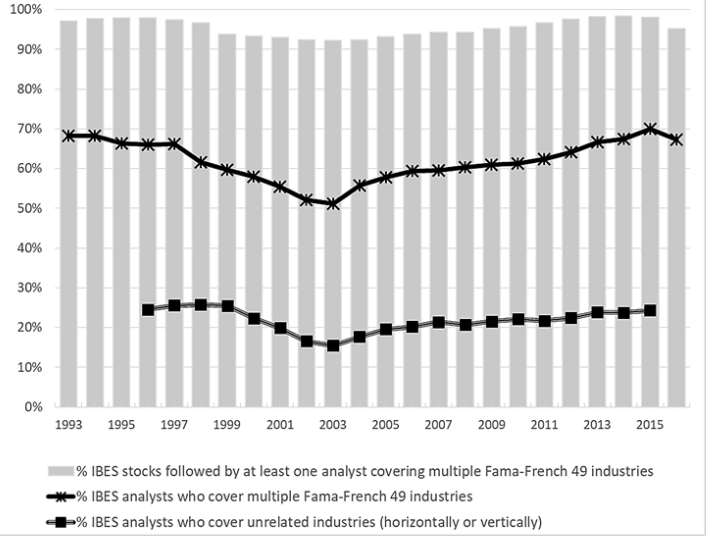
2.2 Data

I obtain individual analyst quarterly earnings forecasts and actual earnings of all U.S. firms from the I/B/E/S Unadjusted Detail database. To avoid imprecision arising from I/B/E/S's rounding of forecasts, I use the CRSP cumulative adjustment split factor to split-adjust the raw unadjusted data. Information about analyst identities and brokerage firms are drawn from the I/B/E/S Recommendations database. Because I/B/E/S recommendation data are only available from 10/29/1993, my sample period starts in 1993Q4 and ends in

2016Q1. I retain analysts that are present in both the Detail and Recommendations databases. For each analyst i making a forecast about firm j for fiscal year-quarter t, I use analyst i's latest earnings forecast issued prior to the announcement of the actual earnings but not later than 30 days after the fiscal quarter-end of t. To identify analyst coverage, I use the annual forecast data and assume that analyst i covers stock j for the whole fiscal year if analyst i issues a forecast for that given fiscal year.

Figure 2.1: Analysts covering multiple industries

This figure contains two graphs: (1) the fraction of stocks out of the universe of the I/B/E/S database that are followed by at least one analyst who covers more than one Fama-French 49 industry in each calendar year of my sample; (2) the fraction of analysts out of the universe of the I/B/E/S database who cover more than one Fama-French industry in each calendar year of my sample; (3) the fraction of analysts out of the universe of the I/B/E/S database who cover unrelated industries in each calendar year from 1996 to 2015. As explained in section 2.4.2, I consider two industries unrelated if they are not in the same three-digit NAICS code industry, have no supplier-customer relationships with each other, and do not belong to the same product market.



Next, I match all firms to Compustat Annual using CUSIPs and fiscal year-end dates, and to CRSP daily using CUSIPs and dates. I retain all matched firms and assign each firm to one of the 49 Fama-French industries based on its historical SIC code (CRSP item HSICCD or Compustat item SICH when HSICCD is missing). Fama-French 49 industry portfolio returns are downloaded from the data library of Kenneth R. French. Figure 2.1 depicts the fraction of I/B/E/S analysts covering at least two Fama-French industries and the average fraction of stocks covered by at least one of those analysts over the period 1993-2016. As shown, almost 70% of I/B/E/S analysts cover two or more industries, and over 90% firms are followed by one of those analysts. For robustness checks, I also consider other industry classifications, such as the Global Industry Classification Standard (GICS) industries, the two-digit SICH code, and the Hoberg-Phillips product market classification.

Finally, I exclude firms that have no analyst covering multiple industries. The final dataset consists of 1,423,192 analyst-firm-quarter observations with 12,175 unique analysts and 9,246 unique stocks. Panel A of Table 2.1 reports the number of unique stocks, the number of unique analysts, the number of unique analyst-stock pairs, the average number of analysts covering a particular stock, and the average number of Fama-French industries and of stocks covered by analysts within each calendar year of my sample period. As shown, the average number of analysts covering a given firm increased from 4.0 in 1993 to 9.7 in 2016. Analysts' workloads have not changed a great deal, remaining around 10 firms in 3 different FF-49 industries. The diversity of coverage industries is thus a common feature throughout my sample period.

Panel B of Table 5.1 reports the summary statistics of the variables used in this study. My main dependent variables of interest are earnings forecast and forecast errors. To measure how different an analyst's forecast is from the consensus among other analysts, I follow prior research and compare her forecast to the average of all analysts who issue forecasts for the same firm i and fiscal year-quarter t (Clement, 1999a; Hong and Kubik, 2003; Kothari et al., 2016a). This controls for any firm-quarter factors that influence all analysts' expectations. I therefore define

$$\text{Adjusted EPS Forecast}_{ijt} = \frac{\text{Raw Forecast}_{ijt} - \text{Mean Forecast}_{jt}}{\text{SD Forecast}_{jt}}, \qquad (2.1)$$

where Raw Forecast_{ijt} is the raw earnings per share forecast in dollars made by analyst i for the earnings of fiscal quarter t of stock j, which is split-adjusted

Table 2.1: Data sample and summary statistics

This table describes the sample and reports the summary statistics of the main variables. Panel A tabulates, for each calendar year in my sample from 1993 to 2016, the number of unique stocks, the number of unique analysts, the number of unique analyst-stock pairs, the average number of analysts covering a particular stock, and the average number of Fama-French 49 industries and of stocks covered by analysts. Panel B reports the summary statistics for the main sample of analyst-stock-quarter observations for the period 1993-2016. The adjusted EPS forecast is computed as in Equation (2.1); forecast error is computed as in Equation (2.3); PMAFE is computed as in Equation (2.2); experience and firm experience are analysts' overall and firm-specific experience, respectively, computed as the number of years between an analyst's current earnings forecast and his/her first ever announced forecast and his/her first forecast for a particular firm; number of stocks is the number of stocks covered by an analyst; number of industries is the number of industries covered by an analyst; and broker size is the number of analysts employed by a broker in a calendar year. Adjusted EPS forecast, forecast errors, and PMAFE are winsorized at the 1% and 99% levels. Detailed definitions of all the variables are presented in Table 2.A.1.

Panel A: Sample

Year	Number of Stocks	Number of analysts	Number of analyst-stock pairs	Avg. analyst coverage	By analyst: Avg. number of industries covered	By analyst: Avg. number of stocks covered
1993	1,749	1,147	9,230	4.0	3.6	10.2
1994	2,074	1,622	12,620	4.8	3.4	9.2
1995	2,190	1,773	13,609	4.9	3.3	9.1
1996	2,479	1,967	15,085	4.7	3.3	9.2
1997	2,691	2,312	16,659	4.7	3.1	8.5
1998	2,772	2,720	18,969	5.3	2.9	7.9
1999	2,694	2,896	19,843	5.7	2.7	7.7
2000	2,499	2,819	18,937	5.7	2.6	7.5
2001	2,375	2,874	19,420	6.4	2.5	7.3
2002	2,340	2,945	21,201	6.7	2.5	7.6
2003	2,326	2,773	20,925	6.8	2.5	8.0
2004	2,587	2,962	23,613	7.1	2.5	8.4
2005	2,763	3,004	24,997	7.2	2.6	8.6
2006	2,807	3,048	25,877	7.2	2.7	8.9
2007	2,941	3,046	26,894	7.2	2.8	9.1
2008	2,886	2,913	26,127	7.3	2.8	9.2
2009	2,772	2,752	26,096	7.8	2.9	9.7
2010	2,839	2,872	28,273	8.4	3.0	10.1
2011	2,872	3,022	30,364	8.6	3.1	10.3
2012	2,900	2,910	31,147	8.8	3.2	11.1
2013	3,058	2,824	32,731	8.9	3.4	11.8
2014	3,279	2,840	33,854	8.5	3.6	12.3
2015	3,432	2,748	34,470	8.5	3.7	12.8
2016	2,078	2,053	12,981	9.7	2.9	9.0

using the CRSP cumulative adjustment split factor from the CRSP Daily file; Mean Forecast$_{jt}$ and SD Forecast$_{jt}$ are, respectively, the mean and standard deviation of forecasts made by all analysts for firm j and fiscal quarter t.

Panel B: Summary Statistics

	N	Mean	St. Dev.	Percentile				
				10th	25th	50th	75th	90th
Dependent variables								
Adjusted EPS forecast	1,423,192	0.00	0.89	-1.14	-0.62	0.00	0.62	1.15
Forecast errors	1,423,192	0.00	0.80	-1.00	-0.38	0.00	0.37	1.00
PMAFE	1,423,192	-0.02	0.67	-1.00	-0.42	-0.05	0.26	0.76
Number of revisions	1,423,192	0.37	0.67	0	0	0	1	1
Forecast revisions (SUF)	314,742	-0.18	0.99	-1.55	-0.89	-0.27	0.60	1.20
CAR(0, 1) (in %)	314,742	-0.13	4.96	-4.26	-1.83	-0.10	1.61	4.04
Main explanatory variable								
Belief Shock	1,423,192	0.02	0.10	-0.09	-0.01	0.01	0.07	0.12
Negative Shock	1,423,192	-0.03	0.07	-0.09	-0.02	0.00	0.00	0.00
Positive Shock	1,423,192	0.04	0.06	0.00	0.00	0.02	0.07	0.12
Explanatory variables for robustness								
Belief Shock (EW)	1,423,192	0.02	0.10	-0.08	-0.01	0.01	0.07	0.12
Belief Shock (Related)	1,031,484	0.03	0.10	-0.07	0.00	0.02	0.08	0.14
Belief Shock (Unrelated)	1,031,484	0.01	0.08	-0.01	0.00	0.00	0.02	0.09
Belief Shock (FF12)	1,423,192	0.01	0.08	-0.06	0.00	0.00	0.06	0.10
Belief Shock (GICS)	1,411,961	0.03	0.10	-0.06	0.00	0.00	0.08	0.14
Belief Shock (Sic2)	1,423,192	0.03	0.10	-0.06	0.00	0.03	0.09	0.14
Belief Shock (HP50)	1,271,360	0.03	0.09	-0.04	0.00	0.00	0.07	0.13
Control variables								
Overall experience	1,423,192	6.47	5.03	0.88	2.39	5.34	9.52	13.81
Firm experience	1,423,192	2.85	3.24	0	0.52	1.73	4.02	7.27
Number of stocks	1,423,192	14.01	7.37	6	9	13	18	23
Broker size	1,423,192	62.85	54.82	11	22	48	89	125
Number of industries	1,423,192	3.61	2.37	1	2	3	5	7
Number of industries (FF12)	1,423,192	2.47	1.42	1	1	2	3	4
Number of industries (GICS)	1,423,192	3.00	2.07	1	2	2	4	6
Number of industries (Sic2)	1,423,192	3.78	2.40	1	2	3	5	7
Number of industries (HP50)	1,423,192	3.35	2.31	1	2	3	4	7
Number of industries (Naics)	1,423,192	3.63	2.52	1	2	3	5	7

The denominator standardizes forecasts such that they are comparable across firms. Note that after demeaning, a forecast below 0 implies that an analyst is more pessimistic relative to her peers covering the same firm at the same time.

As for forecast errors, prior research mostly uses the PMAFE (proportional mean absolute forecast error) to measure analyst inaccuracy (e.g., Clement, 1999a; Malloy, 2005), which is defined as

$$\text{PMAFE}_{ijt} = \frac{\text{AFE}_{ijt} - \text{Mean AFE}_{jt}}{\text{Mean AFE}_{jt}}, \tag{2.2}$$

where AFE_{ijt} denotes the absolute value of the forecast error (forecast minus actual) for analyst i's forecast of firm j for fiscal quarter t, and Mean AFE_{jt}

is the average AFE of all analysts covering firm j for fiscal quarter t. This variable controls for any firm-quarter factors that affect forecast accuracy. Moreover, the sign of the forecast errors is also important when comparing analysts' expectations with firms' actual earnings. Therefore, I follow the intuition behind the PMAFE measure to define

$$\text{Forecast Errors}_{ijt} = \frac{\text{FE}_{ijt} - \text{Mean FE}_{jt}}{\text{Mean AFE}_{jt}}, \tag{2.3}$$

where FE_{ijt} is forecast earnings minus actual earnings.

These three variables and all of the firm-quarter level continuous dependent variables are winsorized at the 1% and 99% levels. Detailed definition of the control variables are presented in Table 2.A.1. The construction of the belief shocks is explained in the next section.

2.3 Empirical Methodology

In this section, I first use a simple conceptual framework to illustrate the role of other coverage industries' performance in shaping analysts' expectations. I then discuss the identification of belief shocks and describe the empirical strategy for estimating the effect of those shocks on analysts' earnings forecasts.

2.3.1 Conceptual framework

Suppose that analyst i's forecast for firm j's earnings of fiscal quarter t is given by

$$F_{ijt} = \sum_{k=1}^{M} \theta_k \cdot \Pi_{jkt} + \sum_{k=1}^{K} \delta_k \cdot P_{ijkt} + \eta_{ijt}, \tag{2.4}$$

where analyst i makes a forecast based on public signals $\Pi_{jt} = (\Pi_{j1t}, \ldots, \Pi_{jMt})'$ and her private information and incentives $P_{ijt} = (P_{ij1t}, \ldots, P_{ijKt})'$ about firm j. Public signals Π_{jt} could be macroeconomic factors such as interest rate hikes and tax cuts, or firm-specific events such as voluntary management disclosures and M&A deals, which are observable to all analysts covering firm j. Private signals P_{ijt} could include private information obtained from the analyst's social network or pressure from the analyst's brokerage house to issue favorable forecasts. This representation of analyst forecasts is motivated by the large body of literature on sell-side analysts' forecasts (see Kothari et al. (2016a) for a recent survey).

This paper tests the idea that analyst i derives some private signals about firm j from collecting and processing information about her other coverage industries. As in the example above, covering the poorly performing transportation industry seems to lower analyst A's earnings expectation about the focal firm COAL. More formally, suppose that analyst i covers stocks in κ other industries, I conjecture that she obtains private signals $\zeta_{ijt} = (\zeta_{ij1t}, \ldots, \zeta_{ij\kappa t})'$ from researching those industries. Conforming to the notion that those private signals could affect analyst beliefs, I refer to them as "belief shocks" in the rest of the paper. Taking these belief shocks into account, I can rewrite Equation (2.4) as

$$F_{ijt} = \theta'\Pi_{jt} + \beta'\zeta_{ijt} + \delta' Z_{ijt} + \eta_{ijt}, \tag{2.5}$$

where $Z_{ijt} = P_{ijt} \setminus \zeta_{ijt}$. That is, the subjective analyst forecast depends on publicly available information, the analyst's belief shocks from other coverage industries, and her other private information and incentives. Next, I explain the identification of belief shocks.

2.3.2 Identifying belief shocks

Consider an analyst i making an earnings forecast for stock j in fiscal quarter t is exposed to potential belief shocks from the other industries she covers in quarter t. I construct the belief shocks (BS) to this analyst with respect to firm j as

$$BS_{ijt} = \sum_{k \in S_{it}^{(-j)}} w_{ikt}^{(-j)} \times IndRet_{ikt}, \tag{2.6}$$

where $S_{it}^{(-j)}$ denotes the set of stocks followed by analyst i in quarter t, *excluding* stocks in the same Fama-French 49 industry as stock j, thereby allowing only shocks from industries other than that of stock j; the weight $w_{ikt}^{(-j)}$ captures how important stock k is to analyst i; and $IndRet_{ikt}$ is the cumulative return of the Fama-French industry of stock k over the quarter before analyst i issues the most recent earnings forecast for stock j in quarter t. If an analyst covers stocks in only one Fama-French industry, I set the BS variable to be zero. I now explain the construction of $w_{ikt}^{(-j)}$ and $IndRet_{ikt}$ in more detail.

First, $w_{ikt}^{(-j)}$ is meant to capture the weight of stock k to analyst i in quarter t. The weighting-scheme is motivated by Harford, Jiang, Wang, and Xie (2018), who find that, because of their career concerns, analysts allocate more attention to firms with relatively larger market capitalization in their portfolio. As a result, analysts' beliefs are more likely to be affected by news events in industries to which analysts devote more research efforts. To this end, I define the weight for each stock k in $S_{it}^{(-j)}$ as

$$w_{ikt}^{(-j)} = \frac{mve_{kt}}{\sum_{l \in S_{it}^{(-j)}} mve_{lt}}, \tag{2.7}$$

where mve_{lt} denotes the market value of equity of stock l at the fiscal year-end preceding fiscal quarter t. Alternatively, I consider equal weights in (2.6) to measure belief shocks and obtain similar results, as shown in Table 2.6.

Second, $IndRet_{ikt}$ is meant to capture the performance of the Fama-French industry of stock k over a period before analyst i issues the most recent earnings forecast for stock j in quarter t. Suppose that analyst i issues the forecast on day τ, I compute the cumulative returns of the Fama-French industry of stock k over the window $[\tau - 90, \tau]$.[3] It is noteworthy that my results are robust to different windows. I have experimented with various window spans, from 60 days to 180 days, and the results are similar to those presented here.

Constructing the belief shocks in this way has the following advantages. First, it relies on stock market performance of industries other than that of stock j, which is arguably exogenous to the characteristics of analyst i. Second, I use industry-level performance rather than firm-level performance to mitigate the potential concern of omitted-variable bias. For instance, analyst

[3]In some cases, the analyst issues the forecast a few days after the fiscal quarter-end day τ_1. For such cases, I compute $IndRet$ over the window $[\tau_1 - 90, \tau]$.

i might become more pessimistic for other reasons (other private signals) and might therefore issue more pessimistic earnings forecasts for both stock j and k. The pessimistic forecast about stock k might also put downward pressure on the price of stock k. In this case, analyst i's more pessimistic forecast about stock j is not due to the bad performance of stock k. However, a single analyst's forecast is unlikely to drive the performance of the whole industry, and thus using industry-level performance resolves this endogeneity issue. Third, industry returns capture industry-wide shocks rather than firm-level idiosyncratic shocks, which are more likely to influence analysts' expectations about the state of the world. Finally, on a cognitive level, extreme returns in an industry are more salient and more likely to affect analysts' beliefs. The BS variable captures this effect because the BS variable moves in the same direction and magnitude with the industry returns by construction.

2.3.3 Estimating the impact of belief shocks

I conjecture that the performance of other industries influences analysts' expectations about the focal firm and thus affects their earnings forecasts. Substituting the constructed measure of belief shocks for ζ_{ijt} in Equation (2.5), I can examine how analysts' forecasts respond to these belief shocks by estimating the following model:

$$y_{ijt} = \alpha_{jt} + \beta \times BS_{ijt} + \varepsilon_{ijt}, \tag{2.8}$$

where i indexes analysts, j indexes firms, t indexes fiscal year-quarters, and y_{ijt} is the dependent variable of interest (e.g., EPS forecast and forecast errors). The main coefficient of interest is β, which measures the effects of belief shocks, BS_{ijt}. This coefficient would be significantly positive if analysts' earnings expectations are affected by the performance of other industries they cover.

The stock $\times$ fiscal year-quarter fixed effects, α_{jt}, allow me to compare earnings forecasts made by two analysts with *different* belief shocks for the *same* firm at the *same* time. These fixed effects capture all publicly available information (i.e., Π_{jt} in Equation (2.5)) and therefore can control for firm-quarter variation driven by factors or events that affect the expectation of *all* analysts, making them more pessimistic (or optimistic) in some quarters than in others. Examples of such events are shareholder litigations and merger rumors. To absorb the firm-quarter fixed effects, I follow the literature (e.g., Clement (1999a); Malloy (2005); Bradley et al. (2017)) to demeaning both the dependent and the independent variables within each firm-quarter group, which gives

$$\widetilde{y}_{ijt} = \beta \times \widetilde{BS}_{ijt} + \widetilde{\varepsilon}_{ijt}. \tag{2.9}$$

The tilde indicates demeaned variables henceforth. Note that the main dependent variables of interest, the adjusted EPS forecast and (absolute) forecast errors, are already demeaned and scaled within firm-quarters by construction.

In addition, I control for some observable analyst-specific characteristics that previous studies have found to affect analysts' forecasts: analysts' overall experience and firm-specific experience in years, the number of industries and stocks covered by analysts, and employer size (Clement, 1999a; Bradley et al., 2017). These variables capture a part of analysts' private information and incentives, i.e., Z_{ijt} from Equation (2.5). Detailed definitions of these variables are presented in Table 2.A.1.

Moreover, I include the calendar year-quarter fixed effects to control for common time trends such as macroeconomic shocks or business cycles, which could influence the expectations of analysts covering different firms but making their forecasts around the same time. I also include the analyst $\times$ stock fixed effects to control for any unobserved but time-invariant factors in Z_{ijt}, such as analysts' skill, education, and industry expertise. I use analyst $\times$ stock fixed effects instead of analyst fixed effects alone to account for unobservable heterogeneity within the same analysts across the different stocks that they cover. For example, analysts might consistently spend more time and effort on a particular firm than on other firms they cover, or they may consistently be more pessimistic or accurate about a particular stock than about other stocks they follow.

Therefore, my final regression model takes the form

$$\widetilde{y}_{ijtq} = \alpha_q + \alpha_{ij} + \beta \times \widetilde{BS}_{ijt} + \gamma' \widetilde{X}_{ijt} + \widetilde{\varepsilon}_{ijtq}, \tag{2.10}$$

where q indexes the calendar year-quarter in which the analyst i issues the forecast, α_q denotes the corresponding year-quarter fixed effects, α_{ij} denotes the analyst $\times$ stock fixed effects, and $\widetilde{X}_{ijt}$ is a vector of control variables demeaned within firm-quarters. I two-way cluster the standard errors by calendar year-quarter and by analyst $\times$ stock to account for possible correlations within cohorts of analysts who make forecasts around the same time and for potential serial correlation within the tenure of an analyst following the same stock. This clustering yields the most conservative standard errors.

To allow for differences in analysts' responses to the sign of the belief shocks, I also estimate the effects of negative and positive shocks separately. If analysts respond to negative and positive shocks differently, not allowing for such potential asymmetry could downward bias the estimate of β in Equation 2.10 towards zero. The following model accounts for the potential asymmetry:

$$\widetilde{y}_{ijtq} = \alpha_q + \alpha_{ij} + \beta_1 \times \widetilde{BS}^{-}_{ijt} + \beta_2 \times \widetilde{BS}^{+}_{ijt} + \gamma' \widetilde{X}_{ijt} + \widetilde{\varepsilon}_{ijtq}, \tag{2.11}$$

where I define negative and positive belief shocks respectively as

$$BS^{-}_{ijt} = \sum_{k \in S^{(-j)}_{it}} w^{(-j)}_{ikt} \times IndRet_{ikt} \times \mathbf{1}(IndRet_{ikt} < 0), \tag{2.12}$$

and

$$BS^{+}_{ijt} = \sum_{k \in S^{(-j)}_{it}} w^{(-j)}_{ikt} \times IndRet_{ikt} \times \mathbf{1}(IndRet_{ikt} > 0). \tag{2.13}$$

To assess the effect of extreme shocks and salient performances, in some specifications I also replace BS^{-}_{ijt} and BS^{+}_{ijt} with the indicator variable of the bottom decile (D1) of BS^{-}_{ijt} and the top decile (D10) of BS^{+}_{ijt}, respectively, where D1 and D10 capture the salient negative and positive performances, respectively. Both β_1 and β_2 would be significantly positive if analysts respond to both negative and positive belief shocks.

The main identifying assumption to obtain an unbiased estimate of β (or $\beta_{1,2}$) is $\text{cov}(BS_{ijt}, \varepsilon_{ijtq}) = 0$. Because the error term ε_{ijtq} contains analysts' *unobserved time-varying private* information and incentives that are not captured by X_{ijt} and α_{ij}, the assumption essentially means that the belief shock variable does not systematically covary with any other unobserved private signals about firm j obtained by the analyst. This assumption is justified because the belief shock variable is constructed based on the performance of other coverage industries, which is arguably exogenous to analysts' unobservable (even time-varying) personal characteristics. It is also highly unlikely that a single analyst can influence the performance of an entire industry. Moreover, any proponent of the existence of confounding factors would have to explain how they relate to the industry shocks and analysts' earnings forecasts for firms in a different industry simultaneously, and why they do not affect other analysts who cover the same firm at the same time.

Another implicit identifying assumption is that analyst coverage is exogenously given and orthogonal to analysts' earnings forecasts and coverage industries' performance. In practice, however, which firms or industries an analyst chooses to cover is certainly not random. I argue that the endogenous nature of analysts' coverage decisions is not likely to contaminate my results. Note that analysts tend to cover the same set of industries throughout their careers, because analysts have information advantages and social connections in industries in which they have experience and expertise, so it is costly for them to switch (Bradley et al., 2017). Thus, using the analyst $\times$ stock fixed effects mitigates the endogeneity concerns by controlling for this time-invariant heterogeneity. Fewer than 15% of the analysts in my sample have changed their industry coverage more than twice during the sample period, and excluding those analysts does not affect my results qualitatively. Moreover, studies on analyst coverage decisions (e.g., McNichols and O'Brien (1997) and Tehranian et al. (2013)) document that analysts are more likely to cover stocks about which they have favorable expectations. I also show in Appendix B that coverage initiation is not associated with more negative belief shocks. Therefore, analysts' endogenous coverage choices would, if anything, actually work against my findings on the effect of negative belief shocks.

2.4 Main Results

This section presents my main empirical results. I document that analysts issue significantly more pessimistic earnings forecasts when they observe (salient) negative performance of other coverage industries. These downward-biased forecasts are less accurate and lower than the actual earnings, which suggests that analysts overgeneralize negative shocks to other industries and become overly pessimistic about the state of the world.

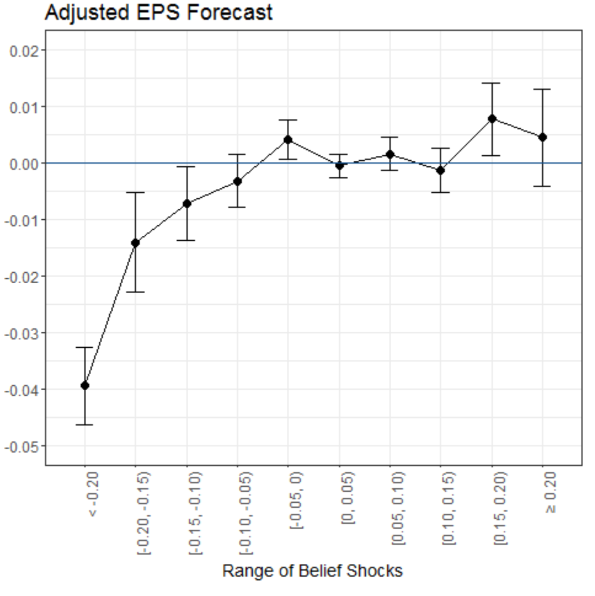
2.4.1 Earnings forecasts

While my main tests are designed to address identification issues, Figure 2.2 shows that the effect of belief shocks on analyst forecasts is pronounced even in the raw data. I divide the data into 10 subsamples based on the domain of belief shocks and compute the mean and the corresponding 90% confidence interval of the adjusted EPS forecasts in each subsample. Because EPS forecasts have been demeaned within each firm-quarter group, negative forecasts imply that analysts are more pessimistic relative to their peers covering the same firm at the same time. The plot displays a strong correlation between analyst forecasts and negative belief shocks. The more negative the belief shock is, the more negative the analyst forecast becomes, which implies that analysts tend to be more pessimistic relative to the consensus when other coverage industries perform worse. Interestingly, analysts seem to respond mostly to negative belief shocks. There is no clear correlation between forecasts and positive belief shocks.

To formally test the effect of belief shocks on analysts' earnings forecasts, I first estimate Equation (2.10) in Table 2.2. The dependent variable is adjusted EPS forecast, which is computed as in Equation (2.1). All of the specifications control for the stock $\times$ fiscal year-quarter fixed effects by demeaning all variables within firm-quarters to compare forecasts issued by *different analysts making forecasts for the same firm in the same quarter*. I also control

Figure 2.2: Analyst forecasts and belief shocks

This graph shows how analysts' adjusted EPS forecasts (y-axis), computed as in Equation (2.1), vary with the value of belief shocks (x-axis). I divide my sample into 10 subsamples based on the domain of the belief shocks. Belief shocks in the first subsample take values smaller than 0.20, belief shocks in the second subsample take on values in $[-0.20, -0.15)$, those in the third one take values in $[-0.15, -0.10)$, and so forth up to the tenth and final subsample taking values larger than 0.20. I plot how the average value of analyst forecasts varies across those subsamples. Error bars indicate 90% confidence intervals. Note that because analyst forecasts have been demeaned within each firm $\times$ fiscal year-quarter group, a negative value implies that an analyst is more pessimistic relative to her peers covering the same firm at the same time.



for an analyst's overall and firm-specific experience, the number of stocks and different Fama-French 49 industries covered by the analyst, the size of the analyst's brokerage house, and the calendar year-quarter fixed effects. In column (1), the coefficient on the belief shock variable is positive and statistically significant ($t = 2.998$), which implies that analysts observing more negative (positive) performance of other industries make significantly more pessimistic (optimistic) earnings forecasts. The coefficient estimate remains similar in column (2), where I additionally include the analyst $\times$ stock fixed effects to control for time-invariant but unobserved analyst characteristics such as talent, education, and industry expertise. Because the full sample contains all analysts, including those covering only one industry, one might be concerned about an unfair control group. In column (3), I resolve this issue by focusing on the subsample consisting only of analysts covering multiple industries. As is shown, the results are not sensitive to the sample selection. If anything, the coefficient on the belief shock variable even increases slightly.

To test the effects of positive and negative belief shocks separately, I estimate Equation (2.11) in columns (4) and (5). Column (5) includes analyst $\times$ stock fixed effects. It is interesting to note that the effect of belief shocks seems to come mostly from the negative performance of the other industries. The estimated coefficient on the negative shocks is large and statistically significant ($t = 2.823$ in column (5)), while that on the positive shocks is negligible and statistically insignificant. Because the negative shock variable only takes negative values, the positive coefficient implies that the analysts issue significantly lower forecasts in comparison to their peers when the performance of the other industries is worse (more negative returns). Going a step further, I replace the negative and positive belief shock variables, respectively, with the indicator variable of the bottom decile (D1) and the top decile (D10) of the belief shock variable in column (6). As is shown, only salient negative performance has a significant impact on analyst forecasts ($t = -3.643$).

The estimated effect of belief shocks is also economically meaningful. From column (5), for example, if analysts observe a negative performance of -10% from the other industries, their forecasts are about 2.7% standard deviations lower than the consensus, relative to other analysts whose coverage industries are not affected by such negative shocks. The estimate in column (6) suggests that upon a salient negative shock resulting in negative returns of -9% and lower (the bottom decile), affected analysts become on average about 4.5% more pessimistic about the firm's earnings relative to their peers.

Table 2.2 confirms the asymmetry shown in Figure 2.2 that the effect of positive belief shocks is negligible. This kind of asymmetry in the reaction to negative and positive shocks is, however, not exceptional. Studies in both economics and finance have provided abundant evidence that individuals or financial agents often react more or exclusively to negative news or shocks than

Table 2.2: Impact of belief shocks on analysts' EPS forecasts

This table reports the estimated effect of belief shocks on analysts' EPS forecasts. The dependent variable is adjusted EPS forecast, which is computed as in Equation (2.1). In column (2), I additionally include the analyst × stock fixed effects. Column (3) shows the results of the subsample consisting of only analysts following multiple industries. In columns (4) and (5), I estimate Equation (2.11) to examine the effects of positive and negative belief shocks separately. In column (6), the negative and positive belief shock variables are replaced with, respectively, the indicator variable of the bottom decile (D1) and top decile (D10) of the belief shock variable. Standard errors are two-way clustered at the calendar year-quarter level and analyst × stock level, and the corresponding *t*-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	Adjusted EPS Forecast					
	(1)	(2)	(3)	(4)	(5)	(6)
Belief Shock	0.151***	0.133***	0.176***			
	(2.988)	(2.705)	(3.243)			
Negative Shock				0.220**	0.266***	
				(2.437)	(2.823)	
Positive Shock				0.074	−0.019	
				(1.373)	(−0.369)	
D1						−0.045***
						(−3.643)
D10						0.001
						(0.253)
Overall experience	−0.003***	−0.002**	−0.003***	−0.003***	−0.002**	−0.002**
	(−5.179)	(−2.457)	(−3.076)	(−5.194)	(−2.471)	(−2.457)
Firm experience	0.002***	−0.003*	−0.003	0.002***	−0.003*	−0.003*
	(2.913)	(−1.750)	(−1.503)	(2.908)	(−1.757)	(−1.757)
Number of industries	−0.013**	0.000	0.001	−0.009	0.009	0.005
	(−2.465)	(0.060)	(0.129)	(−1.519)	(1.228)	(0.787)
Number of stocks	−0.003	−0.009	−0.010	−0.004	−0.009	−0.009
	(−0.698)	(−1.520)	(−1.492)	(−0.716)	(−1.601)	(−1.576)
Brokerage size	−0.022***	−0.015***	−0.012***	−0.022***	−0.015***	−0.015***
	(−10.528)	(−5.192)	(−3.581)	(−10.542)	(−5.174)	(−5.204)
Stock × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst × Stock FE	No	Yes	Yes	No	Yes	Yes
Sample	Full	Full	Mult. ind.	Full	Full	Full
Observations	1,423,192	1,423,192	1,178,422	1,423,192	1,423,192	1,423,192
R^2	0.001	0.194	0.201	0.001	0.194	0.194

to positive ones (e.g., Kahneman and Tversky, 1979; Barberis, Shleifer, and Vishny, 1998; Tetlock, 2007; Williams, 2014). Psychologists formally refer to this asymmetry as the negativity bias, and it has been investigated and verified in many different domains, especially the formation of impressions (e.g., Baumeister, Bratslavsky, Finkenauer, and de Vohs, 2001; Rozin and Royzman, 2001). Another potential explanation is that analysts tend to be overly optimistic by default (e.g., DeBondt and Thaler, 1990; Abarbanell and Bernard, 1992; Kothari et al., 2016a). Because analysts are already optimistic on average, even though they might respond to positive shocks and adjust their forecasts accordingly, their final forecasts might not deviate sufficiently from other analysts' optimistic forecasts for econometricians to detect any significant difference. The results on forecast revisions provided in Section 2.4.2 lend support to this conjecture. Given the trivial effect of positive belief shocks on the final forecast (see Tables 2.2 and 2.6), I focus on the effect of negative belief shocks henceforth.

In sum, my estimation results show that negative shocks to other coverage industries lead analysts to make significantly more pessimistic earnings forecasts, which is consistent with my conjecture that the performance of other coverage industries plays an important role in shaping analysts' expectations about focal firms' earnings. I next investigate the underlying mechanisms through which analyst forecasts are affected by those negative belief shocks.

2.4.2 Information versus noise

There may be two channels through which industry shocks can influence analysts' expectations. First, shocks to other industries may encompass useful information about the focal firms, and this information is only acquired by analysts covering those industries and is not accessible by other analysts. Covering a particular industry provides analysts with better access to material information, such as through conference calls with firm officials (Cohen et al., 2010). Industry expertise also gives analysts a competitive advantage in their ability to analyze the impacts of the industry shocks (Bradley et al., 2017). Analysts who do not cover the shocked industries may have limited attention and may therefore overlook the impacts of other industries' events on the focal firm (e.g., Hirshleifer and Teoh, 2003; Cohen and Frazzini, 2008). Consequently, analysts covering shocked industries have a comparative information advantage and incorporate superior information in their earnings forecasts. I refer to this channel as the *information* channel.

In contrast, the alternative channel follows the implications of a well-known cognitive bias called overgeneralization. Overgeneralization, also known as hasty generalization, is the tendency to draw a broad conclusion about a population based on evidence from a small sample group that does not accurately represent the entire population (e.g., Walton, 1999). Multi-tasking individuals with this cognitive bias may overgeneralize the outcomes of or experience with one task when making decisions for other tasks even though the outcomes of one task are not at all informative about the other tasks. It is noteworthy that the psychology literature has also found strong evidence for the asymmetric effect of positive and negative shocks: individuals tend to overgeneralize negative events much more than positive ones (e.g., Beck, 1979; Clark et al., 1999). Under this channel, even though the negative industry shocks do not sufficiently represent the whole economy or have any useful information about the focal firms, analysts might still heuristically overgeneralize those shocks to lower their expectations about economic conditions and therefore become more pessimistic about the focal firms. Consequently, the pessimistic forecasts made by affected analysts are driven by noise rather than information. I refer to this second channel as the *noise* channel.

To investigate whether analysts learn from the other industries' performance, the *information* channel, or whether they heuristically overgeneralize shocks to other industries, the *noise* channel, I first test the effect of belief shocks on analysts' forecast accuracy. The information channel predicts that analysts acquire superior information and therefore produce more accurate forecasts, whereas the noise channel predicts that analysts observing more salient performance would incorrectly adjust their expectations and thus produce less accurate forecasts.

Second, I estimate the effect of belief shocks resulting exclusively from arguably unrelated industries, i.e., industries that are neither horizontally nor vertically linked with the focal firm's industry. The information channel predicts that the effect of belief shocks on forecasts is no longer significant because analysts are less likely to acquire useful information from unrelated industries. In contrast, the noise channel still predicts a significant effect of belief shocks on forecasts because the overgeneralization also applies to unrelated industries.

Forecast accuracy

In Table 2.3, I first use signed forecast errors as the dependent variable to estimate the effect of negative belief shocks on analyst forecast accuracy. I consider signed forecast errors because it is important to detect whether forecast errors move in the same direction as the belief shocks. More specifically, does the negative performance of other industries indeed lead to less accurate forecasts that are below the actual realized earnings?

Column (1) estimates Equation (2.10) to test the overall effect of belief shocks on signed forecast errors, while column (2) estimates Equation (2.11) to examine the effects of positive and negative belief shocks separately, but only the coefficients on the negative shock are reported as those on the positive shock are negligible. All specifications include the stock $\times$ fiscal quarter, calendar quarter, and analyst $\times$ stock fixed effects. As is shown in column (2), the coefficients on the negative belief shock variable are positive and statistically significant, which suggests that analysts influenced by those negative shocks make significantly more negative forecast errors than those made by their peers. Estimating the effect from salient negative shocks in column (3) leads to the same conclusion.

Relatively more negative forecast errors could mean that their forecasts are significantly lower than the actual earnings realized by the firm, but it could also mean that their forecast errors are closer to zero when their peers make larger and more positive forecast errors. Note that the former would imply that analysts affected by negative shocks issue less accurate forecasts, whereas the latter would imply that they make more accurate forecasts. To disentangle these two confounding results, I use PMAFE, i.e., the absolute

Table 2.3: Impact of belief shocks on forecast accuracy

This table reports the effect of negative belief shocks on analysts' forecast accuracy. The dependent variable in columns (1) to (3) is the (signed) forecast error, which is computed as in Equation (2.3). The dependent variable in columns (4) to (6) is the PMAFE, which is computed as in Equation (2.2). In columns (1) and (4), I estimate Equation (2.10). In the other columns, I estimate Equation (2.11) to examine the effects of positive and negative belief shocks separately, but only report the coefficients on the negative shock, as those on the positive shock are negligible. Standard errors are two-way clustered at the calendar year-quarter and analyst × stock level, and the corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	Forecast Errors			PMAFE		
	(1)	(2)	(3)	(4)	(5)	(6)
Belief Shock	0.147***			−0.228***		
	(3.203)			(−6.819)		
Negative Shock		0.271***			−0.209***	
		(2.937)			(−5.549)	
D1			−0.050***			0.047***
			(−3.923)			(5.987)
Overall experience	−0.002**	−0.002**	−0.002**	−0.001	−0.001	−0.001
	(−2.177)	(−2.193)	(−2.179)	(−0.765)	(−0.767)	(−0.766)
Firm experience	−0.003**	−0.003**	−0.003**	−0.003***	−0.003***	−0.003***
	(−2.092)	(−2.104)	(−2.102)	(−2.767)	(−2.770)	(−2.747)
Number of industries	−0.002	0.007	0.003	0.010**	0.011**	0.006
	(−0.320)	(0.910)	(0.542)	(2.067)	(2.124)	(1.200)
Number of stocks	−0.010*	−0.010**	−0.010**	−0.011**	−0.012**	−0.011**
	(−1.905)	(−1.991)	(−1.977)	(−2.403)	(−2.409)	(−2.329)
Brokerage size	−0.014***	−0.014***	−0.014***	−0.005*	−0.005*	−0.005**
	(−5.671)	(−5.656)	(−5.682)	(−1.944)	(−1.940)	(−1.964)
Stock × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst × Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,423,192	1,423,192	1,423,192	1,423,192	1,423,192	1,423,192
R^2	0.187	0.187	0.187	0.170	0.170	0.170

forecast errors, as the dependent variable in column (4) to (6).

As shown, the coefficient on the negative belief shock variable in column (5) is negative and statistically significant, which suggests that the magnitude of the forecast errors increases significantly when the performance of the other industries are more negative. Analysts affected by negative belief shocks issue significantly less accurate earnings forecasts than their peers do. The coefficient estimate of -0.209 suggests that if other coverage industries experience a negative return of -10%, the forecast accuracy of affected analysts drops by about 2.1% relative to their counterparts, whose coverage industries do not experience such negative shocks. This effect is economically sizable, as analysts need about 7 years more of firm-specific experience to offset this inaccuracy.

To assess the effect of salient negative shocks, I focus on the coefficient of the bottom decile (D1) variable in column (6), which is 0.048 and statistically significant (t = 5.987). If analysts observe a salient negative performance from the other industries in the bottom decile (lower than -9%), their forecast accuracy is about 4.7% lower than that of their peers, which analysts need about 15.7 years more of firm-specific experience to offset.

Moreover, I find no evidence that those negative shocks encompass information that improve analysts' accuracy in the future, as lagged belief shocks have no significant impact on forecast errors (see Appendix C). Another potential concern is whether my findings are driven by analysts' overreaction to their past forecast errors. If analysts form diagnostic expectations (Bordalo, Gennaioli, Ma, and Shleifer, 2018a; Bordalo, Gennaioli, and Shleifer, 2018b), they might overcorrect to past forecast errors and make overpessimistic forecasts subsequently if their previous forecasts have been overoptimistic. I show in Appendix D that the effects of belief shocks are not affected by analysts' response to past forecast errors.

Overall, the results are strongly in favor of the noise hypothesis: analysts overgeneralize negative shocks in other industries and lower their expectations about firms' earnings incorrectly, resulting in inaccurately low earnings forecasts relative to the actual earnings. Inconsistent with the information hypothesis, analysts do not provide more accurate forecasts when they experience negative performance in other industries.

Unrelated industries

As discussed by Bradley et al. (2017), analysts with industry experience and expertise are often in limited supply. Even for brokers having analysts with related industry expertise, those analysts have only limited attention and may be too busy to cover new companies. The brokerage houses would therefore have to assign companies to analysts who are available but lack the related industry experience and knowledge. As a result, some analysts might cover firms in two or more unrelated industries.

This misallocation provides a natural setting to distinguish between the information channel and the noise channel. Namely, shocks to unrelated industries are less likely to encompass useful information about focal firms. The information hypothesis predicts that analysts would only respond to related industries' performance and not (or much less) to unrelated industries' performance. The noise hypothesis predicts that analysts would overgeneralize shocks to related and to unrelated industries and make less accurate forecasts regardless of the industry relatedness.

To identify industry relatedness, I use the three-digit NAICS codes as industry classification to compute the belief shock variable. This industry classification allows me to identify vertically linked industries, i.e., industries that have supplier or customer relationships. I detect possible economic links using the 2007 U.S. Input-Output Tables from the Bureau of Economic Analysis, which are based on the NAICS codes and which provide detailed information on the flows of the goods and services among industries[4]. I define supplier-customer industries as those industries with any flows to a given industry. In addition, I detect firm-level customer-supplier links by using the network relationships constructed by Barrot and Sauvagnat (2016). They obtain the identity of large customers of all public US firms, which, under regulation Statement of Financial Accounting Standards (SFAS) No. 131, are obliged

[4] I use the 2007 table of the commodities by industry valued at purchasers' prices under Use Tables/After Redefinitions/Purchaser Value (https://www.bea.gov/industry/io_annual.htm).

to report the identify of any customer representing more than 10% of total reported sales. Moreover, I use the Compustat Segment files to identify business and operating segments of conglomerate companies that have a different industry classification than the company's primary sector. Finally, to detect horizontal links in product markets, I utilize the 10-K text-based network industry classification (TNIC-3) data developed by Hoberg and Phillips (2016).[5]

Specifically, suppose that an analyst covers industries I_1 and I_2, with two different three-digit NAICS codes. When computing the belief shock of this analyst with respect to stock k in industry I_1, I consider industry I_2 *unrelated* if (1) I_1 and I_2 have no flows of goods and services with each other in the Input-Output table (no industry-level supplier-customer links); (2) firm k has no large customer in industry I_2 (no firm-level supplier-customer links); (3) firm k has no subsegment operating in industry I_2; and (4) firm k has no product market rivals in I_2 (i.e., none of the firms in I_2 is associated with firm k in the TNIC-3 database). Otherwise, I_2 is classified as a related industry for firm k. Figure 2.1 plots the percentage of analysts in the I/B/E/S database who cover companies in such unrelated industries. As is shown, around 20% of I/B/E/S analysts cover neither horizontally nor vertically linked industries. Because I only have TNIC-3 data for the period from 1996 to 2015, the following analysis focuses on this subperiod of my sample.

In Panel A of Table 2.4, I restrict the sample to the firms with at least one analyst who covers an unrelated industry, and I use the specification in columns (2), (5), and (6) from Table 2.2 and columns (1) to (3) from Table 2.3 to estimate the effect of the belief shocks resulting exclusively from unrelated industries. As is shown, the results are similar to the baseline results based on all other coverage industries: the estimated coefficient on the negative belief shock variables is significantly positive, while that on the indicator variable D1 of salient negative shocks is significantly negative, implying that negative shocks to unrelated coverage industries lead analysts to make incorrectly lower earnings forecasts for the focal firms. The magnitude and t-statistics of the

[5] I thank the authors of Barrot and Sauvagnat (2016) and Hoberg and Phillips (2016) for making their data publicly available.

Table 2.4: Impact of shocks to unrelated industries

This table shows how industry relatedness affects my results for the period from 1996 to 2015. In both panels, the dependent variables are the adjusted EPS forecast in columns (1) to (3) and forecast errors in columns (4) to (6). Panel A estimates the effect of belief shocks resulting exclusively from unrelated industries for firms with at least one analyst who covers an unrelated industry. Panel B estimates the effects of shocks to related and unrelated industries simultaneously for analysts who cover both related and unrelated industries. Standard errors are two-way clustered at the calendar year-quarter and analyst × stock level, and the t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

Panel A: Subsample of firms with at least one analyst covering an unrelated industry

	Adjusted EPS Forecast			Forecast Errors		
	(1)	(2)	(3)	(4)	(5)	(6)
Belief Shock (Unrelated)	0.024			0.030		
	(0.900)			(1.230)		
Negative Shock (Unrelated)		0.126^{*}			0.129^{**}	
		(1.699)			(2.016)	
D1 (Unrelated)			-0.027^{**}			-0.029^{***}
			(-2.521)			(-2.738)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst × Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,031,484	1,031,484	1,031,484	1,031,484	1,031,484	1,031,484
R^2	0.196	0.196	0.196	0.190	0.190	0.190

Panel B: Subsample of analysts who cover both related and unrelated industries

	Adjusted EPS Forecast			Forecast Errors		
	(1)	(2)	(3)	(4)	(5)	(6)
Belief Shock (Related)	0.090^{*}			0.095^{**}		
	(1.904)			(2.310)		
Belief Shock (Unrelated)	0.019			0.021		
	(0.756)			(1.009)		
Negative Shock (Related)		0.201^{**}			0.209^{**}	
		(2.031)			(2.377)	
Negative Shock (Unrelated)		0.108^{*}			0.098^{**}	
		(1.892)			(1.982)	
D1 (Related)			-0.038^{***}			-0.034^{**}
			(-2.698)			(-2.537)
D1 (Unrelated)			-0.025^{***}			-0.026^{***}
			(-2.619)			(-2.866)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst × Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	474,943	474,943	474,943	474,943	474,943	474,943
R^2	0.160	0.160	0.160	0.151	0.151	0.151

coefficients are smaller than those in the baseline, which is not surprising because the effects of belief shocks from related industries are omitted in this setting. Nevertheless, the results in Panel A provides evidence in support of the noise hypothesis: even though negative shocks to unrelated industries are not likely to be informative about the focal firms, they still influence analysts' expectations for the focal firms.

An interesting follow-up question is how analysts respond differently to the performance of related and unrelated industries. Is it possible that the information hypothesis applies to related coverage industries while the noise hypothesis applies to unrelated ones? In Panel B of Table 2.4, I focus on a subsample of analysts who cover related and unrelated industries, and estimate their response to belief shocks from related and unrelated industries separately. The specifications are the same as in Panel A.

As is shown, negative shocks to both related and unrelated coverage industries lead analysts to make inaccurate pessimistic forecasts, which is difficult to reconcile with the information hypothesis. Despite the substantial reduction in the sample size, the statistical significances of negative belief shocks from unrelated industries still increases slightly relative to those in Panel A, after controlling for the effects of belief shocks from related industries. The coefficient estimates of belief shocks from related industries are larger than those from unrelated industries, which suggests that analysts' expectations are more influenced by related industries' performance. This findings is reasonable because analysts are more likely to believe that shocks from related industries are informative about the focal firms than those from unrelated industries. However, unreported tests of the equality of coefficients indicate that the difference in the two coefficients is not statistically significant in any of the specifications. The effects of belief shocks from related and unrelated industries seem to add up to the total effects from our baseline results in Tables 2.2 and 2.3.

Taken together, the results in this section suggest that the negative performance of other coverage industries leads analysts to incorrectly lower their expectations and produce less accurate forecasts. Belief shocks from both related and unrelated industries seem to only provide noise rather than useful information about the focal firms. These findings lend support to the noise channel and soundly reject the information channel. Analysts heuristically overgeneralize negative shocks to their other coverage industries, become more pessimistic about the state of the world, and therefore issue downward-biased earnings forecasts relative to their peers who do not cover the shocked industries.

Forecast revisions

One potential concern is whether my results are driven by analyst distraction. Kempf et al. (2017), for example, study investor distraction by using extreme positive and negative industry returns as a proxy for attention-grabbing events. One may argue that shocks to other industries do not influence analysts' expectations, but rather distract their attention from the focal firms. As a result, distracted analysts issue relatively conservative earnings forecasts that turn out to be less accurate. This argument has difficulty explaining why analysts are not distracted by salient positive industry performance, and it is difficult to reconcile with the large body of literature showing that, if anything, sell-side analysts are optimistic by default. Nevertheless, I formally investigate the possibility of analyst distraction by examining analyst forecast revisions.

If analysts were just distracted by other coverage industries with extreme returns, they would allocate less effort to the coverage firm and revise their forecasts less frequently than usual. In Table 2.5, I estimate Equation (2.11) in columns (1) and (2) with the total number of revisions as the dependent variable. The belief shock variables are computed here over the period from the earnings announcement date of fiscal quarter $t-1$ to that of fiscal quarter t. As is shown, neither negative nor positive extreme performance of other coverage industries affects analyst revisions significantly. Thus, there is no evidence that analysts spend less effort on the coverage firms or revise their forecasts less often.

In addition, I use the following specification to estimate the effect of belief

Table 2.5: Impact of belief shocks on forecast revisions

This table reports the effect of belief shocks on analysts' forecast revisions. The dependent variable is the number of forecast revisions issued by analyst i for firm j regarding fiscal year-quarter t in columns (1) and (2), the magnitude of forecast revision, which is measured as the standardized unexpected forecast (SUF) computed as in Stickel (1992a) in columns (3) and (4), and the cumulative abnormal returns around the announcement of analyst i's forecast revision in columns (5) and (6). Standard errors are two-way clustered at the calendar year-quarter and analyst $\times$ stock level, and the corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	Number of Revisions		Forecast Revision		CAR(0,1)	
	(1)	(2)	(3)	(4)	(5)	(6)
Negative Shock	0.013		0.467***		−0.004	
	(0.573)		(5.013)		(−0.663)	
Positive Shock	−0.019		0.293***		0.002	
	(−0.713)		(3.930)		(0.536)	
D1		−0.004		−0.063***		0.001
		(−0.831)		(−7.092)		(1.411)
D10		−0.001		0.027***		0.000
		(−0.170)		(2.788)		(0.552)
Forecast revision					0.002***	0.002***
					(11.699)	(11.677)
Overall experience	−0.001	−0.002**	0.002	0.002	0.000	0.000
	(−1.325)	(−2.095)	(1.184)	(1.195)	(0.645)	(0.643)
Firm experience	0.016***	0.015***	−0.003	−0.003	−0.000	−0.000
	(11.862)	(12.166)	(−1.160)	(−1.154)	(−0.784)	(−0.782)
Number of industries	−0.015***	−0.016***	−0.002	−0.000	−0.000	−0.000
	(−3.121)	(−3.631)	(−0.155)	(−0.031)	(−0.110)	(−0.079)
Number of stocks	0.031***	0.044***	−0.010	−0.010	−0.000	−0.000
	(6.904)	(9.124)	(−0.998)	(−1.009)	(−0.026)	(−0.037)
Brokerage size	0.029***	0.035***	0.008*	0.008*	0.000	0.000
	(10.519)	(12.569)	(1.686)	(1.677)	(0.328)	(0.333)
Stock × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst × Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,178,422	1,423,192	315,792	315,814	315,792	315,814
R^2	0.219	0.225	0.250	0.249	0.314	0.314

shocks on the magnitude of analyst j's r-th forecast revision for firm i in fiscal year-quarter t:

$$\widetilde{SUF}_{ijtr} = \alpha_{q(t)} + \alpha_{ij} + \beta_1 \times \widetilde{BS}^{-}_{ijt[r-1,r]} + \beta_2 \times \widetilde{BS}^{+}_{ijt[r-1,r]} + \gamma' \widetilde{X}_{ijt} + \widetilde{\varepsilon}_{ijtr}, \quad (2.14)$$

where r indexes forecast revisions (within each analyst-firm-fiscal year-quarter pair). The dependent variable is the standardized unexpected forecast (SUF) computed as in Stickel (1992a) and Malloy (2005) to measure revision magnitude. The belief shock variables are computed as described in section 2.3.2 but over the window between the announcement date of an analyst's most recent forecast r and the announcement date of her previous forecast $r-1$ for the same firm and fiscal quarter. All of the variables are demeaned within firm-quarters to control for the stock $\times$ fiscal year-quarter fixed effects. The sample contains 314,742 forecast revisions. The estimation results are shown in columns (3) and (4) of Table 2.5. The direction and magnitude of analyst forecast revisions are strongly associated with the (salient) performance of other coverage industries: analysts revise their forecasts downwards (upwards) significantly more when the other industries experience sizable negative (positive) shocks. This finding contradicts the notion of analyst inattention, as distracted analysts would not incorporate other industries' shocks into their revisions.

In columns (5) and (6), I examine the stock price impact of forecast revisions associated with belief shocks by estimating Equation (2.14) with the three-day cumulative abnormal returns around the forecast revision announcement date as the dependent variable. This test provides additional evidence to distinguish whether belief shocks encompass information or noise. The forecast revisions made by analysts affected by belief shocks would have a greater impact on stock prices if those analysts bring valuable information to the market. As is shown, conditional on the direction and magnitude of the revisions, belief shocks do not significantly affect the market reactions, which again does not support the information channel. Moreover, the insignificant coefficients on belief shocks suggest that investors do not unravel the biases in analysts' forecasts resulting from belief shocks.

The findings in Table 2.5 also confirm the asymmetric effect of negative and positive belief shocks, although they suggest a less extreme version. In particular, analysts also revise forecasts upwards following positive belief shocks despite the significantly smaller magnitude. This finding lends support to the notion that as analysts are already optimistic on average, even though they might respond to positive shocks and revise their forecast upwards, their final forecasts might not differ sufficiently from other analysts' optimistic forecasts for econometricians to detect any significant difference. The asymmetric effect of negative and positive belief shocks is probably not due only to the negativity bias, but also to the analysts' average optimism.

2.4.3 Robustness

In this section, I show that my main findings are robust to alternative weighting schemes when constructing the belief shock variable, to different subperiods in my sample, and to alternative industry classifications. Table 2.6 presents the results of the robustness tests. In Panel A, I reestimate the specification in columns (2), (5), and (6) from Table 2.2 with the adjusted EPS forecast as the dependent variable. In Panel B, I reestimate the specification in columns (1) to (3) from Table 2.3 with signed forecast errors as the dependent variable.

First, I estimate the effect of belief shocks computed by equally weighting analysts' coverage industries in Equation (2.6), which yields coefficient estimates of similar statistical significance and even slightly larger magnitude.

Second, I divide the main sample period into four subperiods: before and after Regulation Fair Disclosure (Reg FD), the 2008 financial crisis period, and the post-crisis period. Reg FD, which was ratified by the SEC in 2000, prohibited selective information disclosure by firms to a subset of analysts and thus could affect analysts' private information, such as their personal connections to the management (e.g., Cohen et al., 2010). As is shown, the results in the pre- and post-Reg FD periods and in the post-crisis period are similar to the baseline. However, during the financial crisis, analysts seem to only respond to salient negative industry performance. One likely explanation is that analysts were already very pessimistic about the economy because of the

Table 2.6: Robustness tests

In Panel A, the baseline estimates in the first row refer to columns (2), (5), and (6) from Table 2.2, and I reestimate the corresponding specifications in those columns with the adjusted EPS forecast as the dependent variable. In Panel B, the baseline estimates refer to columns (1) to (3) from Table 2.3, and I reestimate the corresponding specifications in those columns with signed forecast errors as the dependent variable. I first present the results of belief shocks computed by using equal weighting for the industries in Equation (2.6). I then show the coefficient estimates when restricting the sample to four different subperiods. Furthermore, I consider five alternative industry classifications: Fama-French 12 industries, GICS industries (three-digit), industries based on two-digit SICH codes, and the Hoberg and Phillips (2016) 10-K text-based 50 industry classification (FIC-50). I compute the belief shock variable using those five alternative industry definitions. For brevity, I only present coefficients of interest, and I suppress the control variables. Standard errors are two-way clustered at the calendar year-quarter and analyst × stock level, and the corresponding *t*-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

Panel A: EPS forecast as the dependent variable

	Table 2.2: (2)	Table 2.2: (5)		Table 2.2: (6)		
	Belief Shock	Negative Shock	Positive Shock	D1	D10	Obs.
Baseline (FF49)	0.133***	0.266***	-0.019	-0.045***	0.001	1,423,192
	(2.705)	(2.823)	(-0.369)	(-3.643)	(0.253)	
Alternative weight:						
Equal-weighting (FF49)	0.138**	0.284***	-0.033	-0.047***	-0.001	1,423,192
	(2.548)	(2.735)	(-0.588)	(-3.281)	(-0.167)	
Subperiods:						
Pre-Reg FD: 1993-2000	0.111*	0.239**	0.019	-0.035*	0.007	281,574
	(1.711)	(2.118)	(0.289)	(-1.936)	(0.568)	
Post-Reg FD: 2001-2006	0.116	0.378***	-0.188	-0.050**	-0.003	344,654
	(1.563)	(2.687)	(-1.596)	(-2.317)	(-0.216)	
Financial crisis: 2007-2009	0.130	0.219	-0.077	-0.052*	-0.011	223,218
	(1.414)	(1.624)	(-0.819)	(-2.001)	(-0.818)	
Post-crisis: 2010-2016	0.168***	0.265**	0.094	-0.038***	0.007	573,746
	(2.709)	(2.285)	(1.327)	(-2.868)	(0.851)	
Alternative industry classifications:						
Fama-French 12	0.123**	0.239*	-0.004	-0.022*	0.003	1,423,192
	(2.095)	(1.959)	(0.064)	(-1.745)	(0.405)	
Three-digit GICS	0.125***	0.303***	-0.002	-0.033***	-0.004	1,409,349
	(3.084)	(3.082)	(-0.067)	(-3.043)	(-0.764)	
Two-digit SIC	0.151***	0.346**	0.013	-0.045***	0.001	1,423,192
	(2.697)	(2.377)	(0.264)	(-3.538)	(0.217)	
Hoberg-Phillips 50	0.121**	0.342***	-0.033	-0.038***	-0.004	1,242,909
	(2.408)	(2.793)	(-0.718)	(-3.092)	(-0.493)	

Panel B: Forecast errors as the dependent variable

	Table 2.3: (1)	Table 2.3: (2)		Table 2.3: (3)		
	Belief Shock	Negative Shock	Positive Shock	D1	D10	Obs.
Baseline (FF49)	0.147***	0.271***	0.005	-0.050***	0.001	1,423,192
	(3.203)	(2.937)	(0.103)	(-3.923)	(0.274)	
Alternative weight:						
Equal-weighting (FF49)	0.156***	0.291***	-0.001	-0.051***	0.001	1,423,192
	(3.118)	(2.887)	(-0.021)	(-3.608)	(0.215)	
Subperiods:						
Pre-Reg FD: 1993-2000	0.097	0.266**	-0.024	-0.042**	0.000	281,574
	(1.572)	(2.166)	(-0.305)	(-2.139)	(0.026)	
Post-Reg FD: 2001-2006	0.177**	0.423***	-0.109	-0.061***	0.008	344,654
	(2.378)	(3.024)	(-1.021)	(-2.798)	(0.623)	
Financial crisis: 2007-2009	0.117	0.171	-0.007	-0.045*	-0.009	223,218
	(1.489)	(1.438)	(-0.102)	(-1.903)	(-0.872)	
Post-crisis: 2010-2016	0.190***	0.320***	0.092	-0.046***	0.003	573,746
	(3.270)	(2.969)	(1.403)	(-3.269)	(0.449)	
Alternative industry classifications:						
Fama-French 12	0.146***	0.283**	-0.003	-0.029**	-0.001	1,423,192
	(2.725)	(2.421)	(-0.046)	(-2.422)	(-0.100)	
Three-digit GICS	0.135***	0.312***	0.009	-0.039***	-0.003	1,409,349
	(3.513)	(3.212)	(0.306)	(-3.757)	(-0.648)	
Two-digit SIC	0.170***	0.375***	0.025	-0.048***	0.003	1,423,192
	(3.382)	(2.861)	(0.575)	(-3.836)	(0.524)	
Hoberg-Phillips 50	0.128***	0.317***	-0.005	-0.042***	-0.005	1,242,909
	(2.738)	(2.722)	(0.118)	(-3.372)	(-0.786)	

crisis. Thus, only extremely negative signals could change their perspective, making them even more pessimistic, which is consistent with the main findings. In sum, the estimation results in the subperiods provide important evidence showing that my main findings are persistent over time and not solely driven by extreme negative events such as the financial crisis.

Furthermore, I consider five alternative industry classifications: Fama-French 12 industries, the three-digit Global Industry Classification Standard (GICS) industries, industries based on two-digit SICH codes, and the Hoberg and Phillips (2016) 10-K text-based 50 industry classification (FIC-50). I compute the belief shock variable using each of these five alternative industry definitions. As is shown in Table 2.6, the statistical significance and magnitude of the point estimates are qualitatively the same as those of my baseline results.

Therefore, my findings are not likely to be driven by a particular industry (mis)classification or by measurement errors.

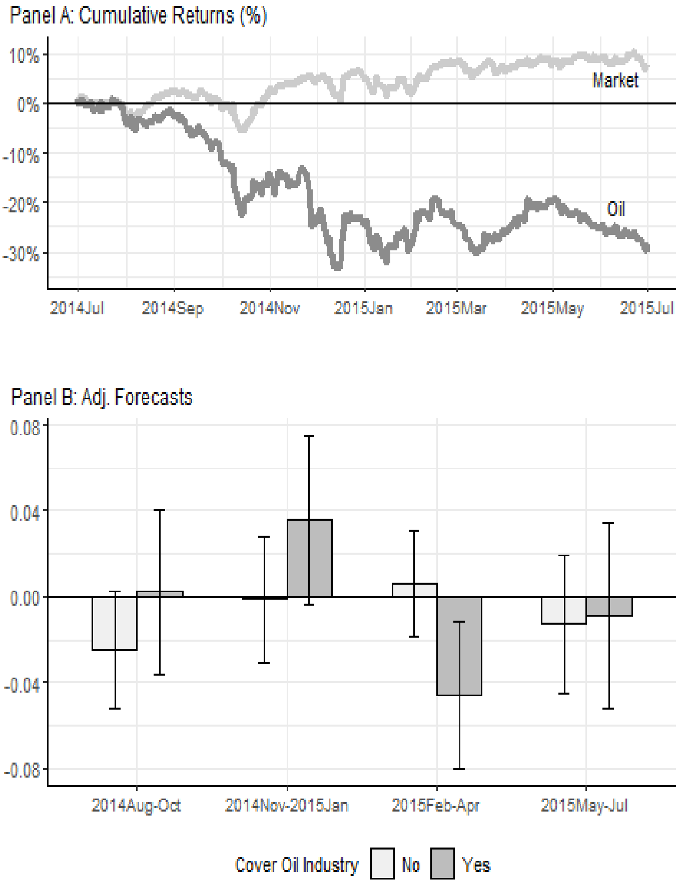
2.5 An Exogenous Industry Shock: the Oil Price Crash in 2014-15

The findings in this paper so far provide strong and robust evidence that negative shocks to other coverage industries make analysts overpessimistic about the focal firms. This evidence is plausibly causal because (1) industry shocks are unlikely to correlate with analyst-specific characteristics; (2) my identification approach compares forecasts made by different analysts for the same firm at the same time, ensuring that differences in firms' fundamentals cannot explain the results; and (3) I include analyst $\times$ stock fixed effects to control for unobserved time-invariant differences across analysts covering the same firm. But even then, one may still argue that (a) analysts' forecasts can affect the firm policies of industry leaders and thereby influence the performance of the overall industry (*reverse causality*), and (b) the belief shock variable fails to capture industry shocks adequately (*measurement errors*). Even though it is difficult to conceive how an individual analyst can cause changes in corporate practices that are immediately value-destroying for the entire industry, or how industry returns can systematically misspecify industry shocks, I further strengthen my identification by exploiting an exogenous industry shock that is clearly orthogonal to analyst opinions.

The oil industry is full of booms and busts. After four years of relative stability, the crude oil price experiences a dramatic decline in 2014-15: from a peak of \$115 per barrel in June 2014, the oil price plunges below \$50 by the end of January 2015. This sharp fall puts severe economic stress on the U.S. oil companies and drives the entire industry (Fama-French 49-industry code 30) down by about 30%, as illustrated in Panel A of Figure 2.3. However, the downturn remains mostly industry-specific and does not have any significant negative spillovers on other industries. Indeed, the overall market actually increases slightly by about 5% over the same period. This contrast

Figure 2.3: Oil price shock in 2014-15 and analyst forecasts

This graph shows how analyst forecasts change amid the oil price shock in 2014-15. Panel A plots the daily cumulative returns of the market portfolio and the oil industry portfolio (Fama-French 49-industry code 30) from 7/1/2014 to 6/30/2015. Panel B plots the average adjusted EPS forecasts made for non-oil firms and issued during each of the four sub-periods from 8/1/2014 to 7/31/2015, separately based on whether the analyst covers the oil industry in both 2014 and 2015. I exclude firms without any analyst covering the oil industry. I also plot the corresponding 90% confidence intervals. Note that because the forecasts have been demeaned within each firm×fiscal year-quarter group, a negative value implies that an analyst is more pessimistic relative to her peers covering the same firm at the same time.



provides a useful case to test whether analysts form divergent expectations following the different performance of their other coverage industries. More importantly, because this crash in oil prices is mainly due to the oversupply from unconventional oil sources, the weakening global demand, and OPEC's renouncement of price support, the resulting negative shock to the oil industry is totally exogenous to analysts' forecasts and recommendations, which addresses the concern of reverse causality.[6]

I use a difference-in-differences (DD) approach and define the treatment group as analysts who cover the oil industry in both 2014 and 2015. The sample is restricted to forecasts made between 8/1/2014 and 7/31/2015 and for *non-oil* firms covered by at least one analyst in the treatment group. I further exclude analysts who have changed industry coverage between 2014 and 2015, ensuring that the results are not contaminated by analysts' endogenous coverage choices. These criteria yield 590 firms with 254 analysts in the treatment group and 951 analysts in the control group.

In Panel B of Figure 2.3, I plot the average adjusted EPS forecasts for each of the four sub-periods from 8/1/2014 to 7/31/2015 across the firms in this subsample, separately based on whether the analyst covers the oil industry. The adjusted forecasts are demeaned within firm-quarters to control for differences in firms' fundamentals. Even in this univariate comparison, the figure shows a clear pattern: before the price crash materially hits the oil industry, analysts in the treatment and control groups make similar forecasts. After the shock (2015 Feb-Apr), analysts in the treatment group become significantly more pessimistic about the focal firms, relative to their peers who do not cover the oil industry. As the forecasts made during 2015 May-Jul are again similar between analysts in the treatment and control groups, this industry shock appears to have a short-lived effect on analysts' beliefs, which is consistent with the baseline results.

In Table 2.7, I test the impact of this oil industry shock on analyst expectations more formally by estimating the following regression specification:

$$\widetilde{y}_{ijt} = \alpha + \beta_1 Cover\,Oil_i \times Post_t + \beta_2 Cover\,Oil_i + \beta_3 Post_t + \gamma' \widetilde{X}_{ijt} + \varepsilon_{ijt}, \quad (2.15)$$

where β_1 gives the DD-estimator, which is expected to be negative if the dependent variable is the adjusted EPS forecast in columns (1-2) and signed forecast

[6] For example, see Arezki, Rabah, and Olivier Blanchard (2015), "The 2014 Oil Price Slump: Seven Key Questions", VoxEU.org

errors in columns (3-4), and positive if the dependent variable is PMAFE in columns (5-6). The control variables are the same as those in Table 2.2 and 2.3. *Post* is a dummy variable that equals one if a forecast is issued between 2/1/2015 and 4/30/2015, indicating the "after shock" period. I cluster the standard errors at the analyst × stock level to account for potential serial correlations (Bertrand, Duflo, and Mullainathan, 2004).

The results confirm the hypothesis that the negative oil industry shock leads analysts who cover oil companies to incorrectly lower expectations for focal firms in other industries. The DD estimate has the expected sign and is statistically significant in all columns. Column (1) shows that analysts who cover the oil industry become about 7.6% more pessimistic about the focal firms after the shock, relative to the peer analysts who do not cover the oil industry. Column (3) and (5) show that their pessimistic forecasts are lower than actual earnings and about 4.4% less accurate. In the other columns, when I further restrict the sample to only IT firms (Fama-French 49-industry codes 32-38), which is arguably unrelated to the oil industry, I still find that analysts who cover the oil industry have significantly lowered their expectations and made overpessimistic forecasts. Therefore, industry relatedness and spillovers are unlikely to confound my results.

Taken together, the evidence presented in this section strengthens the baseline results: negative shocks to one coverage industry makes analysts overpessimistic about firms in other industries. In this particular case, the severe negative shock to the oil industry caused by the oil price crash significantly affects the earnings forecasts made by the treatment analysts for non-oil firms. Because the source of this industry shock is known and exogenous to the opinions of analysts who cover oil companies, the results suggest that the effect of industry shocks on analyst expectations is causal.

2.6 Additional Analysis

The results thus far are consistent with the notion that other coverage industries' negative performance lowers analysts' expectations about the state of

Table 2.7: Impact of oil price shock on analyst forecasts

This table reports the difference-in-differences estimates of the impact of oil price shock in 2014-15 on analysts' earnings forecasts. *Cover Oil* is a dummy variable that equals one if an analyst covers the oil industry in both 2014 and 2015. *Post* is a dummy variable that equals one if a forecast is issued between 2/1/2015 and 4/30/2015. The control variables are demeaned within firm-quarters. The dependent variable is the adjusted EPS forecast in columns (1-2), the signed forecast errors in columns (3-4), and the absolute value of forecast errors in columns (5-6). I restrict the sample to forecasts made between 8/1/2014 and 7/31/2015. In columns (1), (3), and (5), I focus on all firms except those operating in the oil industry itself (Fama-French 49-industry code 30). In columns (2), (4), and (6), I focus on only IT firms (Fama-French 49-industry codes 32-38). Standard errors are clustered at the analyst $\times$ stock level, and the corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	Adjusted EPS Forecast		Forecast Errors		PMAFE	
	(1)	(2)	(3)	(4)	(5)	(6)
Cover Oil × Post	−0.076**	−0.275**	−0.062**	−0.213**	0.044**	0.118*
	(−2.564)	(−2.379)	(−2.418)	(−2.332)	(2.029)	(1.678)
Cover Oil	0.028	0.220***	0.030*	0.178***	−0.010	−0.004
	(1.436)	(2.938)	(1.764)	(3.146)	(−0.813)	(−0.096)
Post	0.019	0.087	0.017	0.040	−0.021*	−0.033
	(1.094)	(1.461)	(1.140)	(0.883)	(−1.665)	(−0.945)
Overall experience	0.001	0.005	0.002	0.007	0.002**	0.001
	(0.578)	(0.800)	(1.432)	(1.451)	(2.097)	(0.327)
Firm experience	0.000	−0.009	0.000	−0.005	−0.004**	−0.005
	(0.003)	(−0.812)	(0.134)	(−0.581)	(−2.269)	(−0.833)
Number of industries	−0.066**	−0.375***	−0.048*	−0.230**	−0.029	−0.117*
	(−2.115)	(−2.944)	(−1.831)	(−2.466)	(−1.408)	(−1.688)
Number of stocks	0.019	0.065	0.008	−0.015	0.030*	0.038
	(0.713)	(0.636)	(0.357)	(−0.190)	(1.818)	(0.605)
Brokerage size	−0.021**	−0.089**	−0.009	−0.042	0.015**	0.021
	(−1.960)	(−2.373)	(−1.077)	(−1.534)	(2.236)	(1.002)
Constant	−0.014	−0.068*	−0.015	−0.040	−0.003	−0.001
	(−1.248)	(−1.769)	(−1.621)	(−1.370)	(−0.357)	(−0.046)
Demeaned controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Excl. Oil	Only IT	Excl. Oil	Only IT	Excl. Oil	Only IT
Observations	17,639	1,390	17,639	1,390	17,639	1,390
R^2	0.001	0.024	0.001	0.020	0.001	0.006

the world. Yet given the large heterogeneity of firms and analysts, the effect of belief shocks might vary for different types of analysts covering different firms. Moreover, because analysts probably hold diverse prior beliefs about each industry and only cover a particular set of firms within each industry, they might not respond identically to belief shocks coming from different coverage industries. In this section, I exploit the heterogeneity in firms and analysts' coverage portfolios to further assess how other industries' performance influences analysts' belief-forming process.

2.6.1 Coverage firms with high information asymmetry

I first try to isolate firm types for which the effect of negative belief shocks is particularly stark. Following the idea that analysts rely more on their private information when the coverage firm is opaque and difficult to analyze, I divide my original sample into firms with different levels of information asymmetry. More specifically, I break down the sample in three ways: firms in high-tech industries (code 3 in Fama-French 5 industries) versus firms in other industries, small firms (with below median market capitalization) versus big firms, and young firms (went IPO in less than 10 years) versus mature firms. Table 2.8 reports the estimated effects for each subset.

In Panel A, I reestimate the specification of column (5) from Table 2.2 for each subsample, with adjusted EPS forecasts as dependent variables. As is shown, the coefficient estimate on negative belief shocks is slightly larger for small and young firms, and it is particularly stronger for stocks in high-tech industries. A negative belief shock of -10% leads analysts covering high-tech stocks to become 4.3% more pessimistic relative to the baseline of 2.7%. A similar pattern emerges from the estimation results in Panel B, where I reestimate the specification of column (2) from Table 2.3 with forecast errors as dependent variables. These findings lend support to the notion that analysts covering opaque firms (especially high-tech firms) are more influenced by the negative performance of other coverage industries relative to other analysts.

2.6.2 Analysts with different numbers of coverage industries

My baseline results suggest that analysts covering two or more industries tend to overgeneralize the negative performance of the other industries. To examine

Table 2.8: Heterogeneous effects of negative belief shocks

This table reports the heterogeneous effects of negative belief shocks on analysts' expectations. I divide the sample into firms with different levels of information asymmetry. Specifically, I break down my sample in three ways: firms in high-tech industries (code 3 in Fama-French 5 industries) versus firms in other industries, small firms (with below-median market capitalization) versus big firms, and young firms (with IPO in less than 10 years) versus mature firms. I reestimate the specification of column (5) from Table 2.2 in Panel A and column (2) from Table 2.3 in Panel B. For brevity, I only report the coefficient estimates of the negative belief shocks, and I suppress the coefficient estimates of the positive shocks and control variables. All of the regression models are estimated with stock × fiscal-year quarter (demeaning variables within firm-quarters), calendar year-quarter, and analyst × stock fixed effects for each subsample. Standard errors are two-way clustered at the calendar year-quarter and analyst × stock level. The corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

Panel A: Adjusted EPS forecast as dependent variable

	Subsample					
	High Tech	Other	Small	Big	Young	Mature
	(1)	(2)	(3)	(4)	(5)	(6)
Negative Shock	0.429***	0.209**	0.269***	0.262***	0.297***	0.257***
	(3.703)	(2.236)	(2.723)	(2.802)	(2.821)	(2.652)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Stock × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst × Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	324,045	1,099,147	711,424	711,768	450,763	972,429
R^2	0.236	0.182	0.213	0.176	0.252	0.185

Panel B: Forecast errors as dependent variable

	Subsample					
	High Tech	Other	Small	Big	Young	Mature
	(1)	(2)	(3)	(4)	(5)	(6)
Negative Shock	0.411***	0.223**	0.267***	0.273***	0.355***	0.242**
	(3.245)	(2.560)	(2.762)	(2.954)	(3.188)	(2.571)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Stock × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst × Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	324,045	1,099,147	711,424	711,768	450,763	972,429
R^2	0.228	0.176	0.208	0.168	0.245	0.178

whether this varies with analysts' portfolio complexity, I split the sample based on the number of industries an analyst covers and reestimate the specification in column (5) of Table 2.2 for each subsample.

In Panel A of Table 2.9, the dependent variable is the adjusted EPS forecast, computed as in Equation (2.1). I first use the full sample to demean all of the independent variables within each firm-quarter, to control for stock × fiscal year-quarter fixed effects, and then estimate the regression models with those demeaned variables, calendar year-quarter, and analyst × stock fixed effects for each subsample. As is shown, all of the point estimates on the negative belief shocks are positive and statistically significant, which suggests that all analysts with multiple coverage industries lower their expectations because of belief shocks, even those covering only two industries.

Similar to Panel A, I divide the sample and reestimate the same specification in column (5) from Table 2.3 with forecast errors as the dependent variable in Panel B. The estimation results in Panel B confirm the baseline findings. Analysts affected by more negative belief shocks produce more negative forecast errors. The effect size is similar to the baseline results and is statistically significant regardless of the number of industries that an analyst covers. There is no clear pattern of effect size increasing over all coverage industries.

To summarize, I have shown that analysts covering multiple industries overgeneralize and consequently make less accurate earnings forecasts, including those covering only two industries. This finding is also interesting from practitioners' perspective. Due to a lack of supply of industry-experienced analysts, brokerage houses face the trade-off between the costs and benefits of assigning non-industry experts (Bradley et al., 2017). One could consider overgeneralization as a potential cost of delegating analysts more industries to cover even though they might be more talented than others.

2.6.3 "Expected" versus "unexpected" belief shocks

As noted above, analysts might have different interpretations of the signals coming from different coverage industries because of their heterogeneous prior

Table 2.9: Analysts with different numbers of coverage industries

This table reports the heterogeneous effects of negative belief shocks on analysts covering different numbers of industries. In both panels, I split the sample based on the number of industries an analyst covers, and I reestimate the specification of column (5) from Table 2.2 in Panel A and column (2) from Table 2.3 in Panel B. For brevity, I only report the coefficient estimates of the negative belief shocks, and suppress the coefficient estimates of the positive shocks and control variables. I first use the full sample to demean all of the independent variables within each firm-quarter to control for stock × fiscal year-quarter fixed effects and then estimate the regression models with those demeaned variables, calendar year-quarter, and analyst × stock fixed effects for each subsample. Standard errors are two-way clustered at the calendar year-quarter and analyst × stock level. The corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

Panel A: Adjusted EPS forecast as the dependent variable

	Number of industries					
	$n = 2$	$n = 3$	$n = 4$	$n = 5$	$n = 6$	$n \geq 7$
Negative Shock	0.298***	0.401**	0.359***	0.509***	0.282*	0.347**
	(3.483)	(2.573)	(2.721)	(3.280)	(1.727)	(2.020)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Stock × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst × Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	319,300	249,656	203,437	147,843	97,967	160,219
R^2	0.267	0.292	0.300	0.305	0.312	0.244

Panel B: Forecast errors as the dependent variable

	Number of industries					
	$n = 2$	$n = 3$	$n = 4$	$n = 5$	$n = 6$	$n \geq 7$
Negative Shock	0.300***	0.420***	0.343**	0.463***	0.388***	0.324**
	(3.815)	(2.840)	(2.516)	(2.971)	(2.641)	(2.218)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Stock × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst × Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	319,300	249,656	203,437	147,843	97,967	160,219
R^2	0.262	0.286	0.294	0.295	0.306	0.237

beliefs about each industry. Analysts might be more likely to change their beliefs when the shocks are unexpected and different from their priors. Recall the example from the introduction: analyst A might have reacted less to the negative shock in the transportation industry if he had already foreseen this shock. However, it is empirically difficult to observe analysts' prior beliefs about these industry shocks.

In Panel A of Table 2.10, I use analysts' earnings surprises as a weak proxy for their prior beliefs and decompose belief shocks into expected and unexpected components. Earnings surprises indicate that it is plausible that the industry shock is not anticipated by the analyst. As earnings surprises are only at the firm level and are probably correlated with unobserved analysts' private signals and therefore might contaminate the coefficient estimates, the results based on this proxy should be interpreted with this caveat in mind. I compute the average earnings surprises across firms in a given industry and identify industries with surprises if the shock and the average earnings surprise have the same sign. Likewise, industries without surprises are those in which the average earnings surprise has a different sign than that of the industry shock.

Using decomposed belief shocks and repeating the analysis from Tables 2.2 and 2.3, I find that both expected and unexpected shocks significantly influence analyst beliefs, with similar magnitudes. Combining the effects of anticipated and unanticipated belief shocks seems to recover the coefficient estimates in the corresponding columns from Tables 2.2 and 2.3. The finding that expected belief shocks also affect analyst forecasts is interesting and suggests that analysts seem to overgeneralize industry-wide shocks rather than firm-level idiosyncratic shocks. Thus, this result supports my approach of using industry-level performance rather than firm-level performance to construct belief shocks.

2.6.4 Industry shocks versus idiosyncratic shocks

As explained before, I use industry-level performance to construct the baseline belief shock variable for identification purposes. In practice, however, analysts

Table 2.10: Belief shocks and earnings surprises

In Panel A of this table, I decompose belief shocks into one component capturing shocks to industries in which the analyst was on average surprised by the actual earnings and another component capturing shocks to industries in which the analyst was not surprised. In Panel B, I contrast industry shocks with firm-level idiosyncratic shocks by including belief shock variables based on firm-level stock market performance as additional control variables to my baseline specifications with belief shock variables based on industry-level performance. In both panels, the dependent variable is the EPS forecast in columns (1) to (3) and forecast errors in columns (4) to (6). The specifications correspond to those in columns (2), (5), and (6) from Table 2.2 and those in columns (1) to (3) from 2.3. All of the specifications include the stock × fiscal year-quarter (by demeaning all of the variables within firm-quarters), calendar year-quarter, and analyst × stock fixed effects. Standard errors are two-way clustered at the calendar year-quarter and analyst × stock level, and the corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

Panel A: Whether the analyst has earnings surprises in the shocked industries

	Adjusted EPS Forecast			Forecast Errors		
	(1)	(2)	(3)	(4)	(5)	(6)
Belief Shock (w/ surprise)	0.081***			0.084***		
	(2.956)			(3.308)		
Belief Shock (w/o surprise)	0.076**			0.085***		
	(2.167)			(2.665)		
Negative Shock (w/ surprise)		0.136***			0.140***	
		(2.861)			(2.980)	
Negative Shock (w/o surprise)		0.147**			0.147**	
		(2.225)			(2.382)	
D1 (w/ surprise)			−0.026***			−0.030***
			(−2.829)			(−3.354)
D1 (w/o surprise)			−0.031***			−0.032***
			(−3.084)			(−3.323)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Stock × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst × Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,423,192	1,423,192	1,423,192	1,423,192	1,423,192	1,423,192
R^2	0.194	0.194	0.194	0.187	0.187	0.187

Panel B: Industry shocks versus idiosyncratic firm-level shocks

	Adjusted EPS Forecast			Forecast Errors		
	(1)	(2)	(3)	(4)	(5)	(6)
Belief Shock (industry-level)	0.076			0.095**		
	(1.619)			(2.157)		
Belief Shock (stock-level)	0.063***			0.057***		
	(5.993)			(6.603)		
Negative Shock (industry-level)		0.190**			0.201**	
		(2.128)			(2.252)	
Negative Shock (stock-level)		0.080***			0.075***	
		(4.748)			(5.405)	
D1 (industry-level)			−0.034***			−0.039***
			(−3.077)			(−3.349)
D1 (stock-level)			−0.035***			−0.035***
			(−5.386)			(−6.361)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Stock × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst × Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,423,192	1,423,192	1,423,192	1,423,192	1,423,192	1,423,192
R^2	0.194	0.194	0.194	0.187	0.187	0.187

cover a particular set of firms within each industry, rather than the entire industry. Because an industry shock affects some firms more than others, analysts covering different firms within the same industry might have diverse perceptions of the shock. Specifically, does a negative industry shock still make analysts more pessimistic when their coverage firms within that industry are actually performing well?

To address this question, I additionally construct a belief shock variable as in Equation (2.6) by value-weighting the stock market performance of analysts' individual coverage firms. In Panel B of Table 2.10, I include stock-level belief shock variables as additional control variables to my baseline specifications with the industry-level belief shocks from Tables 2.2 and 2.3. Controlling for stock-level belief shock variables allows me to examine how analysts respond to the same industry shocks differently regarding the performance of their coverage firms. If analysts only pay attention to industry shocks that substantially affect their coverage firms, the coefficients on industry-level belief shocks would no longer be significant.

Compared to the estimates in Tables 2.2 and 2.3, the coefficients on the baseline industry-level belief shocks are smaller in magnitude, but most remain statistically significant. The coefficients on stock-level belief shocks are in the same direction as those on industry-level variables, with higher statistical significance but smaller magnitudes, which suggests that a negative industry shock lowers an analyst's belief more if her coverage firms in that industry are materially affected by the shock and perform poorly. The effect of industry shocks is still present but diminishes if the coverage firms are not or are less affected. The larger magnitude of industry-level belief shocks relative to stock-level shocks lends further support to the notion that analysts are more likely to overgeneralize industry-wide shocks rather than firm-level idiosyncratic shocks when forming expectations about firms in other industries. Note that combining the effects of industry- and stock-level belief shocks seems to recover the baseline estimates in the corresponding columns from Tables 2.2 and 2.3.

The higher statistical significance of stock-level belief shocks also indicates that analysts' expectations covary more closely with the performance of coverage firms. In an unreported test in which I replace industry-level belief shocks with stock-level belief shocks, the estimated effects of firm-level performance on analysts' beliefs are larger in both statistical significance and economic magnitude. Nevertheless, as discussed before, the effect of firm-level performance cannot be cleanly identified, as it is difficult to rule out reverse causality and other potential confounding factors that drive firm performance and analyst expectation simultaneously.

2.6.5 Does experience or the brokerage house mitigate overgeneralization?

Another interesting question is what factors mitigate the impact of overgeneralization, which results in analyst inaccuracy. I test two candidates: experience and brokerage firm size. As analysts gain experience, they could become better at analyzing firms' financial reports, identifying business cycles, and teasing out noise from information. Clement (1999a) and others have provided empirical evidence showing that forecast accuracy increases with experience.

Table 2.11: Influence of analyst experience and brokerage house

This table shows whether analysts' experience or employers reduces the impact of overgeneralization. I reestimate the specifications of column (5) from Table 2.2 and columns (2) and (5) from Table 2.3, additionally interact the negative belief shock variable with, respectively, analysts' overall experience, firm experience, and brokerage firm size. ***, **, and * denote significance at 1%, 5%, and 10%.

	Adjusted EPS Forecast			Forecast Errors			PMAFE		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Negative Shock	0.235**	0.260***	0.212*	0.274***	0.272***	0.264***	−0.226***	−0.216***	−0.223***
	(2.308)	(2.802)	(1.928)	(2.733)	(2.895)	(2.678)	(−5.012)	(−5.746)	(−3.712)
Negative Shock × Overall experience	0.005			−0.001			0.003		
	(1.184)			(−0.130)			(1.090)		
Negative Shock × Firm experience		0.002			−0.000			0.002	
		(0.347)			(−0.096)			(0.586)	
Negative Shock × Broker size			0.014			0.002			0.004
			(0.770)			(0.128)			(0.264)
Overall experience	−0.002**	−0.002**	−0.002**	−0.002**	−0.002**	−0.002**	−0.000	−0.001	−0.001
	(−2.341)	(−2.471)	(−2.473)	(−2.253)	(−2.193)	(−2.195)	(−0.656)	(−0.768)	(−0.768)
Firm experience	−0.003*	−0.003*	−0.003*	−0.003**	−0.003**	−0.003**	−0.003***	−0.003***	−0.003***
	(−1.754)	(−1.708)	(−1.759)	(−2.103)	(−2.052)	(−2.105)	(−2.768)	(−2.704)	(−2.769)
Brokerage size	−0.015***	−0.015***	−0.015***	−0.014***	−0.014***	−0.015***	−0.005*	−0.005*	−0.005*
	(−5.183)	(−5.174)	(−4.399)	(−5.656)	(−5.655)	(−5.320)	(−1.944)	(−1.943)	(−1.855)
Number of industries	0.009	0.009	0.009	0.007	0.007	0.007	0.011**	0.011**	0.011**
	(1.227)	(1.228)	(1.228)	(0.910)	(0.908)	(0.910)	(2.123)	(2.129)	(2.125)
Number of stocks	−0.009	−0.009	−0.009	−0.010**	−0.010**	−0.010**	−0.012**	−0.012**	−0.012**
	(−1.605)	(−1.601)	(−1.598)	(−1.989)	(−1.990)	(−1.987)	(−2.411)	(−2.410)	(−2.408)
Stock × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Analyst × Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,423,192	1,423,192	1,423,192	1,423,192	1,423,192	1,423,192	1,423,192	1,423,192	1,423,192
R^2	0.194	0.194	0.194	0.187	0.187	0.187	0.170	0.170	0.170

Furthermore, analysts employed by bigger (often more prestigious) brokerage firms are provided with more resources, such as more research assistance from junior analysts and access to soft information through privileged house calls with management, that enable the analysts to filter out more noise from other industries. Table 2.11 shows the results of my test of whether analysts' experience or employer reduces the impact of overgeneralization. I reestimate the specifications of column (5) from Table 2.2 and columns (2) and (5) from Table 2.3, and I interact the negative belief shock variable with analysts' overall experience, firm-specific experience, and the size of the brokerage firm.

If analysts with more experience or those who work for a bigger broker house are less likely to overgeneralize negative performance of the other industries, the coefficient on the interaction terms would be statistically significant and have an opposite sign than that of the negative belief shock variable. As is shown in Table 2.11, however, the estimated coefficient on all of the interaction terms turns out to be negligible. While more experienced analysts employed by larger brokerage houses are on average more accurate, these characteristics do not prevent them from overgeneralizing negative shocks to other coverage industries.

2.7 Conclusion

This paper exploits the diversity of industries that analysts cover, which is common to sell-side equity analysts. I test the hypothesis that the performances of other coverage industries play an important role in shaping analysts' expectations about the state of the world and thereby influence their earnings forecasts for their focal firms.

My main finding is that negative shocks to other coverage industries make analysts more pessimistic about the focal firms. When investigating whether these industry shocks provide analysts with additional valuable information or merely noise, I find strong evidence for the latter. Analysts' pessimistic forecasts turn out to be less accurate and much lower than the realized earnings, which suggests that analysts overgeneralize negative shocks from other coverage industries and unnecessarily lower their expectations about the focal firms.

The results in this paper not only introduce a new determinant of heterogeneous beliefs among financial analysts but also provide a broader implication for studying the decision-making process of multi-tasking agents, who might overgeneralize their experience with or outcome of one task when making decisions for other tasks. Moreover, because overgeneralization leads analysts to lower expectations because of other industries' performance, which is arguably unrelated to the fundamentals of focal firms, this heuristic essentially provides exogenous variation in analysts' disagreement and pessimism. I further exploit this insight in the next chapter.

2.A Appendix

A Variable Descriptions

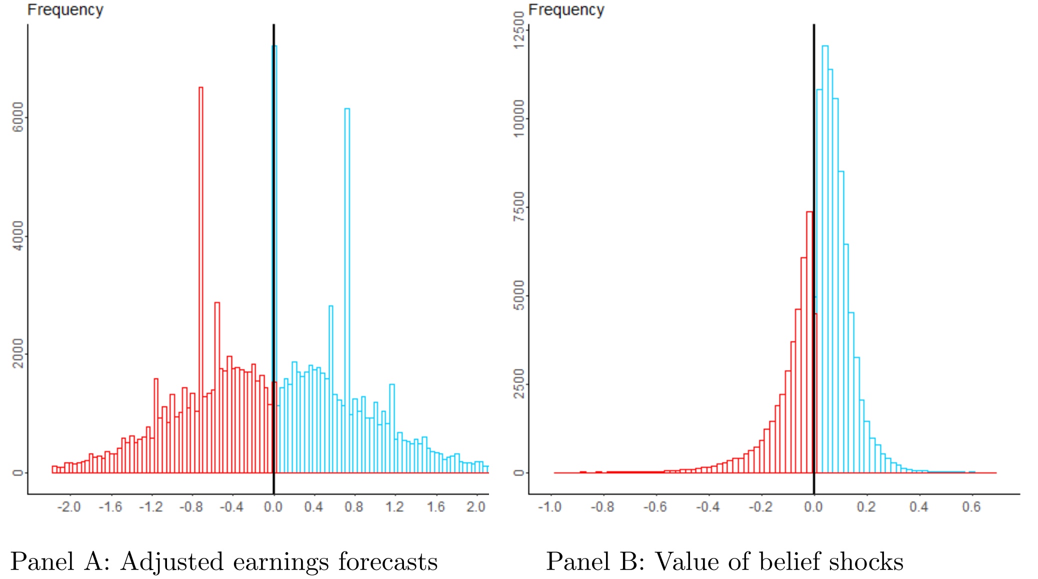
Table 2.A.1: Variable descriptions

Variable	Description
Dependent variables	
EPS forecast	Earnings per share forecasts demeaned and scaled within each firm-fiscal quarter group, computed as in Equation (2.1) (winsorized at the 1% and 99% levels).
Forecast errors	Raw eps forecast minus actual earnings, demeaned and scaled within each firm-fiscal quarter group, computed as in Equation (2.3) (winsorized at the 1% and 99% levels).
PMAFE	Absolute value of forecast errors, demeaned and scaled within each firm-fiscal quarter group, computed as in Equation (2.2) (winsorized at the 1% and 99% levels).
Number of revisions	Logarithm of one plus the total number of forecasts issued by the same analyst for the same firm and fiscal quarter.
Forecast revision	Magnitude of forecast revision measured as the standardized unexpected forecast (SUF) computed as in (Stickel, 1992a) (winsorized at the 1% and 99% levels).
CAR(0, 1)	Market-adjusted cumulative announcement return over the window (0, 1) around the analyst's revision date (winsorized at the 1% and 99% levels).
Explanatory variables	
Belief Shock	Shocks to analyst belief, constructed using Fama-French 49 industries (see Section 2.3.2).
Belief Shock (EW)	Belief shocks constructed using Fama-French 49 industry and equal-weighting.
Belief Shock (Unrelated)	Belief shocks constructed using three-digit NAICS industries, which are arguably unrelated to the industry of the focal firm, as explained in section 2.4.2.
Belief Shock (Related)	Belief shocks constructed using related three-digit NAICS industries, as explained in section 2.4.2.
Control variables	
Overall experience	The overall experience computed as the number of years between an analyst's current earnings forecast and his/her first forecast for any firm.
Firm experience	The firm-specific experience computed as the number of years between an analyst's current earnings forecast and his/her first forecast covering a given stock.
Number of stocks	Logarithm of one plus the number of stocks covered by the analyst in a given year.
Broker size	Logarithm of one plus the number of analysts employed by the brokerage house in a calendar year plus one.
Number of industries	Logarithm of one plus the number of Fama-French 49 industries covered by the analyst in a given year.

B Initial coverage and belief shocks

This appendix provides evidence supporting the identification assumption that analysts' coverage decisions are not driven by bad performance of the other coverage industries. I first identify coverage initiated as when an analyst issues her first earnings forecast on a particular stock (since 1995), and I then examine the distribution of these initial forecasts adjusted within firm-quarters and the corresponding belief shocks prior to those forecasts. If my results are driven by pessimistic analysts initiating coverage following negative belief shocks, I would observe that (1) analysts' initial forecasts are more pessimistic relative to their peers; and (2) more of the corresponding belief shocks take negative values. Figure 2.A.1 depicts the histogram of the forecasts and belief shocks at initial coverage, respectively. As is shown, while there is no obvious bias in the initial forecasts, the corresponding belief shocks are biased towards positive values. In fact, about 48.7% of the initial forecasts are relatively pessimistic, whereas only 34.2% of the belief shocks are negative. This finding suggests that, even though analysts endogenously choose coverage, this selection is unlikely to contaminate my results.

Figure 2.A.1: Histogram of the adjusted earnings forecasts and belief shocks at initial coverage



Panel A: Adjusted earnings forecasts

Panel B: Value of belief shocks

C Effects of lagged belief shocks

The results in Table 2.3 suggest that analysts make less accurate forecasts, which is more in line with the noise channel than with the information channel. However, it may be that the information that analysts acquire takes time to influence focal firms, which will help analysts make forecasts in the future. In this case, even though their current forecasts are inaccurate (which is the analysts' mistake for using the information too soon), their future forecasts would be more accurate. To test this possibility, I examine the effects of lagged belief shocks on analysts' EPS forecasts and forecast errors in Table 2.A.2. As is shown, lagged belief shocks have no significant impact on analysts' forecasts or their accuracy, which is inconsistent with the notion that analysts learn information from industry shocks that is useful in the future.

Table 2.A.2: Lagged belief shocks

This table reports the effects of lagged belief shocks on analysts' EPS forecasts and forecast errors. The dependent variables are the EPS forecast in columns (1-4), signed forecast errors in columns (4-8), and PMAFE columns (9-12). I reestimate specification (5) from Table 2.2, and columns (2) and (5) from Table 2.3 by replacing current belief shocks with lagged belief shock variables. For brevity, I only report the coefficients on the lagged negative shock variables, as those on the positive shocks are negligible. All of the specifications control for the stock × fiscal year-quarter (by demeaning all variables), calendar year-quarter, and analyst × stock fixed effects. Standard errors are two-way clustered at the calendar year-quarter and analyst × stock level, and the corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	Adjusted EPS Forecast				Forecast Errors				PMAFE			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Negative Shock (Lag 1)	−0.003			−0.048	−0.021			−0.055	−0.009			−0.001
	(−0.147)			(−1.250)	(−1.070)			(−1.640)	(−0.413)			(−0.027)
Negative Shock (Lag 2)		0.054		0.057		0.047		0.042		0.023		0.002
		(1.419)		(1.444)		(1.399)		(1.164)		(0.936)		(0.062)
Negative Shock (Lag 3)			−0.016	−0.014			−0.020	−0.020			−0.004	−0.000
			(−0.486)	(−0.387)			(−0.737)	(−0.658)			(−0.167)	(−0.021)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stock × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Analyst × Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,032,928	779,799	602,726	602,726	1,032,928	779,799	602,726	602,726	1,032,928	779,799	602,726	602,726
R^2	0.213	0.224	0.232	0.232	0.206	0.217	0.224	0.224	0.186	0.195	0.202	0.202

D Overreaction to past forecast errors

The results in Tables 2.2 and 2.3 imply that negative shocks to other industries lead analysts to make incorrectly pessimistic forecasts for the focal firms. However, one potential concern is that this finding is driven by analysts who overreact to their past forecast errors. To illustrate the idea, recall the example from the introduction. If the negative shock to the transportation industry also disrupts the earnings of COAL Corp in the previous quarters (2011Q1-Q2), which surprises analyst A who is overoptimistic about COAL (assuming analyst B is not surprised), analyst A might overreact to this individual signal and become subsequently overpessimistic about COAL in 2011Q3. This overcorrection could for example happen if analyst A forms a diagnostic expectation (Bordalo et al., 2018a,b). As a result, his forecast is negatively correlated with his previous forecast errors, and my results might capture this correlation.

To address this concern, I additionally control for the forecast errors from the previous fiscal quarters in column (5) from Table 2.2 and in column (2) from Table 2.3. The results are shown in Table 2.A.3. The estimated coefficients on two- to four-quarter lagged forecast errors are significantly negative, implying that analysts make more pessimistic forecasts if their forecasts for the previous fiscal quarters were overoptimistic, and vice versa. This is consistent with the implications of diagnostic expectations that analysts overcorrect their past forecast errors. Nevertheless, after controlling for overcorrection to past forecast errors, the magnitude and statistical significance of the coefficients on belief shock variables are virtually the same as those in the baseline, which suggests that my main results are not driven by analysts' overcorrection to past errors.

Table 2.A.3: Controlling for the effects of past forecast errors

This table reports the effects of belief shocks on adjusted EPS forecast in columns (1-5) and on signed forecast errors in columns (6-10), after controlling for the effects of past forecast errors. Lag X indicates the X-quarter lagged forecast errors. Standard errors are two-way clustered at the calendar year-quarter and analyst × stock level, and the corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	Adjusted EPS Forecast					Forecast Errors				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Negative Shock	0.270***	0.276***	0.273***	0.279***	0.278***	0.265***	0.263***	0.262***	0.263***	0.261***
	(2.787)	(2.867)	(2.782)	(2.695)	(2.683)	(2.792)	(2.834)	(2.752)	(2.620)	(2.609)
Positive Shock	−0.025	−0.031	−0.032	−0.035	−0.033	−0.005	−0.015	−0.017	−0.010	−0.009
	(−0.480)	(−0.574)	(−0.600)	(−0.653)	(−0.621)	(−0.117)	(−0.324)	(−0.361)	(−0.213)	(−0.187)
Forecast errors (Lag 1)	0.010***				0.015***	−0.007**				−0.002
	(2.806)				(4.260)	(−2.032)				(−0.566)
Forecast errors (Lag 2)		−0.021***			−0.020***		−0.029***			−0.028***
		(−8.917)			(−8.433)		(−12.974)			(−11.713)
Forecast errors (Lag 3)			−0.020***		−0.018***			−0.025***		−0.024***
			(−9.704)		(−8.813)			(−12.982)		(−11.784)
Forecast errors (Lag 4)				−0.015***	−0.015***				−0.020***	−0.020***
				(−8.049)	(−7.760)				(−11.331)	(−10.981)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stock × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Analyst × Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,266,166	1,129,442	1,011,345	909,153	909,153	1,266,166	1,129,442	1,011,345	909,153	909,153
R^2	0.194	0.192	0.190	0.187	0.188	0.187	0.184	0.181	0.177	0.178

Chapter 3

Disagreement, Volatility, and Mispricing[1]

3.1 Introduction

Many investors rely on analysts' forecasts to evaluate firms' future prospects and make trading decisions. The vast literature on the market effects of analysts has found abundant evidence that their forecasts are able to move stock prices (Kothari, So, and Verdi, 2016a). However, different analysts often make divergent forecasts for the same firm at the same time, which may lead investors to hold heterogeneous beliefs about the company and, consequently, may affect the coverage company's trading activities and price movements. Given my findings in the previous chapter that analysts overgeneralize news from other coverage industries and therefore make different forecasts, I hypothesize that this heuristic potentially have more profound effects on financial markets. In this chapter, I first develop a simple trading model to demonstrate how analyst belief shocks could induce trading volume and return volatility, and I then provide empirical evidence conforming to the theoretical predictions.

To derive testable predictions, I follow Harris and Raviv (1993) and Kandel and Pearson (1995) to set up a three-period trading model featuring investors

[1] This chapter is also based on Renjie (2019).

with heterogeneous beliefs. Each type of investors update their beliefs about the underlying risky asset using the forecast of a particular analyst. The key friction is that, while analysts' forecasts may be biased (e.g., because of the belief shocks from the other coverage industries), investors do not discover the potential bias in the forecasts. The model generates two main predictions. First, because dispersion in analyst belief shocks induces analyst disagreement, it leads to higher trading volume and larger return volatility. Second, negative belief shocks lead analysts to make overly pessimistic forecasts, which could exert downward price pressure and induce underpricing.

Next, I take these predictions to the data and find strong evidence consistent with both predictions. First, I find that after controlling for common trends and time-invariant firm heterogeneity, a one-standard-deviation increase in belief shock dispersion translates to 6.4%-8.1% more analyst disagreement about the coverage firms and is associated with up to 13.7% higher daily trading volume and 5.9% larger stock return volatility. Analyst overgeneralization seems to aggravate information asymmetries and increase uncertainty about firms' fundamentals.

Second, firms with more analysts affected by negative belief shocks experience a significant decline in stock price prior to earnings announcements. This downward price pressure effect is more pronounced for firms with *ex-ante* higher information asymmetry. Consistent with the underpricing prediction that the price will reverse when the true information is revealed, a one-standard-deviation more negative belief shocks is associated with a 63.6% higher positive reversal upon earnings announcements relative to the average, conditional on the direction and magnitude of earnings surprises. This effect is about 84.5% for firms with high information asymmetry. These findings imply that analyst overgeneralization has substantial effects on financial markets.

This chapter contributes to the literature that links analyst disagreement to heterogeneous beliefs. Models based on investors with heterogeneous beliefs can explain asset price movements and trading volumes (e.g., Miller, 1977; Harris and Raviv, 1993; Kandel and Pearson, 1995). However, it is difficult to directly measure investors' beliefs. In light of analysts' role as key information intermediaries, many empirical studies, such as Diether, Malloy, and Scherbina (2002), use the dispersion of analysts' earnings forecasts as an empirical proxy for differences in opinions among investors and study how analyst disagreement affects asset returns and trading activities. By exploiting the effect of overgeneralization on analyst disagreement, I put forward a new channel that helps explain both the cross-sectional and time-series variation in analyst disagreement.

In addition, my results provide some implications for other strands of literature. Overgeneralization leads analysts to adjust expectations for reasons that are not related to the fundamental values of focal firms. In other words, this heuristic essentially provides an exogenous variation in analyst disagreement, which can be used to construct instrumental variables for analyst or investor disagreement at the firm level. Moreover, as the resulting analyst disagreement in turn affects stock trading volumes and return volatilities, and the resulting analyst overpessimism leads to temporary underpricing, one could also use overgeneralization to construct instrumental variables for trading activities, volatilities, and mispricing. This insight could be useful for future empirical research on related topics.

The remainder of this chapter is organized as follows. Section 3.2 develops testable predictions. Section 3.3 discusses the data and empirical methodology, and presents the descriptive statistics. Section 3.4 provides the empirical evidence, and Section 3.5 concludes.

3.2 Model

3.2.1 Setup

This simple model follows the setups in Harris and Raviv (1993) and Kandel and Pearson (1995): (1) There is a risk-free asset with a zero rate of return and a risky security with an uncertain payoff R. (2) There are three time periods: at time 1, investors form prior beliefs about the value of the asset; at time 2, they update their beliefs according to analyst forecasts; at time 3, the value of the risky asset is realized and investors consume their wealth. (3) There is a continuum of investors with total mass equal to 1 who maximize mean-variance utility $\mathbb{E}_{i,t}[W_i] - \frac{\lambda}{2}\mathrm{Var}_{i,t}[W_i]$, where λ is the coefficient of absolute risk aversion. (4) There are two types of investors indexed by $i = 1, 2$. They hold prior beliefs that the return R is normally distributed with mean R_i and precision $h_0 = \sigma_0^{-2}$. A proportion α of traders are of type 1; without loss of generality, they are assumed to initially be more optimistic, $R_1 > R_2$. (5) There are two analysts indexed by $i = 1, 2$ who make forecasts for the risky asset R. Their forecasts are given by $F_i = R + \varepsilon_i$, where $\varepsilon_i \sim \mathcal{N}(s_i, h_\varepsilon = \sigma_\varepsilon^{-2})$. The distribution of the noise term ε_i models that analyst i overgeneralizes signal s_i from other coverage industries and makes a biased forecast for R. (6) At time 2, type i investors update their beliefs using analyst i's forecast F_i. However, they trust that analysts make an unbiased forecast for R. That is, they believe that $\varepsilon_i \sim \mathcal{N}(0, h_\varepsilon = \sigma_\varepsilon^{-2})$.

There are two key frictions in this model. The first is that type i investors update their beliefs in a naive Bayesian manner and only update beliefs with respect to analyst i's forecast, without taking into account the information sets and actions of others. In particular, at time 1, they do not take into account that at time 2, prices will be "incorrect" because the other agents are updating their beliefs and trading using different information based on the other analyst's forecast. This assumption is standard in the literature of speculative trading and information diffusion (see Kandel and Pearson (1995), Hong and Stein (1999), and Hirshleifer and Teoh (2003)). The second friction is that investors are unable to debias analyst research and are therefore misled by biased forecasts, which is supported by my results on the market reaction to analysts' revisions. Studies such as Jackson (2005) also provide empirical evidence supporting this assumption.

3.2.2 Equilibrium prices and investor holdings

At time 1, each type i investor optimizes her mean-variance preference

$$\max_{q_{i,1}} \mathbb{E}_{i,1}\left[q_{i,1}(R - P_1)\right] - \frac{\lambda}{2}\mathrm{Var}_{i,t}\left[q_{i,1}(R - P_1)\right].$$

So each type i investor demands

$$q_{i,1}(P_1) = \frac{h_0}{\lambda}(R_i - P_1)$$

of the risky asset. The aggregate demands are $Q_{1,1}(P_1) = \alpha q_{1,1}(P_1)$ and $Q_{2,1}(P_1) = (1-\alpha)q_{2,1}(P_1)$. The market-clearing condition implies that the total demand from all investors must equal the zero net supply. Hence, the market-clearing equilibrium price is

$$P_1^* = \alpha R_1 + (1-\alpha)R_2 = \overline{R}, \tag{3.1}$$

and the equilibrium holdings are

$$q_{1,1}^* = \frac{h_0}{\lambda}(1-\alpha)\Delta R \quad \text{and} \quad q_{2,1}^* = -\frac{h_0}{\lambda}\alpha\Delta R, \tag{3.2}$$

where $\Delta R = R_1 - R_2$. By assumption, $\Delta R > 0$, and the aggregate supply of securities is zero. The second type holds a short position, $q_{2,1}^* < 0$.

After analysts 1 and 2 have issued forecasts F_1 and F_2 at time 2, investors update their beliefs and resume trading. The posterior beliefs of type i investors are given by a normal distribution with mean

$$\mathbb{E}_{i,2}[R|F_i] = \frac{h_0}{h_0 + h_\varepsilon}R_i + \frac{h_\varepsilon}{h_0 + h_\varepsilon}F_i.$$

Similar to period 1, optimizing investors' mean-variance preferences and using the market-clearing condition gives the equilibrium price at time 2:

$$P_2^* = \frac{h_0\overline{R} + h_\varepsilon\overline{F}}{h_0 + h_\varepsilon}, \tag{3.3}$$

where $\overline{F} = \alpha F_1 + (1-\alpha)F_2$. Likewise, equilibrium holdings at time 2 are given by

$$q_{1,2}^* = \frac{(1-\alpha)}{\lambda}(h_0\Delta R + h_\varepsilon\Delta F) \quad \text{and} \quad q_{2,2}^* = -\frac{\alpha}{\lambda}(h_0\Delta R + h_\varepsilon\Delta F), \tag{3.4}$$

where $\Delta F = F_1 - F_2 = \varepsilon_1 - \varepsilon_2 = \Delta\varepsilon$ indicates the difference in analysts' opinions.

3.2.3 Prediction regarding asset volatility and trading volume

When I compare the prices in (3.1) and (3.3), it is clear that the absolute price change between the two periods depends linearly on the new information of analyst forecasts and on the difference in analysts' opinions,

$$|\Delta P^*| = |P_2^* - P_1^*| = \frac{h_\varepsilon}{h_0 + h_\varepsilon}|F_2 - P_1^*| + \frac{\alpha h_\varepsilon}{h_0 + h_\varepsilon}|\Delta\varepsilon|. \tag{3.5}$$

Because a larger fluctuation of the security price is equivalent to higher return volatility, a greater dispersion in analyst opinions could increase the return volatility of the underlying risky asset.

Furthermore, calculating the change in the equilibrium holdings in (3.2) and (3.4), I can show that it is also linearly related to the difference in analyst forecasts. Because the net supply of the risky security is assumed to be zero, the absolute value of the change in the aggregate holdings by type i investors represents the trading volume in period 2. Taking the absolute difference between (3.2) and (3.4) yields the trading volume

$$TV = |Q_{1,1}^* - Q_{1,2}^*| = \frac{\alpha(1-\alpha)h_\varepsilon}{\lambda}|\Delta\varepsilon|. \tag{3.6}$$

The trading volume is therefore also proportional to the difference in analysts' opinions.

Recall that analyst i's bias ε_i is assumed to depend on her belief shock s_i from the other coverage industries, namely, $\mathbb{E}[\varepsilon_i] = s_i$, which implies that $\mathbb{E}[|\Delta\varepsilon|] = |s_1 - s_2| = |\Delta s|$. This leads to an important empirical prediction regarding the impact of analysts' belief shocks on financial markets.

Prediction. *If a larger dispersion in analysts' belief shocks amplifies the difference in analysts' forecasts, it would increase the return volatility and trading volume of the risky security.*

3.2.4 Prediction regarding asset returns

In this simple model, the expected price change depends on the new information in analysts' forecasts $F_{1,2}$, which are essentially determined by the signals (or belief shocks) $s_{1,2}$:

$$\mathbb{E}[P_2^* - P_1^*] = \frac{h_\varepsilon}{h_0 + h_\varepsilon}\mathbb{E}[\overline{F} - \overline{R}] = \frac{h_\varepsilon}{h_0 + h_\varepsilon}\overline{s}, \tag{3.7}$$

where $\overline{s} = \alpha s_1 + (1 - \alpha)s_2$. If an analyst overgeneralizes negative shocks to her other coverage industries and becomes pessimistic about this risky asset such that $\overline{s} < 0$, the expected return on this asset would be negative.

Moreover, the expected difference between P_2^* and the asset's fundamental value R is given by

$$\mathbb{E}[P_2^* - R] = \frac{h_0}{h_0 + h_\varepsilon}(\overline{R} - \mathbb{E}[R]) + \frac{h_\varepsilon}{h_0 + h_\varepsilon}\overline{s}. \tag{3.8}$$

This expression implies that analysts' belief shocks would lead the price to shift away from the asset's fundamental value. Connecting to my key empirical findings above, because analysts lower their expectations based on noise from other coverage industries, their incorrect pessimism would induce underpricing of the security. This underpricing would be more pronounced if more analysts are affected by larger negative belief shocks. Of course, when the true information is revealed to the market (realization of R in the model or firms' announcement of actual earnings in practice), the price will reverse to the fundamental value.

Prediction. *If some analysts receive negative belief shocks, their incorrect pessimism would exert downward price pressure and induce underpricing.*

3.3 Data and methodology

I start with the dataset of the previous chapter and aggregate the data at the firm $\times$ fiscal year-quarter level, resulting in 191,724 observations. The main dependent variables of interest are analyst disagreement, stock trading

Table 3.1: Data sample and summary statistics

This table reports the summary statistics of the main variables. Belief shock dispersion is computed as the standard deviation of the belief shock variables within each pair of firm j and fiscal quarter t. All negative shocks is computed as the sum of the absolute value of negative belief shocks of all analysts covering firm j for fiscal quarter t, scaled by the total number of analysts. Forecast dispersion is the standard deviation of EPS forecasts scaled by the absolute value of the mean EPS forecast, for each firm j and fiscal quarter t. Trading volume is the logarithm of the average daily stock trading volume over the estimation window, which is the period between the earnings announcement date of fiscal quarter t and that of fiscal quarter $t-1$. Realized volatility is the standard deviation of daily stock returns in the estimation window. Implied volatility is the average daily volatility implied from options with a maturity of 30 days in the OptionMetrics database, over the estimation window. EA CAR(-1, 1) is the market-adjusted cumulative announcement return over the window (-1, 1) around the firm's earnings announcement date. Earnings surprise is the actual earnings minus the consensus, divided by the absolute value of the consensus. Market value of equity (mln) is the product of total shares outstanding and fiscal quarter closing stock price. Book-to-market is the book value of equity divided by the current market value of equity. ROA is the operating income before depreciation divided by the lagged total assets. Number of analysts is number of analysts following a particular stock in a given quarter. Multi-industry is the fraction of analysts who follow multiple Fama-French 49 industries for each stock and quarter. Market volatility is average daily VIX index in the same period as when realized and implied volatility are computed. All dependent and control variables are winsorized at the 1% and 99% levels.

	N	Mean	St. Dev.	Percentile				
				10th	25th	50th	75th	90th
Dependent variables								
Forecast dispersion	191,726	0.25	0.62	0.01	0.03	0.07	0.19	0.50
Trading volume	191,726	12.62	1.64	10.51	11.53	12.61	13.69	14.74
Realized volatility	191,726	0.45	0.26	0.20	0.26	0.38	0.55	0.79
Implied volatility	134,541	0.48	0.23	0.25	0.32	0.42	0.58	0.78
EA CAR(-1, 1) (in %)	191,014	0.22	7.92	-8.71	-3.54	0.18	4.11	9.32
Main explanatory variable								
Belief shock dispersion	191,726	0.05	0.04	0.01	0.02	0.04	0.07	0.10
All negative shocks	191,726	0.03	0.06	0.00	0.00	0.00	0.03	0.08
Control variables								
Earnings surprise	191,726	-0.05	1.10	-0.45	-0.08	0.03	0.14	0.43
MVE (in $mln)	191,726	4,196	9,309	141	328	937	3,031	10,362
Book-to-market	191,313	0.65	0.29	0.26	0.42	0.65	0.88	1.00
ROA	176,015	0.03	0.05	0.00	0.01	0.03	0.05	0.07
Number of analysts	191,726	11.72	8.55	4	5	9	16	24
% Multi-industry	191,726	0.55	0.25	0.21	0.37	0.55	0.71	0.86
Market volatility	191,726	20.05	7.57	12.61	14.40	18.44	23.71	28.68

volumes, return volatilities, and earnings announcement returns. I follow Diether et al. (2002) to compute analyst forecast dispersion as the standard deviation of EPS forecasts scaled by the absolute value of the mean EPS forecast. To measure trading activities, I compute the trading volume as the logarithm of the average daily stock trading volume over the period between the earnings announcement date of fiscal quarter t and that of fiscal quarter $t-1$. I choose this estimation window because its length is similar across different firms or within the same firm, and it does not depend on when analysts issue forecasts. Nonetheless, my results are robust to alternative estimation windows. Moreover, I compute realized volatility is the standard deviation of daily stock returns in the estimation window. For robustness checks, I also consider options-implied volatility, which is the average daily volatility implied from options with a maturity of 30 days in the OptionMetrics database within the estimation window.

To estimate the effect of the dispersion in analysts' belief shocks, I compute the standard deviation of the belief shocks within each pair of firm and fiscal quarter, i.e.,

$$\text{BSDispersion}_{jt} = \sqrt{\frac{1}{||I_{jt}|| - 1} \sum_{i \in I_{jt}} (BS_{ijt} - \overline{BS}_{jt})^2},$$

where I_{jt} denotes the set of analysts who make forecast for firm j and fiscal quarter t, and the belief shock variable is computed as in the previous chapter. Note that the belief shock dispersion variable corresponds to $|\Delta s|$ in the model. Thus, I can use the following specification to test the first prediction regarding asset volatility and trading volume:

$$y_{jt} = \alpha_j + \alpha_{q(t)} + \beta \times \text{BSDispersion}_{jt} + \gamma' X_{jt} + \eta_{jt}, \tag{3.9}$$

where $q(t)$ indexes the calendar quarter in which the firm announces its realized earnings of fiscal quarter t, y_{jt} is the dependent variable of interest (forecast dispersion, trading volume, and return volatility), and X_{jt} is a vector of firm-specific control variables such as size and profitability. All of the specifications include firm fixed effects to capture unobserved but time-invariant heterogeneity across firms and calendar quarter fixed effects to account for common trends. If dispersion in belief shocks leads to more analyst disagreement, higher trading volumes, and larger return volatilities, the coefficient estimates of β are expected to positive and significant.

To test the prediction regarding stock returns, I first identify and aggregate analysts' belief shocks that could potentially exert downward price pressure. For each firm-quarter observation, I construct a variable called *all negative shocks*, which is computed as the sum of the absolute value of negative belief shocks (i.e., $|BS_{ijt}^-|$) of all of the analysts covering firm j in fiscal year-quarter t and then scaled by the total number of analysts, i.e.,

$$\text{All negative shock}_{jt} = \frac{1}{||I_{jt}||} \sum_{i \in I_{jt}} |BS_{ijt}^-|.$$

Note that this measure is an empirical proxy for $\bar{s}$ in Equations (3.7) and (3.8). Using this measure, I can estimate the following specification to test the second prediction

$$EACAR_{jt} = \alpha_j + \alpha_{gq(t)} + \beta \times \text{All negative shock}_{jt} + \gamma' X_{jt} + \eta_{jt}, \quad (3.10)$$

where the dependent variable is the three-day (-1, +1) market-adjusted cumulative abnormal return around the focal firm's earnings announcement date. I use industry $\times$ calendar year-quarter fixed effects ($\alpha_{gq(t)}$) to control for any common trend within the same industries, such as the spillover effects from other industries. Moreover, I explicitly control for the direct market impact of earnings surprises to distinguish reversals because of better than expected earnings performance, from reversals that correct underpricing driven by analysts' unmerited pessimism. If more analysts are affected by negative belief shocks and consequently make overly pessimistic forecasts that exert downward price pressure, the announcement return would be more positive as the true information (actual earnings) is revealed. In other words, the theoretical prediction implies a positive and significant β.

Summary statistics and detailed explanations of all used variables are shown in Table 3.1. All dependent and control variables are winsorized at the 1% and 99% levels. The standard errors in all regression specifications are two-way clustered by firm and calendar quarter to account for autocorrelations within the firm and correlations within the quarter.

3.4 Empirical evidence

This section presents the empirical results. I first confirm that dispersion in belief shocks significantly increases analyst disagreement. I show that dispersion in belief shocks also leads to larger trading volumes and higher return volatilities, which is in line with the first theoretical prediction. Furthermore, I document that negative belief shocks induce temporary underpricing, by showing that stocks with more analysts affected by negative belief shocks experience significantly negative returns in days before earnings announcements but reverse upwards significantly more around the announcements.

3.4.1 Disagreement, trading volume, and volatility

Table 3.2 shows the results of my test of whether analysts' opinions diverge more when dispersion in belief shocks increases. I regress quarterly analyst forecast dispersion on the dispersion of analyst belief shocks. I include time-varying firm-characteristics as control variables in column (2), and add calendar year-quarter fixed effects in column (3) and firm fixed effects in column (4). The estimated coefficients on the belief shock dispersion variable are positive and significant in all specifications (with t-values between 2.718 and 6.375). The statistical significance also increases with the strictness of the regression specifications. In economic terms, a one-standard-deviation increase in belief shock dispersion (0.042) is associated with a 6.4% ($= 0.376 \times 0.042/0.246$) to 8.1% ($= 0.474 \times 0.042/0.246$) increase in analyst disagreement, relative to the average level of analyst disagreement (0.246).

Table 3.3 shows the results of my test of whether dispersion in belief shocks leads to higher trading volumes. I regress the logarithm of average daily trading volumes on the dispersion of analyst belief shocks. I include time-varying firm-characteristics as control variables in column (2), and add

Table 3.2: Impact on analyst disagreement

This table reports the effect of the dispersion in analyst belief shocks on the differences of opinions about the stock. The dependent variable is the forecast dispersion, which is computed as the standard deviation of EPS forecasts scaled by the absolute value of the mean EPS forecast for each stock j and fiscal year-quarter t. In column (3), I additionally include the calendar quarter fixed effects. Calendar quarter is the year-quarter in which the firm announces its realized earnings of fiscal year-quarter t. In column (4), I further tighten the identification to include the firm fixed effects. Detailed definitions of the control variables are presented in Table 3.1. Standard errors are two-way clustered at the firm and calendar year-quarter level, and the corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	Forecast Dispersion			
	(1)	(2)	(3)	(4)
Belief Shock Dispersion	0.442***	0.378***	0.474***	0.376***
	(2.718)	(2.849)	(5.503)	(6.375)
Log(MVE)		−0.055***	−0.064***	−0.063***
		(−19.920)	(−22.547)	(−7.586)
Book-to-Market		0.146***	0.125***	0.219***
		(8.960)	(8.801)	(7.686)
ROA		−0.839***	−0.712***	−0.667***
		(−13.574)	(−11.693)	(−6.482)
Number of analysts		0.005***	0.005***	0.005***
		(9.019)	(9.405)	(6.406)
% Multi-Industry		0.098***	0.037***	0.103***
		(8.884)	(3.275)	(10.565)
Year-Quarter FE	No	No	Yes	Yes
Firm FE	No	No	No	Yes
Observations	191,726	175,665	175,665	175,665
R^2	0.001	0.031	0.038	0.210

calendar year-quarter fixed effects in column (3) and firm fixed effects in column (4). The estimated coefficients on the belief shock dispersion variable are again positive and significant in all specifications (with t-values between 3.592 and 7.682). The statistical significance is again the highest when the regression specification is tightest. As for the economic magnitude, a one-standard-deviation increase in belief shock dispersion (0.042) is associated with a 3.1% $(= \exp(0.725 \times 0.042) - 1)$ to 13.7% $(= \exp(3.049 \times 0.042) - 1)$ increase in trading volumes.

Table 3.3: Impact on trading volumes

This table reports the effects of the dispersion in analyst belief shocks on stocks' trading volumes. The dependent variable is the trading volume, which is computed as the logarithm of the average daily stock trading volume within the corresponding investigation window. In column (3), I additionally include the calendar quarter fixed effects. Calendar quarter is the year-quarter in which the firm announces its realized earnings of fiscal year-quarter t. In column (4), I further tighten the identification to include the firm fixed effects. Detailed definitions of the control variables are presented in Table 3.1. Standard errors are two-way clustered at the firm and calendar year-quarter level, and the corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	Trading Volume			
	(1)	(2)	(3)	(4)
Belief Shock Dispersion	3.049***	1.943***	1.519***	0.725***
	(3.998)	(3.592)	(6.161)	(7.682)
Log(MVE)		0.563***	0.491***	0.373***
		(38.873)	(33.268)	(18.615)
Book-to-Market		−0.201**	−0.390***	−0.050
		(−2.467)	(−5.549)	(−0.974)
ROA		−3.318***	−2.318***	0.220*
		(−16.418)	(−11.751)	(1.855)
Number of analysts		0.056***	0.061***	0.028***
		(21.408)	(28.889)	(15.594)
% Multi-Industry		0.566***	0.225***	0.116***
		(10.548)	(6.005)	(7.288)
Year-Quarter FE	No	No	Yes	Yes
Firm FE	No	No	No	Yes
Observations	191,726	175,665	175,665	175,665
R^2	0.006	0.612	0.662	0.899

I further test the prediction by estimating the effect of analyst belief shocks on firms' return volatility. I measure volatility in two ways: (1) realized equity volatility as the standard deviation of daily stock returns in the period between the announcement date of the first analyst forecast and the announcement date of the actual earnings for each firm j and fiscal year-quarter t, and then annualized; and (2) option-implied volatility from OptionMetrics averaged over the same period. Both measures are widely used in the literature.

As shown in Table 3.4, firms' volatility significantly increases with the

Table 3.4: Impact on stock volatility

This table reports the effects of the dispersion in analyst belief shocks on the stock volatility. In columns (1)-(3), the dependent variable is the realized equity volatility as the standard deviation of daily stock returns within the corresponding investigation window. In columns (4)-(6), the dependent variable is the option-implied volatility from Option Metrics averaged over the corresponding investigation window. Both measures are annualized. In column (2) and (5), I additionally include the calendar quarter fixed effects. Calendar quarter is the year-quarter in which the firm announces its realized earnings of fiscal year-quarter t. In column (3) and (6), I further tighten the identification to include the firm fixed effects. Detailed definitions of the control variables are presented in Table 3.1. Standard errors are two-way clustered at the firm and calendar year-quarter level, and the corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	Realized Volatility			Implied Volatility		
	(1)	(2)	(3)	(4)	(5)	(6)
Belief Shock Dispersion	0.630***	0.426***	0.269***	0.544***	0.399***	0.252***
	(6.047)	(7.167)	(7.702)	(5.723)	(7.271)	(8.097)
Log(MVE)	−0.075***	−0.075***	−0.052***	−0.088***	−0.090***	−0.071***
	(−28.430)	(−27.936)	(−6.435)	(−40.026)	(−39.322)	(−10.155)
Book-to-Market	−0.186***	−0.172***	−0.068***	−0.137***	−0.129***	−0.045***
	(−9.374)	(−8.957)	(−5.041)	(−8.214)	(−8.638)	(−3.654)
ROA	−0.825***	−0.801***	−0.343***	−1.074***	−1.019***	−0.326***
	(−13.378)	(−14.428)	(−7.590)	(−18.697)	(−19.645)	(−8.346)
Number of analysts	0.005***	0.005***	0.001***	0.004***	0.004***	0.000
	(12.127)	(13.085)	(2.706)	(10.815)	(11.524)	(0.924)
% Multi-Industry	0.042***	0.063***	0.041***	0.002	0.007	0.014***
	(4.633)	(10.938)	(11.308)	(0.204)	(1.139)	(4.027)
Market volatility	0.014***	0.012***	0.013***	0.010***	0.008***	0.008***
	(18.753)	(8.907)	(11.616)	(26.702)	(4.980)	(8.578)
Year-Quarter FE	No	Yes	Yes	No	Yes	Yes
Firm FE	No	No	Yes	No	No	Yes
Observations	175,665	175,665	175,665	123,695	123,695	123,695
R^2	0.426	0.478	0.679	0.513	0.564	0.771

dispersion in analyst belief shocks. In addition to firm characteristics, I control for market volatility to capture the macroeconomic uncertainty around the same period. The coefficient estimate of belief shock dispersion remains highly significant across all specifications. Economically, a one-standard-deviation increase in belief shock dispersion is associated with a 2.5% ($= 0.269 \times 0.042/0.445$) to 5.9% ($= 0.630 \times 0.042/0.445$) increase in stock volatility, relative to the mean (0.445). The results are almost identical for option-implied volatility, as shown in columns (4)-(6).

Overall, my empirical findings strongly support the first theoretical prediction. A larger dispersion in analysts' belief shocks increases the difference in analysts' forecasts, and as a result, it leads to more trading activities and larger return volatilities.

3.4.2 Underpricing

Figure 3.1 illustrates the impact of negative belief shocks on stock prices. Panel A uses the full sample, while Panel B focuses on the subsample of high-tech firms. I split the sample into two groups: one group of firm-quarters with some analysts affected by negative belief shocks (i.e., all negative shocks > 0), and the other group with no analysts affected by negative belief shocks (i.e., all negative shocks $= 0$). Defining firms' quarterly earnings announcement date as day 0, I trace out the market-adjusted cumulative abnormal returns from 30 days before to 30 days after earnings announcements. I choose 30 trading days before to start because most analysts begin to issue their forecasts by then.

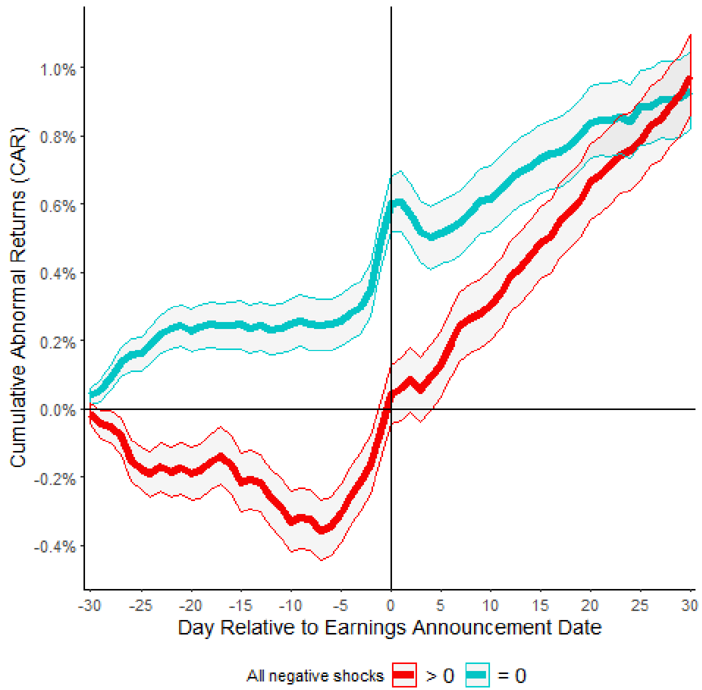
As shown in Panel A of Figure 3.1, there is a significant decline in stock price until five days before the earnings announcement for firms affected by negative analysts' belief shocks. This downward price pressure induces an average price decline of 36 basis points. There is no such pattern for the other group of firms. The price pressure effect is more stark in Panel B. High-tech firms affected by negative analysts' belief shocks decline by as much as 95 basis points, likely due to the severe information asymmetry in those industries. Investors rely more on analysts' opinions to trade those firms. I also find a stronger reversal around the earnings announcement for affected firms, which is consistent with the underpricing prediction.

In Table 3.5, I formally test the underpricing prediction by reestimating Equation (3.9) with the three-day (-1, +1) market-adjusted cumulative abnormal return around the focal firm's earnings announcement date as the dependent variable. If more analysts are affected by negative belief shocks and consequently make incorrect pessimistic forecasts that exert downward price

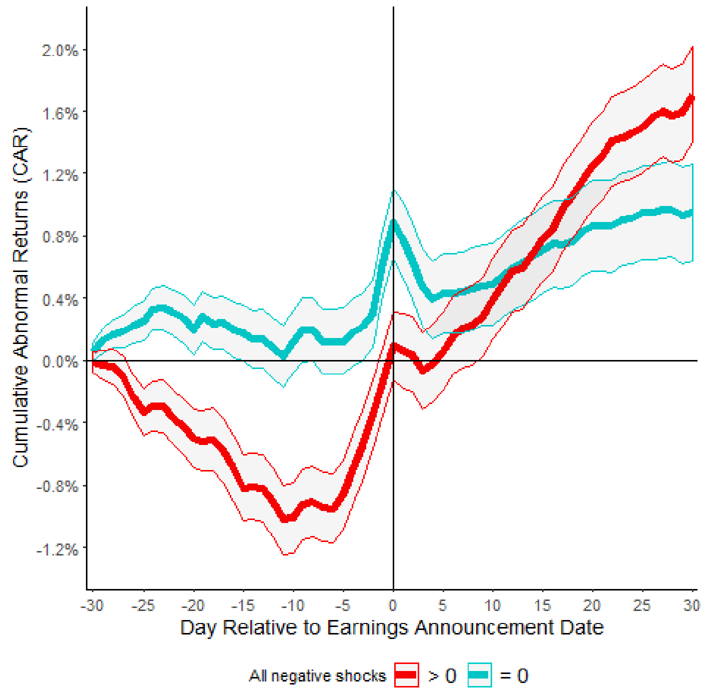
Figure 3.1: Impact of negative belief shocks on stock returns

This graph plots the average daily market-adjusted cumulative abnormal returns of stocks over the window (-30, +30) around the quarterly earnings announcement dates for two groups of firms. One group of firms has at least one analyst affected by negative belief shocks (i.e., all negative shocks > 0), and the other group of firms has no analyst affected by negative belief shocks (i.e., all negative shocks = 0). Panel A is based on the full sample, while Panel B focuses on the subsample of high-tech firms (code 3 in Fama-French 5 industries). Shading areas indicate the corresponding 90 percent confidence intervals.

Panel A: Full sample



Panel B: High Tech



pressure, the announcement return would be more positive because the true information (actual earnings) is revealed. To identify the impact of analysts' negative belief shocks on stock returns, I use industry × calendar year-quarter fixed effects to control for any common trend within the same industries, such as the spillover effects from other industries. Moreover, I explicitly control for the direct market impact of earnings surprises to distinguish reversals because of better than expected earnings performance, from reversals that correct underpricing driven by analysts' unmerited pessimism.

Table 3.5: Impact of negative belief shocks on earnings announcement returns

This table shows the impact of analysts' negative belief shocks on the focal firms' stock returns. The dependent variable is the three-day (-1, +1) market-adjusted cumulative abnormal return around the firm's earnings announcement date. The estimation results in columns (1) and (2) are based on the full sample. Column (2) includes the firm and industry × calendar year-quarter fixed effects. In columns (3) to (5), I focus on the subsample of high-tech firms (code 3 in Fama-French 5 industries), small firms (with below-median market capitalization), and young firms (with IPO in less than 10 years), respectively. Standard errors are two-way clustered at the firm and calendar year-quarter level, and the corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	EA CAR(-1, 1)				
	(1)	(2)	(3)	(4)	(5)
All negative shocks	0.023***	0.018*	0.031**	0.021*	0.025*
	(2.699)	(1.864)	(2.029)	(1.954)	(1.748)
Earnings surprise	0.013***	0.013***	0.013***	0.013***	0.012***
	(33.948)	(32.181)	(19.537)	(27.964)	(19.879)
Log(MVE)	−0.001**	−0.012***	−0.016***	−0.015***	−0.019***
	(−2.512)	(−14.442)	(−8.769)	(−13.032)	(−11.863)
Book-to-Market	0.004**	−0.006**	−0.011**	−0.011***	−0.013***
	(2.290)	(−2.503)	(−1.994)	(−3.221)	(−2.650)
ROA	0.038***	−0.049***	−0.055**	−0.055***	−0.056***
	(5.798)	(−5.166)	(−2.469)	(−4.663)	(−4.224)
Number of analysts	0.000	0.000	0.000	0.000	−0.000
	(1.511)	(0.158)	(0.768)	(1.149)	(−1.153)
% Multi-Industry	−0.003***	−0.002	−0.008**	−0.003*	−0.005**
	(−2.877)	(−1.565)	(−2.283)	(−1.786)	(−2.047)
Industry × Year-Quarter FE	No	Yes	Yes	Yes	Yes
Firm FE	No	Yes	Yes	Yes	Yes
Sample	Full	Full	High Tech	Small	Young
Observations	175,031	175,031	36,547	88,501	65,964
R^2	0.030	0.131	0.131	0.170	0.194

Conforming to the underpricing prediction, more analysts' negative belief shocks lead to larger price reversals around earnings announcements, as the coefficient on the all negative shock variable is positive and statistically significant in column (1). In the three days around the announcement, firms gain an additional 14 basis points (=0.023×0.06) when the analysts experience a one-standard-deviation increase in the negative belief shocks, which is an economically large effect of 63.6% relative to the average announcement return of 22 basis points. After controlling for the industry × quarter and firm fixed effects, the coefficient of interest decreases only slightly and remains significant, which corresponds to a gain of 11 basis points (=0.019×0.06) and therefore a 50% decrease. To test whether this pricing pressure effect is more pronounced for firms with a higher level of information asymmetry, I focus on the following three subsamples: high-tech industries in column (3), small firms (with below-median market capitalization) in column (4), and young firms (with IPO in less than 10 years) in column (5). Relative to the average earnings announcement returns in those subsamples, the estimated coefficient of interest suggests that a one-standard-deviation increase in analysts' negative belief shocks corresponds to a larger reversal of 92.7% (=0.034×0.06/0.0022) for high-tech firms, 72.6% (=0.023×0.06/0.0019) for small firms, and 86.7% (=0.026×0.06/0.0018) for young firms. These findings confirm my conjecture that the underpricing effect is more pronounced for firms with high information asymmetry.

Taken together, the findings in this section demonstrate that analyst overgeneralization has more profound impacts on the financial market. When there is a greater dispersion in the shocks that analysts overgeneralize, their opinions about the firm's future prospects will differ more, inducing significantly higher trading volumes and larger return volatilities. Analyst overgeneralization seems to aggravate information asymmetries and increase uncertainty about firms' fundamentals. Furthermore, when more analysts are affected by negative belief shocks, their resulting pessimism will exert significant downward price pressure and lead to temporary underpricing. This price pressure effect is more pronounced for firms with higher information asymmetry.

3.5 Conclusion

This chapter exploits my earlier finding that analysts overgeneralize bad news from other coverage industries and therefore make overly pessimistic forecasts for their focal firms. I use a simple trading model to demonstrate that, as many investors rely on analysts' opinions to evaluate companies and make trading decisions, this heuristic could have profound impacts on financial markets. I further provide strong empirical evidence supporting the theoretical predictions. Specifically, overgeneralization leads to larger differences in analysts' opinions about firms' future prospects and significantly increases stocks' trading volume and return volatility, aggravating information asymmetries and uncertainties about the underlying assets. Analysts' pessimism resulting from overgeneralization exerts downward price pressure and induces temporary underpricing.

Note that because overgeneralization leads analysts to lower expectations because of other industries' performance, which is arguably unrelated to focal firms' fundamentals, this heuristic essentially provides exogenous variation in analysts' disagreement and pessimism. This insight can be used to empirically study the effects of analyst (investor) disagreement or temporary underpricing in other settings.

Chapter 4

Director Attention and Firm Value[1]

4.1 Introduction

A board of directors has the critical task of actively monitoring and advising top management to ensure that managers act in the best interest of shareholders. However, a directorship is rarely a full-time job. Most directors have other occupations besides their directorships, and many directors serve on multiple boards. Given that attention is not unlimited for directors, we ask whether directors can perform their job effectively when their other occupations require more of their attention. Consequently, we examine how a firm performs when its directors are distracted.

Understanding the effect of director attention is important to evaluate the role and importance of corporate boards in corporate governance. In this article, we empirically study the impact of limited director attention on firm value by exploiting *exogenous* variation in board monitoring intensity from time-variation in how directors allocate attention across their multiple directorships. We find strong evidence that distracted directors spend less time and energy monitoring and advising managers, which gives managers the

[1] This chapter is based on Renjie and Verwijmeren (2019), which is forthcoming in the *Financial Management*.

freedom to shirk at the expense of shareholders, leading to significant declines in firm value.

We rely on a sample of RiskMetrics firms with at least one outside director with multiple directorships in the Directors database. These directors need to distribute attention among their directorships, which provides a useful setting to study the effect of director attention. As we cannot observe exactly how much time or energy directors spend on each of their directorships, our identification strategy is designed to exploit plausibly exogenous variation in how directors allocate attention across their directorships. The following simple thought experiment illustrates our approach. Consider two otherwise identical companies in a given industry and quarter. Director A sits on the board of Company 1 and on the board of firm "Car" in a totally different industry, namely the automotive industry. Director B sits on the board of Company 2 and on another firm that is not in the automotive industry. Suppose now that there is an attention-grabbing event in the automotive industry. Assuming limited attention, Director A may shift attention towards firm Car and away from Company 1. The manager at Company 1 consequently receives less monitoring and advice. In contrast, Company 2 is not affected because its director is not related to the automotive industry. Thus, we can identify the impact of variation in director attention on firm value by studying the changes in the value of Company 1 relative to that of Company 2 around the time Director A is distracted. We assign each firm to 1 of the 49 Fama-French industries and use unusually high volatility as the main empirical proxy for attention-grabbing events. This identification approach is similar to that of Kempf, Manconi, and Spalt (2017), who study how *investor* attention matters for corporate actions. We confirm that our results are robust to alternative industry classifications and various definitions of industry shocks.

To obtain insights into whether our measure of director distraction captures director attention, we start by examining board meeting attendance. We show that directors identified by our measure as distracted attend fewer board meetings. We next employ our measure of director distraction to study how director attention affects firm value. By examining Tobin's Q and stock performance, we find that firm value drops significantly when board members are distracted. A deviation from no distraction to the average distraction level is associated with a 3.3% discount in quarterly Tobin's Q, and a stock market underperformance of about 72 basis points per quarter. This effect is particularly strong when the distracted directors sit on an important committee of the board.

Because our tests either include industry × quarter fixed effects or explicitly control for industry-specific shocks, our results are not likely driven by spillovers among industries or by any variable that does not vary across firms within a given industry and quarter, such as the state of the business cycle. Firm-level time-invariant unobservable factors cannot drive our findings as we also include firm fixed effects. Even with these fixed effects, a remaining concern relates to the endogeneous nature of director appointments. For instance, Company 1 chooses Director A who also holds a directorship in the automotive industry, because the business of Company 1 is related to the automotive industry, whereas this is not the case for Company 2. Thus, shocks in the automotive industry spill over to Company 1 but not to Company 2. To alleviate this concern, we provide three pieces of evidence.

First, we argue that the direction of the spillover effect is mostly consistent with the direction of the industry shock. If the automotive industry experiences a positive shock, the effect spilled over to Company 1 is likely also positive, and vice versa for negative shocks. We therefore examine distraction from positive and negative industry shocks separately. We show that director distraction from both positive and negative shocks in the other industry affects firm value negatively. Secondly, because shocks in the oil and gas industry can especially have spillover effects (also in the opposite direction), we modify our distraction measure by removing shocks from oil and gas industries and we repeat our analysis on a subsample excluding firms operating in those industries. The results remain similar to the baseline results. Thirdly, we ensure that attention shocks come from unrelated industries by excluding shocks from supplier or customer industries, and again we find similar results, which supports the validity of our distraction measure in capturing director attention shocks rather than industry relatedness or comovement.

This article is related to a large literature on the busyness of corporate boards. Some studies find that directors with multiple directorships are too busy to effectively monitor management (Core, Holthausen, and Larcker, 1999; Fich and Shivdasani, 2006; Falato, Kadyrzhanova, and Lel, 2014), whereas other researchers find that busyness reflects the quality of directors, which could provide advantages for firms (Gilson, 1990; Kaplan and Reishus, 1990; Shivdasani and Yermack, 1999; Ferris, Jagannathan, and Pritchard, 2003; Field, Lowry, and Mkrtchyan, 2013). Our study disentangles busyness from director ability and provides evidence on the costs of having busy directors.

A noteworthy feature of our identification strategy is that we consider the source of distraction at the industry-level rather than at the firm level.[2] A firm-level approach has the crucial disadvantage that firm-level shocks could be driven by the ability of the director. For instance, if we classify Director A as distracted when company Car does poorly (as opposed to the whole automotive industry), then this could simply be attributed to the bad performance of Director A. Director A might be a poor monitor and/or adviser, and as a result, both company Car and Company 1 can underperform at the same time. Considering industry-level shocks mitigates this concern as it is less likely that the ability of one single director affects the performance of the whole industry.

Falato et al. (2014) uses 220 sudden deaths of directors at interlocked firms as exogenous shocks to directors' workload. Hauser (2018) uses mergers of interlocked firms as exogenous shocks to directors' outside appointments. However, loss of outside appointments could not only decrease directors' workload but also reduces potentially valuable business relationships of the director. Director deaths at interlocked firms introduces uncertainty about the effect of director replacement. Our identification scheme study director attention while isolating the potential confounding effects resulting from changes to directors' appointments or to interlocked firms' boards. Masulis and Zhang (2018) studies director attention by examining distraction events such as director illness

[2]Stein and Zhao (2016) examines director distraction when the source of distraction is at the firm level.

and winning prestigious awards, and finds that these distracting events lower firm value. It is comforting to know that the effects of these specific shocks are in line with the effects of the more general source of director distraction that we study.

We further investigate multiple potential channels to better understand the negative effect of director distraction on firm value. When managers receive less monitoring from distracted directors, two potential agency problems might be exacerbated: (1) managers engage in empire building and make value-destroying investment decisions (Jensen, 1986), or (2) managers become more passive and "enjoy a quiet life" (Bertrand and Mullainathan, 2003). Alternatively, managers might miss important advice or have to delay making important decisions when it is difficult to schedule meetings with distracted directors for discussion and approval. We find that firms with more director distraction invest significantly less and are less likely to announce takeovers. These changes are due to firms with distracted directors being less active rather than the directors postponing their investments. The acquisitions that are still being announced when directors are distracted do not destroy value. Overall, our article addresses the question of which agency problem the board of directors mitigates. Our results suggest that an effective board of directors prevents managers from shirking or "enjoying a quiet life" at the expense of shareholder value.

Our findings support policies restricting the number of directorships that an individual is allowed to have. Nevertheless, it is important to note that we do not argue that directors with multiple directorships are detrimental to shareholder value per se, as firms could benefit from the knowledge and network of a director who serves on multiple boards (Field et al., 2013). The results in our study provide insights into the trade-off of having busy directors by isolating their busyness from their quality and highlighting that firm value drops when directors are distracted because management becomes less active.

The remainder of this chapter is organized as follows. Section 4.2 discusses our data and presents descriptive statistics. Section 4.3 explains how we construct our director distraction measure. Section 4.4 presents the main findings and Section 5 examines alternative explanations. Section 4.6 concludes.

4.2 Data

We combine data from different sources. Director data are drawn from the RiskMetrics Directors database for 1996-2017. This database contains director-firm-year observations for S&P 1500 firms. We use board affiliation information from RiskMetrics to classify directors who are not employed by the firm as outside directors. We focus on outside directors because distraction by other directorships is less likely for inside directors, given their employment with the firm.[3] We exclude firms that have no outside director with multiple directorships. We match the director data with the Compustat Quarterly database to obtain financial reporting data and exclude regulated financial (SICH 6000-6999) and utility (SICH 4900-4999) firms.[4] We obtain stock price data from CRSP, merger activity data from SDC, and Fama-French 49 industry portfolio returns from Kenneth R. French's data library. We assign each firm to 1 of the 49 Fama-French industries based on its historical SIC code (Compustat data item SICH). When the SICH code is not available, we follow Fama and French (2008) and use the CRSP SIC code (data item HSICCD).

The final director-level dataset consists of 71,752 director-firm-year observations, with 5,875 individual outside directors with multiple directorships. The final firm-level dataset consists of 75,595 firm-quarter observations, with 2,264 unique firms. Table 4.1 reports summary statistics for the variables we use in our study. Detailed definitions of these variables are reported in the Appendix. All continuous dependent variables are winsorized at the 1% level at both tails. Our summary statistics are comparable to previous studies using data from RiskMetrics and Compustat (e.g., Masulis and Mobbs, 2014).

[3]Nonetheless, we examine changes in firm value when executive directors are distracted in Section 4.3.

[4]Our results are robust to these exclusions.

Table 4.1: Summary statistics

This table reports summary statistics for the main sample of firm-quarter observations of RiskMetrics firms with at least one director with multiple directorships over the period 1996-2017. A complete list of variable definitions is provided in Table 4.A.1. All continuous dependent variables are winsorized at 1% at both tails.

	N	Mean	Std. Dev.	Min.	p25	Median	p75	Max.
Dependent variables								
Tobin's Q	75,331	2.08	1.59	0.47	1.26	1.66	2.36	81.28
CAPEX	75,569	0.69	0.18	-1.39	0.59	0.70	0.79	2.37
Acquisition	75,595	0.08	0.27	0	0	0	0	1
Diversifying merger	75,595	0.04	0.19	0	0	0	0	1
Main independent variable								
Distraction	75,595	0.07	0.17	0.00	0.00	0.00	0.10	6.00
Distraction (> 0)	26,982	0.21	0.22	0.00	0.08	0.14	0.25	6.00
Alternative measures								
Distraction (positive)	75,595	0.03	0.08	0.00	0.00	0.00	0.00	3.58
Distraction (negative)	75,595	0.03	0.10	0.00	0.00	0.00	0.00	5.00
Control variables								
Total assets ($million)	75,595	8,632	26,293	124	745	1,927	5,927	347,564
Log(Assets)	75,595	7.71	1.50	2.64	6.61	7.56	8.69	12.06
Cash flow	71,928	0.04	0.03	-0.42	0.02	0.04	0.05	0.17
Board size	75,595	8.17	2.85	1	7	8	10	20
Board busyness	75,595	0.43	0.25	0.06	0.23	0.40	0.58	1
Board independence	75,595	0.74	0.18	0	0.67	0.78	0.88	1
Institutional ownership	72,031	0.76	0.20	0	0.65	0.79	0.90	1
Investor distraction	68,690	0.05	0.04	0.00	0.02	0.04	0.08	0.47
Merger deal variables								
CAR(-2, +2)	5,527	0.00	0.06	-0.41	-0.02	0.00	0.03	0.48
Relative deal size	5,529	0.14	0.37	0.00	0.02	0.05	0.13	11.17
Diversifying deal	5,529	0.52	0.60	0	0	0	1	1
Private target	5,529	0.74	0.44	0	0	1	1	1
Cross-border	5,529	0.26	0.44	0	0	0	1	1
Director-level variables								
Attended < 75% board meetings	71,752	0.02	0.13	0	0	0	0	1
Director distraction	71,752	0.55	0.92	0	0	0	1	10.77
Industry Shock	71,752	0.23	0.43	0	0	0	0.32	4
Director age	71,702	61.88	7.16	28	57	62	67	95
Log(Director age)	71,702	4.13	0.12	3.37	4.06	4.14	4.22	4.56
Independent	71,752	0.91	0.28	0	1	1	1	1
Number of directorships	71,752	2.64	0.95	2	2	2	3	10
Yearly Tobin's Q	68,290	1.91	1.29	0.46	1.18	1.53	2.16	55.73

4.3 Measuring director distraction

4.3.1 Variable construction

The main variable of interest is a firm-level proxy for how much the board members of a given firm f are distracted in a given quarter t. The intuition behind the *Distraction* measure is the same as in Kempf et al. (2017), who examine investor distraction. A given director i of firm f is more likely to be distracted if there is an attention-grabbing event in a different industry in which director i has an additional directorship. For each outside director i at firm f in fiscal quarter t, we compute a director-firm-level distraction score D_{ift} as

$$D_{ift} = \sum_{j \in B_{it} \setminus \{f\}} w_{ijt}^{f} \times 1(Ind_{jt} \neq Ind_{ft}) \times IS_t^{Ind_{jt}}, \tag{4.1}$$

where $B_{it} \setminus \{f\}$ denotes the set of firms other than firm f where director i serves on the board in quarter t; the weight w_{ijt} captures how much director i cares about firm j; $1(Ind_{jt} \neq Ind_{ft})$ indicates whether firm j is in the same Fama-French 49 industry as firm f, thereby allowing only shocks from industries other than that of firm f; and $IS_t^{Ind_{jt}}$ captures whether distracting events occur in the industry of firm j in quarter t. We now explain the construction of w_{ijt}^{f} and $IS_t^{Ind_{jt}}$ in more detail.

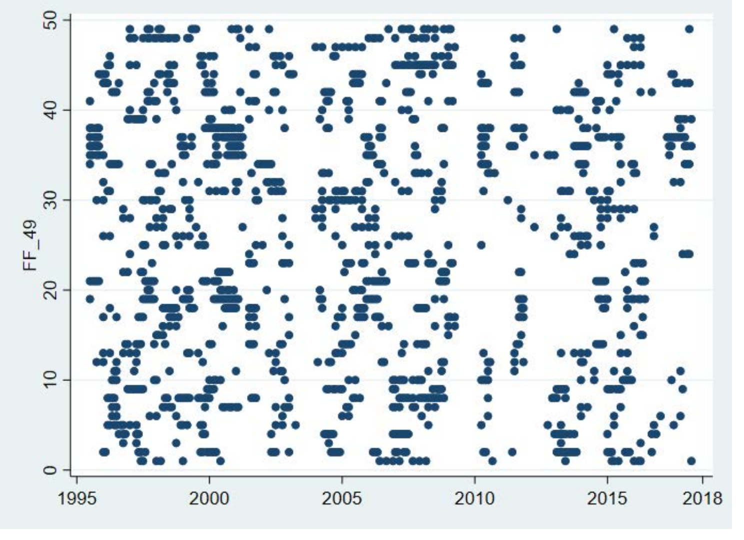
The construction of the weight w_{ijt}^{f} is motivated by Masulis and Mobbs (2014), who find that directors with multiple directorships distribute their time and energy unequally based on the directorship's relative prestige, which they establish by firms' market value of equity. Consequently, we calculate the weight of each directorship (firm) j for director i with respect to the focal firm f in quarter t as:

$$w_{ijt}^{f} = \min\left\{1, \frac{mve_{jt}}{mve_{ft}}\right\}, \tag{4.2}$$

where mve_{jt} and mve_{ft} denote the market value of equity of firm j and that of focal firm f in fiscal quarter t. This weighting-scheme accounts for the notion that directors are less likely to be distracted from their relatively more prestigious directorships, as it assigns a lower weight to attention shocks from directorships that are less important than the focal firm (i.e., when $mve_{jt} < mve_{ft}$).

Figure 4.1: Attention-grabbing industries

This figure shows which Fama-French 49 industries are identified as attention-grabbing in each quarter from 1996 to 2017.



The term $IS_t^{Ind_{jt}}$ is used to identify whether the industry of firm j is attention-grabbing in quarter t. Because attention-grabbing industry shocks are mostly associated with extreme returns and more news releases, which result in high volatility, we define $IS_t^{Ind_{jt}}$ as an indicator variable equal to 1 if the Fama-French 49 industry of firm j has abnormally high volatility relative to the other Fama-French 49 industries in a given quarter t. More specifically, in each quarter t, we first calculate for each Fama-French 49 industry l, its abnormal volatility:

$$\Delta\sigma_{lt} = \frac{\sigma_{lt} - \widehat{\sigma}_{lt}}{\widehat{\sigma}_{lt}}, \tag{4.3}$$

where σ_{lt} is the daily volatility of the Fama-French 49 industry portfolio l in quarter t and $\widehat{\sigma}_{lt}$ is the daily volatility of the FF49-industry portfolio l over the window [-283, -31] relative to the start of quarter t. Then, we sort the 49 abnormal volatilities and consider an industry attention-grabbing if its abnormal volatility is positive and in the top-10 (top-quintile) across 49 industries. Note that if in a given quarter none of the industries has positive $\Delta\sigma_{lt}$, there would be no attention-grabbing industry in that quarter.[5] Figure 4.1 shows which Fama-French 49 industries are considered attention-grabbing over time. For example, IT-related industries (Fama-French industries 34-38) are attention-grabbing during 2000-2002, and finance-related industries (Fama-French industries 45-48) are attention-grabbing during 2008-2010. The dispersed pattern of industry shocks in Figure 4.1 mitigates the concern that our findings are driven by a small number of industries.

To compute firm-level distraction, we aggregate the director-firm-level distraction scores across all directors with outside directorships. Specifically, for firm f in quarter t, we compute its board distraction level as:

$$Distraction_{ft} = \frac{1}{N_{ft}} \sum_{i \in \mathbb{B}_{ft}} D_{ift}, \tag{4.4}$$

where $\mathbb{B}_{ft}$ denotes the set of outside directors with multiple directorships on the board of firm f in quarter t, and N_{ft} denotes the total number of outside directors. However, Ljungqvist and Raff (2018) highlights that directors can strategically substitute or complement co-directors' monitoring effort, which suggests that a larger number of outside directors does not necessarily mitigate the effects of distracted directors. To test whether the scaling is warranted in our setting, in untabulated analysis we have confirmed that firms in our sample with more outside directors are affected significantly less by individual board member distraction. These results are available upon request from the authors.

An important advantage of $Distraction_{ft}$ is that this firm-level director distraction measure is by construction not related to the fundamentals of the firm of interest (firm f), as only shocks from industries other than that of firm f are used to construct D_{ift}. Thus, $Distraction_{ft}$ is a plausible candidate for identifying exogenous shocks to the attention of firm f's board members.

[5] Using different estimation windows to compute $\widehat{\sigma}_{lt}$, or different cutpoints such as top-5 industries (instead of top-10) yield qualitatively similar results. We have also used Fama-French 12 industries and 2-digit SIC industries and obtained similar results.

Another advantage of our identification strategy is that we consider the source of distraction at the industry-level rather than at the firm-level. Exploiting the source of distraction at the firm-level has a crucial disadvantage in that firm-level shocks could be driven by the ability of the director. Considering industry-level shocks alleviates this concern as it is less likely that the ability of one single director affects the performance of the whole industry.

The summary statistics of $Distraction_{ft}$ are presented in Table 4.1. As is shown, this variable is right-skewed and equals 0 in more than 50% of the sample. Therefore, we also report the distribution of the distraction variable with only positive values. About 36% of the firms in our sample have had distracted directors. Henceforth, we use 0.21 as the mean distraction level and refer to distraction values above this mean as high distraction, which involve 11% of our sample.

4.3.2 Board meeting attendance of distracted directors

To test whether our distraction measure captures director distraction, we study the board attendance rate of directors with multiple directorships in Table 4.2. The dependent variable is a dummy variable that equals to one if a director has attended less than 75% of the board meetings of a particular firm in a given fiscal year. The idea is that directors are less likely to miss board meetings when they allocate more time and effort to the firm. We aggregate the explanatory variables accordingly as the dummy dependent variable is at the director-firm-year level. Control variables include the directorship's relative ranking, the number of outside directorships, and other director and firm characteristics. Summary statistics of these variables are presented in Table 4.1.

We start by validating whether our industry shocks can identify attention shocks. In Columns (1-2) of Table 4.2, we test whether directors are less likely to miss board meetings at a firm when its industry experiences abnormally higher volatility. To this end, we aggregate the quarterly industry shocks over

Table 4.2: Director distraction and attendance of board meetings

This table reports the effect of director distraction on directors' attendance of board meetings. We use director-firm-year level observations from RiskMetrics and consider only directors with more than one board seat in a given year. The dependent variable is a dummy variable indicating whether a director has attended less than 75% of the firm's board meetings in a given year. In columns (2), (3), and (6-7), the model is estimated with year fixed effects and firm fixed effects. In column (5), the model is estimated with firm × year fixed effects. In column (6), the indicator variable 1(Negative shock) equals one if at least one of the director's attention-grabbing directorships is hit by a negative industry shock. In column (7), the indicator variable 1(Executive in shocked industry) equals one if the director is an executive in one of the attention-grabbing industries. In all of the specifications, we cluster the standard errors at the director-level. The corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	Attended < 75% board meetings						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Industry shock	-0.003***	-0.002*					
	(-2.776)	(-1.656)					
Director distraction			0.002***	0.002**	0.002**	0.001*	0.001*
			(3.022)	(2.300)	(2.166)	(1.896)	(1.742)
Director distraction × 1(Negative shock)						0.003*	
						(1.776)	
Director distraction × 1(Executive in shocked industry)							0.003
							(1.575)
High ranked directorship	-0.003**	-0.006***	-0.002*	-0.005***	-0.004**	-0.005***	-0.006***
	(-2.281)	(-4.864)	(-1.866)	(-4.513)	(-2.331)	(-4.175)	(-4.557)
Log(Director age)	-0.051***	-0.086	-0.051***	-0.085	-0.023***	-0.085	-0.086
	(-8.048)	(-1.267)	(-8.008)	(-1.261)	(-2.831)	(-1.254)	(-1.277)
Independent	-0.012***	0.005	-0.012***	0.005	-0.005	0.005	0.005
	(-3.766)	(1.446)	(-3.764)	(1.449)	(-1.364)	(1.432)	(1.455)
Number of directorships	0.005***	0.002	0.004***	0.001	0.001	0.001	0.001
	(4.221)	(1.334)	(3.800)	(0.936)	(1.201)	(0.732)	(0.949)
Board size	-0.002***	0.000	-0.002***	0.000	-0.002**	0.000	0.000
	(-5.541)	(1.140)	(-5.410)	(1.248)	(-2.546)	(1.309)	(1.241)
Yearly Tobin's Q	-0.000	-0.000	-0.000	-0.000	-0.001	-0.000	-0.000
	(-0.403)	(-0.574)	(-0.406)	(-0.569)	(-0.255)	(-0.526)	(-0.557)
Observations	68,244	68,244	68,244	68,244	68,244	68,244	68,244
Adj. R^2	0.007	0.092	0.007	0.092	0.053	0.092	0.092
Year FE	No	Yes	No	Yes	No	Yes	Yes
Director FE	No	Yes	No	Yes	No	Yes	Yes
Firm × year FE	No	No	No	No	Yes	No	No

fiscal year y as

$$IS_{ijy} = \sum_{t \in y} IS_t^{Ind_{jt}}, \tag{4.5}$$

where $IS_t^{Ind_{jt}}$ is defined as in Section 4.3.1. We find that directors are significantly less likely to miss board meetings at firms in shocked industries. The coefficient of Industry shocks implies that an interquartile increase in director-firm-level distraction (0.32) is associated with a 4.8% ($= -0.003 \times 0.32/0.02$) lower probability that the director attended less than 75% of board meetings. This result provides evidence that our industry shock measure captures attention-grabbing events that could distract directors.

When directors of Company 1 are distracted and shift time and energy to their other directorships, they might miss more board meetings of Company 1. In Columns (3-5) of Table 4.2, we test whether directors miss more meetings at the focal firms when they are distracted according to our measure. We sum up the director-firm-level distraction in (4.1) over all four quarters in fiscal year y for a particular firm f to obtain a director-firm-year-level measure for director distraction, that is, $\sum_{t \in y} D_{ift}$.

We show in Column (3) that the coefficient of Director distraction is both statistically and economically significant. An interquartile increase in director-firm-level distraction is associated with a 10% ($= 0.002 \times 1/0.02$) higher probability that the director attended less than 75% of board meetings. The effect remains significant after controlling for director and year fixed effects in Column (4), where we exploit the variation at the director level over time. In Column (5), we further exploit the variation at the firm-year level, which isolates the source of variation that comes from pairwise comparisons of distracted directors versus non-distracted directors within the same firm in the same year. The coefficient of Director distraction remains virtually unaffected.

Although our baseline measure captures attention-grabbing industry shocks by means of abnormally higher volatilities, it does not distinguish between the distraction effect of positive and negative shocks. It may be that, conditioning on abnormally high volatility, industries with positive performance shocks demand less director attention than those with negative performance shocks, because directors may face higher pressure when the firm experiences an unfavorable industry shock. We test this possibility in Column (6) of Table 4.2 by estimating whether negative industry shocks lead directors to miss more board meetings than positive industry shocks do. We interact the yearly director distraction measure with a dummy variable indicating whether at least one of the attention-grabbing industries is hit by a negative shock (i.e. with negative cumulative stock returns). As shown, the baseline director distraction measure remains positive and significant, as does the coefficient on the interaction term. When the attention-grabbing industry experiences a negative shock, the affected directors are about 20% (= $0.004 \times 1/0.02$) more likely to attend less than 75% of board meetings. This finding suggests that although industries with both positive and negative shocks are attention-grabbing, industries with negative shocks are significantly more likely to distract directors.

Finally, we show in column (7) of Table 4.2 that our finding is driven not only by directors who are executives in the attention-grabbing industries. We interact our baseline director distraction measure with a dummy variable that equals one if the director is an executive in one of the attention-grabbing industries. The positive coefficient on the interaction term falls slightly short of statistical significance ($t = 1.575$) and thus provides only weak evidence that directors are more likely to miss board meetings of the focal firms if they are executives in the shocked industries as opposed to non-executives. The coefficient of the baseline measure remains positive and significant, which implies that directors with both executive and non-executive positions in attention-grabbing industries are distracted.

A noteworthy limitation of this analysis is that we cannot observe the exact continuous board attendance rate of directors. For example, a meeting attendance drop from 100% to 80% (or from 70% to 20%) is substantial but does not show up in the used binary dependent variable. Because there is relatively little variation in the attendance dummy, we cannot fully exploit the effect of director distraction. Accordingly, we are probably underestimating the effect of distraction on director board meeting attendance. Overall, the results in Table 4.2 suggest that our measure of distraction adequately captures variation in the attention of directors. Directors attend fewer board meetings when they are distracted, but they are less likely to miss meetings of firms in the attention-grabbing industries, consistent with the notion that distracted directors spend less time and energy monitoring and advising management.

4.4 Empirical findings

This section presents our main findings. First, we test the effect of director distraction on firm value. Then, we investigate three potential channels through which director attention could affect firm value. We conclude by studying the distraction effect for different groups of directors.

4.4.1 Main results

In Table 4.3 we examine the effect of director distraction on firm value using Tobin's Q as the dependent variable. In Columns (1) and (2), the model is estimated with quarter and firm fixed effects, which exploits variation within firms. In Column (3) and (4), the model is estimated with industry $\times$ quarter fixed effects and firm fixed effects, which additionally controls for any unobserved time-varying industry heterogeneity. Including the industry $\times$ quarter fixed effects also mitigates the concern that our findings simply result from spillovers among industries. In Columns (2) and (4), we also include firm and board characteristics.

The coefficient of *Distraction* in Columns (1) - (4) of Table 4.3 is between -0.237 and -0.338 (depending on the model specification) and is statistically highly significant, suggesting that firm value decreases significantly when directors are distracted. This negative impact of director distraction is also economically meaningful. A deviation from no distraction to the average distraction level of 0.205 is associated with a 2.3% ($= -0.237 * 0.205/2.084$) to 3.3% ($= -0.338 * 0.205/2.084$) discount in Tobin's Q on a quarterly basis.

Figure 4.2 plots the difference in quarterly Tobin's Q between firms with no director distraction and firms with high director distraction over time. The negative impact of director distraction on firm value is relatively consistent over time.

A potential concern relates to the endogenous nature of director choice. The choice of Company 1 to employ Director A, who also holds a directorship in the automotive industry, is endogenous. The possibility exists that the business of Company 1 is more related to the automotive industry than other

Table 4.3: Effects of director distraction on firm value

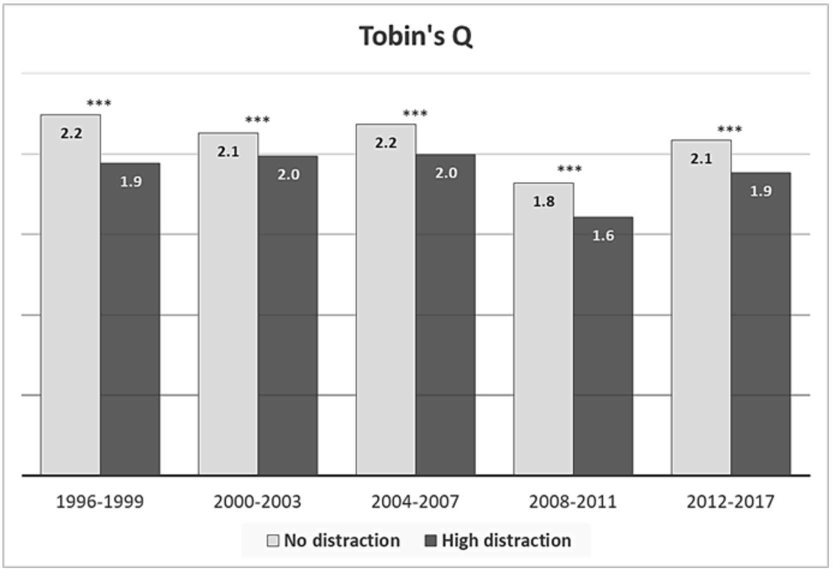
This table reports the effect of director distraction on firm value. The dependent variable is Tobin's Q. In column (1) and (2), the model is estimated with quarter and firm fixed effects, which exploits variation within firms. In column (3) and (4), the model is estimated with industry × quarter fixed effects and firm fixed effects. In column (5) and (6), we consider distraction from positive and negative industry shocks separately. Distraction (positive) uses only industries with abnormally high volatility and positive performance as attention-grabbing industries; distraction (negative) uses only industries with abnormally high volatility with negative performance as attention-grabbing industries. We use Fama-French 49 industries. Standard errors are clustered at the firm level, and the corresponding *t*-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	Tobin's Q					
	(1)	(2)	(3)	(4)	(5)	(6)
Distraction	-0.338***	-0.250***	-0.271***	-0.237***		
	(-5.654)	(-4.874)	(-5.332)	(-5.387)		
Distraction (positive)					-0.230**	
					(-1.965)	
Distraction (negative)						-0.316***
						(-3.495)
Log(Assets)		-0.372***		-0.380***	-0.380***	-0.380***
		(-9.491)		(-10.849)	(-10.849)	(-10.860)
Board size		0.015		0.010	0.011	0.010
		(1.299)		(0.935)	(0.981)	(0.954)
Board busyness		-0.179		-0.074	-0.098	-0.089
		(-1.571)		(-0.711)	(-0.921)	(-0.862)
Board independence		-0.153		-0.189	-0.187	-0.186
		(-1.126)		(-1.403)	(-1.390)	(-1.386)
Observations	75,331	75,331	75,331	75,331	75,331	75,331
Adj. R^2	0.499	0.516	0.574	0.589	0.589	0.589
Quarter FE	Yes	Yes	No	No	No	No
Industry × quarter FE	No	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes

companies are. Thus, shocks in the automotive industry would spillover and affect Company 1 more than other companies. To address this concern, we test the prediction of this endogeneity story that the direction of the spillover effect is likely consistent with the direction of the industry shock. That is, if the automotive industry experiences a positive shock, the effect spilled over to Company 1 is also expected to be positive, leading to an increase in firm value of Company 1. Conversely, if the automotive industry experiences a negative shock, the effect spilled over to Company 1 should be negative, leading to a decrease in firm value of Company 1.

Figure 4.2: Tobin's Q and director distraction over time

The graph plots the average quarterly Tobin's Q for the subgroups of no distraction ($Distraction_{ft} = 0$) firms and high distraction ($Distraction_{ft} > 0.205$) firms over time. ***, **, and * denote significance of the difference between the no distraction and high distraction groups at 1%, 5%, and 10%, respectively.



In column (5) and (6) of Table 4.3, we consider distraction from positive and negative industry shocks separately and reestimate their effect on firm value. Distraction (positive) uses industries with abnormally high volatility and positive performance as attention-grabbing industries, whereas Distraction (negative) uses only industries with abnormally high volatility with negative performance as attention-grabbing industries. The results indicate that the coefficients of the distraction measures have the same negative sign as in the other columns. The magnitude and t-statistics are smaller than those in the other columns, but this is not surprising as each measure ignores many other attention-grabbing cases and sends many firms with high distraction to the control group of firms with low or no distraction. The stronger effect of negative industry shocks is consistent with the idea that industries with negative shocks demand more director attention because directors may face higher pressure when the firm experiences an unfavorable industry shock. The finding that positive shocks to other industries also affect firm value negatively is consistent with our conjecture of director distraction and mitigates the concern that our results are merely driven by industry spillover effects.

In Table 4.4, we test whether our results are robust to alternative definitions of industry shocks and alternative industry classifications. Our main director distraction measure is based on stock volatility to measure attention-grabbing events. Instead, we now follow Barber and Odean (2008) and Kempf et al. (2017) and consider three alternative ways of capturing salient events in a given industry: extreme positive returns, extreme negative returns, and trading volume. For extreme positive (negative) returns, we consider the industries with quarterly stock performance in the top (bottom) decile as attention-grabbing industries. For trading volume, we define the attention-grabbing industries as those that have the highest (top-decile) abnormal trading volume with respect to the previous three quarters, computed as in Equation (4.3). We reestimate the specification from Column (3) and (4) of Table 4.3 using these three alternative definitions of industry shocks. As shown in Table 4.4, using these alternative measures of attention-grabbing events produces results qualitatively similar to our results based on stock volatility.

In addition, we consider three alternative industry classifications, namely the Fama-French 12 industries, the SICH two-digit industries, and the Hoberg and Phillips (2016) 10-K text-based 50-industry classifications (FIC-50).[6] For each industry classification, we measure director distraction using our baseline volatility-based definition of industry shocks as well as the three alternative definitions. Table 4.4 shows that using the alternative industry classifications leads to results qualitatively similar to our results based on the Fama-French 49 industry classification. Overall, the findings in Table 4.4 indicate that our results are not driven by a particular industry classification and are robust to alternative measures of attention-grabbing events within a given industry.

An alternative way to test the effect of director distraction on firm value is to investigate how director attention directly affects firms' stock returns. To

[6] For each two-digit SIC/FIC-50 industry, we construct a value-weighted portfolio using all firms in the CRSP database with a stock price above $5 in that industry.

Table 4.4: Robustness: alternative industry classifications and definitions of industry shocks

In this table we test the robustness of our results for alternative definitions of industry shocks and industry classifications. Besides our baseline volatility-based distraction measure, we use the alternative definitions of industry shocks. Extreme positive (negative) returns consider the industries with quarterly stock performance in the top (bottom) decile as attention-grabbing industries. Trading volume defines the attention-grabbing industries to be those with the highest (in top-decile) abnormal trading volume with respect to the previous three quarters, computed similarly as in Eq. (4.3). We use the Fama-French 12 industries, the two-digit SICH code industries, and the Hoberg and Phillips (2016) 10-K text-based 50 industries (FIC-50) as alternative industry classifications. For each two-digit SICH/FIC-50 industry, we construct a value-weighted portfolio using all CRSP stocks priced above 5 dollars within that industry. We reestimate the specifications from columns (3) and (4) of Table 4.3. For brevity we only report the coefficient of the distraction variables and suppress those of control variables. Standard errors are clustered at the firm level, and the corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

Industry classification	Industry shocks	Firm FE & Industry × quarter FE		FE with controls	
		Coeff.	t-stat.	Coeff.	t-stat.
Baseline:					
Fama-French 49	Volatility	-0.271***	(-5.332)	-0.237***	(-5.387)
Alternatives:					
Fama-French 49	Extreme positive returns	-0.207***	(-3.340)	-0.167***	(-3.091)
Fama-French 49	Extreme negative returns	-0.346***	(-3.530)	-0.318***	(-3.511)
Fama-French 49	Trading volume	-0.224**	(-2.353)	-0.196**	(-2.197)
Fama-French 12	Volatility	-0.216***	(-3.740)	-0.174***	(-3.118)
Fama-French 12	Extreme positive returns	-0.181***	(-3.583)	-0.223***	(-2.802)
Fama-French 12	Extreme negative returns	-0.273***	(-5.646)	-0.268***	(-4.772)
Fama-French 12	Trading volume	-0.224**	(-2.118)	-0.152	(-1.558)
Two-digit SIC	Volatility	-0.313***	(-6.075)	-0.267***	(-5.259)
Two-digit SIC	Extreme positive returns	-0.247***	(-2.981)	-0.206**	(-2.498)
Two-digit SIC	Extreme negative returns	-0.359***	(-5.405)	-0.199**	(-2.328)
Two-digit SIC	Trading volume	-0.276***	(-3.262)	-0.231***	(-3.188)
Hoberg-Phillips 50	Volatility	-0.405***	(-5.739)	-0.334***	(-5.278)
Hoberg-Phillips 50	Extreme positive returns	-0.408***	(-5.055)	-0.370***	(-4.630)
Hoberg-Phillips 50	Extreme negative returns	-0.422***	(-5.756)	-0.366***	(-5.083)
Hoberg-Phillips 50	Trading volume	-0.434***	(-6.166)	-0.367***	(-5.105)

this end, we use monthly stock price data from CRSP and match each month to the corresponding fiscal quarter. Table 4.5 reports the effect of director distraction on firms' stock market performance. In Columns (1) and (2), the dependent variable is the cumulative excess stock returns ($Ret - R_f$) over each fiscal quarter. We also use two risk-adjusted stock returns as alternative measures in Columns (3)-(6), namely, market-adjusted returns (CAPM) and Fama-French risk-adjusted returns (FF4). To compute the market-adjusted returns, we first estimate the CAPM to obtain the market beta for each stock at the beginning of each fiscal quarter using monthly returns data from the past 36 months, and then compute the abnormal return as the excess return over the product of the market beta and the market return in a given fiscal quarter. To compute the Fama-French risk-adjusted returns, we first estimate the Fama-French and Carhart four-factor model ($R_{it} - R_{ft} = \alpha + \beta_{i,mkt} MKT_t + \beta_{i,HML} HML_t + \beta_{i,SMB} SMB_t + \beta_{i,UMD} UMD_t + \varepsilon_{it}$) to obtain factor betas for each stock in the beginning of each fiscal quarter using monthly returns data of the past 36 month, and then compute the abnormal return as the excess return over the product of the factor betas and the four-risk factors in a given fiscal quarter. In Columns (1), (3) and (5), the model is estimated with quarter fixed effects, and in Columns (2), (4), and (6), the model is estimated with stock fixed effects. We further include the returns of the Fama-French 49 industry portfolios to control for industry $\times$ quarter level trends.

Table 4.5 shows that firms' stock performance is significantly worse when their directors are distracted. A deviation from no distraction to the average distraction level of 0.205 leads to an underperformance of about 72 basis points ($= -0.035 \times 0.205$) per quarter. The coefficient of director distraction remains statistically significant when using market-adjusted and Fama-French risk-adjusted returns.

4.4.2 Potential channels

Our results thus far support the notion that firms have lower valuation when their board members are distracted. Next, we test which underlying mechanism could explain the negative effects of director distraction. When managers

Table 4.5: Effects of director distraction on stock performance

This table reports the effect of director distraction on firms' stock performance. In column (1) and (2), the dependent variable is the cumulative excess stock returns ($Ret - R_f$) over each fiscal quarter. We also use two risk-adjusted stock returns as alternative measures in columns (3-6), namely, the market-adjusted returns, CAR (CAPM), and the Fama-French risk-adjusted returns, CAR (FF4). To compute the market-adjusted returns, we first estimate the CAPM model to obtain the market beta for each stock in the beginning of each fiscal quarter using monthly returns data of the past 36 month, and then compute the abnormal return as the excess return over the product of the market beta and the market returns in a given fiscal quarter. To compute the Fama-French risk-adjusted returns, we first estimate the Fama-French and Carhart four-factor model ($R_{it} - R_{ft} = \alpha + \beta_{i,mkt} MKT_t + \beta_{i,HML} HML_t + \beta_{i,SMB} SMB_t + \beta_{i,UMD} UMD_t + \varepsilon_{it}$) to obtain the factor betas for each stock in the beginning of each fiscal quarter using monthly returns data of the past 36 month, and then compute the abnormal return as the excess return over the product of the factor betas and the four-risk factors in a given fiscal quarter. In column (1), (3) and (5), the model is estimated with quarter fixed effects, whereas in the other columns the model is also estimated with stock fixed effects. Fama-French 49 industry portfolios are included to control for industry × quarter level trends. Standard errors are clustered at the stock level, and the corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	Cumulative returns		CAR (CAPM)		CAR (FF4)	
	(1)	(2)	(3)	(4)	(5)	(6)
Distraction	-0.035***	-0.033***	-0.034***	-0.031***	-0.026***	-0.024***
	(-5.262)	(-4.623)	(-5.295)	(-4.646)	(-3.910)	(-3.450)
Log(Assets)	0.000	-0.007***	0.000	-0.007***	0.000	-0.007***
	(0.220)	(-4.480)	(0.429)	(-5.015)	(0.414)	(-4.629)
Board size	0.004***	0.004***	0.004***	0.003***	0.004***	0.003***
	(10.320)	(6.464)	(9.254)	(4.446)	(8.641)	(3.986)
Board busyness	-0.022***	-0.022***	-0.015***	-0.016***	-0.011**	-0.009
	(-5.329)	(-3.907)	(-3.615)	(-2.825)	(-2.438)	(-1.528)
Board independence	-0.009*	-0.013	-0.004	0.001	-0.001	0.004
	(-1.663)	(-1.609)	(-0.760)	(0.114)	(-0.157)	(0.485)
Industry returns	0.936***	0.937***	0.401***	0.397***	0.274***	0.269***
	(66.205)	(65.508)	(31.874)	(31.527)	(20.525)	(19.978)
Observations	75,005	75,005	75,005	75,005	75,005	75,005
Adj. R^2	0.295	0.306	0.073	0.092	0.025	0.043
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Stock FE	No	Yes	No	Yes	No	Yes

receive less monitoring from distracted directors, two potential agency problems might be exacerbated: 1) managers engage in empire building and make value-destroying investment decisions (Jensen, 1986), or 2) they become more passive and enjoy a quiet life (Bertrand and Mullainathan, 2003). Alternatively, director distraction might not lead to higher agency frictions, but 3) managers might miss important advice or have to delay making important decisions when it is difficult to schedule meetings with distracted directors for discussion and approval.

Overinvestment

In Table 4.6 we test whether director distraction leads to managerial empire building by studying firms' capital expenditures to total assets (CAPEX) and merger and acquisition (M&A) activities. In Columns (1)-(6), the model is estimated with industry $\times$ quarter fixed effects to control for the effect of industry-wide investment shocks such as technology innovations and merger waves. We include standard control variables in investment regressions: firm size, one-quarter lagged Tobin's Q, and cash flow, as well as board size, busyness, and independence. In addition, we control for institutional ownership and institutional investor distraction as in Kempf et al. (2017), which could affect corporate investment decisions.

As shown in Table 4.6, we find that firms invest significantly less when directors are distracted. In terms of capital expenditure, a deviation from no distraction to the average distraction level of 0.205 is associated with a drop of 0.6% ($= -0.021 \times 0.205/0.690$) in firms' CAPEX. The effect remains similar and statistically significant when we also control for firm fixed effects.

In addition to capital expenditure, we examine firms' takeover decisions. Acquisitions are sizable and non-routine investments in which management is clearly heavily involved. Because we observe deal announcement dates, we can also study whether managers decide on the timing of the deal conditional on the monitoring intensity of the board. Moreover, we can compute deal announcement returns to examine how the market reacts to the deal, which allows us to get insights into whether the deal creates or destroys shareholder

Table 4.6: Effect of director distraction on firm investment

This table reports the effect of director distraction on firm investment. In column (1) and (2), the dependent variables are firms' capital expenditures (CAPEX). In column (3) and (4), the dependent variable is acquisition which equals to 1 if the firm announces at least one acquisition in the given quarter. In column (5) and (6), the dependent variable is diversifying merger which equals to 1 if the announced acquisition deal is cross-industry. In all those columns, the model is estimated with industry × quarter fixed effects. Column (2), (4), and (6) additionally include firm fixed effects. The standard errors are clustered at the firm level. In column (7) and (8), the dependent variable is the 5-day CARs around the merger announcement date. In those two columns, the model is estimated with industry × year fixed effects, and standard errors are clustered at the industry level. All corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	CAPEX		Acquisition		Diversifying merger		CAR(-2, +2)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distraction	-0.021***	-0.018*	-0.019**	-0.005	-0.010*	-0.008*	0.018	0.018
	(-3.026)	(-1.867)	(-2.494)	(-0.705)	(-1.775)	(-1.870)	(1.296)	(1.250)
Log(Assets)	-0.012***	0.016***	0.022***	0.016***	0.011***	0.009***	-0.002***	-0.002***
	(-4.387)	(3.453)	(11.538)	(5.268)	(6.527)	(4.244)	(-3.197)	(-3.480)
Lagged Q	-0.004*	-0.001	0.005**	0.007***	0.003*	0.004***	-0.002***	-0.002*
	(-1.801)	(-0.609)	(2.463)	(3.877)	(1.907)	(3.243)	(-2.829)	(-1.975)
Cash flow	0.314***	0.226***	0.237***	0.276***	0.111***	0.116***	0.075	0.066
	(3.367)	(3.318)	(4.628)	(4.941)	(3.298)	(3.635)	(0.996)	(0.963)
Board size	-0.008***	-0.005***	-0.002***	-0.003***	-0.001	-0.001*	-0.000	-0.000
	(-6.052)	(-5.324)	(-2.798)	(-2.706)	(-1.303)	(-1.827)	(-0.489)	(-0.494)
Board busyness	-0.093***	-0.042***	-0.015*	-0.009	0.001	0.005	-0.009	-0.010
	(-7.872)	(-4.497)	(-1.888)	(-0.951)	(0.200)	(0.644)	(-1.218)	(-1.319)
Board independence	-0.056***	-0.019	-0.010	-0.018	0.001	-0.007	-0.002	-0.003
	(-4.221)	(-1.560)	(-1.131)	(-1.463)	(0.214)	(-0.704)	(-0.294)	(-0.464)
Investor distraction	0.058**	0.044**	0.032	0.022	0.001	0.005	-0.011	-0.006
	(2.430)	(2.246)	(0.861)	(0.583)	(0.041)	(0.171)	(-0.336)	(-0.193)
Institutional ownership	0.059***	0.068***	0.021**	0.039***	-0.001	0.012	-0.008	-0.006
	(3.677)	(4.889)	(2.158)	(3.407)	(-0.165)	(1.621)	(-1.232)	(-0.967)
Relative deal size								-0.013***
								(-2.860)
Diversifying deal								0.003
								(1.434)
Private target								0.005*
								(1.903)
Cross-border								0.000
								(0.161)
Observations	65,352	65,352	65,359	65,359	65,359	65,359	5,227	5,227
Adj. R^2	0.156	0.576	0.023	0.112	0.022	0.120	0.120	0.120
Industry × quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Firm FE	No	Yes	No	Yes	No	Yes	No	No
Industry × year FE	No	No	No	No	No	No	Yes	Yes

value.

In Columns (3) and (4) of Table 4.6, the dependent variable is a dummy variable that equals one if the firm announces at least one acquisition in the given fiscal quarter. The estimation results suggest that, when directors are distracted, firms are not more likely to announce an acquisition and build an empire. If anything, they are less likely to announce an acquisition.

To test whether managers pursue private benefits when they receive less monitoring, we test in Columns (5) and (6) of Table 4.6 whether firms make more diversifying mergers when directors are distracted. Studies have suggested that managers pursuing private benefits tend to make diversifying merger deals because these reduce CEO human capital risk and offer a chance to venture into industries that are considered fashionable, glamorous, or reputable (e.g., Amihud and Lev (1981), Morck, Shleifer, and Vishny (1990)). Interestingly, we find that firms are actually (about 5.7%) less likely to announce diversifying mergers when their directors are distracted.

Even though firms seem to make fewer acquisitions when their directors are distracted, the deals they make might still be value destroying for shareholders. Therefore, we examine deal announcement returns. The dependent variables are the five-day CARs around the deal announcement date in Column (7) and (8) of Table 4.6. We find that the announcement returns are not negative and significant conditional on director distraction.

In sum, when directors are distracted, firms do not seem to engage excessively in empire building or to make more value destroying investments. On the contrary, firms with high director distraction are significantly less active, have lower capital expenditures, and are less likely to announce an acquisition. Our findings suggest that distracted directors leave room for managers to enjoy a quiet life instead of maximizing shareholder value, which leads to a significant decrease in firm value.

It is also interesting to note that board members seems to play a different role in monitoring the management than institutional investors do. When institutional investors are distracted and reduce monitoring, managers tend to make more value-destroying investments (Kempf et al., 2017). Yet, when directors are distracted, managers seem to enjoy a quiet life rather than engage in empire building. This result is sensible as engaging in empire building when investors are not distracted is likely to lead to activism, whereas a period of relative inactivity is less likely to invoke investor activism.

Quiet life versus delayed decision making

Although the results in the prior subsection are more in line with the quiet life hypothesis (Bertrand and Mullainathan, 2003; Giroud and Mueller, 2010) than with empire building, they do not exclude alternative explanations. Most notably, it may be that managers simply cannot make or implement important decisions such as acquisition deals when it is difficult to schedule meetings with distracted directors for discussion and approval. Managers might also miss valuable advice from these distracted directors. Thus, managers might have to delay important decisions until directors are no longer distracted and can spend more time and energy on the firm.

If managers miss important advice, negative announcement effects might be expected for takeover deals, but director distraction might simply lead managers to postpone their investments. To examine this possibility, we compare firms' activities in times with high director distraction to those in subsequent times with no director distraction. The delayed decision making hypothesis predicts that, after a period in which directors are distracted, firms become significantly more active when director attention returns and managers are able to get advice and execute pending decisions.

We construct a subsample of firms that have two consecutive quarters in which director distraction is high ($Distraction_{ft} > 0$) and two subsequent consecutive quarters when there is no director distraction ($Distraction_{ft} = 0$). We refer to the quarters with high director distraction as the "before" period and to the subsequent quarters without distraction as the "after" period. In Table 4.7, we compare firms' capital expenditure, takeover decisions, and SEC filings in the before-period to those in the after-period. Firms' SEC filings are retrieved from the Edgar databases. We consider filings of all form types disclosed by the firms in our sample and use the filing dates to match the filing activity to our firm-quarters.

Panel A of Table 4.7 reports the means of the variables of interest in the before- and after-periods. The difference between the before- and after-periods is neither statistically nor economically significant for any of the variables. Panel B uses multivariate regressions, in which we include additional control variables and time and firm fixed effects. The coefficient on the dummy variable indicating the after-period is not significant in any of the specifications.

The evidence in Table 4.7 is more consistent with the quiet life hypothesis than with the delayed decision making hypothesis. Nevertheless, our findings do not rule out an effect from managers not being able to make decisions. Managers might miss valuable investment opportunities when they cannot receive approval or advice from distracted directors, and those investment opportunities might have been seized by competitors or have evaporated once director attention returns. Still, it seems unlikely that all investment opportunities would have evaporated the next period. In addition, when managers really want to push a value-increasing investment, there are ways to do this, even when some directors are time-constrained. Overall, our findings suggest that the loss in firm value when directors are distracted results mostly from managers enjoying a quiet life when they receive less monitoring from outside directors.

4.4.3 Effect from different groups of directors

Not every outside directors is assigned the same task. In this subsection, we examine the impact of distraction from various groups of directors on firm value. Important tasks that directors can have is to serve on the audit, nomination and/or compensation committee. We obtain information on committee membership from RiskMetrics. In Table 4.8, the dependent variable is Tobin's Q. In Columns (1)-(5), we interact the baseline *Distraction* variable with a dummy variable indicating whether at least one of the distracted director belongs to the corresponding group.

In Column (1) of Table 4.8, we show that distraction of committee members destroys firm value more than that of non-committee members as the corre-

Table 4.7: Testing the delayed decision making hypothesis

In this table we test the "delayed decision-making" hypothesis. We construct a subsample of firms that have two consecutive quarters in which director distraction is high ($Distraction_{ft} > 0$) while in the subsequent two consecutive quarters there is no director distraction ($Distraction_{ft} = 0$). We refer to the quarters with high director distraction as the "before" period and to the subsequent quarters without distraction as the "after" period. The variables of interests are capital expenditures, takeover decisions, and the number of SEC filings. Panel A reports the mean of the variables of interest in the "before" and "after" period, respectively. Panel B reports the results of multivariate regressions including time and firm fixed effects. In all regressions, $After$ is a dummy variable indicating the "after" period. Standard errors are clustered at the firm level, and the corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

Panel A: difference in means

		Distraction		Difference
		High (before)	No (after)	After - Before
	N	Mean	Mean	t-stat.
CAPEX	4,366	0.68	0.68	-1.01
Acquisition	4,366	0.06	0.05	-0.97
Log(1+Filings)	3,867	2.04	2.11	1.41

Panel B: OLS regressions

	CAPEX	Acquisition	Log(1 + Filings)
	(1)	(3)	(4)
After	-0.007	-0.006	0.001
	(-1.384)	(-0.575)	(0.040)
Log(Assets)	0.018	0.012	0.109***
	(1.099)	(0.840)	(2.739)
Board size	-0.018***	-0.002	-0.010
	(-3.031)	(-0.320)	(-0.487)
Board busyness	-0.065*	0.020	0.179
	(-1.668)	(0.457)	(1.251)
Board independence	-0.023	0.005	0.280
	(-0.495)	(0.120)	(1.377)
Lagged Q	-0.002	0.018**	0.040
	(-0.142)	(2.252)	(1.633)
Cash flow	0.184	0.067	-0.146
	(0.868)	(0.467)	(-0.287)
Observations	4,028	4,028	3,550
Adj. R^2	0.628	0.083	0.713
Quarter FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes

Table 4.8: Effect of different groups of directors

This table reports how distraction of different groups of directors affects firm value. The dependent variable is Tobin's Q. In all columns, the model is estimated with industry $\times$ quarter and firm fixed effects. In columns (1-5), we interact the baseline distraction variable with a dummy variable whether at least one of the distracted director belongs to the corresponding group. In column (6), we estimate the effect of distracted directors who are executives at the focal firm but hold directorships in the attention-grabbing industries. This distraction measure is computed in the same way as that of outside directors, that is, first indicate whether the executives hold any other directorships in the shocked industries, then aggregate individual executive-director's distraction at the firm-level, and finally scale by the total number of executives on the board. In all of the specifications, standard errors are clustered at the firm level, and the corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	Tobin's Q					
	(1)	(2)	(3)	(4)	(5)	(6)
Distraction	-0.126*	-0.199***	-0.233***	-0.186***	-0.254***	-0.238***
	(-1.935)	(-4.101)	(-4.709)	(-3.336)	(-4.658)	(-5.289)
Distraction $\times$ 1(All committee)	-0.173*					
	(-1.956)					
Distraction $\times$ 1(Audit)		-0.104				
		(-1.106)				
Distraction $\times$ 1(Nomination)			-0.018			
			(-0.200)			
Distraction $\times$ 1(Compensation)				-0.139*		
				(-1.917)		
Distraction $\times$ 1(Executive in shocked industry)					0.047	
					(0.551)	
Distraction (Executive-directors)						0.004
						(0.148)
Observations	75,331	75,331	75,331	75,331	75,331	75,331
Adj. R^2	0.589	0.589	0.589	0.589	0.589	0.589
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry $\times$ quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

sponding interaction term is negative and significant. Results in Columns (2)-(4) show that the stronger effect from committee members is mostly driven by distracted compensation-committee members. In fact, the distraction of audit- or nomination-committee members is not more detrimental to firm value than that of non-committee members. In Column (5), we show that firms do not suffer more if some of the distracted board members are executives in the shocked industries. It is important to note that the Distraction variable alone remains negative and highly significantly in all columns. This implies that the reduction in firm value due to distraction is not due to only one type of director; for example, it applies to directors both with and without executive roles in shocked industries.

In the final column of Table 4.8, we consider executive directors who hold directorships in the attention-grabbing industries. Our baseline analysis excludes executive directors because we assume that attention shocks from other directorships are less likely to distract directors from their primary occupation at the focal firms. However, it is possible that our results are partially driven by those distracted executives. We test this possibility by constructing the distraction of executive directors in the same way as that of outside directors and then estimating the effect of their distraction on firm value. As shown in Column (6), the effect of executive-directors' distraction is not statistically significant, and the effect of outside directors' distraction remains virtually identical to the baseline estimate in Table 4.3. These results are in line with executives at focal firms being less likely to get distracted. Furthermore, they indicate that our baseline results are robust to controlling for the effects of executive directors' distraction.

4.4.4 Distraction and directors' career outcomes

Our findings thus far suggest that temporary director distraction leaves room for managers to shirk at the expense of shareholders, which leads to a significant decline in firm value. It is then natural to ask whether shareholders take actions to replace distracted directors. As our study focuses on temporary distractions, this analysis could add to the evidence in Masulis and Zhang (2018) that more permanently distracted directors are replaced. The estimation results indicating whether temporarily distracted directors are more likely to be replaced in the next year are presented in Table 4.9, in which the dependent variable equals one when a director is replaced the next year.

The coefficients of Director distraction and the interaction effects in Columns (1)-(3) of Table 4.9 suggest that directors' temporary distraction because of other attention-grabbing industries does not significantly increase the proba-

Table 4.9: Effect of distraction on directors' career outcomes

This table reports how distraction affects directors' career outcomes. The dependent variable is a dummy variable that equals one if the director is replaced in the next year. Control variables are the same as those in Table 4.2. In Columns (4) and (5), we distinguish between whether the departure is voluntary or forced. We classify a departure as voluntary based on an analysis of news sources around turnover announcements (Alexandridis et al., Doukas, and Mavis, 2018) and/or if the age of the director is 72 or older. The remaining cases are classified as forced departures. Variables are defined in the Appendix. In all of the specifications, standard errors are clustered at the director level, and the corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	Replaced in the next year			Voluntary	Forced
	(1)	(2)	(3)	(4)	(5)
Director distraction	-0.002	-0.003	-0.003	-0.000	-0.003
	(-1.299)	(-1.328)	(-1.539)	(-0.232)	(-1.585)
Distraction × ΔTobin's Q		-0.001			
		(-0.300)			
Distraction × Attended < 75% board meetings			0.019*	-0.002	0.022*
			(1.698)	(-0.897)	(1.933)
ΔTobin's Q	-0.005***	-0.005**	-0.005***	-0.002*	-0.003*
	(-2.899)	(-2.301)	(-2.903)	(-1.952)	(-1.933)
Attended < 75% board meetings	0.062***	0.062***	0.047***	0.011*	0.036**
	(4.593)	(4.593)	(2.938)	(1.712)	(2.421)
Number of directorships	-0.023***	-0.023***	-0.023***	-0.008***	-0.015***
	(-12.433)	(-12.425)	(-12.519)	(-9.064)	(-8.718)
High ranked directorship	-0.012***	-0.012***	-0.012***	-0.000	-0.012***
	(-4.873)	(-4.873)	(-4.872)	(-0.335)	(-4.997)
Log(Director age)	0.225***	0.225***	0.225***	0.329***	-0.104***
	(14.412)	(14.411)	(14.412)	(30.382)	(-6.443)
Independent	-0.049***	-0.049***	-0.049***	-0.014***	-0.035***
	(-7.873)	(-7.874)	(-7.851)	(-5.120)	(-5.906)
Board size	-0.011***	-0.011***	-0.011***	0.000	-0.011***
	(-17.973)	(-17.968)	(-17.974)	(0.168)	(-18.423)
Observations	59,312	59,312	59,312	59,312	59,312
Adj. R^2	0.016	0.016	0.016	0.055	0.014

bility of their departure, even if the distraction is associated with lower firm values (ΔTobin's Q), unless the distraction is also associated with board meeting absence. In other words, temporarily distracted directors are replaced only when the distraction leads them to actually miss board meetings. One interpretation of this result is that shareholders take actions to replace distracted directors once they miss board meetings. An alternative interpretation is that distracted directors who attend fewer board meetings resign voluntarily to be able to focus more on other directorships. To obtain some insights into these different interpretations, we distinguish between voluntary and forced departures in the last two columns of Table IX. We classify a departure as voluntary if an analysis of news sources around the turnover announcement indicates that the director stepped down voluntarily and/or if the age of the director upon the departure is above 72 years, which corresponds to the most common retirement age cited in the policies of S&P 1500 companies.[7] We consider the remaining cases to be more representative of forced departures. Using this classification, the results in Columns (4) and (5) of Table 4.9 show that missed board meetings due to director distraction are significantly related to forced departures, but not to voluntary departures.

Overall, our findings indicate that shareholders take actions to replace distracted directors once the distraction becomes observable in terms of board meeting absence. These findings add to the literature as our measure of distraction is based on temporary attention-grabbing events in unrelated industries, which are events that shareholders of the focal firm might not easily link to perceived director distraction (as opposed to, e.g., severe health issues of a director). In our setting, shareholders may more easily observe the outcome of distraction rather than the cause.

4.5 Alternative explanations and robustness

The results in the previous section are consistent with our conjecture that distracted directors spend less time and energy monitoring and advising managers, which leaves room for managers to shirk and leads to decreases in firm value. In this section, we test and rule out some alternative explanations that could drive our results.

[7] See Jon Lukomnik, "Board Refreshment Trends at S&P 1500 Firms," Harvard Law School Forum on Corporate Governance and Financial Regulation (February 9, 2017), https://corpgov.law.harvard.edu/2017/02/09/board-refreshment-trends-at-sp-1500-firmshttps://corpgov.law.harvard.edu/2017/02/09/board-refreshment-trends-at-sp-1500-firms. The classification based on news sources follows the Alexandridis, Doukas, and Mavis (2018) analysis of CEO replacements. We thank Christos Mavis for his help with this analysis and for sharing data.

4.5.1 Endogeneity of director choice and industry relatedness

An alternative explanation that we explained earlier is related to the endogeneous nature of director choice. Because directors are likely to sit on the boards of firms in related industries, our results could be driven by industry spillover effects (Dass, Kini, Nanda, Onal, and Wang, 2014). Our use of fixed effects and our finding that both positive and negative shocks in a different industry decrease firm value in companies with distracted directors reduce this concern. Nevertheless, one could still argue that a positive shock in one industry can sometimes create a negative shock to another industry, especially when those industries are vertically related. For example, positive oil price shocks are good news for oil producers, but often reduce the profitability of oil consumer industries. In this section, we add two pieces of evidence to alleviate the concern of industry spillovers.

First, as noted, oil and gas industries often experience price shocks that are exogenous to any individual firm, and then spillover to other related industries with opposite effects (e.g., Lamont, 1997). To rule out the spillover effects from energy industries, we modify our distraction measure by removing attention shocks from oil and gas industries, and focus instead on a subsample that excludes firms operating in oil and gas industries.[8] In Table 4.10, we reestimate the baseline specifications from Columns (4)-(6) of Table 4.3. In addition to Tobin's Q, we use CAPEX and Acquisitions as dependent variables. We find that the coefficient estimates of the adjusted director distraction variables are similar to the baseline results. The magnitude and t-statistics are smaller for the distraction variable based on positive and negative attention shocks separately, which is not surprising as each measure now ignores some attention-grabbing cases and sends some firms with high distraction to the control group of firms with low or no distraction.

Second, we disregard shocks from supplier or customer industries. We use the three-digit NAICS code to classify industries, which allows us to exclude industries that are likely to have supplier and/or customer relationships. We detect possible economic links by using the 2007 U.S. Input-Output Tables

[8] Oil and gas industries correspond to Fama-French 49-industry codes 28-31.

Table 4.10: Additional tests concerning industry spillovers

This table provides evidence mitigating the concern that our results are merely driven by industry spillover effects. First, we exclude firms operating in oil or gas industries and disregard attention shocks from those industries. Second, we use the three-digit NAICS code as industry classification to exclude industries that are likely to have supplier or customer relationships. We reestimate the baseline specifications in column (4-6) from Table 4.3 with Tobin's Q, CAPEX, and acquisition as dependent variables in each panel respectively. In all specifications, the model is estimated with quarter fixed effects and firm fixed effects. Included control variables are the same as in Table 4.3 and 4.6 respectively, but are suppressed for brevity. Standard errors are clustered at the firm level, and the corresponding *t*-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

Panel A: Tobin's Q

	Tobin's Q					
	Subsample excl. Oil & Gas			Unrelated Naics Industries		
	(1)	(2)	(3)	(4)	(5)	(6)
Distraction	-0.370***			-0.639***		
	(-5.273)			(-3.985)		
Distraction (positive)		-0.189**			-0.169*	
		(-2.325)			(-1.781)	
Distraction (negative)			-0.283**			-0.876***
			(-2.268)			(-5.783)
Observations	70,722	70,722	70,722	65,359	65,359	65,359
Adj. R^2	0.169	0.168	0.168	0.176	0.176	0.176

Panel B: Capital expenditure

	CAPEX					
	Subsample excl. Oil & Gas			Unrelated Naics Industries		
	(1)	(2)	(3)	(4)	(5)	(6)
Distraction	-0.015*			-0.039**		
	(-1.924)			(-2.441)		
Distraction (positive)		-0.024*			-0.031*	
		(-1.733)			(-1.692)	
Distraction (negative)			0.008			-0.064***
			(0.702)			(-3.265)
Observations	61,467	61,467	61,467	65,352	65,352	65,352
Adj. R^2	0.156	0.156	0.156	0.076	0.076	0.077

Panel C: Acquisitions

	Acquisitions					
	Subsample excl. Oil & Gas			Unrelated Naics Industries		
	(1)	(2)	(3)	(4)	(5)	(6)
Distraction	-0.019**			-0.031*		
	(-2.495)			(-1.777)		
Distraction (positive)		-0.028**			-0.048***	
		(-2.100)			(-2.784)	
Distraction (negative)			-0.012			-0.050***
			(-1.067)			(-2.611)
Observations	61,474	61,474	61,474	65,359	65,359	65,359
Adj. R^2	0.024	0.024	0.024	0.013	0.013	0.013

from the Bureau of Economic Analysis, which are based on NAICS codes and provide detailed information about the flows of the goods and services among industries.[9] We define supplier and customer industries as those that have any flows to or from a given industry.

In Table 4.10, we use director distraction measures constructed based on NAICS codes and attention shocks from plausibly unrelated industries. The magnitude and t-statistic of the coefficient estimates are similar to those in the baseline Tables 4.3 and 4.6, suggesting that our distraction measure does indeed captures director attention shocks rather than just industry relatedness and comovement.

4.5.2 Single-segment firms

Another potential concern is that our results are simply driven by the multi-segment structure of conglomerate firms. Because our sample consists of S&P 1500 firms, which are relatively large, many of the firms in our sample operate in multiple industries. If company 1 in our thought experiment also operates in the automotive industry, then shocks in the automotive industry could directly affect the investment and valuation of company 1, even though the automotive segment is not the primary segment of company 1 (Lamont, 1997; Stein, 1997).

To address this concern, we construct a subsample of single-segment firms, based on the number of segments reported in Compustat's segment files, and reestimate the regressions in Tables 4.3, 4.5, and 4.6. If our results are driven by sub-segments of conglomerate firms, we should find an insignificant effect of director distraction on the investment and valuation of single-segment firms.

As shown in Table 4.11, the effect of director distraction estimated for single-segment firms is similar to that in Tables 4.3, 4.5, and 4.6. This similarity applies to both the magnitude and the statistical significance of the effects. As such, our findings in Section 4.4 do not seem to be driven by the internal capital market of conglomerate firms.

[9]We use the 2007 table of commodities by industry valued at purchasers' prices under the Use Tables/After Redefinitions/Purchaser Value (https://www.bea.gov/industry/io_annual.htm).

Table 4.11: Results of single-segment firms

This table replicates the main results in Table 4.3 and 4.6 for the subsample of single-segment firms. We identify single-segment firms according to the number of segments reported in Compustat's segment files. In all columns, the model is estimated with quarter fixed effects and firm fixed effects. Included control variables are the same as in Table 4.3 and 4.6. Standard errors are clustered at the firm level, and the corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	Tobin's Q	CAR (CAPM)	CAPEX	Acquisition	Diversifying merger
	(1)	(2)	(3)	(4)	(5)
Distraction	-0.262***	-0.034***	-0.034***	-0.022***	-0.010*
	(-3.868)	(-3.820)	(-3.389)	(-2.657)	(-1.688)
Log(Assets)	-0.386***	-0.011***	-0.003	0.020***	0.007***
	(-7.395)	(-4.512)	(-1.120)	(8.382)	(3.523)
Board size	0.015	0.002**	-0.013***	-0.004***	-0.001**
	(0.918)	(2.270)	(-8.164)	(-3.736)	(-2.187)
Board busyness	-0.204	-0.000	-0.093***	-0.022**	0.002
	(-1.294)	(-0.052)	(-6.373)	(-2.322)	(0.400)
Board independence	-0.231	0.003	-0.070***	0.015	0.017**
	(-1.169)	(0.299)	(-4.466)	(1.432)	(2.371)
Lagged Q			0.001	0.008***	0.004**
			(0.509)	(4.200)	(2.209)
Cash Flow			0.045	0.054	0.012
			(0.420)	(1.018)	(0.395)
Investor distraction			-0.025	-0.070*	-0.017
			(-1.063)	(-1.812)	(-0.632)
Institutional ownership			0.032	0.030***	0.009
			(1.636)	(2.699)	(1.204)
Observations	54,316	43,188	47,666	47,670	47,670
Adj. R^2	0.526	0.034	0.065	0.012	0.005
Quarter FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes

4.5.3 Robustness checks: Matching

In addition to OLS estimations, we now use the nearest-neighbor and propensity-score-matching strategies to test the robustness of our results (Abadie and Imbens, 2006). More specifically, firms with high director distraction ($Distraction_{ft} > 0.205$) are in the treatment group, and we construct control groups of firms that have no director distraction ($Distraction_{ft} = 0$) and are matched to the treated firms along a set of relevant and observable characteristics: firm size (logarithm of total assets), one-quarter lagged Tobin's Q, board size, busy board (ratio), board independence (ratio), fiscal year and quarter, and Fama-French 49-industry classification. Each observation in the treatment group is matched with the nearest observation in the control group. Table 4.12 reports the results of the matching analysis.

Table 4.12: Results of nearest-neighbor and propensity-score matching

This table reports the results from nearest-neighbor and propensity-score matching estimation. The considered outcome variables are Tobin's Q, capital expenditure (CAPEX), acquisition likelihood, and diversifying deal likelihood. Firms with high director distraction ($Distraction_{ft} > 0.10$) are in the treatment group, and we construct control groups of firms that have no director distraction ($Distraction_{ft} = 0$) and are matched to the treated firms along a set of relevant and observable characteristics: firm size (the logarithm of total assets), one-quarter lagged Tobin's Q, board size, busy board (ratio), board independence (ratio), fiscal year and quarter, and Fama-French 49 industry. Each observation in the treatment group is matched with the "nearest" observation out of the control group. In Panel A we determine the "nearest" by using a weighted function of the covariates. In Panel B and C we determine the "nearest"by using the propensity scores estimated respectively by the logistic treatment model and probit treatment model. Each panel reports the estimated average treatment effect of high director distraction, robust Abadie-Imbens standard error, corresponding z-statistic, and number of observations in the treatment group. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

Panel A: Nearest-neighbor matching

	Tobin's Q	CAR (CAPM)	CAPEX	Acquisition	Diversiying merger
ATE	-0.130***	-0.031***	-0.014***	-0.005	-0.001
S.E.	0.011	0.005	0.003	0.005	0.004
z-stat.	-11.418	-6.398	-4.852	-0.955	-0.284
N	8,557	7,678	8,571	8,573	8,573

Panel B: (Logistic) Propensity-score matching

	Tobin's Q	CAR (CAPM)	CAPEX	Acquisition	Diversiying merger
ATE	-0.060***	-0.025***	-0.011***	-0.011**	-0.006*
S.E.	0.015	0.005	0.003	0.005	0.003
z-stat.	-4.101	-5.488	-3.284	-2.354	-1.769
N	8,557	7,678	8,571	8,573	8,573

Panel C: (Probit) Propensity-score matching

	Tobin's Q	CAR (CAPM)	CAPEX	Acquisition	Diversiying merger
ATE	-0.077***	-0.022***	-0.012***	-0.015***	-0.008***
S.E.	0.029	0.005	0.004	0.004	0.003
z-stat.	-2.659	-4.831	-2.984	-3.457	-2.667
N	8,557	7,678	8,571	8,573	8,573

In Panel A of Table 4.12, we determine the nearest match by using a weighted function of the covariates. In Panels B and C, we determine the nearest match by using the propensity scores estimated by a logistic treatment model and probit treatment model, respectively. We find a negative and significant effect of high director distraction on firms' valuation and investment in all specifications, consistent with our baseline results in Section 4.4. The matching estimates are even larger in economic magnitude and stronger in statistical significance.

4.6 Conclusion

Boards of directors are tasked with the critical function of actively monitoring and advising top management. By exploiting exogenous shocks to unrelated industries in which directors have additional directorships, we show that director attention affects board monitoring intensity and thereby firm value as management becomes less active. Firms with more director distraction invest significantly less and are less likely to announce takeovers. These changes are due to firms with distracted directors being less active rather than postponing their investments. Our results suggest that an effective board of directors prevents manager from shirking or enjoying a quiet life at the expense of shareholder value.

Our results contribute to the important and lively debate on the busyness of directors. Directors holding multiple directorships have to divide their attention, but the reason they are appointed to multiple boards likely reflects their quality. Isolating busyness from ability is therefore a challenging task, as having multiple directorships might reflect both. Our study is able to disentangle busyness from director ability and provides evidence on the costs of having busy directors. As such, our findings render support for policies restricting the number of directorships that an individual is allowed to have. Indeed, according to the Spencer and Stuart U.S. Board Index 2016 Report, 74% of S&P 500 firms now impose some restrictions on their directors' ability to accept other corporate directorships, compared to 27% in 2006.

4.A Appendix

Table 4.A.1: Variable description

Variable	Description
Dependent variables	
Tobin's Q	Book value of assets plus the market value of common equity minus the book value of common equity and deferred taxes divided by total assets: $(atq + (cshoq * prccq) - ceqq)/atq$
Cumulative returns	Cumulative excess stock returns $(Ret - R_f)$ over each fiscal quarter
CAR (CAPM)	Cumulative market-adjusted returns
CAR (FF4)	Cumulative returns adjusted for the four Fama-French risk factors
CAPEX	Invested capital divided by lagged total assets: $icaptq/atq_{t-1}$
Acquisition	Dummy variable equal to one if a firm announces an M&A transaction in a given fiscal quarter and zero otherwise. We consider all majority-stake acquisitions recorded in SDC between 1996-2014 with a minimum deal value of $10 million.
Diversifying merger	Dummy variable equal to one if a firm announces a cross-industry M&A transaction in a given fiscal quarter and zero otherwise. A deal is cross-industry if the bidder and target are not in the same Fama-French 49 industries.
Explanatory variable	
Distraction	Firm-quarter level director distraction, computed as described in section 4.3
Distraction (positive)	Firm-quarter level director distraction where the attention-grabbing industries not only have abnormally high volatility, but also have cumulatively a positive return in that given quarter
Distraction (negative)	Firm-quarter level director distraction where the attention-grabbing industries not only have abnormally high volatility, but also have cumulatively a negative return in that given quarter
Control variables	
Total assets ($million)	*Atq*
Log(Assets)	Logarithm of total assets: $\log(atq)$
Lagged Q	Previous fiscal quarter's Tobin's Q
Cash flow	Previous fiscal quarter's operating income before depreciation divided by lagged total assets: $oibdpq/atq_{t-1}$
Board size	Number of directors
Busy board	Number of directors sitting on more than one board divided by number of directors

Board independence	Number of independent directors divided by number of directors
Institutional ownership	Fraction of the firm's stock owned by institutional investors as reported in the Thomson Reuters 13f database
Investor distraction	Investor distraction computed as in Kempf et al. (2016) with Fama-French 49 industries; and attention-grabbing industries are the three best and three worst performing industries
Merger deal level variables	
CAR(-2, +2)	Five-day cumulative abnormal return around the merger announcement date with estimation window (-280, -31)
Relative deal size	Value of transaction divided by current quarter's total asset
Diversifying deal	Dummy variable equal to one if the acquirer and target are not in the same two-digit SIC industry
Private target	Dummy variable equal to one if the target firm is private
Cross-border	Dummy variable equal to one if the acquirer and target are not in the same country
Director level variables (from RiskMetrics)	
Director distraction	Director i's distraction regarding firm f in a given fiscal year, computed as summing up D_{ift} from Eq. (4.1) over the four quarters in that fiscal year
Industry Shock	Measure of the attention-grabbingness of a given industry, computed as in Eq. (?)
Attendance $<$ 75% board meeting	Dummy variable equal to one if a director has attended less than 75% of board meetings in a given year: $attend\_less75\_pct$
Director age	Age
Log(Director age)	Logarithm of director age: log(Age)
High ranked directorship	Dummy variable equal to one if the market cap of this directorship is greater than median of the market cap across all firms that the director serves on the board
Independent	Dummy variable equal to one if a director is classified as independent
Number of directorships	Number of total board seats at public companies: $outside\_public\_boards + 1$
Yearly Tobin's Q	Tobin's Q at the end of the current fiscal year

Chapter 5

Labor Markets of Financial Analysts[1]

5.1 Introduction

Workers at more prestigious companies have on average better performance. For example, academic researchers at higher ranked schools have better publication records; attorneys at larger law firms win more court cases; and sell-side equity analysts employed by more reputable brokerage houses produce more accurate earnings forecasts. This performance premium is driven by two distinct effects: the direct effect (influence) of more resourceful employers and the selection effect because of sorting in the labor market, which leads more prestigious companies to hire better candidates. This sorting mechanism creates an endogeneity problem, making it troublesome to establish and quantify the causal effect of employers on workers' performance.

The purpose of this paper is to disentangle those two confounding effects and quantify their relative importance, by estimating a two-sided matching model for the labor market of sell-side equity analysts. Analysts play an important role in gathering, analyzing, and distributing information in financial markets. Their most important outputs are earnings forecasts, and they have strong incentives to accurate predictions. Mikhail et al. (1999), Hong et al.

[1] This chapter is based on Renjie and Xia (2019).

(2000), and Groysberg et al. (2011) show that more accurate forecasts can help analysts avoid job termination or move down to less reputable brokerage firms, especially for early career analysts. Also, Stickel (1992b) and Groysberg et al. (2011) show that analysts with higher forecast accuracy are more likely to be nominated as "All-star" analysts and earn higher compensations.

We find that new analysts working for more reputable brokerage firms are more accurate on average. An analyst employed by the most reputable brokerage is about 6% more accurate than an analyst employed by a minor brokerage, which is equivalent to an advantage of 17.5 years of more experience. This performance premium is driven by the fact that more reputable brokerage firms have more resources that improve analysts' forecast accuracy; and by the sorting effect, whereby more reputable brokerage firms attract more talented analysts who are intrinsically better forecasters. Using a two-sided matching model, we are able to quantify the relative importance of these two distinct effects in determining analyst forecast accuracy. We find that both effects are important, and the influence effect accounts for 73% of the total effect of brokerage firms' reputation on analyst forecast accuracy, while the sorting effect accounts for the remaining 27%.

More reputable brokerage houses can help their new analysts improve their forecast accuracy in several ways. First, analysts working for more reputable brokerage firms may have access to better data and research support (Clement, 1999b). Better information acquisition and analysis in more reputable brokerage houses lead to more accurate forecast results. Second, analysts working for more reputable brokerage firms may have better personal communication opportunities with the management teams they follow (Clement, 1999b), and private interactions with these teams is one of the most influential factors that determine forecast accuracy (Soltes, 2013; Brown et al., 2015). On the other hand, sorting captures the effect that better-talented analysts are attracted to work for more reputable brokerage firms. Therefore, even if brokerage firms' reputations have no direct impact on analysts' forecast performance, we still observe that analysts who work for higher-reputation brokerage firms perform better, because the sorting effect leads to positive assortative matching between analysts' individual talent and broker reputation.

Distinguishing these two effects is challenging. Brokerage firm reputation becomes endogenous when better-talented analysts work for more reputable firms, and analysts' talent cannot be perfectly measured. The unobserved part of talent can then be correlated with the brokerage firm reputation measure, and the estimated effect of brokerage firm reputation will be biased upward. This concern increases when we focus on new analysts where the datasets contain little information on their abilities. The ideal solution to this endogeneity problem is to find an instrumental variable that is independent of an analyst forecasting ability but correlates with the reputation of the brokerage house hiring this analyst. However, the matching decision between analysts and brokerage firms are mutual choices, and it is a complicated process involving a number of observable and unobservable factors. To the best of our knowledge, there is no valid instrument that solve this endogeneity problem.

To circumvent this endogeneity issue, we take a structural approach similar to Sorensen (2007a). Our structural model contains two key elements: first, an outcome equation that models the determinants of analysts forecast accuracy, and second, a one-to-many associative matching model that captures the sorting process. The matching model explicitly models the matching process between analysts and brokerage firms and allows for matching decisions to interact with different agents. The matching decision interaction between agents creates difficulties in estimating the model, but also provides a rank order property that is useful for identification. The rank order property of the two-sided matching model means that the matching decision depends on the relative ranking of the agents in the market. Therefore, it not only depends on the characteristics of the matched agents themselves, but also on the other agents' characteristics. If the agents' characteristics vary exogenously across the market, we can identify the sorting effect by comparing the performance difference between analysts of different quality but match with brokerage firms with similar reputations in different markets. Similarly, we can identify the influence effect by comparing the performance difference between analysts with similar quality but match with brokerage firms with different reputation in different markets.

The key identification assumption is that agents are exogenously assigned across different markets. That is, we need sufficient variation across the new analyst labour market, and agents cannot choose to participate in a particular market for reasons correlated with the agents' characteristics in that market. In this study, we assume the new analyst labour market is segregated by the calendar year and geographically. A similar identification assumption has been made in Sorensen (2007a), Park (2013), Chen (2014), Ni and Srinivasan (2015), Pan (2015), Akkus et al. (2016a), and Xia (2018).

Agents' matching decisions interact, so any analysis of the likelihood function of one agent's decision must also take account of other agents' decisions. The likelihood function then becomes a high dimensional integral function and it cannot be factored out, as in the standard Heckman selection model (Heckman, 1979) where agents' decisions are independent of each other. To overcome the numerical difficulty in solving the high dimensional integration problem, we apply a Bayesian approach, use the Markov Chain Monte Carlo (MCMC) method to transform the integration problem into a simulation problem to make estimation feasible (Tanner and Wong, 1987; Albert and Chib, 1993; Sorensen, 2007a; Park, 2013; Chen, 2014; Ni and Srinivasan, 2015).

Our research contributes first to the literature on the determinants of analyst forecast accuracy. Brokerage firm resources have been found to affect analyst forecast accuracy (Clement, 1999b; Kothari et al., 2016b), and because of the lack of an identification strategy the sorting effect cannot be disentangled from the total impact. Therefore, the influence on analyst forecast accuracy is unknown. Our results not only provide the first quantitative estimates of the influence effect of the brokerage firm but also quantify the relative importance of the influence and the sorting effects.

Second, our study contributes to the literature that uses the two-sided matching model to understand the incentives for agents to match and the outcomes of the matching results in markets such as the venture capital market (Sorensen, 2007a; Akkus et al., 2016a; Fox et al., 2018), the labour market (Agarwal, 2015; Pan, 2015; Matveyev, 2016; Xia, 2018), M&A market (Park, 2013; Akkus et al., 2016b), and the bank lending market (Chen and Song, 2013; Schwert, 2018).

The results of our study also help to understand workers' incentives to work for firms with good reputations, and the incentives for firms to maintain their reputations. Edmans (2011) finds that firms with better reputations on average perform better, and our results suggest that the reputation of a firm can serve as a sorting mechanism to attract talented employees, which is beneficial for firm performance. More talented employees also like to work for firms with good reputations, because they can scale their ability by using the firms' resources and achieve better personal performance and better future career outcomes. Our results suggest that for new analysts the influencing effect of firms' reputations is 2.7 times larger than the sorting effect. Therefore, the benefit of working for high-reputation firms is particularly attractive for new workers.

The remainder of this chapter is organized as follows. In Section 5.2 the data and the OLS estimation results are discussed. Section 5.3 presents the theoretical and empirical model and a discussion of identification. Section 5.4 provides the estimation results. Section 5.5 concludes the paper.

5.2 Data and OLS results

5.2.1 Sample selection and key variables construction

We consider new hires by brokerage firms in each year between 1996 and 2013. Our data comes from the Institutional Brokers Estimate System (I/B/E/S) database, which collects analysts' earnings forecasts and recommendations for companies worldwide. We use the I/B/E/S Detail Recommendations File to identify the brokerage firm an analyst is employed by in any given year. The recommendation file starts in 1992 and expands its coverage over the first three years, so we only consider analysts who started in 1996 or later. We classify an analyst as a new hire in a given year if she appears for the first time in the dataset in that year, and stays at least for the subsequent four years in the dataset and works for the same brokerage firm. We cross-check with the I/B/E/S Detail Earnings History File to further exclude analysts who had previously issued any earnings forecasts, and those who do not issue any earnings forecasts at all. We manually search for the location of the brokerage firms and remove analysts employed by foreign broker houses that do not have any offices in the U.S. Our final sample consists of 1,815 analysts hired by 284 brokerage firms for the period between 1996 and 2013.

Figure 5.1: Geographic distribution of brokerage firms

This figure shows the distribution of the US brokerage firms' headquarters in different states. The darker the states, the more brokerage firms are located in that state.

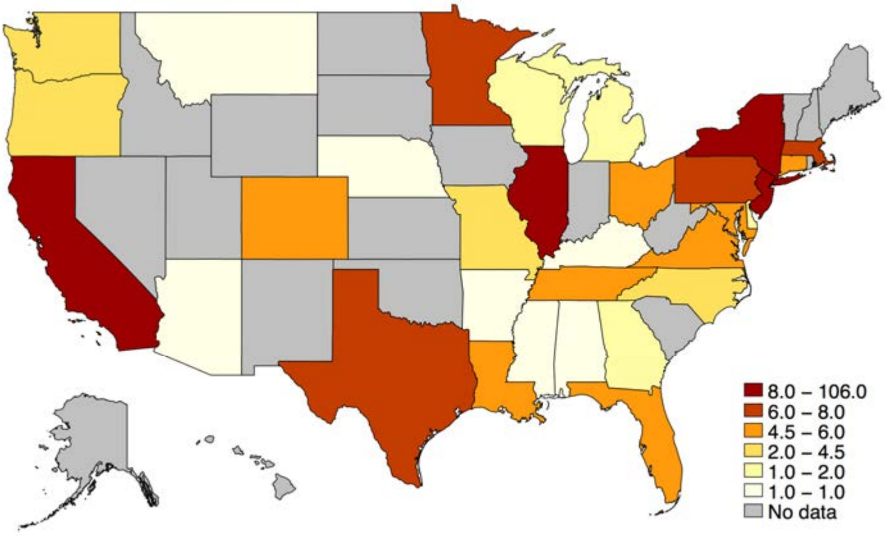


Figure 5.1 illustrates the geographic distribution of brokerage houses from our sample. We plot the number of firms in each state. A clear geographic clustering on the demand side can be clearly seen in the Northeastern states such as NY and MA, accounting for roughly 65% of our sample. We therefore divide the analysts into 36 markets: Northeastern states and the remaining states for 18 years from 1996 to 2013. Note that pooling the other states together into one labour market each year is less of a concern under the assumption that those small local markets are independent of each other.[2]

[2]We also run our analysis by considering all of the states as one big market in each year, and the results are similar.

To measure an analyst's performance, we first determine her accuracy for each stock she covers in a given year and then take the average of this accuracy across all coverage stocks over the first five years of her tenure. Specifically, for analyst i making a forecast for the earnings of fiscal year t of stock j, we compare her absolute forecast error to the average absolute forecast error of other analysts covering the same stock during the same time period. We rank all available absolute forecast errors from small to large and assign a rank that corresponds to the relative ranking of analyst i's forecast error for that stock-year. The analyst ranked n-th (where the most accurate/smallest error is ranked 1st and the least accurate/largest error is ranked Nth) is assigned.

$$rank_{ijt} = 1 - \frac{n_{ijt}}{N_{jt} + 1}. \tag{5.1}$$

The lower the rank, the less accurate the forecast. We aggregate those accuracy ranks for analyst i to determine her overall accuracy as

$$Accuracy_i = \frac{1}{5} \sum_{t=\tau}^{\tau+4} \left(\frac{1}{|J_{it}|} \sum_{j \in J_{it}} rank_{ijt} \right),$$

where J_t denotes analyst i's coverage in year t.

The brokerage firm prestige is measured by using Carter and Manaster (CM) ranking. This ranking measure is based on the order of brokerage firms in firms' IPO tombstone announcements. The measure is developed by Carter and Manaster (1990) and extended by Carter et al. (1998) and Loughran and Ritter (2004). We obtain the data from Jay Ritter's website. On a scale of 0 to 9, the higher the rank, the more prestigious the brokerage firms. Morgan Stanley, Goldman Sachs, JPMorgan, Deutsche Bank, and CITI Group are among the most frequently listed in the highest reputable brokerage groups.

Table 5.1 presents the summary statistics of our variables. The mean growth rate for these brokerage firms is 14.5% yearly, and the median growth rate is 5%. These firms are on average expanding through the sample period. The newly hired analysts on average start by covering slightly more than 8 stocks, less than the average number of stocks covered by analysts in the whole

I/B/E/S universe, which is 14. Most of the analysts cover less than three different industries. The financial analyst labour market is racially dominated by white analysts, based on the surname search, and in our sample we classify less than 17% as nonwhite analysts. Analysts do not cluster in the main industries they cover in our sample. The largest group of analysts (27.9% of the total sample) cover firms in the high-tech industry, followed by 26.8% who mainly cover industries other than those listed in the table. As over half of the U.S. publicly listed firms from 1996 to 2013 are classified in the high-tech industry or in "other" industries, this is a reasonable assumption.

Table 5.1: Summary statistics

This table reports summary statistics of the main variables. We consider an analyst's tenure as her first five years working for the brokerage firm. Broker reputation is the Carter and Manaster rank on a scale of 0 to 9, and the higher the rank the more prestigious the brokerage firm. Broker growth is the percentage of brokerage size increase from last year. Number of stocks and industries is the average number of firms and industries she covers during her tenure. Log(Market Cap) is the logarithm of the total market cap an analyst covers in her first year. Ethnicity indicates whether the analyst is white Caucasian or not based on the analyst's surname (1 indicates not, 0 indicates yes). To include the focus industry fixed effects, we define industries using the Fama-French five industry classifications, and classify an analyst's focus industry as the one in which she covers the most stocks. We indicate the following four industries: Consumer (including retails & wholesales), Manufacturing & Energy, High Tech, and Health. Num IPO indicates the total number of IPOs made in a specific year.

Variables	N	Mean	St. Dev	Percentile				
				10th	25th	50th	75th	90th
Accuracy	1,815	0.514	0.082	0.410	0.473	0.521	0.567	0.609
Broker Reputation	1,815	5.948	3.112	0	5.001	7.001	8.501	9.001
Broker Growth	1,815	0.145	0.467	-0.181	-0.066	0.052	0.191	0.500
Log(Market Cap)	1,815	8.642	1.974	6.052	7.260	8.602	10.119	11.952
Num Stocks	1,815	8.511	4.780	2.6	5	8	11.4	14.75
Num Industries	1,815	1.683	0.771	1	1	1.5	2	2.8
Ethnicity	1,815	0.167	0.374	0	0	0	0	1
I.Consumers	1,815	0.141	0.348	0	0	0	0	1
I.Manuf & Energy	1,815	0.196	0.397	0	0	0	0	1
I.High Tech	1,815	0.279	0.449	0	0	0	1	1
I.Health	1,815	0.116	0.320	0	0	0	0	1
Num IPO	1,815	168.047	148.497	38	60	131	223	384

5.2.2 Naive OLS results

In this subsection, we document a robust and strong empirical correlation between brokerage reputation and newly hired analysts' forecast performances. According to the level of brokerage prestige, we first plot the correlation between brokerage prestige and analyst performance.

Figure 5.2: Relation between brokerage firm prestige and analyst performance

This figure shows the correlation between brokerage firms' reputations and newly hired analysts' forecast accuracy from 1996 to 2013. Our sample is grouped into 10 bins according to broker prestige. The shadow area represents a 95% confidence interval.

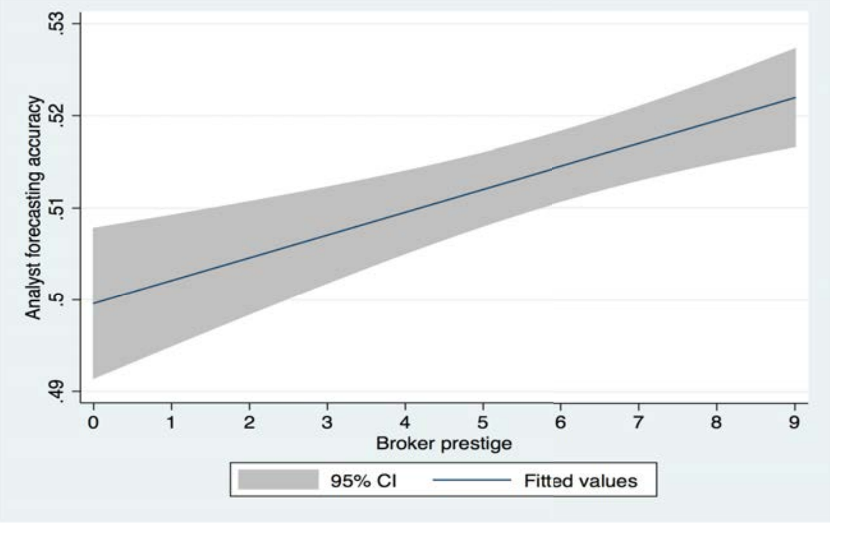


Figure 5.2 illustrates strong positive correlations between broker prestige and analysts' accuracy and their likelihood of becoming an all-star analyst. Analysts who start with the lowest prestige brokerage firms on average exhibited performance of 0.493, while those who start with the highest prestige firms on average exhibited performance of 0.522, and those analysts are on average 6% more accurate. This effect is close to those documented in the literature (e.g. Clement (1999b)), which is equivalent to the advantage of 17.5 years of more experience.

To investigate these relations more formally, we estimate an OLS model for analyst accuracy. Table 5.2 shows that for the entire 1996 - 2013 period, ana-

Table 5.2: Naive OLS Regression

This table reports estimation results of the OLS model for analyst accuracy. Columns (1) to (3) present this relationship by using the whole sample from 1996 to 2013. Column (4) analyzes this relationship using the first half of the sample and column (5) analyzes the relationship using the second half of the sample. Parentheses include the corresponding standard errors. ***,**, and * denote significance at the 10%, 5%, and 1% levels, respectively. Variables are defined in Table 5.1.

	Analyst forecasting accuracy				
	Whole sample			1996 - 2004	2005 - 2013
VARIABLES	(1)	(2)	(3)	(4)	(5)
Broker prestige	0.0025***	0.0025***	0.0026***	0.0022**	0.0031***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Broker growth		0.0005	-0.0032	-0.0045	-0.0017
		(0.005)	(0.005)	(0.006)	(0.008)
Log(Market cap)		0.0006	0.0005	0.0020	-0.0011
		(0.001)	(0.001)	(0.002)	(0.002)
Num stocks		0.0017***	0.0015***	0.0019**	0.0013**
		(0.000)	(0.000)	(0.001)	(0.001)
Num industries		0.0017	0.0022	0.0012	0.0033
		(0.002)	(0.002)	(0.004)	(0.003)
Ethnicity		0.0075	0.0099*	0.0230***	0.0016
		(0.005)	(0.005)	(0.008)	(0.007)
Num IPOs		0.0000**	-0.0002***	-0.0001	-0.0005**
		(0.000)	(0.000)	(0.000)	(0.000)
Market dummy	No	No	Yes	Yes	Yes
Observations	1,815	1,815	1,815	785	1,030
R-squared	0.0088	0.0212	0.0490	0.0570	0.0458

lysts work for higher prestige brokerage firms on average have greater forecast accuracy. The magnitude does vary when we include other broker and analyst characteristics in column (2) and market fixed effect in column (3). In column (4) and column (5) we repeat the analysis on subsamples from 1996 - 2004 and 2005 - 2013. Here, broker reputation is also positively correlated with analyst forecast accuracy. Overall, the positive correlation between broker prestige and analyst forecast accuracy is robust to different controls and split sample regressions. If an analyst moves from the lowest to the highest reputable group of brokerage firms, the analyst forecast accuracy will increase by 4.7% ($= 0.0234 \times 9/0.493$), which is equivalent to 13.8 years of more experience.

In addition to broker prestige, other factors affect newly hired analyst forecast accuracy. From Table 5.2, we observe that the more stocks analysts cover, the more accurate their forecasts are. This observation may appear to contradict previous findings that the more complex the portfolios that analysts are covering, the less accurate their forecasts are. We argue this is less of a concern because our sample only contains newly hired analysts, so the number of stocks analysts cover also contains information on analysts' ability. Another critical factor explaining analyst forecast accuracy is the ethnicity of the analyst. In the whole sample, non-white analysts constitute less than 17% of the total sample but on average they perform better than white analysts. This outperformance is particularly strong in the first half of the sample, possibly because sell-side analyst jobs used to be occupied by white candidates and so the entry bar is higher for non-white candidates. For non-white candidates to get a job, their ability must be better than average, and thus they perform better[3].

As we explain in the introduction, the quality of brokerage firms becomes endogenous when sorting and causes more reputable brokerage houses to employ analysts who are better, along with many dimensions unobserved in the data. Analysts with better unobserved characteristics, as captured by the error term in the regression, match with brokers of better quality. The error term becomes positively correlated with broker size and broker accuracy, and the coefficient estimates are biased upwards relative to the brokers' actual influence. As no obvious instrumental variable is independent of analyst outcome but is related to the quality of the brokerage firm employing this analyst, we adopt the structural model developed by Sorensen (2007a) that exploits the implications of sorting to separate sorting from influence. Sorting implies that in a market with better broker firms, a given firm is pushed down the relative ranking and is left with worse analysts. Hence, a broker's new hire decisions depend on the characteristics of other agents in the market. Nevertheless, the outcome of the analyst is independent of these other characteristics, and the other brokers' characteristics serve as a source of exogenous variation. We now

[3]Similar evidence has been found in the asset management industry, where the entry bar is higher for candidates with low-income family backgrounds. Consequently, to become fund managers these candidates need to be significantly better than those from wealthy families (Chuprinin and Sosyura, 2018)

discuss the model in more detail.

5.3 Model

5.3.1 Two-sided matching model

We model the labor market of sell-side analysts as a one-to-many two-sided matching market, which is based on the college admission model developed by Gale and Shapley (1962) and Roth and Sotomayor (1992) and is similar to the VC-entrepreneur matching model in Sorensen (2007a). Each firm can hire multiple analysts, while each analyst candidate can only be employed by one firm. However, in any given market, brokerage firms are restricted to the number of new analysts they can hire, as firms' hiring capacity is capped because of the limited demands and resources. Each potential match has a valuation (V), which represents the discounted expected future payoff of the possible matched pair. The brokerage firm receives λ fraction of the valuation, and the analyst expects to receive $1 - \lambda$ fraction, where λ is fixed for all possible matches in a market. Such setting rules out transfers and guarantees a unique equilibrium for the model. This assumption is reasonable because analysts are sharing profits of the firm. Even though we do not observe analyst compensation in general, most compensation is paid in the form of a bonus, which is high when a firm's bonus pool expands and low when it shrinks (Groysberg et al., 2011). In addition, because we focus on newly hired analysts, who have little bargaining power at the beginning of their career, it is unlikely that these analysts can negotiate on pay. Therefore, their compensation structure is mostly fixed, and they cannot match more reputable firms by being offered a lower profit share by the firm.

Agents

The matching model has two types of agents: analyst candidates and brokerage firms. In each market m, a set I_m contains all of the analyst candidates, and a set J_m contains all brokerage firms that are looking for new analysts. Each candidate will be employed by by one brokerage firm, and each brokerage firm can hire a limited number of analysts. Let brokerage firm j's quota be q_j, where $q_j > 0$. The set M_m contains all possible matches of analysts and firms in market m, therefore $M_m = I_m \times J_m$. A matching contains observed hirings in market m denoted as μ_m, where $\mu_m \subset M_m$. Denoting that μ_j contains all of the analysts firm i hires and μ_i is the brokerage firm analyst i works for, then a match between firm i and analyst j can be expressed as: $(i, j) \in \mu$, $i = \mu(j)$, or $j \in \mu(i)$.

Agents on both sides of the market choose their matched partners to maximise the matching value, which represents the expected latent joint utility at the time of hiring. Let each possible match have a matching value and let the value of the match i, j be denoted as $V_{i,j}$ regardless of whether i, j is a matched pair or not. The matching values are assumed to be distinct to avoid the possibility that agents can be indifferent between two matches. The matching utility is divided between the brokerage firms and analysts. Firms receive λ share of the matching value and the analysts receive $(1 - \lambda)$ share, and λ is fixed for all matches and $\lambda \in (0, 1)$.

Equilibrium

A matching is an equilibrium if it is stable and no pair of agents would like to deviate from their current matches and form a new match together to become a blocking pair. The stable equilibrium always exists (Gale and Shapley, 1962) and under the fixed sharing rule of the matching value the equilibrium is unique (Sorensen, 2007a). The unique equilibrium is characterised by a set of inequalities based on the no blocking pairs condition.

For i, j to be a stable match, we need no blocking pair to exist for i, j, that is, the opportunity cost of analyst i remaining match with firm j or the opportunity cost of firm j remaining match with analyst i has to be smaller than the matching value of i, j, $V_{i,j}$.

The opportunity cost of analyst i is the maximum value that analyst i can get from the feasible set of deviations of analyst i instead of working for the firm j. The opportunity cost of brokerage firm j is the maximum value that firm j can get from the feasible set of deviations of firm j instead of hiring analyst i. The fixed sharing rule means that finding the maximum value that agents on one side of the market can get is equivalent to find the maximum matching value that a pair of agents can achieve together. We denote OC_i as the corresponding matching value for analyst i's opportunity cost and OC_i is the corresponding matching value for brokerage firm j's opportunity cost. That is,

$$V_{i,j} < max[OC_i, OC_j],$$

where

$$OC_i \equiv max[V_{i,j'}], \forall j' \in J \cap (V_{i,j'} > V_{\mu(j'),j'}),$$
$$OC_j \equiv max[V_{i',j}], \forall i' \in I \cap (V_{i',j} > \min_{i'' \in \mu(j)} V_{i'',j}).$$

If in other circumstances analyst i and brokerage firm j are not matched, then (i, j) cannot become the blocking pair for their current matches. Then it is sufficient that,

$$V_{i,j} > max[V_{i,\mu(i)}, \min_{i''' \in \mu(j)} V_{i''',j}].$$

We denote $\overline{V}_{i,j} \equiv max[OC_i, OC_j]$, and $\underline{V}_{i,j} \equiv max[V_{i,\mu(i)}, \min_{i''' \in \mu(j)} V_{i'''j}]$. For μ to be a stable matching, the following conditions need to hold:

$$V_{i,j} < \overline{V}_{i,j}, \forall (i, j) \notin \mu, \tag{5.2}$$
$$V_{i,j} > \underline{V}_{i,j}, \forall (i, j) \in \mu. \tag{5.3}$$

5.3.2 Empirical Model

The first part of the empirical model is a matching function determining the matching value of the match between two agents. The matching value is unobserved and modelled as a latent variable. Without loss of generality, the matching value of analyst i and brokerage firm j can be written as:

$$V_{i,j} = \alpha W_{i,j} + \eta_{i,j}, \forall (i, j) \in M, \tag{5.4}$$

where $W_{i,j}$ contains characteristics of analyst i and firm j that are observed by econometricians. $\eta_{i,j}$ contains characteristics of analyst i and firm j that are not observed by econometricians but are known for every agent in the market and $\eta_{i,j} \sim \mathcal{N}(0, \sigma_\eta)$.

The second part of the model is the outcome equation. This determines the outcome of all possible matches, which is only observable to those matches that are realised. The outcome of analyst i and brokerage firm j can be written as:

$$Y_{i,j} = \alpha X_{i,j} + \varepsilon_{i,j}, \forall (i,j) \in M, \tag{5.5}$$

where $X_{i,j}$ contains characteristics of analyst i and firm j that are observed by econometricians. $\varepsilon_{i,j}$ contains characteristics of analyst i and firm j that are not observed by econometricians but known for every agent in the market and $\varepsilon_{i,j} \sim \mathcal{N}(0, \sigma_\varepsilon)$.[4]

Directly estimating the outcome equation leads to biased results, as the matching decision between analyst i and firm j is not random but correlated with the error term in the outcome equation, which cannot be observed by econometricians. This problem is captured by a third equation determining the correlation between the error terms in the valuation equation and the outcome equation:

$$\varepsilon_{i,j} = \delta \eta_{i,j} + \xi_{i,j}, \tag{5.6}$$

where $\xi_{ij} \sim \mathcal{N}(0, \sigma_\xi)$. If there is no correlation between the two error terms then $\delta = 0$.

5.3.3 Identification and estimation

We now discuss how we identify and estimate the parameters in the outcome equation. The main feature of the matching market is that the agents' decisions on matching interact with each other, and this leads to better-talented analysts sorting by brokerage quality. If analyst A is hired by brokerage firm 1, then brokerage firm 2 cannot approach analyst A, as analyst A is not avail-

[4]If the outcome is binary, there will be a third part containing a binary outcome function, i.e. $O_{i,j} = 1[Y_{i,j} > 0]$

able anymore. Similarly, if brokerage firm 1 has used up its hiring quota, then other analysts with relatively lower quality than analyst A cannot match with broker 1 anymore. As such, in each market, agents' matching decisions do not only depend on their own qualities, but also correlate with other agents' characteristics.

The sorting and interaction feature helps us identify the direct influence effect from brokerage firms. As we rank all of the new analysts and all brokerage firms based on their characteristics in each market, with the top-ranked analyst candidate matched with the top-ranked brokerage firm, we continue to match the second highest ranked analyst candidate with the top-ranked brokerage firm until the hiring quota is entirely filled, and then we continue to form matches between analysts with the second highest ranked brokerage firm until we fill all of the vacancies in the market. This rank-order property means the matching decision is determined by the relative ranking of the agents on two sides of the market, and partly depends on the agents' own characteristics, and partly on the characteristics of other agents. As the characteristics and quality of "other" agents vary between markets, similar-quality analysts would be matched with brokerage firms with different reputations for exogenous reasons, and can help to identify the parameters in the outcome equation.

The cross-market variation means that same-quality brokerages and same-quality analysts cannot match in two different markets. Assume in market 1, brokerage i and analyst j are matched. In market 2, brokerage i' has the same quality as brokerage i, but assume market 2 contains similar brokerage firms but with more talented analysts. Therefore, an analyst j' with the same quality as analyst j will rank much lower in market 2, and cannot match with brokerage i', and instead is matched with another brokerage firm with lower quality. Brokerage house i' can match with another analyst k who has better quality than analyst j'. The effect from matching is different, but the impact from the brokerage firm influence is the same, and this will lead to differences between outputs from analyst j and analyst k. This will help us identify the effect of matching.

More formally, let Y^*_{ij} denote the observed match (i,j)'s outcome in one market, and then to estimate the coefficients based on the empirical model we have:

$$\begin{aligned}
E[Y_{i,j}|X_{i,j}] &= E[Y^*_{i,j}|X_{i,j},(i,j)\in\mu] \\
&= E[Y^*_{i,j}|X_{i,j},V_{i,j} > \underline{V}_{i,j}] \\
&= \beta + E[\varepsilon_{i,j}|\alpha W_{i,j}+\eta_{i,j} > \underline{V}_{i,j}] \\
&= \beta + E[\delta\eta_{i,j}+\xi_{i,j}|\eta_{i,j} > \underline{V}_{i,j}-\alpha W_{i,j}] \\
&= \beta + \delta E[\eta_{i,j}|\eta_{i,j} > \underline{V}_{i,j}-\alpha W_{i,j}].
\end{aligned}$$

The first equality comes from the equilibrium condition of the matching model, and the fourth equality comes from the error term correlation structure. Therefore, the exogenous variation in this expression identifies outcome equation parameters β, and the expression varies with $\underline{V}_{i,j}$. As $\underline{V}_{i,j}$ is determined by the other agents' characteristics in the market, if the allocation of the other agents in the market is exogenously given, then the parameters in the outcome equation are identified. [5]

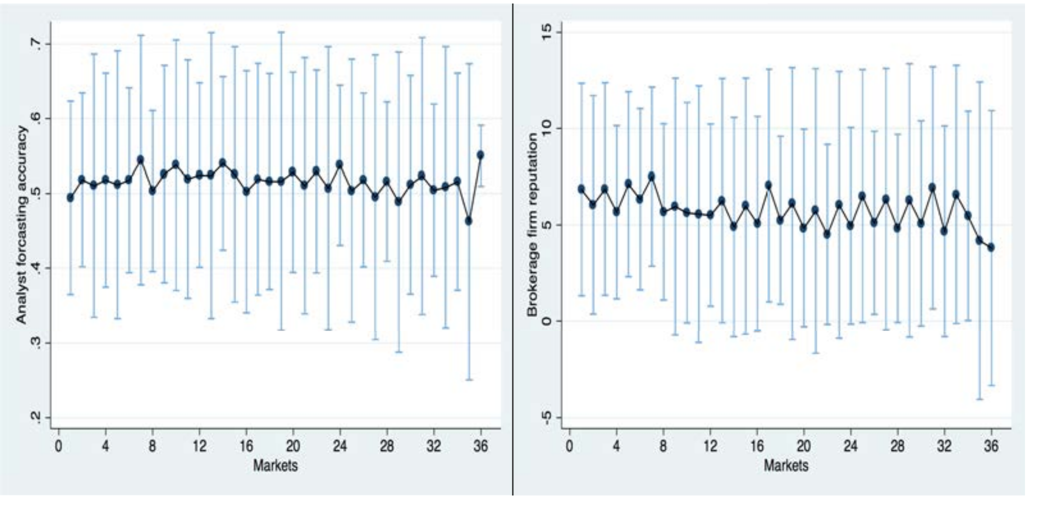
The key identification assumption is that agents are allocated exogenously across markets, which is reasonable because the new analyst labour market is likely to be influenced by macro or financial industry factors instead of agents' sort on different markets (i.e., waiting to hire later because they know there will be better candidates one year later). Figure 5.3 shows that even though the average is reasonably consistent across markets, there are significant variations of main variables within each market, and this variation fluctuates from market to market. Thus, it is reasonable to assume the agents are exogenously allocated across markets.

The estimation method we use is the Bayesian estimation with Markov Chain Monte Carlo (MCMC). The sorting and interaction feature of the model makes estimation difficult. The likelihood function for one pair of agents' matching decisions also depends on the other agents' choices, so all of the error

[5] A complete discussion of the identification strategy can be found in Sorensen (2007b).

Figure 5.3: Variation in main variables across markets

This figure shows the cross and within market variation of the key Y variable: analyst forecast accuracy, and the key X variable: broker reputation. Each subgraph depicts the average of the variable (black solid line) and one standard deviation around the mean (light blue error bar). Subgraph (a) shows the variation of analyst accuracy across different markets. Subgraph (b) shows the variation of brokerage reputation across different markets.



(a) Analyst forecast accuracy

(b) Broker reputation

terms must be integrated simultaneously. To circumvent this high-dimensional integration problem, we take advantage of the Bayesian method with MCMC (Tanner and Wong, 1987; Geweke et al., 1994; Albert and Chib, 1993), and instead of solving the integration problem, we augment the observed data with the simulated value of the latent matching value and the performance of the counterfactual matches. The simulated distribution converges to the augmented posterior distribution. The detailed simulation procedure can be found in Appendix 5.A.

5.4 Estimation results

5.4.1 Main result

In this section, we present and discuss the estimation results. In Table 5.3 Panel B, the coefficients estimated represent agents' preferences. The results show that an analyst prefers to work for brokerage firms with higher reputations and higher growth rates, and brokerage firms prefer to hire analysts who can cover large value portfolios, cover fewer stocks, have less industry focus, and are from a non-white background. Thus, firms prefer non-white analysts who can cover a limited amount of large firms and span less industries. The probability of an analyst match with a broker with the highest reputation score is 90.6%. The probability that brokerage firms prefer a non-white analyst is 55.13%. Compared with a new analyst who can only cover the lowest ten percentile of the portfolio market size, brokerage firms prefer analysts who cover the top ten percentile of the portfolio market size by a probability of 59.5%. Overall, the results from the matching equation suggest analysts have strong preferences in terms of broker reputation, rather than other observed factors that brokerage firms have on analysts. Broker reputation is the most important factor in measuring brokerage firms' quality, while the analysts' ethnicity or portfolio sizes are simply indirect measures of their quality.

Panel C of Table 5.3 represents the effect of sorting on unobserved characteristics. If there is no sorting between unobservables, a matching model is not needed. The result shows δ is positive and 0 is not contained in the 99% highest posterior distribution, and that the sorting effect exists and is significant, indicating that unobserved agents' characteristics affect matching values and also matching outcomes. This also highlights the key point of the study: controlling for matching is crucial given its significant effect.

Panel A of Table 5.3 shows the estimated coefficients in the outcome equation after controlling for endogenous matching. The coefficient associated with broker reputation is positive and 0 is not contained in the 95% highest posterior distribution, which suggests after controlling for sorting, the effect of brokerage reputation is crucial in explaining analyst forecast accuracy. This finding is consistent with channels suggested by Clement (1999b) that brokerage resources (proxied by brokerage reputation in this study) are important in determining analyst forecast accuracy.

Table 5.3: Bayesian estimate of the matching model and the outcome equation

This table reports Bayesian estimation results of two equations from the structure model. The dependent variable in the outcome equation is analyst forecast accuracy, and the dependent variable in the valuation equation is the latent matching value. A detailed description of the variables is given in Table 5.1. Mean, Median, and Standard Dev. are the statistics of the simulated posterior distributions of the parameters. Marginal effects of the valuation equation represent the probability of choosing two matches with only marginal change in one variable, and are calculated by following Sorensen (2007a). Estimates are based on 110,000 simulations of the posterior distribution. The initial 11,000 simulations are discarded for burn-in. ***,**,and * denote that zeros are not contained in the 10%, 5%, and 1% credible intervals, respectively. Variables are defined in Table 5.1.

	Dependent variable: Analyst forecasting accuracy				
	Mean	Median	Marginal effect	Standard Dev.	95% HPD
VARIABLES	(1)	(2)	(3)	(4)	(5)
Panel A: Outcome equation					
Broker reputation	0.0019***	0.0019		0.0007	[0.0006, 0.0033]
Broker growth	-0.0044	-0.0044		0.0043	[-0.0130, 0.0041]
Log(Market cap)	-0.0002	-0.0002		0.0011	[-0.0024, 0.0019]
Num stocks	0.0013***	0.0013		0.0005	[0.0004, 0.0022]
Num industry	-0.0031	-0.0031		0.0027	[-0.0084, 0.0023]
Ethnicity	0.0036	0.0037		0.0052	[-0.0070, 0.0140]
Num IPO	0.0000*	0.0000		0.0001	[-0.0000, 0.0001]
Panel B: Matching equation					
Broker reputation	0.1439***	0.1409	0.0406	0.0261	[0.0974, 0.1952]
Broker growth	0.0651	0.0663	0.0184	0.1219	[-0.1732, 0.3010]
Log(Market cap)	0.0569***	0.0560	0.0161	0.0149	[0.0284, 0.0868]
Num stocks	-0.0095	-0.0091	-0.0027	0.0068	[-0.0233, 0.0034]
Num industry	-0.1763***	-0.1753	-0.0497	0.0316	[-0.2381, -0.1145]
Ethnicity	0.1820***	0.1786	0.0513	0.0664	[0.0556, 0.3115]
Panel C: Variance					
δ	0.0063***	0.0063		0.0037	[8.89e-07, 0.0131]

5.4.2 Relative importance

Although the above analysis clearly shows that broker reputation has a significant direct impact on analyst forecast accuracy because sorting on unobservables also has a significant impact on the outcome, the relative importance of the direct effect of broker reputation, and the indirect effect from sorting is unknown.

In determining the relative importance, we compare the OLS and Bayesian estimated results in Table 5.4. Column (1) presents the OLS regression results, and column (2) the Bayesian estimation results. Figure 5.4 shows how we decompose the total effect into the influence effect and the sorting effect.

Table 5.4: Bayesian estimate of alternative market and comparison

This table compares the outcome equations from models with different market definitions and compares the coefficient estimated from the naive OLS regression for analyst accuracy. Bayesian estimates are based on 110,000 simulations of the posterior distribution. The initial 11,000 simulations are discarded for burn-in and a tune-in factor of 10. Parentheses represent the corresponding t-statistics. ***,**,and * denote significance at the 10%, 5%, and 1% levels, respectively. Variables are defined as in Table 5.1.

	Dependent variable: Analyst forecasting accuracy				
	OLS	Bayesian estimation		Difference with OLS	
		Main	Expanded market	Main	Expanded market
VARIABLES	(1)	(2)	(3)	(4)	(5)
Broker reputation	0.0026	0.0019	0.0019	0.0007**	0.0007**
				(2.3459)	(2.3459)
Broker growth	-0.0032	-0.0044	-0.0004	0.0012	-0.0028
				(0.5741)	(-1.3397)
Log(Market cap)	0.0005	-0.0002	0.0001	0.0008	0.0005
				(1.4906)	(0.8518)
Num stocks	0.0015	0.0013	0.0009	0.0002	0.0006***
				(0.9377)	(2.8132)
Num industry	0.0022	-0.0031	-0.0034	0.0053***	0.0056***
				(5.3979)	(5.7035)
Ethnicity	0.0099	0.0036	-0.0031	0.0063***	0.0130***
				(2.6854)	(5.5420)
Num IPO	-0.0002	0.0000	0.0000	-0.0002***	-0.0002***
				(-9.5108)	(-9.5108)
δ		0.0063	0.0074		
Markets	36	36	18		

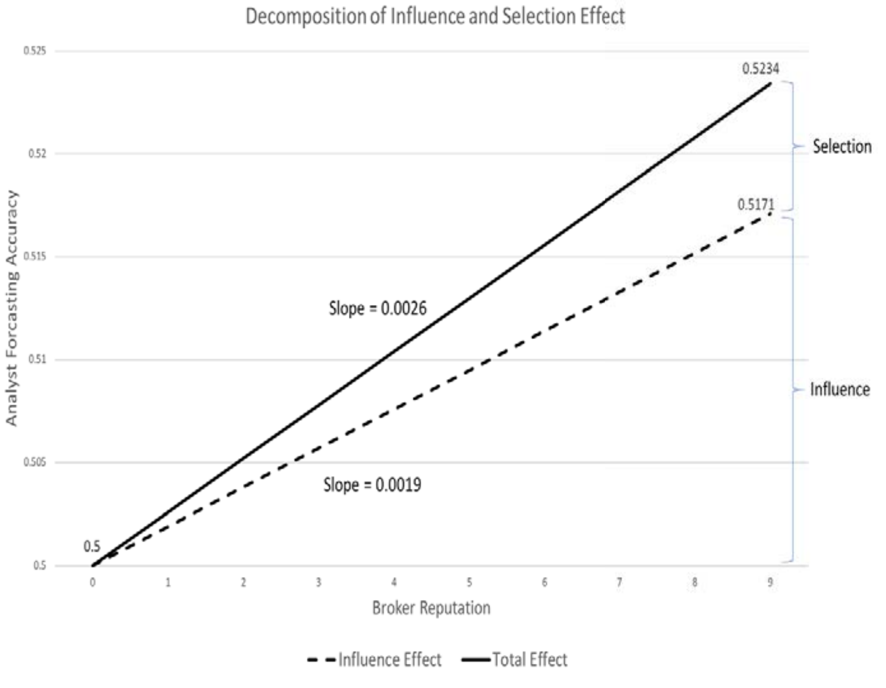
Controlling for sorting effect, analysts employed by the brokerage houses with the highest reputation rank are on average 3.5% ($= 0.0019 \times 9/0.493$) more accurate than those employed by the small brokers. This advantage in accuracy is comparable to 10.2 years more experience. On the other hand, the difference between the OLS and MCMC coefficient estimates of broker reputation indicates the selection effect because of sorting in the labor market, which is both economically and statistically significant. The selection effect accounts for 27% of the total effect estimated by the naive OLS regression, while the influence of brokers accounts for 73% of the total impact.

5.4.3 Alternative market

In our main analysis, our definition of new analyst labour market is by one calendar year but segregated by geographical locations. The market segregation

Figure 5.4: Decomposition of influence and sorting

This figure shows the results comparison between the naive OLS regression results and the sorting controlled outcome equation results in Table 5.4 for the highest reputable brokerage firms. The dashed line represents influence effect of the broker reputation after controlling for the selection effect.



is a critical identification assumption, and will fail if new analyst candidates or brokerage firms choose to participate in the specific market, based on unobserved characteristics of other agents in that market. For example, if the Northeast of the US has more reputable brokerage firms and if that reputation is sufficient to attract analyst candidates, this will lead to analysts sorting between different locations, and so a more appropriate definition of the market is to consider the whole US as a single market.

In this subsection, we expand the market definition to evaluate the robustness of the estimation results. In Column (3) of Table 5.4, we treat the Northeast and the rest of the US as the same market and repeat the analysis. The estimated coefficients are at a similar magnitude and significance level, particularly the key variables of broker reputation and δ. The magnitude of the coefficient associated with broker reputation is robust to different specifications and the statistical significance is also similar. The magnitude of δ increases but the statistical significance is similar. This indicates that minor sorting exists between the geographical locations in the same year, but the baseline Bayesian estimation does not capture this minor effect. For our main purpose of estimating the direct effect of broker reputation, this is less of a concern because this cross-location sorting appears to have little correlation with broker reputation. Overall, the results provide an intuitive robustness test that confirms that the identification assumption is valid and our estimation results are not sensitive to different market definitions.

5.5 Conclusion

Our study focuses on the new analyst labor market. We find new analysts working for firms with higher reputations perform better. This total effect is a combination of the direct influence effect, in which reputable firms can help analysts perform better, and the sorting effect, in which brokerage firms with high reputations can attract more talented analysts. To disentangle these two effects, we utilize a one-to-many two-sided matching model to circumvent the need to find the instrumental variable. The features of the matching model can capture how agents' matching decisions interact, and how the other agents' characteristics determine the relative ranking of the agents' matching decisions, but the other agents' characteristics do not have an effect on the agents' performance. Therefore, the exogenous variation of the other agents' characteristics helps to identify the coefficients of the outcome equation.

In the sample of 1815 new analyst-brokerage firm matched pairs from 1996 to 2013, we find that both the influence effect and the sorting effect have a significant impact on analyst forecast accuracy. The influence effect accounts for 73% of the total impact, and the sorting effect for 27%.

The results of the study have more general implications for understanding the incentives for workers to choose more reputable firms to work for and the incentives for firms to spend resources in maintaining their reputation. High reputation firms provide resources for workers and help them perform better, and in our results the forecast difference between analysts of the same quality working for the lowest and the highest reputation firms is equivalent to 15 years of experience. A firm's reputation is valuable, as it not only motivates current workers but also attracts more talented new workers. Both of these effects are important in understanding the benefit of firms' reputation on workers performances.

5.A Appendix: MCMC estimation procedure

Let the markets be indexed by $m = 1, \ldots, N$, latent valuation variables be $V_m \equiv \{V_{ij}, ij \in M_m\}$, matching characteristics $W_m \equiv \{W_{ij}, ij \in M_m\}$, and exogenous explanatory variables be $X_m \equiv \{X_{ij}, ij \in M_m\}$, for all potential matches $ij \in M_m$ in each market m. The following algorithm shows how to draw from the posterior the distribution of the parameters augmented with the latent valuation variable, V_{ij}, and the missing observations y^*_{ij} for unobserved matches. We are interested in estimating the parameters α, β, and δ. The Markov chain is generated by drawing each individual dimension of the joint posterior distribution conditional on the draws of the other dimensions as follows:

1. Start Gibbs-sampler for $g = 1 : G_{burn-in} + G_{sample}$ total runs.

2. Initialise the sampling by drawing α, β, δ, and σ^2_ξ from prior distributions: $\alpha \sim \mathcal{N}(\alpha_0, A_\alpha^{-1} = 10I_k)$, $\beta \sim \mathcal{N}(\beta_0, A_\beta^{-1} = 10I_p)$, $\delta|\sigma^2_\xi \sim \mathcal{N}(\delta_0, \sigma^2_\xi/A_\delta)$, and $\sigma^2_\xi \sim IG(a = 2.1, b = 1)$.

3. Draw latent valuation variables V_{ij} for all potential matches in each market m, and draw outcome variable Y_{ij} for unobserved matches in each market m, from distributions conditional on parameters $\alpha, \beta, \delta, \sigma^2_\xi$.

4. Update α, β by drawing from a Bayesian Seemingly Unrelated Regression (BSUR) of $[V; Y]$ on $[W; X]$ conditional on δ, σ^2_ξ.

5. Update δ, σ^2_ξ by drawing from a Bayesian regression of $Y - X\beta$ on $V - W\alpha$, conditional on α, β.

6. Go back to step 3 and repeat.

We now describe how to draw from each conditional distribution.

5.A.1 Conditional distribution of valuation variables V_{ij}

The conditional augmented posterior distribution of V_{ij} depends on whether brokerage firm i and analyst j are matched or not:

- when $ij \notin \mu_m$, we draw V_{ij} from $N(W'_{ij}\alpha, 1)$ truncated from above at $\overline{V}_{ij}$;

- when $ij \in \mu_m$, we draw V_{ij} from

$$V_{ij}|\alpha, \beta, \delta, \sigma_\xi^2, Y_{ij} \sim \mathcal{N}\left(W'_{ij}\alpha + (Y_{ij} - X'_{ij}\beta)\frac{\delta}{\delta^2 + \sigma_\xi^2}, \frac{\sigma_\xi^2}{\delta^2 + \sigma_\xi^2}\right)$$

  truncated from below at $\underline{V}_{ij}$.

The expressions for $\overline{V}_{ij}$ and $\underline{V}_{ij}$ are given in the equation.

5.A.2 Conditional distribution of unobserved outcome variables Y_{ij}

We only need to simulate the outcome variable Y_{ij} if $ij \notin \mu_m$, i.e., for unobserved matches. We draw Y_{ij} from

$$Y_{ij}|\alpha, \beta, \delta, \sigma_\xi^2, V_{ij} \sim \mathcal{N}\left(X'_{ij}\beta + \delta(V_{ij} - W'_{ij}\alpha), \sigma_\xi^2\right).$$

5.A.3 Conditional distribution of α and β

We apply a BSUR of [V; Y] on [W;X] to sample α and β,

$$\alpha, \beta|V_{ij}, Y_{ij}, \delta, \sigma_\xi^2 \sim \mathcal{N}\left(M^{-1}N, M^{-1}\right),$$

where

$$M = \begin{pmatrix} \Omega_{1,1}^{-1}W'W & \Omega_{1,2}^{-1}W'X \\ \Omega_{2,1}^{-1}X'W & \Omega_{2,2}^{-1}X'X \end{pmatrix} + A, \quad N = \begin{pmatrix} A_\alpha \alpha_0 \\ A_\beta \beta_0 \end{pmatrix} + \begin{pmatrix} \Omega_{1,1}^{-1}W'V & \Omega_{1,2}^{-1}W'Y \\ \Omega_{2,1}^{-1}X'V & \Omega_{2,2}^{-1}X'Y \end{pmatrix},$$

and

$$\Omega = \begin{pmatrix} 1 & \delta \\ \delta & \delta^2 + \sigma_\xi^2 \end{pmatrix}.$$

5.A.4 Conditional distribution of δ and σ_ξ^2

Draw $\delta, \sigma_{xi}^2|\alpha, \beta, V, Y$ from a Bayesian regression of $\varepsilon = Y - X\beta$ on $\eta = V - W\alpha$:

1. Draw $\sigma_\xi^2 \sim IG(a+N, b+S)$, where N is the number of all potential matches from all markets, and $S = (\varepsilon - \eta d)'(\varepsilon - \eta d) + (d - \delta_0)' A_\delta (d - \delta_0)$, and $d = (\eta'\eta + A_\delta)^{-1}(\eta'\varepsilon + A_\delta \delta_0)$.

2. Draw $\delta | \sigma_\xi^2 \sim \mathcal{N}\left(d, \sigma_\xi^2(\eta'\eta + A_\delta)^{-1}\right)$, truncated from below at 0.

Chapter 6

Summary

This thesis consists of four empirical essays on sell-side equity analysts and boards of directors.

Chapter 2 sheds light on how sophisticated financial agents such as equity analysts form expectations. I show that bad news in other coverage industries makes analysts more pessimistic about the focal firms. The resulting pessimistic forecasts turn out to be less accurate and much lower than the realized earnings. Analysts also overreact to bad news in industries that have no business relationships with the focal firms. I interpret these findings as evidence that analysts overgeneralize bad news in other coverage industries, become overpessimistic about the state of the world, and therefore lower their earnings expectations for the focal firms.

This study contributes to the large literature on analysts, which still has little empirical evidence on the source of analyst disagreement. My results help explain both the cross-sectional and time-series variations in analyst disagreement, which has been widely used to measure the level of heterogeneous beliefs among investors. Moreover, the finding of overgeneralization contributes to the more general literature that studies the impact of experience on decision making in financial markets. I show that multi-tasking agents might overweight information from one task when making decisions for other tasks.

In the second study of this thesis, I further examine the effects of analyst overgeneralization on the financial market. As many investors rely on analyst's opinions to evaluate companies and make trading decisions, the disagreement among analysts could significantly affect firms' trading activities and stock price movements. A simple trading model predicts that, because overgeneralization induces analyst disagreement, it leads to higher trading volumes and larger return volatilities, and the resulting analysts' overpessimism exerts downward pressure and induces temporary underpricing. I find strong empirical evidence supporting both predictions.

In addition, the result in these two chapters also have some insights for other strands of literature. As overgeneralization makes analyst adjust expectations for reasons not related to firms' fundamentals, it essentially provides an exogenous variation in analyst disagreement and pessimism. This insight could be useful for future empirical research that study the effects of investor disagreement and temporary underpricing.

Chapter 4 turns to the board of directors, who play a crucial role in corporate governance. Their job is to actively monitor and advise top management, ensuring that managers act in the best interest of shareholders. However, a directorship is rarely a full-time job. Most directors have other occupations, and many directors serve on multiple boards. Given that attention is not unlimited for directors, those with multiple directorships have to divide their attention, but the reason they are appointed to multiple boards likely reflects their quality. Isolating busyness from ability is therefore a challenging task, as having multiple directorships might reflect both. Our study is able to disentangle busyness from director ability and provides evidence on the costs of having busy directors.

By exploiting exogenous shocks to unrelated industries in which directors have additional directorships, we show that director distraction affects board monitoring intensity and leads to higher level of inactivity by management. Directors attend significantly fewer meetings when they are distracted. Firms with more distracted board members experience a significant decline in firm value and tend to invest less and are less likely to announce takeovers. Our results suggest that an effective board of directors prevents manager from shirking or enjoying a quiet life at the expense of shareholder value.

The final study turns to study the labor markets of financial analysts. Consistent with the general observation that workers at more prestigious companies tend to have better performance, equity analysts employed by more reputable brokerage houses produce significantly more accurate earnings forecasts. An analyst employed by the most reputable brokerage is about 6% more accurate than an analyst employed by a minor brokerage, which is equivalent to an advantage of 17.5 years of more experience.

This performance premium is driven by two distinct effects: on the one hand, more reputable brokerage firms provide better resources that improve analysts' forecast accuracy (influence effect); on the other hand, more reputable brokerage firms are more likely to attract talented candidate analysts (sorting effect). Distinguishing these two effects is however challenging, as the sorting mechanism creates an endogeneity problem. We disentangle these two effects and quantify their relative importance, by estimating a two-sided matching model for the labor market of analysts. The matching model allows for a one-to-many assortative matching process between firms and analysts, which helps control for the selection effect. Using a sample of 1,815 newly hired analysts from 1996 to 2013, we find that both the influence effect and the sorting effect have a significant impact on analyst forecast accuracy. The influence effect accounts for 73% of the total impact, while the sorting effect accounts for the remaining 27%.

An interesting direction for future research is to examine whether this performance premium exists for other agents, e.g., corporate lawyers and mutual fund managers, and to disentangle the direct influence of the employers from the selection effect of sorting. It is also interesting to evaluate policies related to the labor markets of financial agents. For instance, the recent legislation of MiFID II aims to foster market efficiency, resilience, and transparency. However, because MiFID II requires EU investment banks and brokers to separate the costs for research from asset management, it might force many analysts to leave the industry and leads to a decrease in analyst coverage. If so, examining the effects of such exodus of analysts on the information environment of EU (and US) companies provides valuable insights that advise policy-makers.

Another research direction is to further exploit the implications of analyst overgeneralization. As mentioned above, overgeneralization can be used to construct instrumental variables for analyst or investor disagreement. One can build on this insight to establish causality in many related research areas. For example, an interesting question is how information uncertainty influences firms' financing choices. One difficulty in answering this question is that any good proxy for information uncertainty, such as analyst disagreement or volatility, inevitably picks up other confounding factors such as the firm risk, which contaminates the estimation results. However, using shocks to other coverage industries as an IV for analyst disagreement, one can circumvent this endogeneity problem and provide causal evidence on the role of information uncertainty in capital structure. This identification strategy can be applied in other corporate finance and governance settings, such as M&A and corporate voting.

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Nederlandse samenvatting

Dit proefschrift is een bundeling van vier empirische studies over financiële analisten en leden van de raad van commissarissen.

Analisten spelen een belangrijke rol op de aandelenmarkt. Hun meningen kunnen de publieke opinie beïnvloeden en daarmee ook de koersprijzen. Echter hebben analisten vaak grote meningsverschillen over dezelfde bedrijven en maken uiteenlopende voorspellingen, en verschillende studies laten overtuigend zien dat deze meningsverschillen tot markt-inefficiëntie en onjuiste prijzen leiden. Ondanks de zware gevolgen, weten we tot nu toe heel weinig over de oorzaken van de meningsverschillen tussen analisten.

In de eerste studie van dit proefschrift onderzoek ik waar hun meningsverschillen vandaan komen. Ik verzamel hiervoor analistvoorspellingen voor een groot aantal Amerikaanse ondernemingen, en vind sterk bewijs dat nieuws (voornamelijk slecht nieuws) van andere industrieën de voorspellingen van analisten beïnvloeden. Het feit dat veel analisten meerder industrieën tegelijk volgen heeft ervoor gezorgd dat, wanneer het slecht gaat met één van hun portefeuille industrieën, ze meer pessimistisch worden over bedrijven in die andere industrieën, ten opzicht van hun gelijken die dezelfde bedrijven volgen. Dit pessimisme blijkt overdreven te zijn en wijkt aanzienlijk af van de realiteit. Ik toon verder aan dat dit gedrag door over-generalisatie wordt veroorzaakt. Over-generalisatie, ook bekend als "overhaaste generalisatie", is een drogreden waarbij men een algemene conclusie uit te weinig gegevens trekt. In dit geval, kan over-generalisatie verklaren waarom analisten overdreven op nieuws van andere industrieën reageren en daardoor uiteenlopend voorspellingen maken.

De tweede studie bouwt verder op de bevinding dat over-generalisatie van analisten tot meningsverschillen leidt en bestudeert de gevolgen daarvan op de aandelenmarkt. Uit een simpel theoretisch model kunnen we afleiden dat, aangezien veel investeerders naar analisten luisteren om aandelen te verhandelen, meningsverschillen tussen analisten zorgen voor meer handelsactiviteiten en meer fluctuaties van aandelenkoersen. Het over-pessimisme van analisten ten gevolge van over-generalisatie tijdelijk veroorzaakt een neerwaartse druk op de aandelenprijzen en daardoor te lage prijzen. Ik test deze twee stellingen empirisch en heb sterk bewijs van beide gevonden.

De derde studie richt zich op leden van de raad van commissarissen (RvC). RvC van beursgenoteerde bedrijven vertegenwoordigt de aandeelhouders en heeft de belangrijke taken om toezicht te houden op en advies te geven aan het bestuur. Het komt echter vaak voor dat iemand als commissariaat bij meerdere bedrijven tegelijkertijd tewerkgesteld is. Aangezien dat aandacht van een beperkte capaciteit is, zullen deze drukbezette commissariaten niet altijd in staat zijn om alle taken tegelijk uit te voeren. Een belangrijke vraag is dus of deze commissariaten hun werk naar behoren kunnen vervullen. Het antwoord ligt eigenlijk niet voor de hand, want het feit dat deze commissariaten drukbezet zijn is ook een indicatie van hun bekwaamheid. Duidelijk onderscheid maken tussen drukte en bekwaamheid is daarom een uitdaging.

Onze studie pakt dit probleem aan door gebruik te maken van mogelijke afleidende gebeurtissen. We stellen dat commissariaten afgeleid worden door onverwachte gebeurtenissen in niet-gerelateerde industrieën waarbij ze ook op de RvC zitten. Rond deze tijd kunnen ze minder aandacht aan andere bedrijven besteden en kunnen we bekijken hoe het met deze bedrijven gaat. We vinden bewijs dat het bestuur minder actief zijn wanneer de commissariaten afgeleid worden. Bedrijven met meer afgeleide commissariaten investeren veel minder en hun beurswaarde daalt aanzienlijk. Onze onderzoekresultaten benadrukken het belangrijke rol van de RvC in corporate governance om toezicht te houden op het bestuur.

In Hoofdstuk 5 keren we weer terug op financiële analisten en proberen te verklaren waarom analisten van grotere handelshuizen gemiddeld genomen meer accurate voorspellingen maken dan die bij kleinere handelshuizen. Dit prestatieverschil komt voornamelijk door twee factoren: grotere bedrijven bieden meer hulpmiddelen en beter voorzieningen aan hun medewerkers (invloed-effect), en grotere bedrijven trekken in eerste instantie ook betere kandidaten aan (selectie-effect). Duidelijk onderscheid maken tussen deze twee effecten is moeilijk omdat het selectie-effect voor een endogeneiteit probleem zorgt. In deze studie maken we gebruik van een één-op-veelzijdig matching model om voor het selectie-effect te controleren. Onze resultaten laten zien dat beide effecten significant zijn, waarbij het invloed-effect 73% van het totale verschil uitmaakt en het selectie-effect 27%.

In zijn geheel beschouwd geeft dit proefschrift nieuwe inzichten in de gedragen van de belangrijke marktspelers zoals analisten en leden van RvC. Psychologische factoren beïnvloeden de oordelen en beslissingen van analisten en commissariaten, en daardoor ook het koersverloop van bedrijven. Daarentegen vinden we dat hulpmiddelen van werkgevers het besluitvormingsproces van deze marktspelers kunnen vergemakkelijken en verbeteren. Deze studies dragen niet alleen bij aan de wetenschappelijke literatuur, maar hebben ook praktische implicaties voor beleidsmakers en professionals.

About the author

Rex Wang Renjie was born in Shanghai, China on March 30, 1990. He came to the Netherlands in 2005 and attended the Berlage Lyceum in Amsterdam, at which he received his Atheneum diploma in 2010. Afterwards, Rex moved to Rotterdam to study Econometrics at Erasmus University and obtained his Bachelor's degree in 2013 and Master's degree in 2014, both with appellation *cum laude*. The advanced version of his bachelor thesis is published in the *European Journal of Operational Research*. In college, Rex also assisted teaching for several Maths and Statistics courses, participated in many extracurricular activities such as the Bachelor Honors Class and the ESE Research Traineeship, and he did a part-time internship at Robeco's Quant Strategy Department. After graduation, Rex joined the ERIM doctoral program to pursue a PhD in Finance at the Erasmus School of Economics. His work has been presented at many international conferences, including the FMA, CICF, and SFS Cavalcade NA. The article version of chapter "Director Attention and Firm Value" of his dissertation is published in the *Financial Management*. During his PhD, Rex assisted teaching for the master course Advanced Corporate Finance and Governance, and supervised bachelor and master students. He visited the USC Marshall School of Business in spring 2018. Rex is now an Assistant Professor of Finance at the VU University Amsterdam. His research interests include behavioral finance, corporate finance and governance, and labor markets of financial agents.

Portfolio

Publications

- "Director attention and firm value" with P. Verwijmeren
  Financial Management, forthcoming.

- "An improved method for forecasting spare parts demand using extreme value theory"
  with S. Zhu, R. Dekker, W.L. van Jaarsveld, and A.J. Koning
  European Journal of Operational Research, 2017, 261(1): 169-181.

Conferences presentations

- **2019:** Helsinki Finance Summit, SFS Cavalcade North America (Pittsburgh), China International Conference in Finance (Guangzhou)

- **2018:** EUROFIDAI Paris December Finance Meeting, Research in Behavioral Finance Conference (Amsterdam)

- **2017:** Australasian Finance and Banking Conference (Sydney), Financial Management Association Annual Meeting (Boston), International Corporate Governance Society Conference (Rome)

- **2016:** Paris Financial Management Conference, German Finance Association Annual Meeting (Bonn), Research in Behavioral Finance Conference (Amsterdam), Corporate Finance Day (Antwerpen), Multinational Finance Society Annual Conference (Stockholm)

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Teaching

- **Advanced Corporate Finance and Governance (2015-2018):** teaching tutorial classes, holding office hours, and grading exams. This course is part of the Financial Economics program of the MSc Economics and Business at Erasmus School of Economics.

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