SEBASTIAN STIRNKORB

Changes in the Information Landscape and Capital Market Communication

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Veranderingen in het informatielandschap
en kapitaalmarktcommunicatie

Thesis

to obtain the degree of Doctor from the
Erasmus University Rotterdam
by command of the
rector magnificus

Prof. dr. A.L. Bredenoord
and in accordance with the decision of the Doctorate Board.

The public defence shall be held on
4 November 2021 at 1:00pm

by

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Erasmus Research Institute of Management – ERIM
The joint research institute of the Rotterdam School of Management (RSM)
and the Erasmus School of Economics (ESE) at the Erasmus University Rotterdam
Internet: www.erim.eur.nl

ERIM Electronic Series Portal: repub.eur.nl/

ERIM PhD Series in Research in Management, 536
ERIM reference number: EPS-2021- 536- F&A
ISBN 978-90-5892-615-9

Design: PanArt, www.panart.nl
Print: OBT bv, www.obt.eu

This publication (cover and interior) is printed on FSC® paper Magno Satin MC.

FSC www.fsc.org MIX Papier van verantwoorde herkomst FSC® C022586

TO MY PARENTS

RENATE AND FRITZ

Acknowledgments

This dissertation is the result of a five-year PhD journey at the Rotterdam School of Management. I would not have been able to complete this work without the support of a number of people who I am extremely grateful for.

First and foremost, I am deeply indebted to both of my supervisors, Erik and Marcel. Erik, you have brought me to Rotterdam and you were my first contact at Erasmus University. Thank you for welcoming me with open arms and an open door policy. Not only did I benefit from your ingenious insights and suggestions. Your profound belief in me and my work also helped me to stay motivated, focused and curious, especially when things did not turn out how I had hoped. Marcel, I cannot begin to express my thanks for the great amount of assistance I have received from you over the years. You continuously provided me with valuable advice, constructive criticism and useful guidance. I have learned so much from you. Whenever I work on a project or revise other researchers' work, your words of advice pop up in my mind. I am very glad to have met you and Erik and I look forward to working further with you in the near future.

I would like to extend my sincere thanks to the members of my dissertation committee Anna Gold, Jessen Hobson, Mark Peecher, Evelien Reusen, Erik Roelofsen and Victor Maas. Many thanks for your time and effort to review my dissertation. During this last phase of my PhD and earlier, you all provided me with useful comments and suggestions that had a profound impact on my way of thinking.

As co-authors on several projects, I would like to thank Mark and Jessen for their trust in my abilities and their unwavering support. Spending one semester with you at the University of Illinois at Urbana-Champaign was the highlight of my PhD trajectory. Mark, I am indebted to you not only for assisting me with my thesis, but also for being the great mentor that you are. I truly appreciate your ingenious guidance, encouragement and patience. You are my academic role model and I feel honoured to work with you. Likewise, I am extremely grateful to you, Jessen, for the tremendous time and effort you invested in our projects. Your positive energy, friendly manner and passionate way of doing research provided me with encouragement and inspiration throughout the duration of this journey. Our weekly meetings at UIUC have taught me how to read literature, develop ideas and push them forward. Thank you for your guidance.

Working on my thesis was often challenging, but the great colleagues I have had at RSM always made it enjoyable for me to go to campus. I will miss the lunches, meetings, workshops and spontaneous coffee talks with you Caspar, Christian, Florian, Iuliana, Jing, Marc, Max, Miriam, Nadine, Nicola, Paolo, Sandra, Saskia, Solomon, Stephan and Steven. I also wish to thank my PhD colleagues Albert, Aljaz, Anouk, David, Jeroen, Karin, Laura and Mengfan for being such a great support group. Special thanks go to the fellow researchers outside of Rotterdam who assisted me in my development, among them Olof Bik, Tjibbe Bosman, Jan Bouwens, Brian Gale, Steph Grant, Judith Künneke and Devin Williams.

One thing I have learned during my PhD is how to push boundaries. An even more important thing I have learned is how to respect boundaries to stay mentally healthy and sane. I want to thank my friends for their emotional support during these past five years. Nadja and Iris, thank you for acting as my paranymphs and being there for me during this special day. I am also grateful to Claudia, Elisa, Kristin, Moyo, Jacqueline Janssen, Vincent, the teams of Jaja Opera, Jeunesses Musicales, and all of my former housemates at Herman Robbersstraat, especially Abigail, for helping me to disconnect from academia and connect with each other via music, sports or dinner parties.

Finally, I would like to extend my deepest gratitude to my family for always supporting and believing in me. When the pandemic hit us last year and Europe went into lockdown, my parents welcomed me in my old boy’s room where I managed to finish the last chapter of my dissertation. Mom and Dad, I want to thank you for your unconditional support, your generosity and love. Danke!

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Chapter 1

Introduction

This dissertation examines how changes to the information landscape affect the quality of information reported by companies, its dissemination in capital markets, and investors' processing of the information. In particular, I investigate experimentally how public scrutiny of companies, changes to the structure of transaction fees, and the framing of company disclosure affect capital market communications.

Companies operate in a rapidly evolving environment that is transforming the traditional information landscape (Miller and Skinner 2015). With recent advances in technology, investors nowadays have additional means to scrutinize managers more closely on social media and engage with company disclosures (Brown, Gale, and Grant 2020; Cade 2018). Non-accounting regulators are also increasingly involved in shaping companies' information landscape. To illustrate, in January 2018 Markets in Financial Instruments Directive II (MiFID II), a major European investor protection regulation came into effect and significantly reduced the quantity of sell-side research available to investors (Fang, Hope, Huang, and Moldovan 2020; Guo and Mota 2020). Finally, one of the key drivers of companies' information lanscape is managers' decisions whether and how to share news with the public. In light of adverse events, such as the current global pandemic, it is vital that managers structure disclosure in a way that does not jeopardize the market's perception of their credibility.

The three studies comprising this dissertation examine how these changes to companies' information landscape affect each step of the communication process - the quality of information reported, its dissemination in financial markets and, finally, its usage by investors.

In chapter 2, which is joint work with Jessen Hobson, we investigate how the increasing investor scrutiny of managers can adversely affect the quality of information reported by these managers. The traditional earnings management literature is based on the premise that managers face strong incentives to report earnings that meet or beat important thresholds, such as the analyst consensus forecast (Burgstahler and Dichev 1997; Skinner and Sloan 2002). To mitigate this dishonest reporting, the literature offers at least two ways to curb earnings management - scrutinizing managers (Jensen and Meckling 1976) and screening out manipulative personalities that are particularly likely to manage earnings (Majors 2016). In this chapter, we challenge these two beliefs by examining a setting where a conscientious manager's true earnings meet the market's expectation exactly, the manager has no direct financial incentives to over-state earnings, but nevertheless succumbs to non-monetary incentives to over-state earnings. Drawing on insights from impression management literature, we reason that the increasing public scrutiny of managers enhances managers' awareness of skeptical investors. One way to overcome these image concerns is to simply report a more believable outcome. That is, we expect managers to manage earnings to appear truthful.

To test our predictions, we run an abstract experiment in the tradition of experimental economics. Student participants face incentives generalizable to the roles of investors and managers. The payoffs reflect managers' incentives to report earnings truthfully when true earnings meet or beat a threshold and over-state them when they miss the threshold. We manipulate between-subjects whether or not participants in the role of managers are identifiable to others. Further, we measure participants' "Dark Triad" personality, which captures a person's narcissism, Machiavellianism and psychopathy (Jones and Paulhus 2014). We find that, as public scrutiny increases, managers are more likely to misreport when true earnings meet a threshold exactly. Consistent with theory as well, this effect is more pronounced for conscientious managers, as captured by a low score on the Dark Triad personality measure. From a practical point of view, our results suggest that there are unintended costs to hiring "non-dark" personalities as executives and we suggest a cost-benefit analysis when screening out managers based on their personality.

Chapter 3 is motivated by a recent European regulation that requires broker-dealers to separate their clients' payments for investment research and trade execution (MiFID II). Concurrent archival research examines how transaction cost unbundling is associated with the quality and quantity of information available to investors (Fang et al. 2020; Guo and Mota 2020; Pope, Tamayo, and Wang 2020). In this study, I examine whether this new way of accessing and paying for sell-side investment research also changes how investors use information subsequent to obtaining it. Drawing on transaction decoupling theory (Soman and Gourville, 2001), I predict that transaction cost unbundling induces investors to rely more strongly on forecasted information when transaction costs are sufficiently high to provoke feelings of regret. To test this prediction, I conduct 16 experimental asset markets.

Attendant findings are consistent with my theory that investors fall prey to a sunk cost fallacy when they face unbundled transaction costs. That is, investors' estimates of asset values reflect costly forecasted information more strongly when higher transaction fees are unbundled than when they are bundled with other fees. The use of an experimental asset market further allows me to examine how this recent change in the information landscape impacts the dissemination of information in competitive markets. Consistent with my second prediction, I find that investors integrate other available information into their judgment at a slower pace when markets move towards a system of unbundled payments. This occurs because unbundled payments draw investors' attention to the forecasted information item and away from other information. Finally, my study exemplifies how an alternative to unbundling transaction costs, transparent cost disclosure, does not amplify the fixation on sunk costs. Hence, I suggest that regulators consider disclosure-oriented solutions in their future policy making.

Chapter 4 is joint work with Erik Peek and Marcel van Rinsum. In this chapter, we offer a theory for why investors sometimes disagree when interpreting a company's disclosure. More specifically, we examine how an adverse event disclosure affects the management credibility judgments of shareholders and outside investors differently, contingent on the locus of attribution employed by management in the disclosure. Relying on Social Identity theory (Tajfel and Turner 1986) and prior empirical findings, we posit that shareholders identify with the company they are invested in (see also Bernard, Cade, and Hodge 2018; Elliott, Grant, and Hodge 2018).

We find, in a richly contextualized experiment, that identified shareholders and non-identified outside investors make different judgments about managerial credibility when managers employ external attribution. Theory suggests that outside investors view such attributions as more deceptive, and hence judge the explanation given by management to be less credible than shareholders do. We manipulate investor roles as either current shareholders or outside investors and find that company identification has a strong positive effect on credibility given external attribution, while we find no significantly different effect under internal attribution. We also provide evidence that the difference in credibility perceptions between identified shareholders and outside investors leads to different valuation judgments with higher valuations for identified shareholders, thus indicating disagreement among investors. Prior literature assumes investor homogeneity in response to information and finds that disclosure reduces information asymmetries across investors (Diamond and Verrecchia 1991; Leuz and Wysocki 2016). In contrast, our study identifies a disclosure-induced distortion in shareholders' and outside investors' perceptions of management credibility and their asset valuations. That is, adverse event disclosure can induce disagreement among investors.

In sum, the three studies that form this dissertation build upon the notion that the information landscape is fluid and constantly evolving. Advances in technology, regulatory changes, and the occurrence of events outside of management's control all have important ramifications for how managers disclose information and how investors process this information. Each chapter tests one such implication by drawing on the comparative advantage of experiments (Libby, Bloomfield, and Nelson 2002). In chapter 2, my coauthor and I purposefully abstract from a real-world context to provide a powerful test of our theory that managers manage earnings to appear truthful – even though it is costly (Swieringa and Weick 1982). In chapter 3, I design and evaluate an alternative policy intervention (more transparent cost disclosure instead of unbundling) that does not yet exist in the real world (Kachelmeier and King 2002). In chapter 4, my co-authors and I measure insightful process variables that are not directly observable in the real world, yet explain why we observe a causal relationship between investors' company identification and their perception of a manager's crediblity (Asay, Guggenmos, Kadous, Koonce, and Libby 2019). In sum, theory and findings in these three studies collectively shed light on how developments in companies' information landscape can have a profound impact on preparers of financial statements and their intended users.

Chapter 2

Managing Earnings to Appear Truthful: The Effect of Public Scrutiny on Exactly Meeting a Threshold

2.1 Introduction

Recent research finds that despite increasing costs of earnings management related to Sarbanes-Oxley and other regulatory changes, firms continue to manage earnings to meet and beat earnings thresholds, such as zero earnings and analyst forecasts (Bird, Karolyi, and Ruchti 2019; Burgstahler and Chuk 2015, 2017; Gilliam, Heflin, and Paterson 2015; Keung, Lin, and Shih 2010). Keung et al. (2010) report an increase in meeting or beating analyst forecasts over time, while Gilliam et al. (2015) find less of a decrease relative to other

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This paper is joint work with Jessen Hobson. We thank Suzanne Bijkerk, Jivas Chakravarthy, Jeremy Douthit, Brian Gale, Steph Grant, Saskia Kohlhase, Ben van Landuyt, Mark Peecher, Erik Peek, Evelien Reusen, Dan Rimkus, Marcel van Rinsum, Steve Salterio, Steve Smith, Blake Steenhoven, Monte Swain, Shankar Venkataraman, David Wood, two anonymous reviewers for the 2020 AAA Annual Meeting and the 2020 ABO Section Meeting, and seminar participants at the 2020 AAA Annual Meeting, 2020 ABO Section Meeting, BYU's Accounting Brown Bag, ENEAR's Online Seminar Series, Erasmus University's Brown Bag and the University of Texas Reading Group for helpful comments. We also thank Bethany Brumley, Brian Gale, Alex Johanns, Sydney Kim, Ryan Sommerfeldt, Jeffrey Williams and Michael Yip for research assistance.

benchmarks (e.g. zero-earnings). However, investors and analysts are increasingly skeptical of earnings that just meet or barely beat forecasts (Ji, Rozenbaum, and Welch 2017). Since the early 2000s, earnings response coefficients for these earnings have decreased more significantly than earnings response coefficients to other earnings releases (Keung et al. 2010). Our study explores a potential implication of this heightened skepticism. We examine whether scrutinized managers attuned to how others perceive them avoid exactly meeting a benchmark, even when they must incur a monetary cost to alter unmanaged earnings. Thus, we examine a new incentive to manage earnings: misreporting to appear truthful. This research question is important since it identifies a counterintuitive incentive for otherwise honest managers to misreport unmanaged earnings that exactly meet a benchmark. Further, as described below, the pressure to misreport for this reason is increasing and particularly acute for managers who are psychologically attuned to others' perceptions of them. This incentive to misreport is most likely to affect conscientious managers who value perceptions of honesty, two factors that would normally be considered natural controls preventing earnings management.

Our new explanation for avoiding exactly meeting expectations is important because of the increasing public scrutiny of managers. Recent advances in technology allow investors to publicly scrutinize and pressure managers for behavior that does not conform to investors' own views. Further, these advances allow managers to be more immediately aware of different forms of scrutiny (Miller and Skinner 2015). For example, investors publicly criticize firms on social media (Cade 2018) and crowdsourced investment platforms (Chen, De, Hu, and Hwang 2014; Farrell, Green, Jame, and Markov 2018), and even participate in conference calls via solicited questions (Elliott, Grant, and Hobson 2019). Also the more traditional media outlets have expanded their coverage of firms in recent years, and there is evidence that CEOs anticipate potentially negative media coverage by holding back material information (Baloria and Heese 2018). Finally, interest groups exert substantial influence over firms by launching public campaigns (Brav, Jiang, Partnoy, and Thomas 2008; Levit 2019). While prior research acknowledges that this scrutiny creates pressure and incentives for managers to engage in earnings management (Kolev, Marquardt, and McVay 2008; Stein 1989), the literature often assumes that managers follow their financial incentives and misreport *in order to meet* important benchmarks (Burgstahler and Dichev 1997; Dechow and Skinner 2000; Degeorge, Patel, and Zeckhauser 1999; Dhaliwal, Gleason, and Mills 2004). Our research discusses the role of our theoretical construct, public scrutiny, when the manager has met earnings thresholds absent any form of misreporting and has no direct financial inventive to misreport. In this setting, we manipulate the intensity of public scrutiny of the manager and measure the manager's individual predisposition to be sensitive to and concerned about investors' perceptions of them. We then examine how these two theoretical constructs jointly moderate the effect of meeting earnings exactly on managerial reporting choices.

Our theory building draws on insights from social psychology that individuals do not want to be perceived as deceptive (Vrij, Granhag, and Mann 2010), but rather want to be seen in the best possible light (Asch 1955; Leary 1986). We reason that the enhanced visibility that is inherent to public scrutiny mechanisms reinforces image concerns and will amplify the desire to be seen as honest (Lacetera and Macis 2010). Thus, in line with archival evidence that investors are increasingly skeptical of earnings that exactly meet a benchmark (Keung et al. 2010), we predict that higher public scrutiny will lead managers to become more aware of this skepticism and thus be more likely to misreport when exactly meeting an earnings benchmark. Similar image concerns are absent when managers miss or easily exceed an earnings threshold since such reports send a more credible signal that earnings have not been altered. In order to provide further insights into the process that underlies image concern-driven earnings management, we examine how Dark Triad (DT) personality affects misreporting to avoid exactly meeting benchmarks. A core feature of the DT personality is that those who measure high in DT have less empathy and more aversion and callousness toward others (Book, Visser, and Volk 2015; Hobson et al. 2019; Jonason and McCain 2012). In our setting, where slightly overstating earnings has negligible benefits over meeting market expectations exactly, managers low in DT are more likely to worry about potentially skeptical responses from exactly meeting a benchmark, since they are more attuned to how they are viewed by others. Thus, when public scrutiny increases, we expect managers low in DT, the managers we generally consider to be the good actors (low in narcissism, Machiavellianism, and psychopathy), to be more likely than managers high in DT to misreport to avoid exactly meeting a benchmark.

To test our research question, we use a low-context experiment in the tradition of experimental economics, which allows us to capture the interactive reporting process between managers and investors and precisely measure earnings management and the Dark Triad (Kachelmeier 2018).[2] In our two-player, extensive form game, participants in the role of managers privately observe true earnings for each round, which either miss, meet, or beat market expectations. Investors receive managers' earnings reports and, depending on their assessment of earnings quality, accept or reject them. We instantiate a setting in which managers have nonmonetary incentives to overstate earnings when they exactly meet a benchmark because investors believe a "meet" report is an overreporting of earnings that actually miss the benchmark. However, managers maximize their monetary payoff by truthfully reporting when they meet or beat the earnings benchmark and overstating earnings when they miss the benchmark. Investors have incentives to detect deception (Dyck, Morse, and Zingales 2010). In sum, managers whose true earnings meet the benchmark have a financial incentive to report truthfully but an impression management incentive to misreport that their earnings beat the benchmark, since investors likely believe earnings actually missed the benchmark. This tradeoff between impression management and financial incentives is the point of our study.

Our experiment has a 3 x 2 x 2 mixed-design. Though our research question is primarily concerned with behavior when a manager exactly meets a benchmark, we make the effortful choice to operationalize the full spectrum of true earnings missing, meeting or beating the earnings benchmark, within subjects to authentically capture the natural setting's reporting space, which allows for accurate incentives, repeated play, and omitting deception. We

[2] As discussed in more detail in the methods section, following well-established precedent in the experimental economics paradigm, our study appropriately uses undergraduate student participants and avoids contextual terms and labels. However, to ease comprehensibility and efficiency our discussions throughout the paper uses the contextual labels of the natural setting.

manipulate our first moderator, public scrutiny in each round, between subjects as whether manager-participants are (high public scrutiny) or are not (low public scrutiny) asked to stand up one by one before issuing their report, such that they are identifiable and visible to (i.e. scrutinized by) other investor-participants in the room (Sutton and Galunic 1996). We measure our second moderator, Dark Triad, using the Short Dark Triad (SD3) questionnaire prior to the experiment (Jones and Paulhus 2014).

We find that, as expected, managers report truthfully when beating their benchmark and over- or underreport when missing the benchmark, regardless of our moderating variables. More importantly, we find that a considerable proportion of managers deviate from the Bayesian Nash-equilibrium prediction of truthful reporting when true earnings meet earnings expectations, depending upon the presence of public scrutiny and level of DT, in the manner predicted by our theory. Specifically, when earnings exactly meet a benchmark, managers are more likely to manage earnings when public scrutiny increases, even though this costly action decreases their expected payoffs. Consistent with the notion that image concerns motivate earnings management, we find that this effect is stronger for low versus high DT managers. Further, we show that deviating from truthful reporting in this way increases managers' belief that the market will accept their future reports. Thus, managers accept potential monetary losses for the perception of truthfulness.

Our results contribute to research and practice. We examine a natural but unexplored consequence of the increased investor scrutiny of managers meeting earnings benchmarks—managers misreporting to appear truthful. While extensive research examines financial and labor market incentives to manage earnings (Graham, Harvey, and Rajgopal 2005; Healy 1985; Healy and Wahlen 1999), recent work suggests that managers also have personal incentives to over-state earnings that are not solely tied to monetary benefits. (Asay 2018; Brown 2014; Seybert 2010). We complement this growing stream of literature by examining another motive for earnings management—managers do not want to be seen as deceptive, which may ironically induce them to decrease the quality of their earnings reports when true earnings meet expectations in order to avoid the perception that they actually missed the target and misreported. While prior research in the management accounting literature documents that managers have a preference for honest reporting (Evans, Hannan, Krishnan, and Moser 2001; Rankin, Schwartz, and Young 2008), and image concerns seem to, at least partially, drive this effect (Abdel-Rahim, Hales, and Stevens 2019), these specific studies assume that truthful reporting aligns with being perceived as a good steward. This is not necessarily true in an environment that incentivizes misreporting and continuously communicates performance expectations. Notably, Maas and Van Rinsum (2013) provide evidence that managers also misreport to help others when they have monetary incentives to overstate performance. Unlike their study, we purposefully abstract from reality by introducing monetary disincentives to misreport, which provides a strong test of our research question. We extend psychology research that documents how individuals underreport performance when their performance looks too good to be true (Choshen-Hillel, Shaw, and Caruso 2020) by showing that honesty-related image concerns arise endogenously in the financial reporting setting and we identify two important moderators that are within governance bodies' control.

The examination of public scrutiny speaks to the increasing exposure of managers to their skeptical investor base. Prior research in accounting has focused on how investors react to managers' increasing presence on social media (Elliott, Grant, and Hodge 2018) and their visibility within firm disclosure (Asay, Libby, and Rennekamp 2018b). We suggest that these effects are not one-sided, approaching this issue from the other direction to contemplate how the enhanced visibility of the CEO affects financial reporting practices. Understanding how anticipated public scrutiny influences firm reporting enhances our knowledge of how critical investor attention affects the quality of disclosure even before investors react to the report. Finally, our study complements related literature on the implications of managers' personality in financial reporting. A growing body of literature is concerned with the DT personality of managers, employees and auditors (Hobson et al. 2019; Majors 2016; Wang 2017). For example, Majors (2016) conjectures that high DT managers are more likely to report aggressively than low DT managers, which implies that firms are better off screening out high DT managers. Yet, our results indicate that low DT managers' high empathy makes it more difficult to resist scrutiny and take welfare-enhancing reporting choices. Importantly, this type of misreporting may extend beyond an earnings benchmark setting and apply to other instances where managers' reporting is subject to public scrutiny and a salient reporting benchmark. Hence, we identify a potential cost to hiring low DT managers and suggest that governance boards can take measures to mitigate these costs.[3]

[3] Firms could, for example, attach more names to corporate reporting decisions to alleviate managerial perceptions of public scrutiny.

2.2 Literature Review and Hypotheses Development

2.2.1 Meeting Earnings Benchmarks Exactly and Market Skepticism

Despite evidence that earnings management activities have decreased and/or migrated to real-earnings management activities (Bird et al. 2019; Cohen, Dey, and Lys 2008; Gilliam et al. 2015; Zakolyukina 2018), evidence exists that managers continue to manage earnings to meet or beat common thresholds/benchmarks. For example, Keung et al. (2010) report an increase in meeting or beating analyst forecasts. At the same time, investors and analysts are increasingly skeptical of earnings that just meet or barely beat forecasts (Ji et al. 2017), as noted by significantly lower earnings response coefficients for these earnings relative to other reports since the 2000s, but no significant differences in prior periods (Keung et al. 2010). Thus, the earnings-announcement calculus that leads to the "widely held belief that every firm manages earnings to hit targets" (Graham et al. 2005, p. 67) appears to still be in force, but to have shifted so that the general assumption that hidden "cockroaches" (Graham et al. 2005, p. 29) exist when a company misses earnings benchmarks has transferred to companies that exactly meet benchmarks. [4] To illustrate, archival research in accounting uses the incidence of exactly meeting the analyst consensus as a proxy for low earnings quality (Burgstahler and Dichev 1997). Keung et al. (2010, p. 107) state that "it is likely that even firms that report a zero or small positive earnings surprise 'truthfully' are penalized in the backlash". These findings form the backdrop

[4] Keung et al. (2010) find that the market has become more skeptical of earnings that meet and also narrowly beat earnings expectations, within a range of 1 cent around the market consensus. Our study does not distinguish meeting from narrowly beating expectations. Instead, we consider an earnings number to meet expectations as long as it falls into a range of investor scrutiny. Future research may investigate the exact magnitude and cut-off points of this skeptical range.

of our investigation of how managers respond when faced with meeting an earnings benchmark. In particular, we examine two potential moderators—public scrutiny and Dark Triad personality.

2.2.2 Public Scrutiny

Public scrutiny is a multi-faceted construct, which has been defined as persistent attention to a leader, close performance monitoring, frequent interruptions and relentless questions about past, current and future actions (Sutton and Galunic 1996). Firms face such scrutiny from a variety of sources. For example, governments closely monitor managers and their decisions (Tracy 2020; Tweh 2020), regulatory oversight bodies have intensified their inspection efforts over financial reporting and auditing since Sarbanes-Oxley (Gunny and Zhang 2013; Robinson, Xue, and Yu 2011), media outlets negatively slant firm coverage to alter corporate decisions (Baloria and Heese 2018; Core, Guay, and Larcker 2008; Dyck, Volchkova, and Zingales 2008), institutional shareholders increasingly connect and exert their voice in the form of public campaigns (Levit 2019), and even retail investors, aided by advances in information technology, take part in corporate reporting processes such as conference calls (Elliott, Grant, and Hobson 2019) or confront firms on social media (Cade 2018). Accordingly, there is a large stream of literature that investigates how public scrutiny affects financial reporting in light of clearly defined financial incentives for managers to over- or understate their performance (Hall 1993; Key 1997; Kolev et al. 2008; Stein 1989).

While public scrutiny over financial reporting fulfills an important corporate governance role by disciplining managers who engage in opportunistic reporting activities, prior research also documents unintended consequences. For example, in the context of SEC scrutiny over Non-GAAP reporting, Kolev et al. (2008) document positive and negative effects of public scrutiny on firms' reporting decisions. While they find that SEC interventions discourage certain firms from issuing low-quality Non-GAAP earnings, they also find the perverse effect of firms decreasing the quality of their reports by classifying more recurring items as special items. Relatedly, Gneezy, Kajackaite, and, Sobel (2018) examine in an experimental setting the conditions under which individuals tell partial lies. The authors find that participants tend to lie more when the outcome of their actions is observable, i.e. when there are reputational concerns.

Financial reporting decisions are influenced by personal motives and attributes of reporters that go beyond considerations of immediate increases in personal wealth. For example, an important consideration for managers in their decision making is their reputation to be seen as competent and in control (Graham et al. 2005; Seybert 2010). Following the literature on managerial traits and earnings management, we posit that managers who care about how others view them likely care about maintaining not only an impression of competence, but also perceptions of honesty. Indeed, prior literature argues that managers have self-presentational goals and employ impression management (Abdel-Rahim et al. 2019; Asay, Libby, and Rennekamp 2018a; Bloomfield 2002; Chen and Loftus 2019). Impression management is the process by which people seek to control the impressions others form of them (Leary and Kowalski 1990). This literature posits that people naturally worry about how they are perceived by others (Leary 1986). For example, Asch (1955) posits that individuals have self-presentational goals. Under certain circumstances, these self-presentational goals may be so strong that individuals act contrary to their own preferences. Communication research argues that such a situation arises, for example, when people are interrogated because they dislike the prospects of being perceived as deceptive. Even if they have nothing to hide, they are concerned with the impressions that others form of them (Vrij et al. 2010). Relatedly, Abeler, Nosenzo, and Raymond (2019) find in a meta-study on truth-telling that the best theoretical models capture both preferences for being honest and preferences for appearing honest, which speaks to our proposition that people generally want to be seen as honest.

Image concerns are particularly likely to arise in the context of financial reporting due to the inherent information asymmetries between the investing public and privately informed managers, which impedes the ability to credibly signal private information (García Osma and Guillamón-Saorín 2011; Jensen and Meckling 1976; Stocken 2000). Thus, skepticism is a natural part of the setting, especially when managers send a suspicious-looking reporting signal, such as reporting to have met an earnings benchmark exactly. Investors more likely conclude that the manager has engaged in the manipulation of earnings to reach this outcome to avoid the typically negative market reaction for reporting a negative earnings surprise (see e.g. Skinner and Sloan, 2002).[5] Given the recent advances in information technologies, investors now have better means to censure managers. In light of this elevated public scrutiny over financial reporting, managers who observe that true earnings meet expectations exactly face a conflict between reporting truthfully and issuing a more believable earnings report (i.e. maintaining one's image by reporting untruthfully). Consequently, we argue that greater scrutiny makes managers

[5] Note that this prediction does not hold if investors can fully anticipate the manager's self-presentational concerns. Investors and managers are unlikely to predict these anticipated concerns as prior research consistently documents limits on iterated thinking in strategic games (Camerer 2003).

more aware of investors' potentially skeptical reactions to their report of meeting and therefore more likely to react to these concerns. Specifically, managers have an increased awareness of the adverse signal that a truthful "meet" report will send. One way to relieve these image concerns is to simply report higher (or lower) earnings – a more believable but untruthful signal.

H1: *Managers' propensity to misreport earnings increases with public scrutiny when exactly meeting a benchmark, but less so when not meeting it.*

Note that when true earnings beat or miss expectations the effect of public scrutiny is generally mitigated in the natural setting. When true earnings exceed the benchmark, managers may not be fully believed, but it is even more costly for them to report lower than true earnings. When true earnings miss the benchmark, managers have a well-known incentive and they are even expected to overreport earnings (Dechow and Skinner 2000, p. 248; Graham et al. 2015). Thus, our design, described in more detail below, incentivizes managers whose earnings beat a benchmark to report truthfully and managers whose earnings miss a benchmark to overreport earnings. It is this incentive, in fact, that leads to the possibility that managers whose earnings meet the benchmark be perceived by investors as having misreported (because they missed the benchmark and overreported). Thus, public scrutiny plays less of a role when true earnings beat or miss the benchmark in our setting. Consequently, our hypotheses focus on manager behavior when true earnings meet expectations.

2.2.3 Dark Triad Personalities

The effect hypothesized above is more likely to occur when the manager is more attuned to the impression she is making on others. One measure of interpersonal awareness and concern, recently examined in the accounting literature, is the Dark Triad (DT) personality. The DT describes the extent to which individuals score high on three distinct personality constructs that share a common core: psychopathy, narcissism and Machiavellianism (Paulhus and Williams 2002). DT personalities are characterized by social averseness, low levels of empathy and callousness, which is why we focus on the Dark Triad as our theoretical core construct (Book et al. 2015; Jonason and McCain 2012). Both high and low DT personalities are represented in the business world (Babiak, Neumann, and Hare 2010; Hobson et al. 2019; Judge, Piccolo, and Kosalka 2009; Mathieu, Hare, Jones, Babiak, and Neumann 2013) and DT components have been linked to fraud (Johnson, Kuhn, Apostolou, and Hassell 2013), white-collar crimes (Lingnau, Fuchs, and Dehne-Niemann 2017; Murphy 2012) and tax avoidance (Olsen and Stekelberg 2016). Other work in accounting shows evidence consistent with the conjecture that high DT managers behave with less integrity when pursuing their own goals (Majors 2016; Wang 2017).

On the other hand, high DT managers may be better able to resist market scrutiny when exposed to others' judgment, since high DT personalities are primarily characterized by low empathy or callousness in the context of relationships with others (Book et al. 2015; Hobson et al. 2019; Jonason and McCain 2012; Jones and Figueredo 2013). It is this lack of empathy that normally induces high DT personalities to pursue their own benefits, even at the detriment of others (Majors, 2012). Yet, under certain circumstances, this lack of empathy may also be beneficial to others. Recent research has shown that auditors higher in DT personality are less likely to mistakenly trust managers due to increased social interaction (Hobson et al. 2019). We employ a similar reasoning and argue that high DT managers are less likely to experience image concerns when disclosing an earnings "meet" under public scrutiny. The reason is that their impaired emotional ability makes them less concerned about how others will react to their report. In contrast, we expect individuals who score low on the DT measure to worry more strongly about potentially skeptical market responses to a meet report when public scrutiny increases. That is, managers face a conflict between being honest and being perceived as honest when reporting earnings that truly meet expectations. We posit that high (low) Dark Triad managers in this situation will worry less (more) about maintaining their reputation as public scrutiny increases. The most likely way for managers to overcome this concern is to report a more credible signal (e.g. beating) even if it means that they have to engage in earnings management. Following this reasoning, we predict that low DT managers are more likely than their high DT counterparts to react to public scrutiny by engaging in earnings management when they meet, as opposed to missing or beating a benchmark.

> **H2:** *Managers' higher propensity to misreport when public scrutiny increases and true earnings meet a benchmark, as opposed to not meeting it, is more pronounced for low than high Dark Triad managers.*

2.3 Methodology

2.3.1 Experimental Setting

We model a financial reporting setting in which an expectation exists for earnings, such as a consensus analyst forecast or zero earnings. True earnings are often above or below the expectation, and occasionally exactly at the expectation. The manager alone learns whether true earnings miss, meet, or beat the expectation and then reports the true outcome—miss, meet, or beat—or an adjacent option (e.g., if earnings miss the benchmark the manager can report miss or meet but not beat, see Figure 2.1). Investors view this report and accept or reject it. Managers are better off reporting the truth when earnings meet or beat, and reporting meet when earnings miss. They are always better off when investors accept their report. Investors are better off accepting truthful reports, but rejecting otherwise. Thus, our setting is a simplified financial reporting setting with information asymmetry in which expectations exist.

We use a low-context, two-player extensive-form game to capture the key institutions and incentives to examine the above setting in the lab.[6] Figure 2.1 shows the payoff structure in three separate matrices, one for each possible realization of true earnings (for the full game tree in extensive form, see the appendix). Managers are first movers and begin by privately observing the underlying true earnings. The realization of earnings follows a distribution where missing and beating the market consensus have equal probabilities (P(miss)= P(beat)= 37.5%). Meeting the consensus exactly is the least likely outcome (P(meet)= 25%). The likelihood distribution of true earnings is

[6] Following appropriate experimental practices (Davis and Holt 1993; Friedman and Cassar 2004; Kagel and Roth 1995; Smith 2003) we do not try to capture all factors of the real world. Rather, we model the key incentives and institutional aspects that are fundamental to our setting (e.g., Bowlin, Hobson, and Piercey 2015), as discussed below. Following appropriate experimental economics research methods, we avoid all unnecessary contextual labels, in order to maximize internal validity (Haynes and Kachelmeier 1998; Kachelmeier and King 2002; Smith 1982 p. 923). We use contextual labels in the manuscript for ease of reading. External validity of our modeling the manager disclosure setting as a two player game requires the belief that the CEO's individual choices affect firm disclosure policy (Davidson Dey and Smith 2019; Ge Matsumoto and Zhang 2011; Ham Seybert and Wang 2018) and that the CEO can envisage the recipients of their disclosure (Graham, Harvey, and Rajgopal 2005). This latter assumption seems likely given the increasing presence of activist and vocal investors (Bebchuk, Brav, Jiang, and Keusch 2019; Brav, Jiang, Partnoy, and Thomas 2008). At the least, managers are likely to know and communicate with their top shareholders.

common knowledge and communicated several times to participants. We randomly select one pattern of actual realizations before the experiment and use that pattern for each experimental session.

Figure 2.1: Payoff Matrix

True Earnings (probability):

BEAT (37.5%)

MANAGER \ INVESTOR	ACCEPT	REJECT
BEAT	50, 50	40, 40
MEET	40, 30	30, 40

MEET (25%)

MANAGER \ INVESTOR	ACCEPT	REJECT
BEAT	40, 30	30, 40
MEET	50, 50	40, 40
MISS	10, 30	0, 40

MISS (37.5%)

MANAGER \ INVESTOR	ACCEPT	REJECT
MEET	40, 30	30, 40
MISS	20, 50	10, 40

(manager payoff, investor payoff)

We operationalize the managerial reporting process as a two-player game in extensive form. At the beginning of the game, the manager observes an exogenous private earnings signal (i.e. true earnings), which either beats a benchmark, meets it, or misses it. Beat occurs 3/8 of the time, Meet ¼ of the time, and Miss 3/8 of the time. In the next step, the manager sends a report to the investor about the true earnings. The set of possible messages is conditioned on true earnings as specified in the matrix above. The investor only observes the earnings report, not the true earnings and chooses then between two options. He maximizes his payoff by accepting truthful earnings reports and rejecting untruthful reports. Managers maximize their payoff by reporting Beat and Meet truthfully and overstating earnings whenever a Miss occurs.

2.3.2 Incentive Structure

Participants' payoffs are a function of managers' actions, investors' responses and the underlying earnings (i.e. chance). Each round, managers decide whether or not to report earnings truthfully. If managers decide to misstate, we restrict their action space such that they can only report one earnings realization higher or lower than true earnings. This restriction of players' action space follows from our focus on earnings management and real earnings management around benchmarks, not large-scale fraud (Zakolyukina 2018). Adjustments around benchmarks involve relatively small interventions (Bhojraj, Hribar, Picconi, and McInnis 2009; Kothari 2001) and larger deviations from true earnings are more costly (Bird et al. 2019). This also allows us to avoid complicating the theoretical model further with additional dominated strategies. Investors (see Figure 2.1) have incentives to accurately identify the underlying economic fundamentals of a firm from its reporting (Leuz and Wysocki 2016), which includes deception detection (Dyck, Morse, and Zingales 2010). Hence, investors in our game earn the highest payoff, 50 points, when they accurately assess the earnings quality of a truthful report to be high and accept it. Inaccurately assessing a truthful report to be of low earnings quality by rejecting it reduces their payoff to 40 points. Similarly, investors have incentives to accurately reject an untruthful report, culminating in a payoff of 40 points.[7] Erroneously accepting such reports reduces their

[7] The fact that managers receive the same payoff from the acceptance of an untruthful beat report and the rejection of a truthful meet report does not necessarily reflect real-world payoffs. However, these parameters are critical to the design of our study as it helps us exclude the alternative explanation that managers misreport for pecuniary gains. Simplifying the real-world setting in this manner allows for a strong test of theory (Peecher and Solomon 2001).

payoff to 30 points.[8] Hence, the incentive structure reflects that the cost of wrongly accepting an erroneous report looms larger than the cost of foregoing an investment opportunity.

Managers can reach the highest payoff of 50 only if they report truthfully to have met or beaten the consensus and investors find their report credible (i.e. investors choose Accept when true earnings meet or beat). Thus, managers whose earnings truthfully meet the benchmark, have a financial disincentive to misstate, as further discussed in the sections below. If managers have missed a benchmark, then a truthful report that convinces the investor reaches a payoff of 20 points, but an untruthful report could reach up to 40 points. The maximum payoff of the game (50) is not attainable here since managers incur manipulation costs to report a 'meet' (10 points). These costs may include things such as time spent on deceiving auditors, resources spent on bribing other members of the accounting department and other opportunity costs of inefficient decision making (Strobl 2013). Prior literature finds that firms who report to have missed the earnings consensus, even just by a cent, experience disproportionately lower abnormal returns (Bartov, Givoly, and Hayn 2002; Skinner and Sloan 2002) and the respective managers face negative career outcomes (Matsunaga and Park 2001). We therefore design our model in such a way that the lowest managerial payoffs are realized whenever managers truthfully or untruthfully report to have missed the benchmark. As a consequence, managers have strong incentives to misreport when earnings miss the consensus.

[8] Unlike managers, investors are not affected by true earnings, but only the report of the manager. The investors we consider are therefore not necessarily shareholders, but they can invest in other, lower risk assets. That is, they can make a profit independent of whether the firm misses or beats a benchmark.

Strategies

As noted earlier, investors only see a manager's report, not true earnings. They are aware of the incentive structure. Hence, whenever they receive a report that management has met market expectations exactly, it could mean three things. First, the firm may have beaten the benchmark, but decided to report a lower earnings number. This outcome is unlikely as a benchmark-meeting report is dominated for the manager by truthfully reporting a beat. Second, it could mean that management has indeed met the market consensus exactly; however, this outcome is the least likely event and occurs only one in four times. Finally, it is likely that the firm has missed the earnings target and overreported earnings. Thus, importantly, observing an earnings report that claims to meet the market consensus induces investors to be skeptical of earnings quality, which is in line with insights from the archival literature (Keung et al. 2010). However, it is important to note that managers have no monetary incentive to overreport when they exactly meet earnings expectations, since all other choices are weakly dominated. That is, managers at best can receive 40 if they untruthfully report to beat the benchmark, but this is only if investors accept, while they cannot earn less than 40 if they report truthfully. This, then allows us to test our hypotheses by analyzing whether managers anticipate investors' skeptical reactions and deviate from optimal reporting choices to avoid being seen as deceptive.

The sequential game that we designed has a pure-strategy solution. As noted by Kachelmeier and Van Landuyt (2017), this design choice affords the benefits of simplicity and allows us to test for deviations from the unique solution of the incomplete information extensive-form game. The Bayesian Nash equilibrium is characterized by managers always reporting truthfully when the underlying earnings meet or beat the market consensus. For earnings that miss the benchmark, managers always misreport "meet". Investors in turn accept reports that claim to have beaten the benchmark and reject those reporting to have met the benchmark. For details of the equilibrium solution, see the appendix.

2.3.3 Summary of Setting and Incentives

While we abstract from features of mundane realism to avoid loaded terms that potentially induce noise and reduce the internal validity of our experiment (Swieringa and Weick 1982), we are able to capture the key concepts of our intended financial reporting setting. First, managers have clear incentives to opportunistically adjust discretionary accruals when they miss the market consensus by small amounts, as can be seen in Figure 2.1, if a manager reports meet after missing the benchmark. Creating clear incentives to manage earnings in order to meet the market consensus is in line with findings from the archival literature on earnings management around benchmarks (Burgstahler and Dichev 1997; Degeorge, Patel, and Zeckhauser 1999; Jacob and Jorgensen 2007). Second, earnings management is costly since managers spend valuable time and resources on the presentation of results while neglecting their value-creating duties, which in turn increases litigation and reputational costs (Bird et al. 2019). This can be seen in Figure 2.1 as managers' payoff decreases by 10 points whenever they report untruthfully, except when earnings miss expectations. Third, manipulation costs make it unprofitable to deviate from truthfully reporting benchmark-meeting earnings, since managers lose money on expectation by deviating from reporting the truth. This allows us to isolate the effects of impression management versus economic incentives on managers' decisions.

Fourth, investors in our game can earn higher returns by accurately assessing firm reporting, as seen in Figure 2.1 by noting that investors' highest payoff is when they select Accept when the manager has reported truthfully. Knowledge of firm fundamentals helps in forecasting earnings and leads to higher future returns. Being either overly skeptical or credulous of a report's veracity are both income-decreasing strategies. Fifth, investors always have an outside option to trade lower risk assets, which is captured by the constant payoff of 40 when rejecting reports in our game. Finally, the economic game limits reporting options of managers depending on the underlying true earnings, since managers can only report one earnings category higher or lower than true earnings. This captures the institutional reality that discretionary earnings management, which is mostly used for benchmark meeting and beating, is only feasible in small amounts (Degeorge et al. 1999). In sum, our setting creates a dilemma for managers that allows us to test our research question. When the underlying earnings meet the market consensus, truthful reporting weakly dominates reporting falsely—managers have no financial incentive to not report the truth. However, it is likely that investors believe this report is untruthful, since on average, a meet report is likely deceptive. Thus, managers might trade off the benefit of higher payoffs with the psychological "cost" of being perceived by investors as misreporting. Our hypotheses examine the effect of our independent variables on this choice.

2.3.4 Detailed Procedures, Design, and Independent Variables

Participants are 156 undergraduate students who have attended at least one course in accounting, finance or economics at a major U.S. business school. They are on average 20 years old and 60% are female. We conduct six separate experimental sessions with ten rounds each.[9] Each session lasts approximately 60 minutes and participants earn on average USD 29.37 with a range from USD 21.20 to USD 32.60. The experiment uses a 3 x 2 x 2 mixed design that manipulates *true earnings* within subjects (miss, meet or beat) and *public scrutiny* between subjects (low or high). We also measure whether a participant classifies as high or low DT personality. In order to program the interactive experiment, we use z-tree software (Fischbacher 2007).

Once participants sign up for our study, they receive an e-mail with a link to a survey that measures their DT personalities using the Short Dark Triad (SD3) questionnaire (e.g., Hobson et al. 2019; Jones and Paulhus 2014; Majors 2016; Wang 2017). Participants rate their agreement with each of the 27 items of the SD3 questionnaire. Responses are given on a 5-point Likert scale. Our measurement is reliable as indicated by a Cronbach alpha of 0.78. We therefore collapse all answers into a single construct. We create our DT measure with a median split, such that individuals who score above the median are classified as high DT managers. Thirty-four manager-participants are classified as high DT and 44 as low DT.[10] The survey also measures

[9] Randomization tests confirm that none of our demographic variables are significantly correlated with our between-subjects factors with the exception of gender. Significantly more women (n=29) than others (n=15) are classified as low DT managers (p=0.03). Controlling for gender in our main tests does not change inferences, so we exclude this covariate from subsequent analyses. The number of participants per session is 28 in three sessions and 22, 24 and 26 in one session each. Controlling for session number does not change inferences.

[10] Similar to Hobson et al. (2019), we exclude from the original instrument the question "I have never gotten into trouble with the law" due to its sensitive nature. The median (mean) DT score of all participants is 2.78 (2.81). Scores range from 1.93 to 4.26 with a standard deviation of 0.39, comparable to scores in prior accounting studies (Majors 2016; Wang 2017). DT persoanlities are randomly allocated across public scrutiny conditions (p=0.36). Using a different median split, such that participants are classified as high DT when they score at or above the median does not change inferences with one exception. While we still find a significant interaction in support of H1, the marginally significant simple effect of public

Leary's Brief Fear of Negative Evaluation scale to differentiate the DT-related lack of empathy from dispositional concerns about being scrutinized (Leary 1983). We separate the measurement of these personality variables in time from the actual experiment to avoid confounding effects.

Upon arrival to the laboratory, participants randomly receive a card with a number assigning them to one of the 30 workstations in the group room. All workstations are separated by partitions such that it is very difficult to see a neighbor's screen. Seeing a person who is standing up at the other end of the room is however perfectly possible from all of investors' workstations. We arranged the workstations in such a way that investors and managers are seated at opposite sides of the room. After participants sign a consent form, the administrator reads the instructions aloud while participants follow along at their screens. We incorporate several comprehension questions within the instructions to ensure that our participants have an adequate understanding of the experimental procedures and the payoff structure. In addition to presenting subjects with the payoff matrix on their screens, we also provide everyone with a printed copy and explain the matrix in detail. Before the game starts, a screen reminds all participants that managers may report untruthfully.[11] At the beginning of each round, the computer matches every participant with a different player. Similar to Majors (2016), we repeatedly re-match participants with a new subject each round and inform participants only at the end of the experiment about their aggregate payoff and the actual

scrutiny given that true earnings meet the benchmark exactly increases slightly from 0.09 to 0.11, one-tailed.

[11] Specifically, our instructions state that the manager "may report a state of the world [earnings] different than the true state of the world" and that their "payment is determined by what other people think about him or her". While these instructions may increase the level of effect sizes, they cannot explain the interactive patterns predicted by our hypotheses.

realization of true earnings. Avoiding repeated interactions precludes the formation of reputation from feedback.[12] This in turn strengthens construct validity since we predict that managerial reporting is based on managers' anticipation of investors' response, not their actual response.[13] Managers get a private signal about the true earnings of the current period. This is our manipulation of true earnings. In total, the underlying earnings meet and beat the market consensus three times each and miss it four times.

This is followed by our manipulation of public scrutiny. *Public scrutiny* manipulates the level of scrutiny present when managers report to investors each period using a compound manipulation. In the condition with low public scrutiny, managers simply respond to the question "Which state of the world [earnings] do you want to report?" In the high public scrutiny condition, managers respond to the same question but prior to that, each manager must stand up for 10 seconds and hold up a card with their player number such that they are visible to all investor-participants in the room (for a similar compound manipulation see Maas and Van Rinsum, 2013). All participants' computer screens indicate which participant is currently standing and a timer with the remaining time is displayed. Most importantly, investors in this condition (but not in the condition with low public scrutiny) are informed

[12] The instructions repeatedly mention that participants "will be paired with a different player each round". We also explain that each period, investors will see the manager's report, not true earnings. Since we never reveal the outcome of an individual round and managers are unaware of who they are paired with, we can rule out multi-period reputational concerns and feelings of accountability as an alternative explanation for our findings.

[13] Incorporating actual feedback would likely make our results even stronger since investors reject most of the earnings reports that indicate that the consensus was met. As noted by Majors (2016), this design choice also allows our results to generalize to instances of managerial reporting that do not generate direct instantaneous feedback. In addition, investors in financial markets do not always (sometimes never) learn the true underlying earnings signal that would have occurred in the absence of earnings management.

about which player they are paired with. Managers are not told with whom they are matched. Thus, for managers, this manipulation heightens the salience of their role as reporting to an investor. We have opted for this compound manipulation since visibility and identifiability are both important parts of the multi-faceted construct public scrutiny. Asking manager-participants to stand up without revealing to investors who they are paired with would not allow the investor to scrutinize individual managers. In turn, manipulating identifiability without exposing managers to investors would not capture the persistent attention and intense monitoring which are inherent to our theoretical construct (Sutton and Galunic 1996). Our manipulation analogizes to any setting that heightens managers' exposure to critical market scrutiny, such as increased media attention, shareholder activism, and investor scrutiny on social media or investment platforms.

Next, managers decide what outcome to report and we measure their anticipated acceptance of their report. In the public scrutiny condition, we also ask investors each round whether they know the person they are matched with before the next round starts. After 10 rounds, participants complete a post-experimental questionnaire, which measures demographic variables and manipulation check questions. Finally, all participants are informed about the aggregate realization of true earnings and their payoff. We pay participants in cash at the rate of USD 0.06 per point earned along with a show-up fee of USD 5.00.

2.4 Results

2.4.1 Comprehension Checks

To test the salience of our manipulations, we asked participants several questions. First, we measure one a on a 7-point Likert scale participants' agreement with the statement "In this game, GREEN players [investors] know which other BLUE player [manager] they are paired with". As expected, we find a significant difference among the responses of individuals in the conditions with low (μ=1.18) and high public scrutiny (μ=5.45, t=16.60, p<0.01).[14] Second, we include in the exit survey an item related to our manipulation of *true earnings*. Approximately 94% of participants remember correctly that meeting earnings exactly is the least likely outcome.[15]

2.4.2 Descriptive Statistics

Table 2.1 presents the frequency of earnings management by *true earnings* (miss, meet or beat) and *public scrutiny* (low or high). Figure 2.2 visualizes these results by *true earnings* in Panel A, splits them up for individuals classified as low and high DT in Panel B, and collapses true earnings by whether they meet expectations or not in Panel C. Consistent with the economic incentives implied by our game, the descriptive numbers in Panel A of Figure 2.2 suggest that managers' decisions to manage earnings vary strongly with true earnings. While missing the benchmark induces managers to manage earnings 85.6% of the time, the frequency is lower for meeting (10.7%) and beating (3.0%). These numbers seem to be largely in line with the Nash equilibrium prediction that managers truthfully report when they beat or meet a benchmark but misreport when missing a benchmark. Since our main interest is whether managers' decision to misreport benchmark-meeting earnings is moderated by

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[14] Responses to this manipulation check question are not influenced by the Dark Triad measure (p=0.84).

[15] The actual question was "Based upon the instructions you received before the start of the study, which state of the world [earnings] was the least likely to occur in this game with a probability of 25%?" with the option to choose DOWN, MIDDLE or UP [miss, meet or beat]. Responses to this question are neither influenced by the factor *dark triad* nor *public scrutiny* (p>0.51). Excluding participants who fail this manipulation check does not change our inferences.

our independent variables *public scrutiny* and *dark triad*, we redefine *meeting* as a binary variable that takes the value 1 when manager-participants' earnings exactly meet the benchmark and zero otherwise (i.e. when true earnings either miss or beat the benchmark).[16] Panel C of Figure 2.2 visualizes these results.

[16] As we collapse two categories our statistical models no longer distinguish between managers reporting to miss or beat the benchmark when true earnings meet expectations exactly. However, as predicted and reported below, we find that *public scrutiny* and *dark triad* do not influence managers' reporting decisions when earnings beat (vs. not beat) or miss (vs. not miss) the benchmark. Further, out of the 25 instances where managers misreport true earnings that meet expectations, participants choose to over-state earnings 21 times (84%). We include all 25 observations since managers reporting miss may be doing so strategically to send a signal about honesty, choosing to benefit the investor but harm themselves by not overreporting.

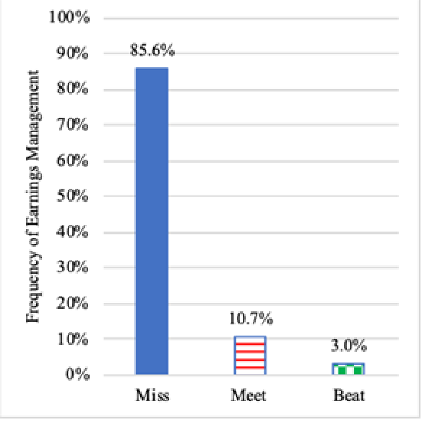
Table 2.1: Frequency of Earnings Management (%) by True Earnings and Public Scrutiny

	Public Scrutiny		
True Earnings	Low	High	Mean (sd)
Miss	0.853	0.859	0.856
	(0.36)	(0.35)	(0.35)
	n=156	*n*=156	*n*=312
Meet	0.085	0.128	0.107
	(0.28)	(0.34)	(0.31)
	n=117	*n*=117	*n*=234
Beat	0.026	0.034	0.030
	(0.16)	(0.18)	(0.17)
	n=117	*n*=117	*n*=234
Mean (sd)	0.374	0.392	
	(0.48)	(0.49)	
	n=390	*n*=390	

Table 2.1 represents the frequency of earnings management by experimental condition. *Frequency of earnings management* aggregates the frequency of issuing a report that is not identical to true earnings. *True earnings* is manipulated at three levels (miss, meet, beat) within-subjects and embodies the earnings signal that managers privately observe. *Public scrutiny* is manipulated between-subjects (low or high). In the condition with high public scrutiny, manager-participants were asked, prior to reporting earnings, to stand up for 10 seconds such that they are identifiable and visible to other investors in the room. With low public scrutiny they report anonymously.

Figure 2.2: Propensity to Manage Earnings by Sub-Samples

Panel A: Frequency of Earnings Management by True Earnings



Panel B: Frequency of Earnings Management by True Earnings, Public Scrutiny and Dark Triad

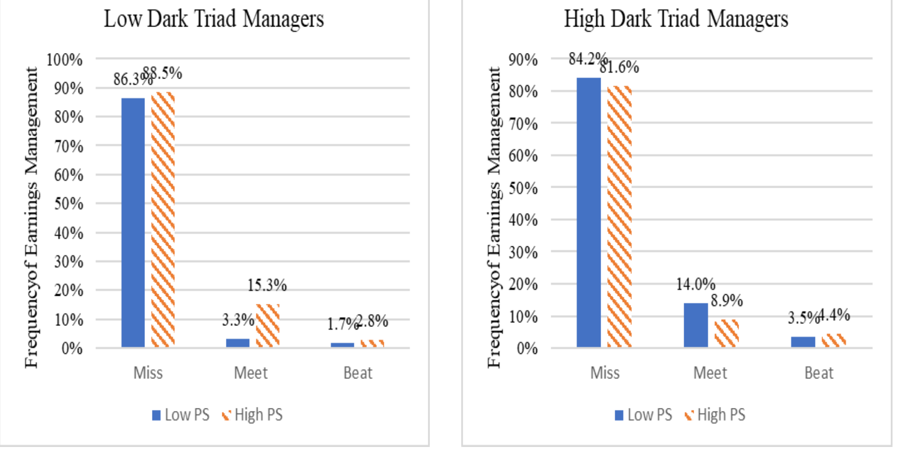
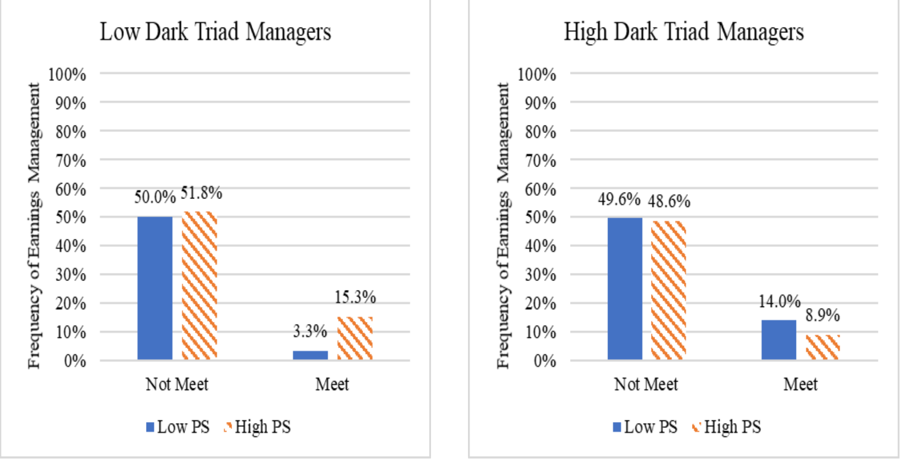


Figure 2.2 (continued)

Panel C: Frequency of Earnings Management by Meeting, Public Scrutiny and Dark Triad



The above figures graphically represent the frequency of earnings management by experimental condition. *Frequency of earnings management* aggregates the frequency of issuing a report that is not identical to true earnings. *True earnings* is manipulated at three levels (miss, meet, beat) within-subjects and embodies the earnings signal that managers privately observe. *Public scrutiny* is manipulated between-subjects (low or high). In the condition with high public scrutiny, manager-participants were asked, prior to reporting earnings, to stand up for 10 seconds such that they are identifiable and visible to other investors in the room. Participants in the condition with low public scrutiny reported anonymously. *Dark triad* is a measured variable and allocates participants to either condition depending on whether they scored at and above (high) or below the median (low) on the SD-3 questionnaire. *Meeting* collapses *true earnings* into a binary variable and indicates whether true earnings meet or do not meet (i.e. beat or miss) the benchmark.

2.4.3 Hypothesis Test Results

Our first hypothesis states that managers' propensity to misreport earnings increases with public scrutiny when meeting a benchmark, but less so when not meeting it. In order to test this hypothesis, we examine the interaction of *meeting* and *public scrutiny*. We estimate a mixed effects logistic regression for the propensity to manage earnings with the factors *meeting*, *public scrutiny*, and *dark* *triad*, and their interactions as independent variables, and include a random intercept for each participant.[17] Table 2.2 reports the frequency of misreporting by *meeting*, *public scrutiny* and *dark triad* type in Panel A. Panel B reports the regression results and Panel C shows planned simple effects tests.

[17] We use a Generalized Linear Model statistical analysis since our observations are dependent within subject. Mixed-effects models correct for these dependencies by treating our manipulations as fixed effects and estimating a random effect for each participant. The random effect parameter corrects for the effect of unmeasured noise from each participant (Verbeek, 2016, pp. 381–383; see also Elliott, Gale, and Hobson 2019; Hurley 2019; Majors 2016).

Table 2.2: Managers' Propensity to Manage Earnings – All Manager Types

Panel A: Frequency of Earnings Management (%) - Mean and Standard Deviation

Full Sample (n = 780)

Meeting/ Public Scrutiny	Low	High
Meet	0.085	0.128
	(0.28)	(0.34)
	n=117	n=117
Not Meet	0.498	0.505
	(0.50)	(0.50)
	n=273	n=273

Meeting/ Dark Triad (DT)	Low DT	High DT
Meet	0.098	0.118
	(0.30)	(0.32)
	n=132	n=102
Not Meet	0.510	0.492
	(0.50)	(0.50)
	n=308	n=238

Public Scrutiny/ Dark Triad (DT)	Low DT	High DT
Low	0.360	0.390
	0.48	0.49
	n=200	n=190
High	0.408	0.367
	0.49	0.48
	n=240	n=150

Sub-Samples

Low Dark Triad Managers (n = 440)

Meeting/ Public Scrutiny	Low	High
Meet	0.033	0.153
	0.18	0.36
	n=60	n=72
Not Meet	0.50	0.518
	0.50	0.50
	n=140	n=168

High Dark Triad Managers (n = 340)

Meeting/ Public Scrutiny	Low	High
Meet	0.140	0.089
	0.35	0.29
	n=57	n=45
Not Meet	0.496	0.486
	0.50	0.50
	n=133	n=105

Table 2.2 (continued)

Panel B: Mixed Effects Logistic Regression Model for Earnings Management

Independent Variable	Coefficient	Std. Error	z	p-Value
Intercept	0.00	0.17	0.00	1.00
Meeting	-1.8	0.42	-4.29	<0.01
Public Scrutiny (PS)	1.54	0.86	1.78	0.07
Dark Triad (DT)	-0.02	0.24	-0.06	0.95
Meeting x PS (**H1**)	-1.58	0.83	-1.92	0.03*
DT x PS	0.11	0.35	0.33	0.74
Meeting x DT	-1.57	0.85	-1.85	0.07
Meeting x PS x DT (**H2**)	-2.06	1.08	-1.90	0.03*

Panel C: Simple Effects Tests

Source	*df*	Chi-Square	p-Value
Effect of Public Scrutiny given Meet	1	1.80	0.09*
Effect of Public Scrutiny given Not Meet	1	0.02	0.90
Effect of Meeting given High Public Scrutiny	1	40.52	<0.01
Effect of Meeting given Low Public Scrutiny	1	36.57	<0.01

* indicates a one-tailed test given our directional prediction. This table presents a mixed effects logistic regression of our earnings management measure on all manipulations and their interactions. Panel A presents descriptive statistics for all combination of variables, Panel B shows the main results and Panel C reports the related simple effects tests. We define *meeting* as a binary variable that takes the value 1 when managers privately observe to meet a benchmark and zero otherwise. *Dark triad* is a measured variable and allocates participants to a condition depending on whether they scored above (high DT) or at and below the median (low DT) on the SD-3 questionnaire. *Public scrutiny* is manipulated between-subjects (low or high). In the condition with high public scrutiny, manager-participants were asked, prior to reporting earnings, to stand up for 10 seconds such that they are identifiable and visible to other investors in the room. Participants in the condition with low public scrutiny report anonymously. *Earnings management* is a binary variable that is equal to one when managers issue a report that is not identical to true earnings and zero otherwise.

Panel B of Table 2.2 shows that there is a main effect of meeting benchmarks ($ß=-1.80$, $z=-4.29$, $p<0.01$), and a marginally significant effect of public scrutiny ($ß=1.54$, $z=1.78$, $p=0.07$). Most importantly, both variables interact ($ß=-1.58$, $z=-1.92$, $p=0.03$ one-tailed) indicating that public scrutiny increases instances of misreporting when managers meet benchmarks exactly, but less so when not meeting them. The respective means in Panel A demonstrate that managers misreport earnings more frequently when they meet the benchmark and report under high ($\mu=12.8\%$) versus low ($\mu=8.5\%$) public scrutiny, and that the difference is marginally significant ($\chi^2=1.80$, $p=0.09$, one-tailed). On the other hand, when earnings do not meet expectations, reporting is not influenced by *public scrutiny* ($\chi^2=0.02$, $p=0.90$).[18] These results support our first hypothesis. Given that managers have no economic incentives to misreport earnings when meeting the benchmark, we conclude that the observed decrease in earnings quality occurs out of a desire to signal high quality earnings: misreporting to appear truthful.

In order to provide further support to the theorized process (Asay, Guggenmos, Kadous, Koonce, and Libby 2019), we test in H2 whether the interactive effect between *meeting* and *public scrutiny* differs for managers who score high or low on the DT measure of personality. Our second hypothesis states that managers' higher propensity to misreport when public scrutiny

[18] Public scrutiny does not influence the frequency of misreporting when considering the sub-samples of missing and beating benchmarks separately (both $p>0.67$). This gives support to our reasoning that the interactive effect between public scrutiny and meeting is not just driven by only one of these no-meeting conditions. Finding that public scrutiny does not reduce the high frequency of misreporting when earnings miss the benchmark is consistent with archival findings on the ambiguous effects of scrutiny on financial reporting quality (Kolev et al. 2008). Similarly, when we repeat the entire analysis with a dummy variable for either beating or missing instead of meeting, we find no significant interactive influence of this dummy variable and *public scrutiny* on reporting behavior, nor a three-way interaction for either of the two alternative models (all $p>0.15$).

increases and true earnings meet a benchmark, as opposed to not meeting it, is more pronounced for low than high DT managers. Consistent with this prediction, we find a significant three-way interaction (ß=-2.06, z=1.08, p=0.03, one-tailed, Panel B on Table 2.2). The means in Panel A of Table 2.2 support our prediction that low DT managers who meet a benchmark manage earnings more frequently when they are more strongly scrutinized (μ=15.3%) than if public scrutiny is low (μ=3.3%) and this contrast is significant ($\chi^2(1)=41.32$, $p<0.01$, one-tailed). For high DT managers, the propensity to manage earnings in the meeting condition decreases with public scrutiny from 14.0% to 8.9%, but not significantly ($\chi^2(1)=0.63$, $p=0.43$). Figure 2.2 visualizes these results graphically.[19] In order to further explore this interactive effect, we run separate analyses for the samples of managers classified as DT or not.

[19] We also find a marginally significant *meeting* x *DT* interaction (p=.07). The interaction is driven by two significant simple effects. Both high and low DT managers misreport less when true earnings meet (vs. not meet) the benchmark (both p<0.01, not tabulated), and the effect is more pronounced for low DT managers. This indicates that low DTs are more sensitive to changes in true earnings. Yet, there is no direct effect of the DT on misreporting whether true earnings meet the benchmark or not (both p>0.35). This latter results alleviates the concern that DT types differ in their overall understanding of the game's incentives as we find no differences across types.

Table 2.3: Low Dark Triad Managers' Propensity to Manage Earnings

Panel A: Frequency of Earnings Management (%)

	Public Scrutiny		
Meeting	Low	High	Mean (sd)
Meet	0.033	0.153	0.098
	(0.18)	(0.36)	(0.30)
	n = 60	*n* = 72	*n* = 132
Not Meet	0.500	0.518	0.510
	(0.50)	(0.50)	(0.50)
	n = 140	*n* = 168	*n* = 308
Mean (sd)	0.360	0.408	
	(0.48)	(0.49)	
	n = 200	*n* = 240	

Panel B: Mixed Effects Logistic Regression Model for Earnings Management

Independent Variable	Coefficient	Std. Error	z	*p*-Value
Intercept	0.00	0.17	0.00	1.00
Meeting	-3.37	0.74	-4.56	<0.01
Public Scrutiny	0.07	0.23	0.31	0.76
Meeting x Public Scrutiny	1.58	0.82	1.92	0.03*

Panel C: Simple Effects Tests

Source	*df*	Chi-Square	*p*-Value
Effect of Public Scrutiny given Meet	1	4.38	0.02
Effect of Public Scrutiny given Not Meet	1	0.10	0.75
Effect of Meeting given High Public Scrutiny	1	24.28	<0.01
Effect of Meeting given Low Public Scrutiny	1	20.77	<0.01

* indicates a one-tailed test given our directional prediction.

This table presents a mixed effects logistic regression of the probability that a low DT manager engages in earnings management. We use our measure of individuals' DT personality to create the sub-sample of low DT managers. Only individuals who score at or below the median value are included in this sub-sample test. *Earnings management* is a binary variable that is equal to one when managers issue a report that is not identical to true earnings and zero otherwise. *Public scrutiny* is manipulated between-subjects (low or high). In the condition with high public scrutiny, manager-participants were asked, prior to reporting earnings, to stand up for 10 seconds such that they are identifiable and visible to other investors in the room. Participants in the condition with low public scrutiny report anonymously. *Meeting* is a binary variable that takes the value 1 when managers privately observe to meet a benchmark and zero otherwise.

To better understand the nature of the three-way interaction, we restrict the sample to observations from low DT managers (n=440). As before, Table 2.3 shows that low DT managers' propensity to manage earnings increases with public scrutiny when receiving a meet signal (μ_{Low}=3.3%, μ_{High}=15.3%), but not significantly otherwise (μ_{Low}=50.0%, μ_{High}=51.8%). Panel B of Table 2.3 shows that the interaction between *meeting* and *public scrutiny* is significant (ß=1.58, z=1.92, p=0.03, one-tailed) and that there is a negative main effect of meeting the benchmark (ß=-3.37, z=-4.56, p<0.01).[20] Follow-up simple effects tests in Panel C demonstrate that the effect of public scrutiny materializes only when meeting an earnings benchmark ($\chi^2(1)$=4.38, p=0.02, one-tailed) and there is no effect when not meeting (beating or missing) the benchmark ($\chi^2(1)$=0.10, p=0.75).[21] Consistent with our theory, we do not find a significant interaction for the sample of high DT managers (z=-0.47, p=0.50, not tabulated).

In sum, we find strong support for our second hypothesis. Our results extend prior research on the diverse effects of public scrutiny on firm reporting (Kolev et al. 2008) and produce insights on the importance of managerial character traits for the quality of financial reports. We demonstrate that low DT managers are more likely to engage in earnings management when public scrutiny increases, especially when true earnings meet expectations exactly. These findings are somewhat counterintuitive in light of

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[20] Additional analysis shows that this main effect is driven by the high likelihood of misreporting in the miss condition. As expected from Table 2.1, misreporting is actually more likely in the meet than in the beat condition (p=0.03).

[21] We find similar results when considering the sub-samples of missing and beating benchmarks separately. When beating a benchmark, low DT managers are equally likely to manage earnings whether they report under low (μ=1.7%) or high (μ=2.8%) public scrutiny (p=0.67). Also, when missing the benchmark, there is no difference between the conditions with high (μ=88.5%) and low (μ=86.3%) public scrutiny (p=0.75).

the literature on personality types and unethical behavior, which generally finds that individuals scoring high on the Dark Triad measure of personality are more likely to engage in unethical behavior (Majors 2016; Wang 2017). Rather, we find that low DT managers are the ones misreporting as public scrutiny increases, although they do so in an effort to be perceived as honest.[22] In this situation, the callousness of high DT managers is actually beneficial to firms, since they better resist market scrutiny.[23] This increased resistance exemplifies one of the costs to employing low DT managers.[24]

2.4.4 Additional Analyses

Anticipated Acceptance of Earnings Reports

We predict that managers misreport when true earnings meet benchmarks because they are concerned investors will not believe a true report. Our

[22] If we consider only the situation where managers miss a benchmark (n=312), we find that high and low DT managers do not differ in their propensity to misreport (p=0.79). While this seems to contradict other authors' conjecture that high DT managers report opportunistically (Majors 2016), it is likely due to a ceiling effect. We created strong incentives to misreport in order to induce skepticism of financial reports, which is a crucial part of our setting and theory.

[23] Our theory and results suggest that high DT managers' lack of empathy makes them less susceptible to public scrutiny when meeting earnings. To examine the alternative explanation that high DT managers are equally attentive to investor skepticism, but simply do not care about other people's judgments, we measured participants' individual predisposition to expect, apprehend and distress over others' negative evaluations, as captured by the Brief Fear of Negative Evaluation scale (Leary 1983). Controlling for this variable in our main test does not change our inferences and the respective control variable does not reach significance (both p>0.46), supporting our theory.

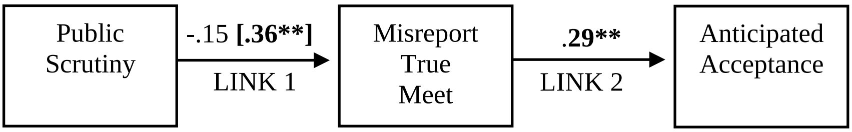
[24] Since our theory is based on the DT personality of managers, which is primarily characterized by a lack of empathy, we are interested in the shared variance of the three DT personality types, narcissism, psychopathy and Machiavellianism and not necessarily in any one of the three personality types. When we replace our DT measure in our analysis of H2 with a dummy variable for individuals classified at or above the median of the individual narcissism and Machiavellianism measures, we do not find a significant interaction between *meeting*, *public scrutiny* and the respective personality measure (both p>0.53). However, using psychopathy, we find marginal support for H2 (p=0.10). Since psychopaths are predominantly marked by the lack of interpersonal skills, we see this as additional support for our theory that high DT managers experience weaker image concerns (Mathieu et al. 2013) and are therefore less affected by our manipulation of public scrutiny.

support for this prediction borrows heavily from psychology insights on impression management (Leary 1986; Leary and Kowalski 1990). In order to give additional support to our theory, we measure managers' anticipated acceptance of their reporting choice. Prior literature argues that people have strong concerns about the impressions that their behavior creates in others (Leary and Kowalski 1990). We measure these self-presentational goals by asking manager-participants in each round whether they believe the investor they are paired with will classify their earnings report as truthful or deceptive.[25] Since participants answer *after* they have issued their earnings report each period, we interpret responses to this question as capturing anticipated acceptance, i.e. a manager's belief that investors will rate earnings as high quality and informative. We code the variable *anticipated acceptance* as equal to one when managers believe that investors will accept their report and zero otherwise.

[25] The actual question was decontextualized and asked "What do you think the GREEN player [investor] you are paired with this round will choose?" with the option to choose LEFT [accept] or RIGHT [reject].

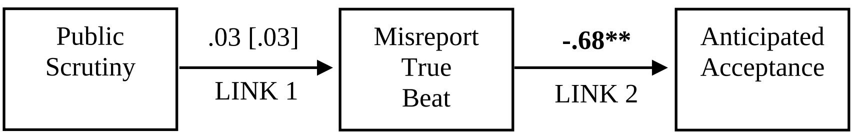
Figure 2.3: Two-Group Path Analysis for high [low] Dark Triad Managers' Anticipated Acceptance

Panel A: Misreporting True Meet Increases Anticipated Acceptance



Panel B: Misreporting True Miss Decreases Anticipated Acceptance

Panel C: Misreporting True Beat Decreases Anticipated Acceptance



Note: *p<0.10, **p<0.05, ***p<0.01, all p-values for predicted relations are one-tailed and bold.

This figure illustrates three path models that estimate the relation between our independent variables, dependent variable and secondary outcome variable for all manager-participants. In order to link the within-subjects variable *true earnings* with the between-subjects measurements, we collapse all observations by participant. The models estimate link 1 for high and low DT managers separately. Link 2 is constrained to be equal across DT groups. *Misreport true meet (miss/beat)* is the frequency of earnings management in response to true earnings that meet (miss/beat) the benchmark, *anticipated acceptance* captures each period participants' response to the question "What do you think the GREEN player [investor] you are paired with will choose?" within the respective sub-sample. The variable is equal to one if they state that investors will accept their report and zero otherwise. We aggregate these responses by participant and *true earnings*. *Public scrutiny* is a dummy variable equal to one when manager-participants stand up to identify themselves prior to issuing a report and zero otherwise. The model in Panel A has a good global fit with $\chi^2(3)=0.94$, $p=0.82$, $\chi^2/df=0.31$, Root Mean Square Error of Approximation (RMSEA)=0.00, Standardized Root Mean Square Residual (SRMR)=0.04. The model in Panel B has an acceptable global fit with χ^2 $(3)=4.40$, $p=0.22$,

χ^2/df=1.47, RMSEA=0.11, SRMR=0.09. The model in Panel C also has an acceptable global fit. Fit indices amount to $\chi^2(3)=4.46$, p=0.22, χ^2/df=1.49, RMSEA=0.11, SRMR=0.09. Acceptable fit is indicated by insignificance of χ^2(Kline 2015), χ^2/df below 2:1 (Tabachnik and Fidell, 2007), $RMSEA<0.07$ (Steiger 2007) and $SRMR<0.11$ (Kline 2015).

Figure 2.3 illustrates three different path models that we estimate with a Maximum likelihood distribution for our sub-sample of manager-participants. Due to dependency between observations, we collapse all observations by participant (n=78) and aggregate for each one the frequency of earnings management either in response to earnings that truly meet (*misreport true meet,* Panel A), truly miss (*misreport true miss,* Panel B) or truly beat expectations (*misreport true beat,* Panel C). Thus, we have one measure for each individual for each subsample of true earnings. We estimate each of the three models with a two-group path analysis on link 1 to differentiate the interactive effect of *public scrutiny* and *meeting* for high and low DT managers. Since we do not expect link 2 to differ across DT types, we constrain it to be equal. The global fit of all three models is within commonly accepted threshold levels.[26] The path coefficients are also in line with our predictions. The significant link 1 in Panel A replicates our three-way interaction from Table 2.2 by showing that only low DT managers are more likely to misreport a "meet" when they are more strongly scrutinized (ß=0.36, z=1.68, p=0.05, one-tailed), whereas high DT managers are unaffected (ß=-0.15, z=-0.56, p=0.58). We find that misreporting a meet increases *anticipated acceptance* as indicated by a significant

[26] The model in Panel A has a good global fit with $\chi^2(3)=0.94$, p=0.82, χ^2/df=0.31, Root Mean Square Error of Approximation (RMSEA)=0.00, Standardized Root Mean Square Residual (SRMR)=0.04. The model in Panel B has an acceptable global fit with χ^2 (3)=4.40, p=0.22, χ^2/df=1.47, RMSEA=0.11, SRMR=0.09. The model in Panel C also has an acceptable global fit. Fit indices amount to χ^2 (3)=4.46, p=0.22, χ^2/df=1.49, RMSEA=0.11, SRMR=0.09. Acceptable fit is indicated by insignificance of χ^2(Kline 2015), χ^2/df below 2:1 (Tabachnik and Fidell, 2007), $RMSEA<0.07$ (Steiger 2007) and $SRMR<0.11$ (Kline 2015).

link 2 (ß=0.29, z=1.74, p=0.04, one-tailed). The indirect effect of *public scrutiny* on *anticipated acceptance* is positive for low DT managers, but does not reach significance at conventional levels (ß=0.10, z=1.21, p=0.11, one-tailed), whereas it is negative and insignificant for high DT managers (ß=-0.05, z=-0.53, p=0.59).

The models below consider the effect of misreporting true miss (Panel B) and true beat earnings (Panel C). Both models produce insignificant coefficients on link 1 for both high and low DT managers (all p>0.66), which is in line with the interactive effect from H1. In addition, the indirect effect of public scrutiny on anticipated acceptance is also insignificant in both models (both p>0.66). Interestingly, we find that link 2, the effect from misreporting on anticipated acceptance, becomes negative in Panel B (ß =-.29, z=-1.82, p=0.03, one-tailed), and in Panel C (ß=-0.68, z=-1.93, p=0.03, one-tailed). This indicates that both types of managers anticipate skeptical market responses when they misreport missing or beating earnings (i.e. they report untruthfully to have met expectations). Note that misreporting earnings that beat expectations is not explained by theory and is possibly a mistake. We nevertheless present the results in Panel C for completeness and note that our results are robust to excluding these observations. In sum, we interpret the path model results as additional support for our theory that managers who privately observe a "meet" decrease the quality of their earnings because they want to be seen as truthful, even though this is economically irrational, and that they do so because they anticipate that their misreporting will be believed.

Investor Acceptance of Earnings Reports

As a final step, we consider investors' responses to managers' earnings reports. While our primary interest is managerial reporting behavior, we also consider investors' responses to assess whether managers can successfully signal higher earnings quality by decreasing the quality of earnings that meet expectations. In our experiment, investors assess earnings reports and either accept or reject them. We code the variable *investor acceptance* as equal to zero when investors reject and equal to one when they accept managers' earnings reports. We estimate a mixed-effects logistic regression with *investor acceptance* as the dependent variable, *meet report* and *public scrutiny* as independent variables while controlling for the occurrence of *earnings management*. *Meet report* is a variable equal to one when managers report that they met the benchmark and zero otherwise. All other variables are as defined previously. The results are presented in Table 2.4.[27]

[27] In the condition with high public scrutiny, we ask investors whether they know the person they are paired with outside of the laboratory since this condition is fully transparent about the identity of the manager. We exclude 20 observations from individuals who declare that the manager they are paired with in a particular round is either a friend or an acquaintance. Hence, we run all analyses for investors with 760 observations. Results are qualitatively similar when not omitting these observations.

Table 2.4: Investor Acceptance of Earnings Reports

Panel A: Investor Acceptance of Earnings Reports (%)

	Public Scrutiny		
Earnings Report	Low	High	Mean (sd)
Meet Report	0.358	0.330	0.345
	(0.48)	(0.47)	(0.48)
	n =243	*n* =227	*n* = 470
Not Meet Report	0.748	0.699	0.724
	(0.44)	(0.46)	(0.45)
	n = 147	*n* = 143	*n* =290
Mean (sd)	0.505	0.473	0.489
	(0.50)	(0.50)	(0.50)
	n = 390	*n* = 370	n=760

Panel B: Mixed Effects Logistic Regression Model for Investor Acceptance

Independent Variable	Coefficient	Std. Error	z	*p*-Value
Intercept	1.43	0.31	4.62	<0.01
Meet Report	-2.23	0.30	-7.41	<0.01
Public Scrutiny	-0.29	0.43	-0.68	0.50
Meet Report x Public Scrutiny	0.12	0.39	0.31	0.76
Earnings Management	0.10	0.21	0.46	0.64

This table reports investors' acceptance of the earnings reports they receive. *Investor acceptance* is equal to one when investors accept an earnings report and zero otherwise. *Earnings report* is the actual report that managers send to investors and therefore different from our manipulation of *true earnings* in the tables above. The binary variable *meet report* is equal to one when managers report to have met earnings, and zero otherwise. *Public scrutiny* is manipulated between-subjects (low or high). In the condition with high public scrutiny, manager-participants were asked, prior to reporting earnings, to stand up for 10s such that they are identifiable and visible to other investors in the room. Participants in the condition with low public scrutiny report anonymously. *Earnings management* is a binary variable that is equal to one when managers issue a report that is not identical to true earnings.

Panel A of Table 2.4 shows that investors accept fewer meet reports (μ=34.5%) than other reports (μ=72.4%). The mixed model in Panel B confirms that there is a significant main effect of *meet report* (ß=-2.23, z=-7.41, p<0.01), which indicates that investors reject meet reports, consistent with the Nash prediction. Additionally, there is no significant interaction between the variables *meet report* and *public scrutiny* (ß=0.12, z=0.31, p=0.76).[28] Our results suggest that environments of higher public scrutiny do not directly affect investors' response to firm disclosure (ß=-0.29, z=-0.68, p=0.50), so investors do not anticipate managers' self-presentational concerns. These results are obtained after controlling for the presence or absence of earnings management (ß =0.10, z=0.46, p=0.64). Hence, while our results suggest that managers are more likely to misreport benchmark-meeting earnings under increasing scrutiny, investors seem to be unaffected. In sum, our results are consistent with archival findings showing that higher scrutinization of firms affects managers in anticipation of potentially skeptical market responses (Baloria and Heese 2018; Kolev et al. 2008).

Alternative Explanations

The design of our experiment helps us to rule out several alternative explanations for our findings. First, we sought to minimize the concern that participants were confused by the design of our economic game by guiding participants through all possible outcomes of the game and providing them a print-out of the game matrix. In addition, our instructions contained several comprehension questions about the incentive structure of the game, which were answered correctly in 86.4% of the cases. We find that responses by the 14 manager-participants who misreport benchmark-meeting earnings at least once do not differ significantly from the remaining 64 participants' answers (t_{76}=0.23, p=0.81). Second, we seek to mitigate the concern that manager-participants rationally misreported these instances by designing the study such

[28] Controlling for investors' DT personality does not change inferences, nor does the coefficient itself reach significance (p=0.89).

that truthfully reporting earnings that meet expectations weakly dominates any alternative action. Further, misreporting when earnings meet expectations is also not an optimal strategy in the long term since participants face a different player each round, which is common knowledge to all participants. Third, it is unlikely that fairness concerns explain our results since the type of misreporting predicted and found in our study decreases not only one's own payoff, but also the other participant's payoff. Finally, given our interactive pattern of results, it is unlikely that high DT managers are simply better economists than low DT types since we do not compare behavior of these personality types directly. Instead, we provide evidence of how they report earnings under higher public scrutiny given a low probability event—earnings meeting market expectations exactly.

2.5 Conclusion

We find a novel and heretofore hidden incentive for earnings management that is a potential consequence of the market's increased skepticism of firms that just meet or narrowly beat analyst forecasts (Keung et al. 2010). Specifically, managers exposed to high public scrutiny are more likely to misreport earnings when they meet a benchmark exactly. Consistent with the notion that these reporting choices are motivated by self-presentational goals, we find that managers who lack empathy, as measured by their score on the Dark Triad, are less prone to this effect than their low DT counterparts. Further, we find that when managers misreport earnings that meet the benchmark exactly, they are more likely to perceive that investors will accept their report. Overall, the evidence is in line with our reasoning that certain managers decrease the quality of earnings to signal high quality earnings, or put differently, they misreport to appear truthful.

An important implication of our results is that regulators, who are currently still debating how much guidance to give firms with regard to their social media activity (Armstrong and Wigglesworth 2019), should not only consider how investors acquire and process accounting information that is disclosed on platforms such as Twitter. The use of this media also influences managerial behavior, and thus, financial reporting quality. Our results also contribute to the literature on earnings management incentives. We build on the notion that executives' personalities matter for firm reporting and offer a previously unexamined motive for earnings management to the literature. Similar to prior work that sees a CEO's personality and psychology as central to manipulative performance reporting (Asay 2018; Brown 2014; Majors 2016; Seybert 2010), we find that the DT personality is associated with earnings quality. However, in contrast to the existing literature that relates the DT to unethical behavior (Majors 2016; Murphy 2012; Wang 2017), we find that DT managers are *better* able to resist market scrutiny and act in accordance with their own and the firm's economic incentives. While some prior research finds that more empathic managers are willing to cheat in order to help their colleagues (Eskenazi, Hartmann, and Rietdijk 2016), we document that they misreport even at the detriment of others because they want appear honest. Thus, our study documents one of the downsides to hiring low DT managers. The lack of image concerns seems to help managers take decisions that are in their own and their organization's interest. This could be one of the reasons why narcissists, Machiavellians and psychopaths are still prevalent in the corporate business world despite their deceptive nature (Babiak et al. 2010; Judge et al. 2009).

Our study has limitations and provides opportunities for future research.

For example, we assign managers exogenously to a level of public scrutiny, but one may expect that managers are able to steer media attention or change their social media activity as they see fit. Notwithstanding, research indicates that disclosure policies are sticky since there are negative market consequences to, for example, eliminating conference calls (Bushee, Matsumoto, and Miller 2004), ceasing to issue CSR disclosure (Garavaglia, White, and Irwin 2017), or eliminating earnings guidance (Chen, Matsumoto, and Rajgopal 2011). We therefore expect that managers also face constraints in altering their social media activity or reducing their contact with the press. Second, financial reporting decisions are often the joint outcome of a group of executives, not a single individual's decision. Future research may examine whether self-presentational goals persist in a group context. Finally, our results speak to earnings management in general, but not one specific method. Other scholars may examine whether self-presentational concerns extend more directly to real earnings management decisions (Gunny 2010) or if they motivate other forms of disclosure, such as the issuance of opportunistic Non-GAAP earnings (Curtis, Mcvay, and Whipple 2014).

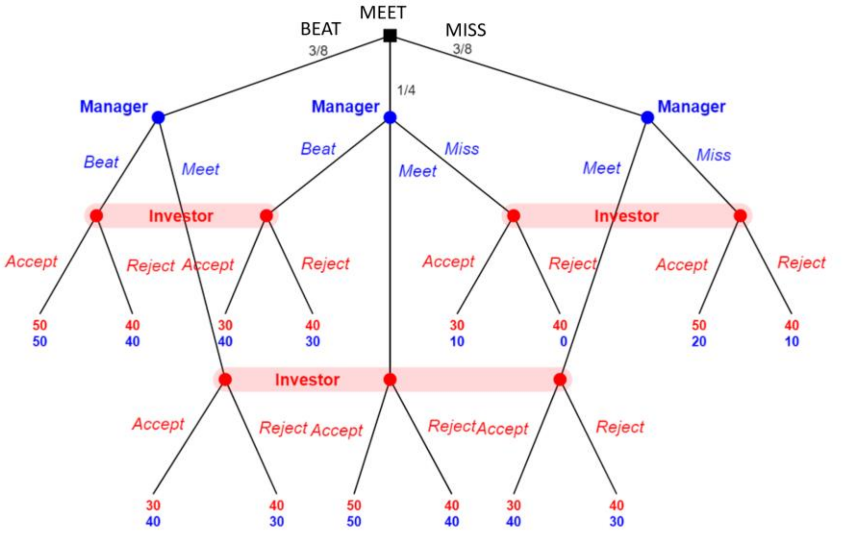
Appendix

Informed Nash Equilibrium Strategy for Investors and Managers

The game that we present in this paper is a variation of the signaling game (see e.g. Spence 1973) and adapted from a related study (Soraperra, Suvorov, van de Ven, and Villeval 2019). It is a two-player sequential game with incomplete information in extensive form, where we endow the first mover with a set of three possible actions. At the beginning of the game, the manager observes an exogenous private earnings signal (i.e. true earnings), which either beats a benchmark (i.e. Beat), meets it (i.e. Meet), or misses it (i.e. Miss). Beat occurs 3/8 of the time, Meet ¼ of the time, and Miss 3/8 of the time.

In the next step, the manager sends a report to the investor about the true earnings. The set of possible messages is conditioned on true earnings. Specifically, the manager cannot report Beat when true earnings miss expectations, nor can she report Miss when true earnings beat.

The investor only observes the earnings report (i.e. message), not the true earnings. He chooses then between two options. He either accepts the earnings report or he rejects the report. The sequential nature of the game is visualized in the figure below using Game Theory Explorer software (Savani and von Stengel 2014).



The solution to the game is straightforward. For true earnings that beat and meet expectations, the incentives of investors and managers are fully aligned (see also Figure 2.1 for ease of comparison). The manager reports Beat and Meet in accordance with true earnings since truthful reporting strictly dominates any deviation. Since a true Beat only occurs in these two sets, the investor always accepts when he hears Beat.

For true earnings that miss, the manager prefers to report a Meet but this affects the investor's payoff. Conditional on the investor accepting the report, the investor gets 30 when the manager plays Meet but 50 when she plays Miss. The incentives are misaligned. By means of backwards induction the investor knows that whenever he hears Meet, the probability of the report being truthful is 2/5:

P(Meet | Meet report) =

$$\frac{P(Meet)*P(Meet\ report\ |\ Meet)}{P(Meet\ report)} = \frac{\frac{1}{4}*1}{\frac{1}{4}*1+\frac{3}{8}*0+\frac{3}{8}*1} = \frac{2}{5}$$

If the investor always chooses Accept, his payoff would be 42.5:

Π_G = P(Beat) * U(Beat,Accept) + P(Meet) * U(Meet,Accept) + P(Miss) * U(Miss,Accept)

= 0.375 * 50 + 0.25 * 50 + 0.375 * 30 = 42.5

If the investor always chooses Reject, his payoff would be 43.75:

Π_G = P(Beat) * U(Beat,Accept) + P(Meet) * U(Meet,Reject) + P(Miss) * U(Meet,Reject)

= 0.375 * 50 + 0.25 * 40 + 0.375 * 40 = 43.75 > 42.5

Hence, the investor will always reject in response to Meet. Note that the investor cannot improve on his strategy by mixing the actions available to him. When he chooses Accept with probability 2/5 and Reject with probability 3/5 in response to Meet, his payoff is as follows:

Π_G = 0.375 * 50 + 0.25 * (2/5 * 50 + 3/5 * 40) + 0.375 * (2/5 * 30+ 3/5 * 40) = 43.25 < 43.75

The expected return for the manager from reporting true earnings Beat and Meet truthfully and Meet when true earnings miss expectations is as follows:

Π_B= 0.375 * 50 + 0.25 * 40 + 0.375 * 30 = 40

In sum, the pure strategy Nash equilibrium of this game consists of the manager always reporting the true earnings Beat and Meet truthfully. When the true earnings miss expectations, she reports Meet. The investor accepts all Beat reports but rejects all Meet reports.

Chapter 3

The Effect of Transaction Cost Unbundling on Investors' Reliance on Investment Research: Evidence from Experimental Asset Markets

3.1 Introduction

Investors face a multitude of costs when engaging in capital market transactions, including charges for trade execution and investment research (i.e., costly forecasts of asset value) (Collins and Fabozzi 1991; Fama 1991). While a large stream of literature examines how the level of these transaction costs generates market inefficiencies, recent regulatory changes have led to variations in the structure of transaction costs (Bhushan 1994; Mashruwala,

I am very grateful to Kees Camfferman, Eddy Cardinaels, Henry Eyring, Brian Gale, Anna Gold, Katlijn Haesebrouck, Jessen Hobson, Stephan Kramer, Frank Moers, Alexander Nezlobin, Mark Peecher, Erik Peek, Peter Pope, Susanne Preuss, Evelien Reusen, Sandra Schafhäutle, Wim Van der Stede, Marcel van Rinsum, David Veenman, Jacco Wielhouwer, Jacob Zureich and seminar participants at Bocconi University, Erasmus University Rotterdam, KU Leuven, London School of Economics, Maastricht University, Tilburg University, Vrije Universiteit Amsterdam and University of Amsterdam for helpful comments and suggestions.

Rajgopal, and Shevlin 2006; Ng, Rusticus, and Verdi 2008). Under current regulation, broker-dealers in the United States charge their clients with a bundled transaction fee that varies with individual trading volume. In Europe, broker-dealers charge separate (i.e., unbundled) fees for investment research and trade execution. In this study, I examine how transaction cost unbundling influences investors' attention to information costs and their subsequent reliance on costly forecasted information.

In January 2018, regulation "Markets in Financial Instruments Directive II" (MiFID II) came into effect in the European Economic Area, mandating broker-dealers to collect unbundled payments for investment research and trade execution from their clients (European Union 2014). Existing archival research and political debate on MiFID II primarily focus on how this regulatory change has decreased the supply of, and demand for, sell-side investment research (Fang, Hope, Huang, and Moldovan 2020; Guo and Mota 2020; Pope, Tamayo, and Wang 2020; Riding 2019). In order to fully evaluate the implications of this new policy, it is vital to understand the effect of transaction cost unbundling on investor use of information subsequent to obtaining it. First, the European Securities and Markets Authority (ESMA) justifies the recent innovation in payments, stating that it is the institution's aim to "increase transparency [and] better protect investors" (European Union 2014). Hence, this regulation means to reduce agency conflicts among investors and asset managers through cost transparency.[30] However it could also affect investors' processing of the information and lead to unintended

[30] More specifically, the provision of research in a bundle with other services may induce asset managers to trade excessively with a certain broker-dealer to ensure the continuous flow of research (Allen et al. 2019). MiFID II makes transaction costs fully transparent, allows investors to monitor asset managers and, thereby, reduces the potential for agency conflicts.

consequences. Because investors now are required to pay for each accessed research report, cost-of-advice research suggests they may over-rely on information (Gino 2008), such that the anticipated capital market benefits associated with transparency (i.e., higher returns through resolved agency conflicts) will not materialize fully.

Second, there is evidence suggesting that MiFID II diminished the informational efficiency of financial markets (Fang et al. 2020). This finding is surprising because since MiFID II, demand for investment research decreased, competition among research providers tightened and the quality of sell-side investment research increased (Guo and Mota 2020). To the best of my knowledge, prior research has not considered that cognitive biases may explain this finding. Building upon the notion that cognitive biases can affect aggregate market outcomes (Bloomfield, Libby, and Nelson 2003; Dietrich, Kachelmeier, Kleinmuntz, and Linsmeier 2001; Elliott, Hobson, and White 2015; Ganguly, Kagel, and Moser 1994), my findings offer one possible explanation for this outcome and further enhance our understanding of transaction cost unbundling and its impact on financial markets.

Especially retail investors, the agents targeted by MiFID II and focus of this study, are subject to substantial transaction costs (Barber and Odean 2013) and behavioral information processing biases (Brown et al. 2020; Elliott et al. 2015; Hales 2007; Kadous, Koonce, and Thayer 2012). Therefore, I examine how the structure of transaction costs interacts with their cost level.

I draw on transaction decoupling theory from the marketing literature, which combines insights from mental accounting theory with sunk cost literature, to develop my predictions (Gourville and Soman 1998; Soman and Gourville 2001). The central tenet of transaction decoupling is that when individuals enter a transaction, such as a forecast purchase, they allocate the cost of the transaction to a mental account that stays open until the transaction is finished (Thaler 1980, 1985). When there is a clearer link between these costs and the associated benefit, research finds that transaction costs exert a stronger influence on individuals' judgment and decision making (Soman and Gourville 2001). In my setting, with unbundled payments, investors can be certain of the initial transaction cost at the moment they receive the information item, yet they are still uncertain about the respective benefit of their purchase. Thus, they can direct their attention toward the information cost and link their expenses to the forecast (Soman and Gourville 2001). When investors face high expenses, they may experience regret over spending large sums of money on potentially unused advice (Gino 2008). My first hypothesis is that, in order to reduce these feelings, investors will integrate more costly information more strongly into their decision making (i.e., rely more on it) (Maines and McDaniel 2000). In contrast, with bundled payments, this effect is less likely to occur because investors' expenditures depend on future trading volume and are, therefore, uncertain. I then investigate how this stronger reliance on research affects patterns of investor learning. Since attention is a scarce resource (Blankespoor, DeHaan, and Marinovic 2020; Thayer 2011), I expect investors to pay relatively less attention to other value-relevant information that arises gradually during trading (e.g., market prices and posted offers). To the extent that prices reflect individual beliefs, higher reliance on the forecast thus implies that price errors decrease less strongly over time, which is my second hypothesis.

To test my hypotheses, I run 16 continuous double auction markets, where investors trade several one-period life certificates that pay a liquidating dividend. Investors receive two predictions of dividend value: a private information signal at the beginning of trading and a less noisy forecast midway through trading. I elicit individual dividend expectations before and after receipt of the forecast and measure the extent to which investors adjust their dividend estimates in the direction of the forecast, using the well-established measure weight of forecast (WOF) (Gino 2008; Kadous, Leiby, and Peecher 2013; Yaniv 2004).

My study employs a 2 x 2 +2 +2 partially nested between-markets research design. I manipulate whether participants pay for the forecast and execution of trades individually (unbundled payment) or a bundled fee that varies with individual trading volume (bundled payment). Also, I manipulate the level of transaction costs as lower or higher by varying the amount charged. This manipulation allows me to establish whether sunk cost effects are in play: I should observe stronger effects with higher costs. In addition, I include two partially nested manipulations to disentangle the multi-dimensional unbundling construct and test the robustness of my predictions.[31] In half of the markets with bundled payments, I inform participants about the relative proportion of information costs to execution costs (disclosure present), but not in the other half of those markets (disclosure absent). In half of the markets with unbundled payments, participants may choose to acquire the forecast (endogenous information acquisition), whereas in the other market half they receive the information by default (exogenous information acquisition). The experimental design purposefully deviates from the setting of

[31] Transaction cost unbundling is a multi-dimensional construct because it simultaneously varies three components of transaction costs. Unbundled payments are not only more certain than bundled payments, but they also transparently disclose to the investor what total cost they will incur. Further, investors can choose to acquire and pay for investment research with unbundled payments, but they do not have this choice with bundled payments.

interest by holding the content and quality of investment research constant across all conditions. It is this abstraction from reality that lets me examine whether part of the stronger market reactions to sell-side research observed after MiFID II (Fang et al. 2020; Guo and Mota 2020) is driven by aspects other than information quality.

The study results align with my prediction that transaction cost unbundling induces investors to rely more on investment research when they trade in markets with higher transaction costs. I find that transaction cost unbundling does not influence information usage to the same extent in markets with lower transaction costs, which is also consistent with theory. I find marginal support for my second hypothesis that this stronger reliance on investment research adversely affects investor learning. Specifically, in the higher cost condition with unbundled payments, absolute price errors do not decrease significantly over time, whereas with bundled payments they do. This result suggests that unbundled payments reduce investors' ability to integrate other value-relevant trading information into their judgments because they focus more strongly on the forecast.

Additional analyses further support my theoretical predictions and the robustness of my findings. First, I strengthen my theoretical claims that differences in payments, not cost transparency, cause my results. I show that the disclosure of higher cost proportions neither induces a sunk cost fallacy nor reduces investor learning. Second, I provide theory-consistent evidence that the information processing results I observe are attributable to regret-driven attention to costs rather than deeper processing of the information. Transaction cost unbundling generates more heterogeneous beliefs in markets with higher than lower transaction costs. Third, I check on the robustness of my results. Supplementary tests suggest that giving investors the choice to acquire higher cost information does not diminish their reliance on the forecast. Hence, I expect my results to generalize to a setting where investors have a choice to acquire investment research.

The findings of my study contribute to literature and practice. From an academic perspective, my findings speak most directly to the information cost literature (Ackert, Church, and Zhang 2018; Sunder 1992). Prior research in this field indicates that investors' use of information is influenced by a variety of information costs, such as processing costs (Bloomfield 2002; Grant 2020; Hobson 2011), access costs (Gale 2020), effort and time (Nelson and Tayler 2007; Thayer 2011), and direct monetary expenditures (Kachelmeier 1996). My research illustrates that investors' reliance on information is influenced by not only the level, but also the structure of these costs. More specifically, it indicates that investors place greater emphasis on costly forecasted information when payments are unbundled compared to bundled. These insights are troublesome in light of archival evidence on MiFID II showing that analyst reports have become more optimistic to curry favor with management (Lang, Pinto, and Sul 2019).

Furthermore, the results suggest that this shift in information use leads to an adverse aggregate market consequence in the form of diminished investor learning. This result extends prior accounting literature that documents patterns of investor learning (Bossaerts, Frydman, and Ledyard 2014; Chen, Francis, and Jiang 2005), relating these effects to investor sophistication (Bonner, Walther, and Young 2003), availability (Gong, Qu, and Tarrant 2020), and precision of public disclosure (Barron and Qu 2014). I add to this growing stream of research by showing that transaction cost unbundling reduces investors' ability to extract and use information that arises during trading. Prior research suggests that investors process costless accounting information more deeply when its core components are separated out and salient, which ultimately makes markets more efficient (Elliott et al. 2015). My study shows that these desirable effects of separating information do not hold universally. In a setting where information is costly, I find that regret-focused attention to information costs causes investors' biased information processing.

My study also aids regulators, as they consider the strengths and weaknesses of either system of payments (European Commission 2020; U.S. Securities and Exchange Commission 2019b). The ESMA introduced regulation MiFID II with the intention to make markets more transparent and to protect investors (European Union 2014). My study complements existing archival research that raises concerns about the efficacy of transaction cost unbundling at reaching these goals. Fang et al. (2020) find that both the accuracy of and market reactions to analyst forecast revisions have increased post-MiFID II but, somewhat surprisingly, the informational efficiency of markets has decreased. These dynamics are consistent with the theory set forth in this study that transaction cost unbundling induces investors to rely on investment research to a greater extent, simply because the investors pay a considerable and explicit amount for it.

One implication of my findings is that regulators should consider alternative payment systems which allow for cost transparency and avoid the documented challenges associated with transaction cost unbundling. Capitalizing on the comparative advantage of experimental research to disentangle compound constructs (Kachelmeier and King 2002; Libby, Bloomfield, and Nelson 2002), I examine an alternative form of charging investors that does not yet exist in the real world. Mandatory transaction cost disclosure does not lead to comparable effects while simultaneously creating full cost transparency. Hence, I encourage regulators to contemplate the adoption of disclosure-oriented policies as a cost-effective alternative to transaction cost unbundling.

3.2 Literature Review and Hypotheses Development

3.2.1 Background

Transaction costs constitute a market friction; they constrain informed trades that are necessary for markets to reach informational efficiency (Bartram and Grinblatt 2021; Diamond and Verrecchia 1987). As a result, several market anomalies have been attributed to these costs, such as limited arbitrage (Bernard 1989) and the existence of post-earnings announcement drifts (Bhushan 1994; Ng et al. 2008). Transaction costs particularly affect small investors because a substantial proportion of their returns is absorbed by the fees they pay (Barber and Odean 2000; Lesmond, Ogden, and Trzcinka 1999).[32] In relative terms, small investors face higher transaction costs than institutional investors, which places them at a disadvantage (Barber and Odean 2013). In order to reduce the impact of these expenses on investment portfolios and markets, regulatory efforts have long focused on making investors aware of these costs by issuing educational materials (e.g. U.S. Securities and Exchange Commission 2019a).

Another major attempt at increasing the transparency of transaction costs was made in January 2018 when MiFID II became effective in the European

[32] In addition to research reports from their broker-dealers, non-professional investors also may purchase investment research from independent agencies or investment platforms, such as SeekingAlpha. With the introduction of MiFID II, new investment apps targeting non-professional investors have become available (see e.g. Research Tree Ltd 2020).

Union. This directive requires broker-dealers to unbundle the cost of investment research from other costs, such as trade execution and advisory services. That is, broker-dealers now charge two separate prices. Transaction cost unbundling replaces the historical practice of bundling research and execution services into a variable fee dependent on trading volume, also known as soft dollar arrangements (Edelen, Evans, and Kadlec 2012).[33] Archival research finds that market participants are not willing to pay explicitly for investment research. This lower demand for investment research lead to a significant drop in analyst coverage and spurred competition among remaining research providers (Pope et al. 2020). As a consequence, the quality of sell-side research and market reactions to analyst forecast revisions increased (Fang et al. 2020; Guo and Mota 2020). In the following, I develop theory that part of these stronger market reactions occur because these recent innovations in payments have changed how investors use investment research.

3.2.2 Theory

Reliance on Investment Research

A substantial proportion of broker-dealers' charges to investors covers information cost, or the expenditures individuals incur to become informed (Grossman and Stiglitz 1976). According to prior research, information costs affect individuals' decision to acquire information (Ackert et al. 2018; Sunder 1992), their subsequent use of it (Bastardi and Shafir 1998; Nelson and Tayler 2007), and, on a market level, their capacity to aggregate diverse information into prices (Copeland and Friedman 1991). While prior studies consider non-

[33] It is noteworthy that under hard dollar arrangements (i.e., unbundled transaction costs), expenses on investment research no longer vary with trading volume and effectively become a fixed cost. Notwithstanding, I use the term "transaction costs" throughout the paper to refer to both investment research and trade execution costs. Current regulation in the United States follows a system of bundled transaction costs and classifies investment research accordingly.

monetary information costs, such as effort or time spent acquiring information (Bloomfield 2002; Gale 2020; Thayer 2011), my research examines how monetary information costs influence investor judgment in a market setting.

Conventional economic theory holds that rational individuals base the decision to engage or to not engage in a future transaction exclusively on the expected incremental transaction costs and benefits (Thaler 1980; Thaler 1999). Yet, much of previous research shows that individuals commonly violate this fundamental economic principle and consider historic, nonrecoverable costs in their decision making, an effect commonly known as the sunk cost fallacy (Arkes and Blumer 1985; Brockner 1992; Thaler 1980). Even financially sophisticated investors are subject to this bias (Tan and Yates 1995), and evidence supports that it affects financial markets (Barberis and Huang 2001). With respect to the origins of the sunk cost fallacy, different research fields offer unique explanations. While economic research attributes the sunk cost fallacy to individuals' tendency to seek risks when losing money (i.e., they are loss-averse) (Kahneman and Tversky 1979; Thaler 1980; Whyte 1986), psychology research argues that individuals anticipate regret over the acquisition of forward-looking information and seek regret-minimizing strategies (Acker 1997; Zeelenberg, Beattie, Van Der Pligt, and De Vries 1996; Zeelenberg and Pieters 2007). Importantly, both explanations establish that historical costs affect judgment only when individuals pay attention to them (Gourville and Soman 1998).

The sunk cost fallacy not only impacts decision making, but also influences how investors evaluate and weigh information, a process known as reliance (Maines and McDaniel 2000). In the context of advice taking, research shows that individuals more heavily weigh advice from others when it is costly compared to when it is free. Stronger reliance on more costly advice is consistent with the notion that investors attempt to "avoid the regret of wasting money on unused advice" (Gino 2008). Note that this reasoning does not hinge on the assumption that investors make better use of the advice or engage in deeper processing. Instead, the literature suggests that higher information costs imply a stronger, but rather unstructured and regret-driven reliance on information cues while disregarding the decision usefulness of the information (Bastardi and Shafir 1998).

To fully understand how investors respond to information costs, it is important to consider how these costs are conveyed to them (Elliott et al. 2015; Hodge, Kennedy, and Maines 2004; Maines and McDaniel 2000). Therefore, I exploit transaction decoupling theory from the marketing literature to examine how transaction cost unbundling influences investor reliance on costly investment research (Soman and Gourville 2001). The literature combines insights from the sunk cost literature with mental accounting theory (Thaler 1980; Thaler 1999) to show that individuals are particularly likely to fall prey to a sunk cost effect if they place the cost of an economic transaction in the same mental account as the subsequent transaction (i.e., they are coupled) (Gourville and Soman 1998; Prelec and Loewenstein 1998; Van Dijk and Zeelenberg 2003).[34] Underlying this theory is a clear, one-to-one connection between costs and their related benefit, which triggers a stronger attention to these costs (Soman and Gourville 2001). In an

[34] Marketing literature shows that such tight coupling occurs, for example, when customers pay for products and services in cash. Yet, when consumers finance their purchases by credit card, the moment of purchase and payment are decoupled, which makes the moment of purchase more enjoyable because it minimizes thoughts of payment (Prelec and Loewenstein, 1998).

investment context, such tight coupling could occur when investors are charged with an explicit (i.e., unbundled) fee for investment research; investors know with full certainty how much they are paying and can thus immediately allocate these costs to the same mental account as the information item. Since bundled transaction costs depend on uncertain future trading volume, it is less likely that information costs impact decision making at the moment of purchase, even if investors expect on average similar expenditures. Hence, I expect that transaction cost unbundling increases reliance on investment research.

These effects are likely to differ with the level of transaction costs. As discussed above, more costly advice induces anticipatory feelings of regret over spending money on unused advice (Gino 2008). With unbundled transaction costs, investors are better able to associate these feelings with the information item. Accordingly, I expect transaction cost unbundling to interact with the level of transactions costs, such that transaction cost unbundling increases investors' reliance on investment research more strongly as transaction costs increase over a reasonable range.

> **H1:** T*ransaction cost unbundling increases reliance on investment research more strongly with higher costs.*

Investor Learning

My first hypothesis predicts that investors' reliance on information is driven by the structure and level of transaction costs. Stronger reliance on information may benefit investors that do not use the information sufficiently in the first place, but it likely harms investors that use the information adequately prior to the regulatory change. The latter may overrely on the information. These countervailing effects do not allow for a statement about their desirability. Yet, it is possible that a shift in attention affects investors' use of other information disseminated in competitive markets (Sunder 1995). To illustrate, financial traders may infer value-relevant information from market prices (Gong, Qu, and Tarrant 2017), posted offers and trade disclosure (Bloomfield and O'Hara 1999; DeJong, Forsythe, Lundholm, and Watts 1991), and the timing of trades (Camerer and Weigelt 1991). Appropriately weighting each of these information signals seems a daunting task that takes time and experience (Ackert, Church, and Zhang 2004).

Building on the notion that attention is a scarce resource due to working memory constraints, prior literature identifies trade-offs in investor use of unique pieces of information (Brown, Gale, and Grant 2020; Peng and Xiong 2006; Thayer 2011). It is intuitive that a stronger reliance on investment research also impacts how investors use other value-relevant market information. Specifically, investors may neglect the information contained in market prices, posted bids, and asks. Unlike investment research, this market information arises endogenously over time and becomes increasingly accurate as investors engage in trades that, at least partially, reveal private information (Copeland and Friedman 1991). Hence, I expect that the hypothesized stronger reliance on investment research in markets with unbundled and higher transaction costs lessens investors' ability to gradually reduce price errors within a trading period (i.e., learning effects are weakened).

H2: *Transaction cost unbundling lowers investors' ability to gradually reduce price errors within a trading period more strongly with higher transaction costs.*

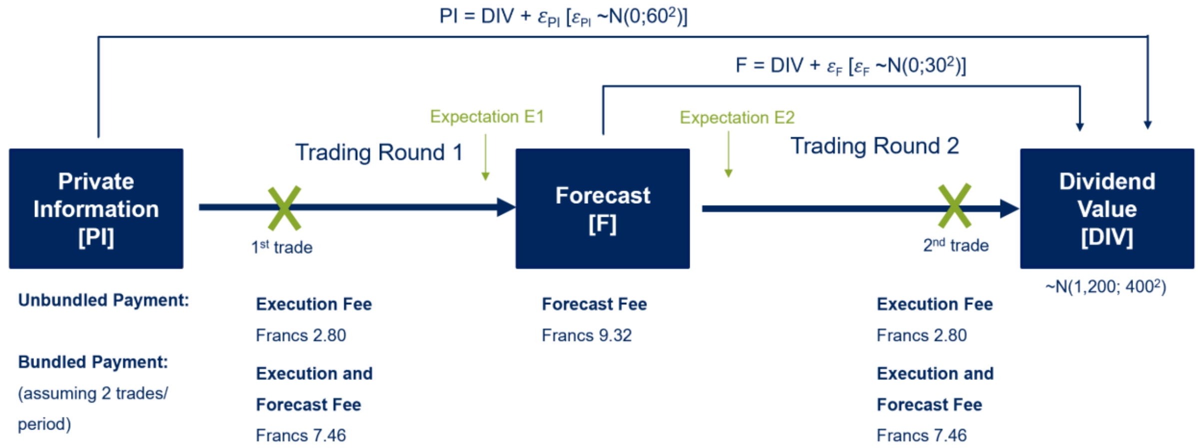
At first glance, the prediction in H2 may seem somewhat mechanical in light of H1. Indeed, finding that investors attach more weight to investment research implies that they will attach relatively less weight to other information. Notwithstanding, the examination of this prediction is theoretically interesting because investor learning effects arise at the aggregate market level. According to prior research, some cognitive biases at the individual level may not persist in a market setting, for example, because competitive forces move the market to equilibrium (Ganguly et al. 1994) or because investors adjust their trading behavior (Kachelmeier 1996). Thus, examining H2 illustrates how the effect predicted in H1 persists at the aggregate market level.

3.3 Methodology

3.3.1 Market Design

In order to test my hypotheses, I run 16 continuous double auction markets programmed in z-tree (Fischbacher 2007). Participants, in groups of five to eight, trade ten certificates, one at a time, for two consecutive trading rounds of 90 seconds each. Each certificate pays a liquidating dividend at the end of the second trading round. Trades are denominated in the fictional currency Francs (FR) and participants start with an endowment of zero. They may borrow unlimited certificates and cash to purchase or short-sell certificates (Elliott, Gale, and Hobson 2019; Elliott et al. 2015). I use the term "trading period" to refer to the 180 seconds during which participants trade a single certificate and the term "market" to describe each set of ten trading periods. Figure 3.1 visualizes the timeline of a single trading period.

Figure 3.1: Timeline of one Trading Period



Notes: Figure 3.1 graphically depicts the timeline of a single trading period. At the beginning, each participant receives a private information signal that predicts the dividend value with noise. After 90 seconds, participants can receive the forecast, which predicts the dividend value more accurately than the private information signal. Participants are not informed about the exact distribution of error terms. Before and after the issuance of the forecast, participants give their best estimate of dividend value. Cash and certificate balances carry forward from the first to the second trading round, but not to the next trading period. At the end of the second trading round, the dividend value is revealed and participants are charged with transaction fees. In the condition with unbundled payments, participants pay a fixed amount for the forecast and a variable fee for the execution of their trades. In the condition with bundled payments, they pay a fee that varies with individual trading volume. I conduct all markets with unbundled payments and exogenous information acquisition first and, based on the average trading volume per cost condition, I elicit cost parameters as visualized in Figure 3.3. The cost parameters at the bottom of the screen refer to the higher cost condition. If participants traded on average two times, a bundled fee of FR 7.46 would keep total expenditures across unbundling conditions equal.

My experimental design resembles the informational structure used in Verrecchia's (1982) theoretical security market model At the beginning of the first trading round, participants acting as investors receive a private information signal. The private information signal, PI, helps participants to predict dividend values, where PI = DIV + ε_{PI}, and ε_{PI} is an independently and normally distributed random variable with a mean of zero and standard deviation of 60. Participants can infer others' private information from market prices during the trading round. At the end of the first trading round, participants may acquire a dividend forecast F, where F= DIV + ε_F and ε_F is independently and normally distributed with a mean of zero and standard deviation of 30. Thus, all information signals offer an unbiased prediction of the dividend but differ in their precision. Dividend values, DIV, are determined by drawing from a normal distribution with a mean of 1,200 and a standard deviation of 400. Operationalizing investment research with a dividend forecast is akin to the type of ancillary services the European Union is targeting with MiFID II (European Union 2014). Actual dividend values are revealed to participants at the end of each period, before trading of the next certificate starts.[35]

Participants are informed about the process that generates dividends and information signals, but the distribution used to generate the noise terms ε_{PI} and ε_F is not disclosed. Instead, participants can try to infer these values from realized dividends and predictions. Each participant sees a total of 22 such values: a set of ten historic values in the instructions, two in the practice session, and ten in the main session. All information values were determined

[35] Certificates and cash balances carry forward from the first to the second trading round, but not to the next trading period. Participants are not informed of the total number of trading periods to avoid end-of-game effects.

prior to the study and kept constant across markets to enhance comparability (Ackert et al. 2018; Cason and Friedman 1996). Table 3.1 presents the sequence of realized dividend values and forecasts used in my study.[36]

[36] Dividend values range from 541 to 1,803 with an average of 1,198.55 and a standard deviation of 304.44. Forecasts range from 459 to 1,785 and their unsigned error has a mean of -1.14 and a standard deviation of 40.37. The number of private signals varies with the number of traders in a market, from five to eight. The eight noise terms of private signals have a mean (standard deviation) of 7.73 (62.13), 0.27 (55.67), 1.41 (69.56), -7.45 (71.64), -5.00 (65.04), -2.41 (64.57), 16.00 (54.39), and 1.82 (55.36). None of these random parameters differs significantly from their theoretical distribution (all $p > .10$).

Table 3.1: Realized Values of Dividend and Forecast

Period	Dividend	Forecast	Forecast - Dividend	Forecast Error (%)
I	877	816	-61	-6.96%
I	541	459	-82	-15.16%
I	1324	1287	-37	-2.79%
I	994	1011	17	1.71%
I	1613	1624	11	0.68%
I	1399	1379	-20	-1.43%
I	1098	1129	31	2.82%
I	1307	1355	48	3.67%
I	1803	1785	-18	-1.00%
I	1079	1115	36	3.34%
P	1482	1502	20	1.35%
P	1224	1188	-36	-2.94%
1	717	671	-46	-6.42%
2	1200	1160	-40	-3.33%
3	1237	1279	42	3.40%
4	1724	1674	-50	-2.90%
5	968	995	27	2.79%
6	1071	1108	37	3.45%
7	1088	1087	-1	-0.09%
8	1419	1477	58	4.09%
9	1099	1139	40	3.64%
10	1104	1103	-1	-0.09%
Average	1198.55	1197.41	-1.14	-0.55%
Std. Dev.	304.44	315.24	40.37	4.63%

Notes: Table 3.1 presents descriptive statistics about dividend realizations and the respective forecasts. Within each market, participants receive a total of 22 forecasts and dividend realizations that may help them to estimate the accuracy of the forecast-generating process. Ten of these values are conveyed to participants in the instructions (I), two more during the practice period (P), and one more during each trading period (1-10).

Before and after the receipt of the forecast, participants give their best estimate of dividend value. The two-round setting allows me to focus on individual belief revisions in response to the forecast and filter out idiosyncratic noise (see also Barron and Qu 2014).

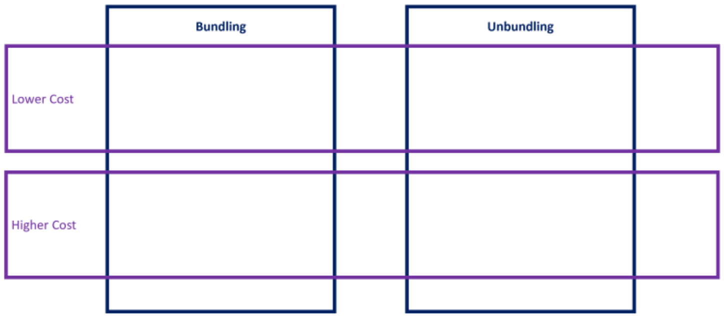
All information signals are independently drawn and predict the dividend with noise, which means the markets are characterized by aggregate uncertainty (Lundholm 1991). Prior research shows that, unlike markets with perfect information, these markets disseminate information more slowly and prices may not converge to full efficiency (Forsythe and Lundholm 1990; Lundholm 1991; Plott and Sunder 1988). Additionally, Verrecchia (1982) shows in his model that traders in these markets have incentives to acquire and use costly information, which makes this setting advantageous to test for differences in investor reliance on investment research.

3.3.2 Experimental Design and Cost Parameters

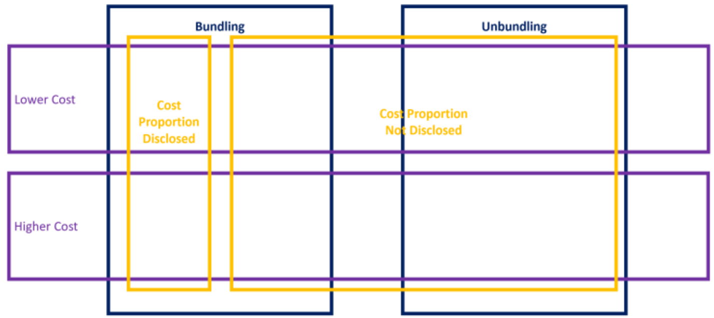
My experiment has a partially crossed 2 x 2 +2 +2 between-markets design. I use the 2 x 2 (unbundling x cost) part to test my hypotheses and the +2 partially nested disclosure conditions to provide a cleaner test of my theoretical predictions. I use the +2 information acquisition conditions in additional analyses at the end of the study to test the robustness of my findings. Figure 3.2 visualizes the experimental design graphically, and Table 3.2 presents the 16 experimental markets conducted.

Figure 3.2: Partially Crossed Research Design

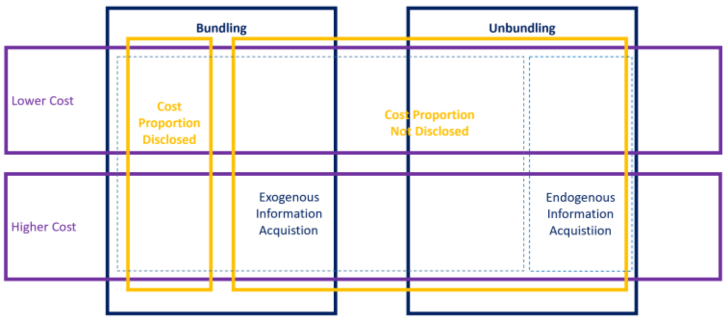
Panel A: 2 x 2 Part of Research Design



Panel B: Additional +2 Research Design



Panel C: 2 x 2 +2 +2 Research Design



Notes: see next page.

Notes: Figure 3.2 graphically illustrates the partially crossed 2 x 2 +2 +2 between-markets research design of this study. The 2 x 2 part (Panel A) refers to the fully crossed factors unbundling and cost. In the condition with unbundled payments, participants are charged with a fixed fee for the forecast and a variable fee for the execution of trades. In the condition with bundled payments, participants pay a fee that varies with trading volume and covers both items. Cost is manipulated as lower or higher. In the higher cost condition, the unbundled fees are four times higher than in the lower cost condition. Bundled fees are elicited based on the average trading volume in the unbundled condition with exogenous information acquisition (see Figure 3.3). Disclosure (Panel B) manipulates whether participants have full transparency about how their fees are allocated to trade execution and the forecast (disclosure present) or not (disclosure absent). Note that the condition with unbundled payments always discloses this proportion by default. Information acquisition (Panel C) manipulates whether participants receive the forecast automatically (exogenous information acquisition) or whether they need to choose to acquire the costly forecast (endogenous information acquisition).

Table 3.2: Summary of Experimental Market Design

Market Number	Payment Type	Cost Level	Information Acquisition	Cost Proportion Disclosure
1-2	Unbundled	Lower	Exogenous	Yes
3-4	Unbundled	Higher	Exogenous	Yes
5-6	Bundled	Lower	Exogenous	No
7-8	Bundled	Higher	Exogenous	No
9-10	Bundled	Lower	Exogenous	Yes
11-12	Bundled	Higher	Exogenous	Yes
13-14	Unbundled	Lower	Endogenous	Yes
15-16	Unbundled	Higher	Endogenous	Yes

Notes: Table 3.2 presents the 16 markets conducted with their respective manipulations. The payment type is manipulated as unbundled or bundled. In the condition with unbundled payments, participants are charged with an explicit forecast fee and a variable fee that covers the execution of their trades. With bundled payments, participants pay a bundled fee that covers both items. Cost level is manipulated as lower or higher. In the higher cost condition, the unbundled fees are four times higher than in the lower cost condition. Bundled fees are elicited based on the average trading volume in the unbundled condition with exogenous information acquisition (see Figure 3.3). Information acquisition manipulates whether participants receive the forecast by default (exogenous information acquisition) or they need to choose to acquire the costly forecast (endogenous information acquisition). Since prior to MiFID II, all investors had access to their broker-dealers' research reports, participants receive the information item in the condition with bundled payments by default. Disclosure manipulates whether participants have full transparency about how their fees are allocated to trade execution and the forecast (disclosure present) or not (disclosure absent). Note that the condition with unbundled payments always discloses this proportion by default.

Participants acting as investors in my markets are required to pay for investment research and trade executions. I vary four properties of these payments between markets. First, I manipulate transaction cost unbundling by asking participants to pay an explicit (i.e., unbundled) amount of money for the forecast ("forecast fee") and a variable fee for each trade ("execution fee") or by charging them a variable (i.e., bundled) fee per trade that covers both items. In order to keep total expenditures across conditions equal, I run all markets with unbundled payments and exogenous information acquisition first. Based on the trading volume within these markets, I calculate cost parameters for the experimental condition with bundled payments, as visualized at the bottom of Figure 3.1.[37]

I then manipulate the level of transaction costs using a compound manipulation: I vary the execution and forecast fees simultaneously. Varying only the forecast fee would change the proportion of forecast to execution fees across cost conditions and, thus, may alter the relative importance the participants attach to the forecast. Since my study is interested in the effect of unbundling given a certain level of transaction costs, it is vital to keep the relative proportion of fees across cost conditions constant. Figure 3.3 presents the actual parameters used.

[37] Note that total expenditures could still differ across conditions if trading volume changes in the markets with bundled payments. Yet, the important thing is that, conditional on the absence of changes in trading volume, participants will incur the same expenditures across unbundling conditions. I find that trading volume does not differ across markets with bundled and unbundled payments ($p>.10$).

Figure 3.3: Cost Parameters Elicited by Experimental Condition

Unbundled Payment	Bundled Payment
Lower Cost Condition	**Lower Cost Condition**
Forecast Fee = 2.33 Execution Fee = 0.70 per trade	Bundled Fee = 5.26/4.18 = 1.26 per trade
Average Trading Volume: 4.18 per period	Cost Proportion: Forecast Fee = 0.56 per trade, Execution Fee = 0.70 per trade
Average Fees = 2.33 + (4.18 * 0.70) = 5.26	Average Fees = 4.18 * 1.26 = 5.26
Higher Cost Condition	**Higher Cost Condition**
Forecast Fee = 9.32 Execution Fee = 2.80 per trade	Bundled Fee = 17.25/2.83 = 6.09 per trade
Average Trading Volume: 2.83 per period	Cost Proportion: Forecast Fee = 3.29 per trade Execution Fee = 2.80 per trade
Average Fees = 9.32 + (2.83 * 2.80) = 17.25	Average Fees = 2.83* 6.09 = 17.25

Notes: Figure 3.3 presents the actual cost parameters used. In the condition with unbundled payments, I set the values such that the fees are four times higher in the condition with higher costs. In order to keep total expenditures across conditions equal, I calculate the bundled fee based on the average trading volume in markets with unbundled payments and exogenous information acquisition. Cost proportions are only disclosed in the condition where disclosure is present. The proportion keeps the execution fee constant within a cost condition, and the residual between this fee and the bundled fee is disclosed as the forecast fee.

I manipulate cost to take two levels – lower and higher. In the lower cost condition with unbundled payments, participants pay FR 0.70 per trade in execution fees and FR 2.33 per trading period for the forecast. In the higher cost condition, the cost parameters are multiplied by a factor of four, such that the execution fee equals FR 2.80 and the forecast fee equals FR 9.32. Note that I purposefully use decimal numbers to ensure that information processing difficulty does not vary across experimental conditions. Figure 3.3 shows that trading volume in the unbundled/lower cost condition is on average 4.18 trades per period, leading to a bundled fee of FR 1.26 per trade. In the higher cost condition, participants engage in 2.83 transactions per period, resulting in a bundled fee of FR 6.09 per trade.[38]

Next, I manipulate the partially nested factor disclosure within the bundled condition. The primary purpose of including this variable is to disentangle the compound unbundling construct. That is, cost unbundling not only creates certainty for investors about how much they are going to pay, but also discloses transparently the part of their expenditures allocated to the forecast. Therefore, half of the markets with bundled payments transparently disclose the proportion of fees allocated to the forecast and the proportion for trade executions.[39] In the condition without disclosure, this proportion is not revealed.

Information acquisition varies within the unbundled condition: investors either have the opportunity to purchase the costly forecast at the end of the first trading round or they are automatically provided with (and charged for) the forecast.[40] This additional treatment addresses the concern that unobservable participant characteristics determine their choice to acquire the forecast (or not), which may limit the generalizability of my findings. While

[38] To ensure that the cost parameters in the unbundled condition are sufficiently high to provoke feelings of regret, but not too high to discourage trading, I ran a pilot study with unbundled and high transaction costs first. The pilot comprises of two markets and 13 participants. Since trading volume was positive in all trading periods, I use the same cost parameters in the actual experiment.

[39] Since the execution fee does not vary across unbundling conditions, this proportion simply discloses the variable execution fee, whereas the residual difference between the bundled fee and the execution fee constitutes the proportional forecast fee (see Figure 3.3). Note that unbundled payments always disclose this proportion automatically.

[40] Prior to MiFID II, all investors had access to their broker-dealers' research reports. Hence, I do not vary information acquisition within the bundled conditions.

the choice to acquire costly information resembles real-world procedures, it creates self-selection concerns in the experiment. Therefore, I report results from the unbundled condition with choice separately in additional analyses at the end of the paper.

In order to increase the strength of my manipulations, all participants see a factual reminder of the transaction cost structure when receiving the forecast in round 2. The actual fees incurred also are presented to participants at the end of each period. Appendix 1 shows the respective screens participants see in the lower cost/unbundled condition.

3.3.3 Detailed Procedures

Participants are 109 students beyond their first year of training in Business and Economics from a major Western European business school. They are, on average, 20 years old, 36% are female and 88% have previously invested in stocks and trust funds or plan to do so in the future. Participation in my study does not require prior knowledge or trading experience and, thus, is low in integrative complexity (Elliott, Hodge, Kennedy, and Pronk 2007). Therefore, I conclude that the sample is adequate to test my theory about non-professional investors' judgment.

Once participants arrive at the laboratory, they sit at one of 16 computer workstations and follow the experimental instructions on their screen as the experimenter reads the instructions out loud. The experiment contains two sets of instructions with several quiz questions throughout to reinforce important information.[41] The first instruction set explains the distributional properties of dividends and predictions, trading mechanism, and incentives.

[41] In total, participants answer 93% of all questions correctly, which indicates a good understanding.

To ease understanding of the informational structure, I provide participants with a graph depicting all possible dividends and associated probabilities. In addition, I provide a history of ten dividends, private signals, and forecasts to allow them "to assess the accuracy of the private signal and the forecast". Due to the importance of this information, a handout at each workstation includes a copy of the distribution of dividends and the historical predictions.[42] The second instruction set explains the structure of transaction costs and the trading interface (see Appendix 1). Participants learn that all transaction fees are deducted from their cash balance at the end of a period. This design choice avoids time discounting effects (Frederick, Loewenstein, and O'Donoghue 2002). After reviewing the instructions, participants trade two certificates in a non-incentivized practice session, followed by the main trading session, and a post-experimental questionnaire (PEQ).

Throughout the experiment, participants earn francs (FR) by trading profitably. In addition, one of their estimates of dividend value is selected at random to encourage accuracy. If it is within five percent of the actual dividend value, participants get an additional FR 100. After running all experimental sessions, francs are converted to Euros using the following formula: Payment in Euros = (Net Gain or Loss in Francs + Adjustment Factor) x Exchange Rate (Elliott et al. 2015; Nelson, Krische, and Bloomfield 2003). The adjustment factor and exchange rate are chosen such that every participant receives at least their show-up fee of EUR 10.00, and the average participant acting as an investor earns EUR 25.00.

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[42] The instructions also inform participants that they will see a list with market prices during trading. This information may help them to "obtain information about other traders' private signals." While such explicit guidance may increase the overall level of investor learning, it should bias against finding weaker evidence of investor learning in the higher cost/unbundled condition (for a similar design choice, see Bloomfield and Libby 1996).

3.3.4 Measurement of Dependent Variable

To test my first hypothesis, I use the well-established measure weight of forecast (WOF) from psychology research (Gino 2008; Harvey and Fischer 1997; Yaniv 2004), which has been used in prior accounting studies (Estep 2019; Kadous, Leiby, and Peecher 2013). The WOF measures individuals' use of forecasted information by dividing the absolute difference between their first and second dividend estimate by the absolute distance between the initial estimate and the forecast (WOF = $\frac{|Estimate_2 - Estimate_1|}{|Forecast - Estimate_1|}$). Thus, greater values indicate stronger use of the forecast. For example, a value of zero implies that the participant did not change their estimate in response to the forecast (i.e., $Estimate_2$ = $Estimate_1$), whereas a value of one suggests that they fully relied upon the signal (i.e., $Estimate_2$ = Forecast).

The measure has a few limitations that may require transformation of the data (see, e.g., Harvey and Fischer 1997; Gino 2008, Estep 2019). First, the WOF has a lower bound of zero but no upper bound. If participants over-adjust their initial estimate in the direction of the forecast, the measure may reach values above one. This happens, for example, due to rounding of estimates. To illustrate, one participant gave an estimate of 1,115 in round 1 but adjusted to 1,100 after receiving the forecast of 1,108, resulting in a WOF of 2.14. I find that 238 out of 1,090 observations (21.8%) have a score greater than one. Following common procedures in the literature, I set these values equal to one, reflecting full reliance on the forecast (see, e.g., Estep 2019; Gino 2008; Gino and Moore 2007; Yaniv 2004).[43] Second, the WOF does not

[43] Due to scaling effects, the WO can result in large outliers and distort inferences. An alternative to winsorizing the data would be to adjust for outliers. If, instead of winsorizing, I exclude the 16 individual observations that are more than three standard deviations away from the mean, inferences are identical to the ones reported for tests at the individual level of

distinguish whether a participant moves towards or away from the dividend forecast. A participant with an initial estimate of 1,000 who receives a forecast of 1,100 would yield the same WOF whether their second estimate was 1,050 or 950. Moving away from the forecast cannot occur due to rounding errors, so these observations are likely mistakes (e.g., typos). The nature and significance of results does not change if I drop the 50 observations (4.6%) where estimates move away from the forecast, so I retain them for all analyses. Finally, I follow common practice (Gino 2008; Gino and Moore 2007; Yaniv 2004) and exclude one observation where the participant's initial estimate is equal to the dividend forecast, since dividing by zero yields undefined values. This adjustment results in a total sample size of 1,089 observations.

3.4 Results

3.4.1 Manipulation Checks

The post-experiment questionnaire (PEQ) contains four questions to assess whether the manipulations have been successful. To check on the manipulation of cost, participants rate their agreement with the statement "I think the fees were rather low" on a 7-point Likert scale. Participants in the lower cost condition score significantly higher (μ=5.11) than those in the higher cost condition (μ=3.91, $p<.01$ one-tailed), suggesting a successful manipulation.[44] To assess whether the manipulation of transaction cost unbundling was successful, participants answer the open-ended question "How did you predict dividend values?". I find that a significantly higher percentage of participants (25 out of 50, i.e., 50%) mentions the word

observations. Keeping these extreme values in, I fail to find support for my predictions ($p>.10$).

[44] I report one-tailed p-values throughout the paper to test directional predictions. All other p-values are two-tailed.

"forecast" in the unbundled compared to the bundled condition (20 out of 59, i.e., 34%, p=.04, one-tailed); this finding is consistent with greater attention to the forecast.

Two more binary questions check on the accuracy of the partially crossed factors. Related to my manipulation of disclosure, 84% of participants correctly state whether they "knew which portion of total fees was charged for the dividend forecast and which part went to the execution of trades". I also find that the manipulation of information acquisition was successful, as 93% of participants correctly indicate whether "every participant received the dividend forecast" according to their condition.[45]

3.4.2 Reliance on Investment Research

In the following, I test my hypothesis that transaction cost unbundling increases investors' reliance on investment research more strongly with higher transaction costs. This analysis excludes observations from the condition with endogenous information acquisition to avoid bias from self-selection. Thus, the sample size for the following tests amounts to 839. To test my first hypothesis, I use a mixed linear model of the following form:

$$\begin{aligned} WOF = \alpha_0 &+ \beta_1\ Unbundling_{i,t} + \beta_2\ Cost_{i,t} + \beta_3\ Disclosure_{i,t} \\ &+ \beta_4\ Unbundling_{i,t}\ x\ Cost_{i,t} + \beta_5\ Disclosure_{i,t}\ x\ Cost_{i,t} \\ &+ \beta_6\ Period_{i,t} + \beta_7\ Market\ Size_{i,t} + Investor\ RE \end{aligned} \tag{1}$$

where index i refers to investors and t refers to trading period. *Unbundling*

[45] Excluding 23 individuals who fail these binary manipulation checks does not alter inferences, but three confidence levels at the individual level of analysis. First, the simple effect of unbundling given higher costs predicted in H1 is no longer significant at the 5%, but instead at the 10% level. Second, the unpredicted *disclosure* x *cost* interaction reported in Table 3.3 becomes significant at the 5% level. Finally, the theory-consistent evidence that higher costs induce anticipatory feelings of regret is no longer supported by the data (p=.11, one-tailed), even though the means still point in the same direction. In sum, I find weaker evidence to support my theory when excluding participants, which is likely due to the reduction in sample size and statistical power.

is a dummy variable that takes the value 0 in the bundled and 1 in the unbundled condition. *Cost* takes the value 0 in the lower cost and 1 in the higher cost condition, and *disclosure* takes the value 0 in the absence and 1 in the presence of cost disclosure. *Market size* refers to the number of traders in a market and ranges from 5 to 8. I control for the size of a market in all tests; markets with a higher number of traders receive more private information signals, which changes the relative importance of the forecast.[46] *Period* indicates during which of the 10 trading periods the participant gave their estimate. Since observations are dependent by individual, I add a random intercept for each participant.[47] Results of the mixed model regression are presented in Table 3.3, Panel C. Table 3.3, Panel A reports adjusted means by unbundling and cost conditions, Panel B reports these means by disclosure and cost, and Panel D shows simple effects tests.

[46] Lundholm (1991) shows that increasing the number of traders in a market does not necessarily improve its informational efficiency or the speed of information aggregation. He argues that individual differences in information processing become more pronounced in markets with aggregate uncertainty, like the ones examined in my study. Likewise, Sunder (1995) notes, "as the number of traders increases, so does the range between the extremes, making it more difficult for traders to draw consistent inferences from data". Hence, I also control for *market size* in my tests of investor learning.

[47] The use of this multilevel type of regression lends itself to the analysis of my data for several reasons. First, the experiment is designed to generate hierarchical data at three levels. Observations are grouped at the individual level, which are clustered at the higher level factors cost, unbundling, and disclosure. The linear mixed model combines fixed and random effects, which accounts for the hierarchical structure of the data (Schielzeth and Nakagawa 2013). Second, the linear mixed model corrects for the correlation of responses within each cluster by using random effects, similar to a repeated-measures ANOVA (McLean, Sanders, and Stroup 1991). The experimental factors, their interactions, and control variables serve as fixed effects. Prior accounting research has employed similar models in the analysis of experimental data (Hurley, Mayhew, and Obermire 2019; Kowaleski, Mayhew, and Tegeler 2018).

Table 3.3: Weight of Forecast (WOF)

Panel A: WOF - Adjusted Means by Payment Type and Cost Level Collapsed Across Disclosure

	Lower Cost	Higher Cost
Bundled	0.68 (0.03) n=289	0.56 (0.03) n=300
Unbundled	0.64 (0.08) n=120	0.68 (0.05) n=130

Panel B: WOF - Adjusted Means by Disclosure and Cost Level Collapsed Across Unbundling

	Lower Cost	Higher Cost
No Disclosure	0.60 (0.06) n=130	0.63 0.04 n=150
Disclosure	0.70 (0.03) n=279	0.57 (0.03) n=280

Panel C: Mixed Effects Linear Regression on WOF

Independent Variable	Coefficient	Std. Error	z	p-Value
Intercept	0.63	0.26	2.45	0.01
Unbundling	-0.03	0.10	-0.37	0.71
Cost	-0.01	0.07	-0.14	0.89
Disclosure	-0.06	0.05	-1.18	0.24
Unbundling x Cost	0.16	0.09	1.81	**0.04**
Disclosure x Cost	0.17	0.09	1.79	0.07
Period	0.00	0.00	0.31	0.76
Market Size	-0.00	0.04	-0.11	0.91

Table 3.3 (continued)

Panel D: Simple Effects Tests

Source	df	Chi-Square	p-Value
Effect of Unbundling Given Lower Cost	1	0.14	0.71
Effect of Unbundling Given Higher Cost	1	3.34	**0.03**
Effect of Cost Given Bundled Payment	1	9.69	<0.01
Effect of Cost Given Unbundled Payment	1	0.23	0.63
Effect of Disclosing Given Lower Cost	1	1.84	0.17
Effect of Disclosing Given Higher Cost	1	1.40	0.24
Effect of Cost Given No Disclosure	1	0.23	0.63
Effect of Cost Given Disclosure	1	10.32	<0.01

Definition of variables:
WOF: Weight of forecast is the dependent variable and measures the extent to which participants adjust their value estimate in the direction of the forecast. The measure divides the absolute difference between participants' first and second dividend estimate by the absolute difference between participants' first estimate and the forecast. For a discussion of the measure, see section 3.4.
Unbundling: Unbundling is a dummy variable that takes the value of 0 when participants are charged with a bundled fee for both the forecast and the execution of their trades, and 1 when participants face separate charges for the forecast and the execution of their trades.
Cost: I manipulate the level of transaction costs as lower or higher. In the condition with higher costs, participants face four times the unbundled fees as in the lower cost condition. Bundled fees are elicited based on the average trading volume in the unbundled condition with exogenous information acquisition (see Figure 3.3).
Disclosure: This variable manipulates whether participants have full transparency about how their fees are allocated to trade execution and the forecast (disclosure present) or not (disclosure absent). Note that the condition with unbundled payments always discloses this proportion by default.
Period: This control variable controls for the trading period and ranges from 1 to 10.
Market Size: This control variable adjusts for the number of traders within a market and ranges from 5 to 8.

Notes: This table presents results from a mixed linear model and includes 839 observations (only markets with exogenous information acquisition included). The model includes a random intercept for each participant. P-values in **boldface** are one-tailed given my directional prediction (all other p-values are two-tailed).

Table 3.3, Panel C reveals two insignificant main effects of *unbundling* (p=.71) and *cost* (p=.89), as well as a significant *unbundling* x *cost* interaction (p=.04, one-tailed). This finding is consistent with my theory that the effect of transaction cost unbundling varies with the level of transaction costs. The descriptive data in Table 3.3, Panel A help further explore the nature of this interactive effect. Adjusted means indicate that participants in the higher cost condition assign a greater weight to the forecast when transaction costs are unbundled (μ=0.68) compared to bundled (μ=0.56) and simple effects tests reported in Table 3.3, Panel D show that this difference is significant (p=.03, one-tailed). In contrast, I find no effect of unbundling when costs are lower (p=.71). Overall, these results support the first hypothesis that transaction cost unbundling induces investors to rely more strongly on investment research as transaction costs increase. Consistent with the dynamics of a sunk cost fallacy, this effect is more pronounced with higher versus lower transaction costs. Hence, it appears as though regulatory interventions that alter the structure of transaction costs indeed change the importance investors attach to investment research. Most importantly, this result is attributable to changes in the structure of transaction costs, not the transparency of information, because the model controls for the presence or absence of cost disclosure.

The regression analysis also allows me to examine how cost disclosure affects investors' reliance on investment research. Knowing how much they pay for information may have a direct effect on investors' reliance on information. The data in Panel B of Table 3.3, however, does not support this alternative explanation. While there is a marginally significant *disclosure* x *cost* interaction (p=.07), I find that *disclosure* affects WOF neither in the presence of higher costs (p=.24), nor lower costs (p=.17).[48] This finding is in line with transaction decoupling theory because cost disclosure does not vary the certainty of transaction costs. In other words, simply disclosing cost proportions does not induce a sunk cost fallacy. This result has practical implications because it shows that it is possible to create full cost transparency without distorting investors' information processing. It is this transparency that allows investors to gauge what part of their commissions is spent on investment research and to monitor their asset managers' expenditures, which are among the anticipated benefits of MiFID II. Some interest groups are urging the SEC to require periodic transaction cost disclosure instead of unbundling payments (Allen, Gellasch, and Schacht 2019). My findings substantiate these claims. Before I test my second hypothesis, I offer evidence in the following section that the dynamics implied by transaction decoupling theory underlie my findings.

3.4.3 Investor Disagreement

Transaction decoupling theory predicts that investors are subject to a sunk cost fallacy, especially if transaction cost unbundling allows them to allocate their feelings of regret to the same mental account as the costly forecast. Theory suggests that investors' stronger reliance on forecasts is unlikely

[48] The analysis also shows that participants discount forecasts as transaction costs increase in the presence of cost disclosure (p<.01), but not in its absence (p=.63). This finding seems to be in line with an ambiguity aversion interpretation. It appears that investors neglect forecasted information in order to reduce perceptions of ambiguity. When bundled costs increase, the potential cost range also widens; this is particularly transparent in the presence of disclosure. Prior research shows that investors exhibit ambiguity aversion (White 2017; Williams 2015) and discount information with an ambiguous component (Gale 2020). This conjecture is supported by the finding that higher costs also reduce reliance in the bundled condition. As reported in Table 3.5, I show that these information discounting effects do not alter price efficiency, as evidenced by an insignificant *disclosure* x *cost* interaction (p=.23). Therefore, I conclude that the information discounting effects that arise in my experiment are of no particular concern in this setting.

motivated by a better informed and, thus, more uniform use of the information, but rather by uninformative belief adjustments that reduce the anticipation of regret (Gino 2008; Zeelenberg et al. 1996). To test this conjecture, I create the variable *belief jumbling*, which subtracts within each market period the correlation between dividend estimates in the first and second trading round from one:

$$Belief\ Jumbling = 1 - corr(E_{1,j,t};\ E_{2,j,t}) \qquad (2)$$

Where index j refers to market and t refers to trading period. This measure indicates the extent to which participants differ in their revision of beliefs. That is, if all participants in a market adjust their expectations in exactly the same manner, thre is a perfect correlation and this measure equals zero. Higher values indicate more heterogeneous adjustments (Bamber, Barron, and Stober 1997). Using the 120 market periods in the exogenous information acquisition condition, I run a mixed linear model regressing *belief jumbling* on the same independent variables specified in equation (1). The model includes a random intercept for each market, since the markets are the unit of analysis. Table 3.4 presents descriptive statistics and results.

Table 3.4: Investor Disagreement Created by Forecast

Panel A: Belief Jumbling - Adjusted Means by Payment Type and Cost Level

	Lower Cost	Higher Cost
Bundled	0.54 (0.08) n=40	0.38 (0.08) n=40
Unbundled	0.69 (0.17) n=20	0.81 (0.11) n=20

Panel B: Belief Jumbling - Adjusted Means by Disclosure and Cost Level

	Lower Cost	Higher Cost
No Disclosure	0.55 (0.14) n=20	0.70 0.10 n=20
Disclosure	0.61 (0.07) n=40	0.43 (0.07) n=40

Panel C: Mixed Effects Linear Regression on Belief Jumbling

Independent Variable	Coefficient	Std. Error	z	p-Value
Intercept	0.82	0.59	1.39	0.16
Unbundling	0.15	0.22	0.70	0.48
Cost	0.06	0.15	0.39	0.70
Disclosure	-0.26	0.13	-2.1	0.04
Unbundling x Cost	0.27	0.20	1.38	**0.08**
Disclosure x Cost	0.33	0.22	1.46	0.14
Period	0.01	0.01	0.86	0.39
Market Size	-0.06	0.09	-0.62	0.53

Panel D: Simple Effects Tests

Source	df	Chi-Square	p-Value
Effect of Unbundling Given Lower Cost	1	0.50	0.48
Effect of Unbundling Given Higher Cost	1	7.70	**<0.01**
Effect of Cost Given Bundled Payment	1	2.79	0.09
Effect of Cost Given Unbundled Payment	1	0.51	0.48

Definition of Variables and Notes: see next page.

Definition of Variables:
Belief Jumbling: The dependent variable in this model is obtained by subtracting within each market period the correlation between dividend estimates from the first and the second trading round from one.
Unbundling: Unbundling is a dummy variable that takes the value of 0 when participants are charged with a bundled fee for both the forecast and the execution of their trades, and 1 when participants face separate charges for the forecast and the execution of their trades.
Cost: I manipulate the level of transaction costs as lower or higher. In the condition with higher costs, participants face four times the unbundled fees as in the lower cost condition. Bundled fees are elicited based on the average trading volume in the unbundled condition with exogenous information acquisition (see Figure 3.3).
Disclosure: This variable manipulates whether participants have full transparency about how their fees are allocated to trade execution and the forecast (disclosure present) or not (disclosure absent). Note that the condition with unbundled payments always discloses this proportion by default.
Period: This control variable controls for the trading period and ranges from 1 to 10.
Market Size: This control variable captures the number of traders within a market and ranges from 5 to 8.

Notes: Table 3.4 shows the results from a mixed linear regression model (only markets with exogenous information acquisition included). The model is specified with a random intercept for each market to correct for dependencies among observations. This analysis uses the 120 observations from the condition with exogenous information acquisition. P-values in **boldface** are one-tailed given my directional prediction (all other p-values are two-tailed).

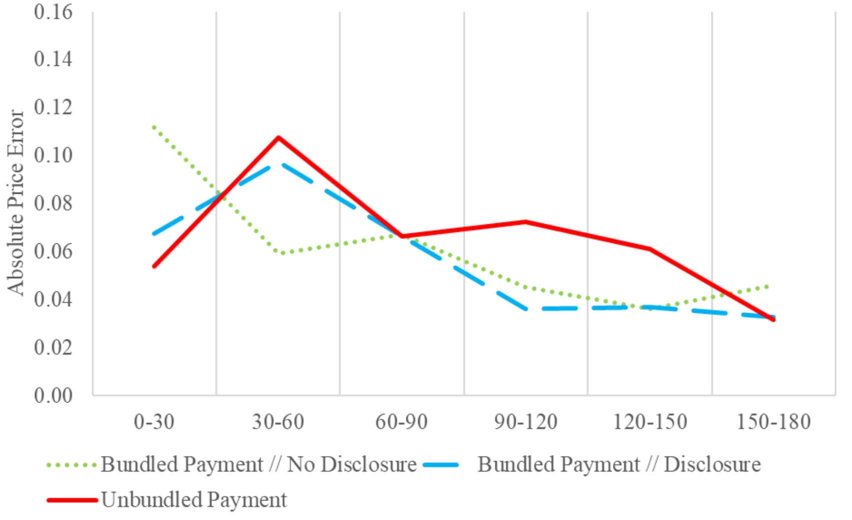
The means in Table 3.4, Panel A report higher levels of *belief jumbling* in the higher cost/unbundled (μ=0.81) compared to the higher cost/bundled condition (μ=0.38), and the difference is significant ($p<.01$, one-tailed). In the lower cost condition, transaction cost unbundling does not generate different levels of disagreement ($p=.48$). Most importantly, this pattern is qualified by a marginally significant *unbundling* x *cost* interaction ($p=.08$, one-tailed). In sum, these results are indicative of uninformative and regret-driven belief adjustments when transaction costs are higher and payments unbundled. This finding supports my theory that transaction decoupling causes stronger reliance on more costly forecasts as markets transition to explicit payments for costly investment research. Next, I analyze a possible consequence of my findings at the aggregate market level – weakened investor learning.

3.4.4 Investor Learning

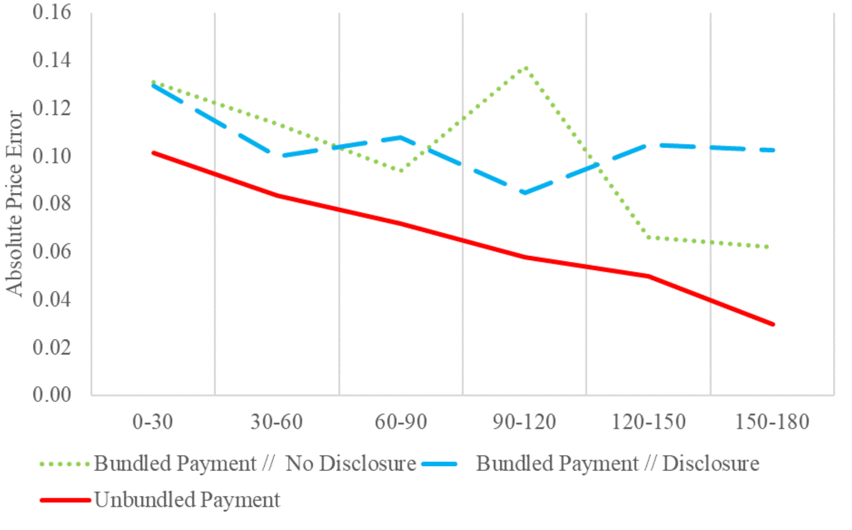
The analysis so far has shown that investors rely more on costly forecasts under a system of unbundled compared to bundled payments. My second hypothesis states that this effect also materializes at the aggregate market level in the form of weaker investor learning. In order to test for differences in the gradual convergence to equilibrium, I first provide visual evidence of investor learning and examine their empirical significance in a second step.

Figure 3.4: Investor Learning over Time

Panel A: Higher Transaction Cost Subsample



Panel B: Lower Transaction Cost Subsample



Notes: Figure 3.4 depicts the evolution of absolute price errors over time by experimental condition. Absolute price errors are defined as the absolute deviation of a transaction price from a full efficiency benchmark, divided by this benchmark. I calculate the absolute price error for each 30-second time interval by averaging the absolute price errors of all transactions per condition.

Figure 3.4 presents, in Panels A and B, how absolute price errors develop over time in markets with higher and lower transaction costs. Absolute price errors are defined as the absolute deviation of the market price from a full efficiency benchmark, divided by this benchmark (see also Gong et al. 2020).[49] I calculate the absolute price error for each 30-second time interval by averaging absolute price errors of all transactions per condition. Consistent with transaction decoupling theory, the higher cost/unbundled condition results in volatile changes in absolute price errors, while in the higher cost/bundled condition they decrease in a more stable fashion. These differences are less pronounced with lower costs.

To substantiate these claims, I run a 2 x 2 +2 between-markets mixed linear model on absolute price errors using the data presented in Figure 3.4.[50]

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[49] Specifically, I calculate the full-efficiency benchmark using the expected dividend value V ~ N(1,200, 400^2), the private information signal PI with error term ε_{PI} ~ N(0, 60^2), and the forecast F with ε_F ~ N(0, 30^2). Hence, the benchmark is calculated for participant i in period t as

$$\mathrm{EV}(\mathrm{DIV}_{i,t}) = \frac{1{,}200 * \frac{1}{400^2} + \sum_{i=1}^{n} PI_{i,t} * \frac{1}{60^2} + F_t * \frac{1}{30^2}}{\frac{1}{400^2} + n * \frac{1}{60^2} + \frac{1}{30^2}}$$

I modify this benchmark for transactions that occur in the first trading round. For these observations, I calculate the expected value solely based on prior expectations of the dividend and private information signals.

[50] Unlike prior analyses, this model does not contain ten observations per participant; I treat every 30-second time interval within each market period as one observation. In 129 out of 720 time intervals, no trade occurs, which decreases the sample size. Further, I exclude one market period in which only a single trade occurs, as it renders the analysis of learning effects over time impossible. Thus, the total sample size for this testing procedure equals 590 observations. To alleviate the concern that my sample suffers from bias, I replace missing observations with the mid-point of each time interval's posted bids and asks. In two time intervals where no offers are posted, I use the estimate from the preceding interval. Replacing missing observations does not change inferences, but two confidence levels (see Table 3.5). While the simple effect of *TT* in the higher cost/unbundled condition becomes marginally significant (p=.08), the p-value of the predicted unbundling x cost x TT interaction falls below the 5% threshold supporting H2 (p=.04). Hence, I conclude that my results are robust to this reduction in sample size.

I use the model specified in equation (1) and interact all terms with *time of trade* (TT). *TT* indicates within which time interval (1 to 6) the outstanding bid or ask offer was accepted. A negative coefficient indicates that transactions occurring at a later stage are better informed, so I interpret this as evidence of investor learning (for a similar approach, see Bossaerts, Frydman, and Ledyard 2014). Due to the continuous nature of time, I treat *TT* as a continuous numeric variable and specify the mixed model accordingly as a regression analysis. Because I analyze time trends at the aggregate market level, not at the investor level, I replace the investor random intercept with a random intercept for each of the 120 trading periods. Support for my hypothesis is indicated by a positive *unbundling* x *cost* x *TT* interaction. Table 3.5 reports regression results in Panel A, simple effects for the higher cost condition in Panel B, and for the lower cost condition in Panel C.

Table 3.5: Investor Learning

Panel A: Mixed Effects Linear Regression on Absolute Price Error

Independent Variable	Coefficient	Std. Error	z	p-Value
Intercept (β_0)	0.07	0.08	0.88	0.38
Unbundling (β_1)	0.03	0.04	0.74	0.46
Cost (β_2)	-0.06	0.03	-1.98	0.05
Disclosure (β_3)	-0.05	0.03	-1.50	0.13
Time of Trade (TT) (β_4)	-0.01	0.00	-2.75	<0.01
Unbundling x Cost (β_5)	-0.02	0.04	-0.51	0.61
Disclosure x Cost (β_6)	0.05	0.04	1.19	0.23
Unbundling x TT (β_7)	-0.01	0.01	-1.27	0.20
Cost x TT (β_8)	0.00	0.01	0.37	0.71
Disclosure x TT (β_9)	0.01	0.01	1.32	0.19
Unbundling x Cost x TT (β_{10})	0.01	0.01	1.39	**0.08**
Disclosure x Cost x TT (β_{11})	-0.01	0.01	-1.14	0.26
Period (β_{12})	-0.01	0.00	-4.96	<0.01
Market Size (β_{13})	0.02	0.01	1.57	0.12

Panel B: Effect of Time of Trade by Experimental Condition - Higher Cost

Source	Coef. (Std. Error)	z	p-Value
Unbundled Payment ($\beta_4 + \beta_7 + \beta_8 + \beta_9 + \beta_{10} + \beta_{11}$)	-0.01 (0.00)	-1.61	0.11
Bundled Payment and No Disclosure ($\beta_4 + \beta_8$)	-0.01 (0.00)	-2.14	**0.02**
Bundled Payment and Disclosure ($\beta_4 + \beta_8 + \beta_9 + \beta_{11}$)	-0.01 (0.00)	-2.52	**<0.01**

Table 3.5 (continued)

Panel C: Effect of Time of Trade by Experimental Cell - Lower Cost Condition

Source	Coef. (Std. Error)	z	p-Value
Unbundled Payment ($\beta_4 + \beta_7 + \beta_9$)	-0.01 (0.01)	-2.49	**<0.01**
Bundled Payment and No Disclosure (β_4)	-0.01 (0.00)	-2.75	**<0.01**
Bundled Payment and Disclosure ($\beta_4 + \beta_9$)	-0.00 (0.00)	-0.93	**0.18**

Definition of variables:
Absolute Price Error: This is the dependent variable and measures the informational efficiency of market prices within each 30-second time interval of each market period. The variable is defined as the absolute deviation of the average market price from a full efficiency benchmark, divided by this benchmark. The full efficiency benchmark weighs prior dividend expectations, the forecast, and all private signals within a trading period by their precision. Higher values indicate lower informational efficiency.
Unbundling: Unbundling is a dummy variable that takes the value of 0 when participants are charged with a bundled fee for both the forecast and the execution of their trades, and 1 when participants face separate charges for the forecast and the execution of their trades.
Cost: I manipulate the level of transaction costs as lower or higher. In the condition with higher costs, participants face four times the unbundled fees as in the lower cost condition. Bundled fees are elicited based on the average trading volume in the unbundled condition with exogenous information acquisition (see Figure 3.3).
Disclosure: This dummy variable manipulates whether participants have full transparency about how their fees are allocated to trade execution and the forecast (disclosure present) or not (disclosure absent). Note that the condition with unbundled payments always discloses this proportion by default.
Time of Trade (TT): This variable indicates during which 30-second time interval within a trading period the trade was executed and ranges from 1 to 6.
Period: This control variable controls for the trading period and ranges from 1 to 10.
Market Size: This control variable captures the number of traders within a market and ranges from 5 to 8.

Notes: Table 3.5 shows the results from a mixed linear regression model and includes six observations from each trading period (only markets with exogenous information acquisition included). The model includes a random intercept for each trading period. Due to missing observations, there are 590 observations. See section 3.4.4 for a discussion of missing observations. P-values in **boldface** are one-tailed given my directional prediction (all other p-values are two-tailed).

Table 3.5, Panel A reveals a marginally significant *unbundling* x *cost* x *TT* interaction (p=.08, one-tailed). I use linear combinations of regression coefficients to examine the nature of this interaction. Results in Table 3.5, Panel B provide evidence of investor learning in the higher cost/bundled conditions (both p<.05, one-tailed), but a lack of similar effects with unbundled transaction costs (p=.11). This result is analogous to the findings from the analysis on individual reliance. In other words, transaction cost unbundling increases reliance on investment research, but weakens investor learning. In markets with lower costs, I find that trades become more informative over time when payments are unbundled (p<.01, one-tailed).[51] With bundled payments, investors learn to reduce price errors only in the absence of cost disclosure (p<.01, one-tailed), not when it is present (p=.18, one-tailed).[52] These results are in line with my expectation that transaction cost unbundling reduces investor learning effects when transaction costs are high, but to a smaller extent when transaction costs are low. Differences across conditions are indicated by the marginally significant three-way interaction. Note that the results in Table 3.5 also rule out the alternative explanation that investor learning decreases because the average market price efficiency differs across unbundling and cost conditions. Specifically, the *unbundling* x *cost* interaction does not reach significance (p=.61). In sum, I find

[51] Finding evidence of investor learning when transaction costs are unbundled and lower, but not when they are high, may occur because investors in the higher cost condition learn more within the first time interval t1. A lower starting point could explain why price errors do not decrease over time. However, absolute price errors in t1 do not differ significantly across the two conditions with lower and higher unbundled transaction costs (p=.13), which mitigates this concern.

[52] While the latter simple effect suggests that lower cost disclosure harms investors' processing of information, this result needs to be interpreted alongside other findings. As reported in Table 3.4, cost disclosure leads to more homogeneous beliefs across investors (β=-0.26, p=.04). This uniform use of information seems to preclude investors from learning each other's private information. Future research could further explore this explanation.

some, albeit weak, evidence in support of my second hypothesis that investor learning decreases as a result of transaction cost unbundling.

3.4.5 Additional Analyses

Anticipation of Regret

The post-experimental questionnaire contains several questions to provide theory-consistent evidence and to rule out alternative explanations. First, my theory is based on the sunk cost fallacy, which has been linked to the experience of anticipated regret over spending money on unused advice (Gino 2008). I measure participants' anticipated regret by asking them to indicate on a 7-point Likert scale their agreement with the statement "After obtaining the dividend forecast, I anticipated to feel regret if I could not recover the money spent on it".[53] To test for differences across conditions, I regress the item on a mixed model with the factors *unbundling*, *cost*, *disclosure*, *choice*, and their respective interactions, while controlling for *market size*. *Choice* is a binary variable, which is equal to zero (one) in the condition with exogenous (endogenous) information acquisition. Note that I have one observation per participant (i.e., 109 observations). I use the entire sample for this analysis; every participant obtains the forecast at least once and the questions in the PEQ ask specifically about this situation. Hence, there are no self-selection concerns.

The (untabulated) results reveal a significant main effect of the cost level (β=1.21, z=1.69, p=.05, one-tailed), whereas all remaining factors are insignificant (all p>.10). The positive coefficient suggests that higher

[53] In addition, participants indicate to what extent they would feel happy if they could not recover the money spent on the forecast. Measuring anticipated regret with a direct measure of anticipated regret and a measure of negative emotions is common practice (for an overview, see Brewer, DeFrank, and Gilkey 2016). However, results of a factor analysis indicate that this second reverse-coded item does not load, so I use only the first one.

transaction costs lead to anticipated regret and speaks to the construct validity of my manipulation. Yet, only participants in the unbundled condition are able to couple these feelings with the forecast and, therefore, assign a greater weight to the information.

Alternative Explanations

I measure two additional processes to rule out alternative explanations. First, I test to see if higher costs have a direct effect on perceptions of forecast quality. Marketing literature suggests that individuals often use price as an indicator of quality (Monroe 1973). Even though participants acting as investors in my markets are informed that a random selection process generates forecast errors, I test whether forecast quality perceptions differ across conditions. Participants indicate on a 7-point Likert scale whether "the dividend forecast was of high quality". Regressing this item on the same model as above, a significant *disclosure* x *cost* interaction (β=1.39, z=2.19, p=.03) and a negative main effect of *unbundling* (β=-1.33, z=-2.32, p=.02) emerge. The *unbundling* x *cost* interaction is marginally significant (β=1.14, z=1.87, p=.06).[54] Most importantly, controlling for perceptions of forecast quality in my tests at the individual level of observation (i.e. H1) does not change the nature and significance of inferences. Hence, the regret-driven weighting of information identified in prior analyses is independent of these

[54] Follow-up tests indicate that the *disclosure* x *cost* interaction is driven by two marginally significant effects. Higher costs increase perceptions of forecast quality in the condition without disclosure (μ_{Low}=3.93, μ_{High}=4.93, p=.07), and so does disclosure in the lower cost condition (μ_{Absent}=3.93, $\mu_{Present}$=4.84, p=.06). Further, I find that unbundling decreases quality perceptions in the lower cost condition ($\mu_{Bund.}$=5.22, $\mu_{Unbund.}$=3.89, p=.02), but not in the higher cost condition ($\mu_{Bund.}$=4.67, $\mu_{Unbund.}$=4.48, p=.68). Since this pattern is different from the one predicted in H1 and documented in Table 3.3, this effect is unlikely to drive results. Rather, it seems as if investors use costs as an indicator of forecast quality, which opens up avenues for future research.

differences in quality perceptions.

Moreover, prior literature argues that individuals experience cognitive dissonance when receiving information that is inconsistent with their preferences (Festinger 1957). Investors may reduce this dissonance by engaging in motivated reasoning, such that their preferences influence their beliefs (Hales 2007). Within my markets, higher costs could lead to such a process if participants fear that the forecast may be inaccurate, while they prefer for it to be precise. I measure cognitive dissonance by adapting a validated scale from Sweeney, Hausknecht, and Soutar (2000). Participants rate their agreement with the seven items listed in Appendix 2 on a 7-point Likert scale. I average their responses to form one index (Cronbach coefficient alpha = 0.85). Regressing this measure on the mixed model, I find (in untabulated results) that none of the coefficients reaches significance (all $p>.10$), which rules out this alternative explanation.

Endogenous Information Acquisition

The markets analyzed so far provide participants exogenously with investment research. I opt for this design for purposes of internal validity, as it allows me to separate payment effects from differences in information acquisition. Prior to MiFID II, investors had access to their broker-dealers' research reports by default; now, they must choose to acquire the information. In the following, I present results from four additional markets, where investors may opt in to acquire the dividend forecast. This analysis addresses the concern that investors who anticipate regret over their use of investment research may decide not to acquire the information. However, choosing to remain uninformed may also induce feelings of regret, which makes this an empirical question. Conducting this analysis thus sheds light on whether the previously identified sunk cost fallacy is generalizable to markets with endogenous information choice.

To test for the effects of information acquisition, I use a linear mixed effects model to regress WOF on *choice*, *cost*, and their interaction, while controlling for *period* and *market size*. I run this model on the eight markets with unbundled payments (n= 198).[55] The repeated-measures data in this model is fully crossed, and I add a random intercept by participant. The (untabulated) results reveal two insignificant main effects (both p>.10) and a marginally significant *cost* x *choice* interaction (β=.14, z=1.78, p=.08). The interaction term indicates that the cost level has a stronger positive effect on investors' use of the information when they have a choice.[56] This finding implies that higher costs affect reliance especially when investors are personally responsible for acquiring the forecast and remains consistent with prior research on the sunk cost fallacy (Gino 2008; Staw 1976). For the purpose of this study, it is important to test whether and to what extent these effects interfere with my results (i.e., in the higher cost condition). Importantly, my research indicates that the type of information acquisition has no additional effect on investors' reliance on the forecast, neither in the higher cost condition (p=.27), nor in the lower cost condition (p=.16). Thus, I expect

[55] Participant traders acquire the costly forecast in 79.2% of cases. I exclude 52 observations where participants do not purchase the forecast, which allows me to examine the robustness of my findings within the self-selected sample. None of the nine demographic variables measured in the PEQ (such as gender, age, risk aversion, and prior participation in trading simulations) significantly influences the likelihood to acquire the forecast (all p>.10), and, therefore, mitigates self-selection concerns. Participants also rate to what extent they "felt frustrated about spending money on the dividend forecast". Responses do not differ by choice condition (p=.55), suggesting that the exogenous provision with costly forecasts did not frustrate participants in my main experiment and cannot explain the pattern of results.

[56] The corresponding simple effects indicate that this interaction is driven by the marginally stronger positive influence of costs in the choice condition (p=.10), while there is no effect of *cost* in the condition without choice (p=.44).

my results to generalize to a real-world setting where regulators switch from bundled to unbundled payments, giving investors the option to purchase research that was previously available by default.

3.5 Conclusion

Overall, my study indicates that investors are more likely to rely on investment research when they are charged with unbundled compared to bundled fees and the effect is particularly strong when they anticipate regret over unused information (i.e., transaction costs are high). As a result, investors' ability to gradually reduce price errors vanishes. These findings, together with the results from additional analyses, point towards a dominant role of payment types in inducing regret-focused attention to information costs.

My study contributes to research and practice by documenting how the structure of transaction costs affects investor judgment. To the best of my knowledge, prior literature on information costs has not examined how the specific form of cost conveyance affects investors' attention to information costs. Finding that transaction cost unbundling harms investors' processing of other market information exemplifies an adverse consequence of MiFID II and merits regulatory attention. Specifically, my results suggest that an alternative to unbundling, transparent cost disclosure, neither induces a sunk cost fallacy nor weakens investor learning. Requiring broker-dealers to disclose the proportion of information costs and other charges per executed trade seems to be a cost-efficient method to overcome the behavioral challenges associated with transaction cost unbundling.

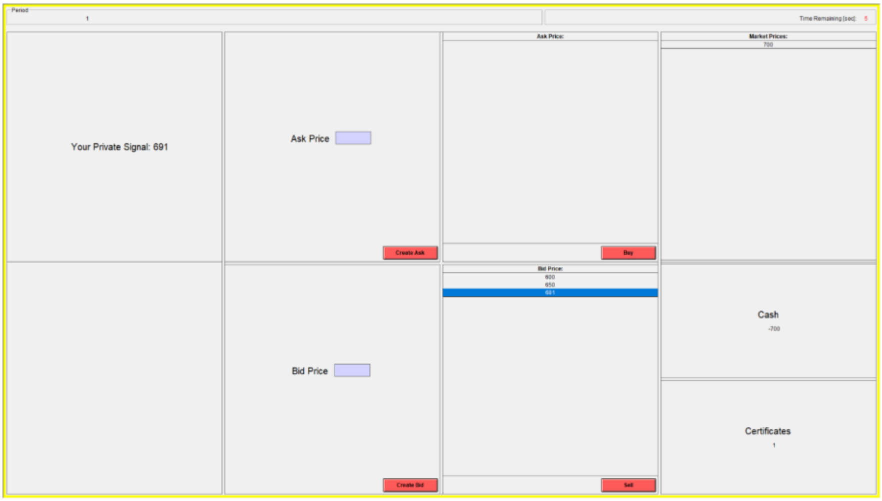
The use of an experiment allows me to isolate the effect of transaction cost unbundling on judgment and decision making, but it cannot capture all aspects that arise in the real world, due to complex regulations like MiFID II. Therefore, my study has limitations common to experimental research (Kachelmeier and King 2002) and offers ideas for future research. First, the introduction of MiFID II in Europe was aimed at reducing the potential for conflicts of interest between investors and asset managers who make trading and research purchasing decisions on their clients' behalf. My study does not speak to these agency conflicts, but acknowledges that retail investors also may purchase and trade on investment research themselves (Kelly, Low, Tan, and Tan 2012; Mikhail, Walther, and Willis 2007). I document a direct effect on investors' judgment and decision making that merits attention by regulators and investors alike.

Second, the decontextualized market setting in which investors trade necessarily abstracts away from financial analyst and forecast characteristics that influence investor decision making (Bonner Hugon and Walther 2007; Hirst Koonce and Simko 1995). To illustrate, prior research documents that analyst reports are often overly optimistic (Jegadeesh, Kim, Krische, and Lee 2004) and contain different degrees of ambiguity (Winchel 2015) as well as trading recommendations and strategies (Kelly et al. 2012; Nelson et al. 2003). Yet, this abstract setting allows me to disentangle my predicted effects from other confounding factors, such as differences in information acquisition and cost transparency. Specifically, I find that cost disclosure does not incrementally induce sunk cost effects, while it likely mitigates agency conflicts. Such an alternative policy may distort firms' incentives to disclose the true cost of research, but note that these concerns are also present with unbundled transaction costs and require regulatory overview.

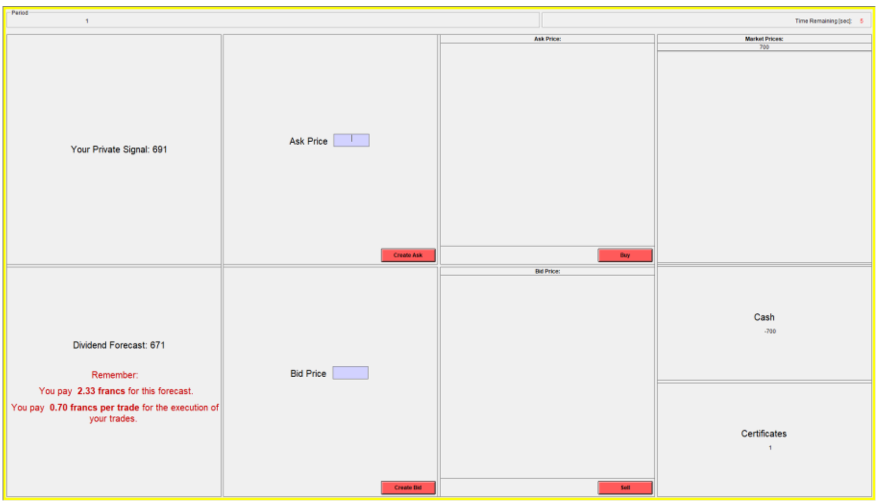
Third, it is possible that investors who experience regret do not use research more heavily, but spend time and effort to acquire freely available research. Yet, prior research shows that these processing costs also increase investor reliance on information (Nelson and Tayler 2007), which would add to the effects documented in this study. Examining the additivity of processing costs and monetary transaction costs is a potential avenue for future research.

Appendix 1: Trading Interface

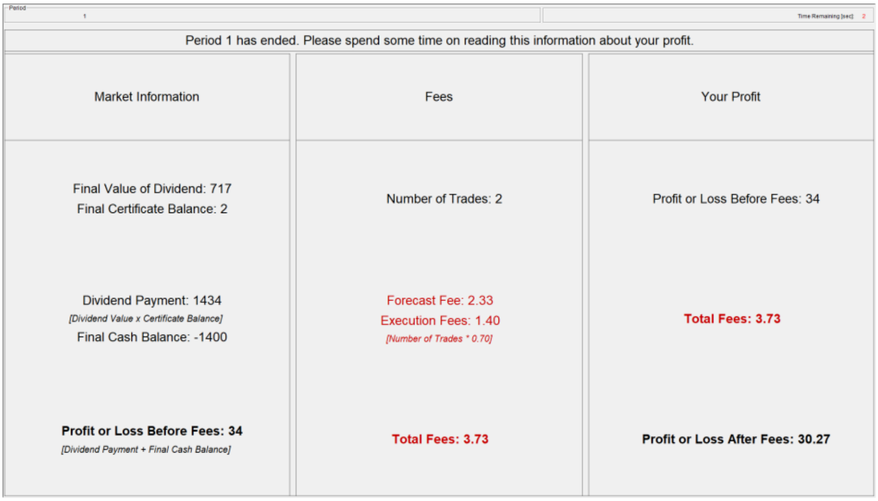
Panel A: Trading Interface First Trading Round – All Conditions



Panel B: Trading Interface Second Trading Round – Unbundled Payment with Lower Cost



Panel C: Overview After Second Trading Round – Unbundled Payment with Lower Cost



Notes: These pictures show the screens that participants see within each trading period. Panel A presents the trading interface that is displayed during the first 90 seconds of trading. Participants may post offers or accept other participants' posted bids and asks. They also see a list with realized market prices and their own cash and certificate balance on the right side of the screen. Panel B shows the trading interface that participants see during the second trading round. Participants see the dividend forecast on the left side of the screen together with a reminder of their respective transaction cost structure. In the lower cost/unbundled condition, the reminder says, "Reminder: You pay 2.33 francs for this forecast. You pay 0.70 francs per trade for the execution of your trades". In the endogenous information acquisition condition, the first part of the sentence appears only if investors decide to purchase the forecast. In the bundled condition, the wording changes to, "Reminder: You pay 1.26 francs per trade for both this forecast and the execution of your trades". In the condition with disclosure, it says, "Reminder: You pay 1.26 francs per trade: 0.56 francs for this forecast and 0.70 francs for the execution of your trades". In the higher cost condition, participants see the cost parameters specified in Figure 3.3. Panel C shows the overview that participants see at the end of each trading period. The screen presents to each participant the realized dividend value, number of transactions incurred, total fee expenditures, and the net profit or loss from a trading period.

Appendix 2: Cognitive Dissonance

Items Used to Measure Cognitive Dissonance

1. I resented spending money on the dividend forecast.
2. I felt disappointed with myself after spending money on the dividend forecast.
3. I felt I'd let myself down after spending money on the dividend forecast.
4. I wonder if I really needed the dividend forecast.
5. I wonder whether I should have obtained any information at all.
6. After obtaining the dividend forecast, I wondered if I'd been fooled.
7. After obtaining the dividend forecast, I wondered whether there was something wrong with the deal I got.

Notes: I use these seven items to measure the level of cognitive dissonance that participants experience from purchasing the forecast. Participants rate their agreement with each item on a Likert scale ranging from 1 (Strongly Disagree) to 7 (Strongly Agree). This questionnaire is adapted from Sweeney et al.(2000).

Chapter 4

Not Seeing Eye-to-Eye: Differences between Identified and Non-Identified Investors' Reactions to Adverse Event Disclosure

4.1 Introduction

Public company disclosure is often followed by large trading volumes, reflecting disagreement among investors about the disclosure's consequences for the future payoffs of the company (Bamber, Barron, and Stevens 2011; Beaver 1968). While prior literature identifies different initial information endowments as one driver of investor disagreement (e.g., Karpoff, 1986; Kim and Verrecchia 1991; Atiase and Bamber 1994), another key driver is that

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This paper is joint work with Erik Peek and Marcel van Rinsum. We are grateful to Jeffrey Hales for sharing his research instrument with us. Furthermore, we would like to thank seminar participants at the Emerging Researchers Consortium in Accounting 2017, the XV. International Accounting Research Symposium, the European Network for Experimental Accounting Research Conference 2019, the 2019 Accounting Behavior and Organizations Research Conference, and Erasmus University Rotterdam, as well as Kai Bauch, Beatriz García-Osma, Mark Peecher, Laura Gómez-Ruiz, Ryan Guggenmos, Frank Hartmann, Stephan Kramer, Christian Pietsch, Amedeo Pugliese and Evelien Reusen for their helpful comments.

investors can interpret the same information differently (Kim and Verrecchia 1994, 1997; Bamber, Barron, and Stober 1999). Little is known about what factors can cause such interpretation differences. Our study sheds light on one of the origins of investor disagreement by examining how investor identification with the company affects investors' perceptions of managerial credibility, while holding information endowments constant. Consistent with Social Identity (SI) theory, we define company identification as investors' self-categorization as in-group members of a particular company (Bernard, Cade, and Hodge 2018; Henri, Tajfel, and Turner 1986).

Non-professional investors identify with a company for various reasons, for instance because they are consumers of its products (Bhattacharya and Sen 2003) or agree with its corporate values (Elliott, Jackson, Peecher, and White 2014). Many of these reasons may also make these individuals become a shareholder in that company. Shareholders are, in fact, a prominent example of investors who likely see themselves as part of 'their' company, develop a unique perspective on the organization (Aspara and Tikkanen 2008), and gradually become (more) identified. An important catalyst for this process is shareholders' stake in the company (Bernard et al. 2018), and their resulting engagement in monitoring and company communication. Specifically, common disclosure characteristics, such as addressing shareholders as 'we', and communication with management via social media cultivate a social bond with the company and its management (Elliott, Grant, and Hodge 2018). In line with the above, and with prior findings indicating that shareholders indeed strongly identify with their companies (Aspara and Tikkanen 2008, 2011), we utilize shareholders' likely identification in our theorizing and design.

We argue that investors who identify more strongly with their companies exhibit affect-based in-group favoritism, leading them to perceive these companies' management and its disclosures as more credible than do non-identified investors. We examine how the strength of this effect varies with locus of attribution, that is, whether managers attribute company results to internal or external causes in their disclosures (Elliott, Hodge, and Sedor 2012). Finally, we analyze how differences in perceived management credibility have real economic effects on company valuations.

We investigate a setting in which investors receive 'bad news' in the form of a disclosure of an adverse event and a related earnings guidance cut, because negative news in particular can affect investors' trust in a company or manager (Chen, Han, and Tan 2016; Elliott, Grant, and Hodge 2018). Building on ultimate attribution error theory (Coleman 2013; Pettigrew 1979), we argue that non-identified investors see external attributions as rather deceitful, while they see internal attributions as more credible. In contrast, the in-group favoritism of identified investors (e.g., shareholders) evokes a trusting response toward all management communication, irrespective of managers' attribution. Therefore, we predict that company identification positively affects perceived management credibility under external attribution, but not under internal attribution, which all investors judge as credible regardless of their company identification level.

To test our prediction, we run a 2 x 2 between-subjects experiment. We incentivize participants as stock market investors and ask them to choose to invest in one of two hypothetical companies, both based on actual data from the same real company (Hales, 2007). We then manipulate participants' company identification, while showing all participants the same company announcement of bad news. That is, we randomly assign half of the subjects to review press releases of the company they have chosen (i.e., as shareholders; higher company identification), whereas we make the other half review the exact same press releases, adapted from the same real company, but for their non-chosen company (i.e., as outside investors; lower company identification). Next follows an announcement of an earnings guidance cut, including either an internal or an external attribution, representing our second manipulation. We measure our main dependent variable, management credibility, after administering the manipulations.

Our results show that when management employs external attribution in disclosures containing negative news, identified investors (shareholders) judge managers of the disclosing company to be more credible than non-identified (outside) investors do. In contrast, when a manager employs internal attribution, company identification does not lead to significant variation in credibility perceptions. We show that in-group favoritism plays an important role in driving the effect of company identification.

We also investigate the role of the two components that make up credibility: trustworthiness and competence (Mercer 2005). Consistent with our reasoning, we find that trustworthiness, rather than competence, drives the interactive effect of company identification and locus of attribution (Chen et al. 2016; Elliott et al. 2018). Moreover, we investigate if differences in credibility also influence company valuations, and our findings corroborate that they do. Overall, these results tally with our theory, showing that identification-induced investor disagreement mitigates the generally accepted positive effect of financial disclosure on resolving information asymmetries (Healy and Palepu 2001; Leuz and Verrecchia 2000).

Our study contributes to the literature in several ways. First, our research adds to the disclosure literature on investor disagreement. While the finance and accounting literature highlights several important consequences of investor disagreement, such as bubbles and crises (Harrison and Kreps 1978; Hong and Sraer 2016; Scheinkman and Xiong 2003), elevated trading volumes (Hales 2009; Harris and Raviv 1993; Karpoff 1987; Kim and Verrecchia 1991, 1994, 1997), and cost of capital-belief sensitivity (Bloomfield and Fischer 2011), it pays less attention to what behavioral factors cause such heterogeneous beliefs. Generally, the literature finds that disclosure reduces information asymmetries across investors (Diamond and Verrecchia 1991; Leuz and Wysocki 2016). In contrast, holding information endowments constant, our study identifies a disclosure-induced distortion in identified versus non-identified investors' perceptions of management credibility and their company valuations.

Second, our study is among the first to show how, under identical economic conditions, simple alternative framing of an event can lead to widely varying perceptions of management credibility. While prior studies show that investors' credibility perceptions are sensitive to managerial attributions for negative news (Cade, Kaplan, and Loftus 2019; Chen, Han, and Tan 2016; Elliott, Hodge, and Sedor 2012), these findings do not imply that identical disclosure may cause investors to disagree. Our results show that employing external attribution is particularly disadvantageous to unidentified investors' credibility perceptions and company valuation. Thus, somewhat counterintuitively, companies are better off acknowledging own responsibility for failures by using internal attribution, rather than pointing toward outside factors. This holds particularly for (young) companies that seek to actively widen their investor base by reaching out to prospective investors.

Finally, our findings contribute to the larger accounting literature on the determinants of managerial credibility (Mercer 2004). This literature provides evidence that investors' assessment of managerial credibility is sensitive to disclosure language fluency (Rennekamp 2012), the medium of disclosure (Grant, Hodge, and Sinha 2018), peer firm reporting (Maletta and Zhang 2014), the plausibility of managerial explanations (Barton and Mercer 2005), and disclosure forthcomingness (Mercer 2005). Implicit in these studies is the idea that investors do not differ in their response to financial disclosure. Recent research however supports the idea that stock ownership can affect investors' tastes and preferences (Bernard et al. 2018). We expand upon this idea and provide evidence on how shareholder identification influences perceptions of management credibility, dependent on management attribution.

The remainder of this paper is structured as follows. Section 4.2 discusses related literature and develops our hypotheses. Section 4.3 describes the methodology, section 4.4 discusses our empirical results, and section 4.5 closes with a conclusion and discussion.

4.2 Literature Review and Hypothesis Development

Research on corporate disclosure identifies several benefits of increased transparency, such as a reduction in companies' cost of capital (Francis, Khurana, and Pereira 2005; Lambert, Leuz, and Verrecchia 2007; Leuz and Schrand 2009) and an increase in share liquidity as a result of lower information asymmetry among investors (Lang, Lins, and Maffett 2012; Leuz and Verrecchia 2000). While these welfare-enhancing benefits of disclosure are well documented (Leuz and Wysocki 2016), the literature typically does not distinguish differential effects of disclosure across different types of investors. Even though the liquidity and trading effects of investor disagreement about company disclosures are studied and understood (Bamber, Barron, and Stober 1999; Karpoff 1986; Kim and Verrecchia 1994, 1997) the origins of such disagreement and, more specifically, the potential role of investor characteristics in disclosure interpretation receive less attention. To our knowledge, only Hales (2007) provides a related study. He finds that directional preferences induce long and short investors to agree with information that suggests they might make money, leading to heterogeneous beliefs. While his study centers on investors' incentives, our study focuses on disagreement due to divergence in perceived management credibility.[58]

Credibility is a key concern of management and affects its communication with investors (Graham, Harvey, and Rajgopal 2005). Following Mercer (2005), we define management credibility as a "trait of a firm's managers, referring to investors' perceptions of managers' *competence* and *trustworthiness*" (Hovland, Janis, & Kelley, 1953, p.21, emphasis added). We define a manager as trustworthy if perceived as possessing the characteristics of a person that can be trusted (Mayer, Davis, and Schoorman 1995). As a major component of credibility, managerial trustworthiness is an important input to investors' judgment and decision making (Elliott et al. 2012; Hewitt, Hodge, and Pratt 2019).

[58] Hales' (2007) results do not imply a systematic difference in perceptions of managerial credibility between identified investors (shareholders) and non-identified (outside) investors, nor such difference depending on locus of attribution. Further differences ensue from Hales' (2007) consideration of the market for derivatives, versus our examination of the spot market. Investors have no directional preferences when considering a company they are not invested in; i.e. they do not hold a short position, thus benefiting from lower earnings, as in Hales (2007).

Prior experimental accounting literature investigates determinants of managerial credibility and its role for investors' interpretation of financial disclosures. Specifically, research shows that the means that managers can use to influence perceptions of their credibility include forthcomingness about negative news (Mercer 2005), norm-compliant accounting choices (Clor-Proell 2009), disclosure readability (Rennekamp 2012), locus of attribution (Chen et al. 2016), disclosure medium (Grant et al. 2018), and social media use (Cade 2019). The literature also shows that managers benefit from increased credibility, as it helps mitigate unfavorable investor responses to bad news (e.g., Chen et al. 2016; Cade 2019), increases investors' willingness to investment (Clor-Proell 2009; Grant et al. 2018), or increases investors' reliance on the information disclosed (Rennekamp 2012). Of key importance to our study is prior studies' finding that especially disclosures conveying *negative* news can affect investors' perceptions of managerial trustworthiness, and hence, credibility (Chen et al. 2016; Elliott, Grant et al. 2018; Mercer 2005). We investigate how company identification and locus of attribution jointly affect perceived management credibility, and consequently valuation, in a setting involving negative news.

4.2.1 Company Identification

Based on Social Identity Theory, we posit that non-professional investors identify with the companies they are invested in. This theory describes identification as a process that makes individuals self-categorize as members of an "in-group" and think like representatives of a group instead of as individuals (Tajfel and Turner 1986). Such self-categorization leads to emotional and evaluative commitment to the in-group, also known as in-group favoritism (Bauer 2015; Tajfel 1981, p. 229).

Prior research finds that auditors, specialists and employees identify with their organizations, clients and colleagues (Bauer 2015; Bauer, Estep, and Malsch 2019; Kelly and Presslee 2017). In a similar vein, we argue that shareholders, who actively and voluntarily decide to become owners of a company, will start feeling associated with their invested-in company. As they review financial disclosures, they associate the valence of the news with the organization and experience it as their own success or loss (Akerlof and Kranton 2000; Gino and Galinsky 2012; Kelly and Presslee 2017). Despite the potential for conflicts of interest (Jensen and Meckling 1976), shareholders find themselves in circumstances that are conducive to social identification. That is, they share a common fate with managers (e.g. benefits from an increase in stock price) (Cameron 2004), have similar goals (i.e. fundamental economic development of the company) (Rousseau 1998) and are, like team members, mutually dependent on collaboration (e.g. accepting management proposals at annual meetings) (Towry 2003). Managers' use of inclusive language in company disclosures addressing shareholders (as 'we'), makes shareholders' stake and involvement in the company even more salient, such that they categorize themselves and management as part of the in-group.[59] Moreover, shareholders are likely aware of competing companies in the same industry (i.e. the "out-group"), which fosters their bond with the invested-in company (Hinkle, Taylor, Fox-Cardamone, and Ely 1998). Thus, shareholders likely identify with the company they are invested in — more so than outside investors. This premise is in line with survey evidence in the marketing

[59] For example, in his speech at the firm's annual general meeting in June 2016, Volkswagen AG's CEO Matthias Müller addressed the recent emissions scandal in the following way: "*From discussions I have had, (...) I am fully aware that for many of you, Volkswagen represents much more than just a simple investment. Many shareholders feel a profound attachment to the group and its employees (...).*" (Müller 2016)

literature showing that shareholders experience affect-based investment motivation towards their companies (Aspara and Tikkanen 2008; Usul, Özdemir, and Kiessling 2017).

Although shareholders represent a prime example of investors who likely identify with their company, the insights we generate are likely to generalize to other investors who identify with a company.[60] Therefore, company identification represents our core construct. In the hypotheses section, we argue that company identification affects perceived management credibility, in tandem with locus of attribution.

4.2.2 Locus of Attribution

Managers frequently provide causal explanations along with disclosures such as earnings guidance (Baginski and Hassell 2004), linking them to internal (i.e. their actions or inactions) or external causes (e.g. actions of third parties, market circumstances). While we acknowledge that economic reality can influence how managers present the causes of negative news, managers nevertheless have leeway in how to frame their causal explanations. Without stretching the truth, managers can communicate news about an adverse event by emphasizing their own responsibility, or by attaching importance to the situational circumstances that have led to the event (see e.g. Chen et al. 2016). Internal and external attributions can have various effects.

External attributions can make managers appear deceptive, self-absorbed and ineffectual 'excuse-makers', who fail to assume responsibility (Kim, Dirks, Cooper, and Ferrin 2006). However, external attributions can also help reduce

[60] It is also possible that investors identify with companies before they invest in them, for example because they are consumers of the products and services (Bhattacharya and Sen 2003) or because they sympathize with the company's purpose (Hoegele, Schmidt, and Torgler 2014).

blame and anger if verified by the receiver of the communication (Crant and Bateman 1993; Weiner, Amirkhan, Folkes, and Verette 1987). For internal attribution, the literature also documents varying investor interpretations. On the one hand, internal attribution signals that a manager accepts responsibility and might therefore take corrective actions in the future (Kim et al. 2006). On the other hand, internal attribution focuses investors' attention on managers' misdeeds (Shaw, Wild, and Colquitt 2003).

Studies on how investors use and interpret different attributions of adverse events provide mixed evidence. Baginski and Hassell (2004) find that external rather than internal attributions drive abnormal market returns to management forecasts, which they ascribe to external attributions' greater verifiability. Relatedly, Elliott et al. (2012) provide experimental evidence showing that attributions provided in an earnings restatement interact with disclosure medium. When the restatement is presented in a video, the authors argue that heightened sensory information emphasizes the deceitful character of external attributions, and find that trust suffers. A textual format, however, leads to equal levels of trust for internal and external attributions. Overall, the mixed research findings suggest that investors' response to locus of attribution varies based on the circumstances. In the next section, we surmise that locus of attribution interacts with company identification to influence perceived management credibility.

4.2.3 Hypothesis

In this section, we predict how company identification and locus of attribution in adverse news disclosure interact to affect credibility. Psychology research suggests that competence and trustworthiness are two distinctly different dimensions of credibility, so we consider them separately in our theory building (Cuddy, Glick, and Beninger 2011).

Negative news, such as a guidance cut, sends an adverse signal about the *competence* of management. Individuals like to see themselves and their in-group as successful, skilled, intelligent and qualified, or in short, as competent (Ashforth and Mael 1989; Haslam et al. 2006). Hence, we argue that shareholders who identify themselves with the company (Aspara and Tikkanen 2008; Tajfel 1981p. 229) will be less inclined than non-identified outside investors to interpret negative news as adversely diagnostic of a manager's competence. Note that we do not expect that locus of attribution influences perceptions of competence. In a setting similar to ours, Chen et al. (2016) show that investors' competence judgments are not influenced by the framing of attributions in earnings guidance.

To predict the effect of our independent variables on the second component of credibility, *trustworthiness*, we build on ultimate attribution error theory. We first examine the effect of company identification on trustworthiness under *external attribution*. Ultimate attribution error theory holds that individuals who identify with others are more likely to attribute undesirable outcomes realized by in-group members to external forces, such as bad luck, while they are inclined to internally attribute similar outcomes for the out-group (Pettigrew 1979). The reason for this attribution bias is that individuals are usually aware of their own environment and the external forces that directly affect them and their in-group members, but they are less familiar with external forces that affect others (Bloomfield and Luft 2006; Miller and Ross 1975). Based on the insight that investors likely come up with external attributions for negative events that affect their in-group, we argue that they are also inclined to believe a given external attribution provided by management (Hewstone 1990). Therefore, we expect that they will perceive management as trustworthy. For non-identified investors, external attributions likely have a contrasting effect. They see management as part of the out-group, and according to ultimate attribution error theory, they are likely to attribute any failure by management to internal rather than external causes (Brewer 2017). We therefore expect them to see external attributions provided by management as a deceptive attempt to shift attention away, evoking a perception of low management trustworthiness. Thus, we expect that company identification has a positive effect on trustworthiness, given external attribution.

In case of *internal attribution,* we expect that company identification does not affect trustworthiness. Following ultimate attribution error theory, absent any explanation and attribution by management, non-identified investors likely attribute an adverse event to internal causes. Thus, when managers take responsibility by providing an internal attribution, this fits the prior beliefs of non-identified investors and is likely perceived by them as trustworthy. Identified investors, on the other hand, may be more inclined to attribute the adverse event to external causes, absent any explanation by management. However, because they exhibit in-group favoritism and have an inclination to trust management (Bauer 2015; Bernard et al. 2018; Tajfel and Turner 1986), the mismatch between their and management's attributions is not likely to reduce trustworthiness. Instead, we argue that identified as well as non-identified investors likely see an internal attribution as believable and trustworthy, given that such admission of responsibility conflicts with managers' inclination and incentive to shift the blame away from themselves.

Thus, we argue that company identification is not likely to affect investors' trustworthiness perceptions when management makes use of internal attribution.

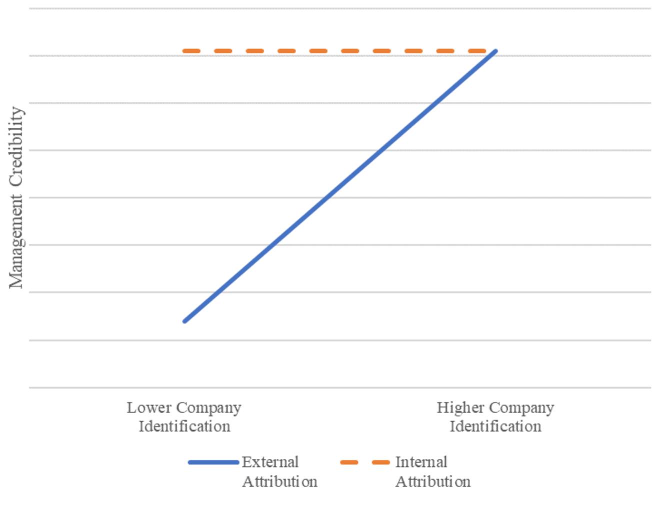
Above, we have separately evaluated the effects on competence and trustworthiness, and we now finalize our prediction regarding credibility. We argue that trustworthiness is the main component driving differences in credibility perceptions, for two reasons. First, as outlined above, for competence only a main effect of company identification is indicated, while we expect our independent variables to interact in influencing trustworthiness. Thus, even if the two components of credibility would have equal weight, the data pattern is likely to be mostly shaped by the interactive effect on trustworthiness. Second, and more importantly, psychology research demonstrates that judgments of trustworthiness are made more intuitively and quicker than judgments of competence (Willis and Todorov 2006), and that they have a greater impact on overall attitudes towards others (Cuddy et al. 2011; Wojciszke, Bazinska, and Jaworski 1998). In line with this, Elliott et al. (2018) show that perceptions of managerial trustworthiness can mitigate negative investor responses to unfavorable earnings news. Moreover, accounting research utilizes investor perceptions of managerial trustworthiness as a proxy for credibility, because competence perceptions appear to be of lesser importance (Chen et al. 2016). Based on these studies, we expect trustworthiness to be the primary driver of management credibility.

In sum, we therefore predict similar effects for credibility as for trustworthiness. We expect that locus of attribution moderates how investors' level of company identification affects perceived management credibility. Both identified and non-identified investors will likely judge internal attribution as credible, but the latter group will judge external attribution as less credible than the former group. As a result, company identification affects perceptions of management credibility more strongly under external than internal attribution. Our expectation is visualized in Figure 4.1, Panel A, and formally stated below:

> **H1**: *Company identification and locus of attribution jointly influence perceptions of management credibility, such that company identification positively affects perceived management credibility under external attribution, but not under internal attribution.*

Figure 4.1: The Effect of Company Identification on Management Credibility for External and Internal Attributions

Panel A: Management Credibility - Predicted Pattern



Panel B: Management Credibility – Results

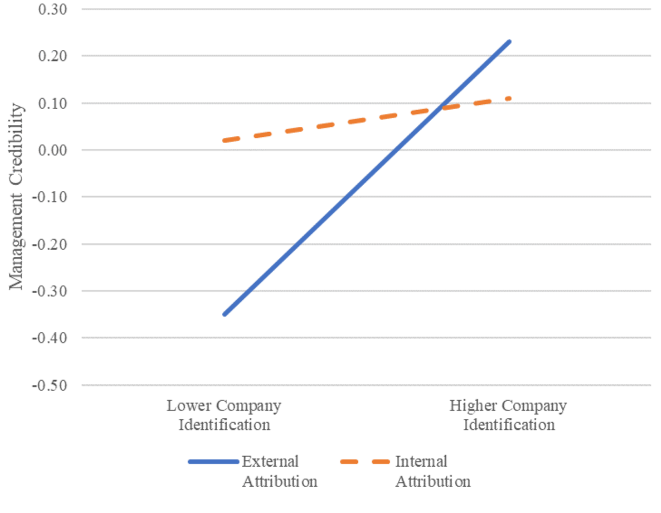


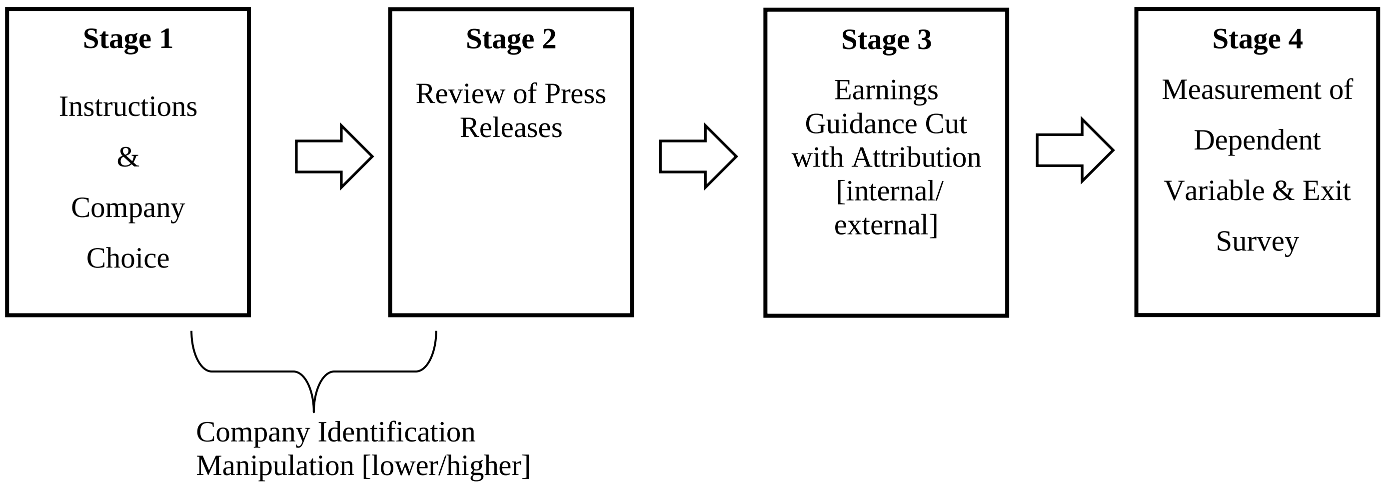
Figure 4.1 reports the predicted pattern (Panel A) and actual results (Panel B) for investor perceptions of management credibility.

4.3 Method

4.3.1 Experimental Design

We conduct a 2 x 2 between-subjects experiment in which we manipulate *company identification* (lower or higher) and *attribution* (internal or external). Figure 4.2 visualizes the structure of our experiment. Upon arrival at the lab, participants sign an informed consent form, are randomly assigned to conditions, and receive instructions about the experimental procedures. We ask participants to take an investment decision in which they choose between two hypothetical airline companies with data based on actual company data, as explained in more detail below. They are incentivized as investors. To reinforce participants' understanding of the payoff structure, we ask them related comprehension questions before they continue. They proceed to review their chosen (higher identification) or non-chosen (lower identification) company's press releases, after which they receive an earnings guidance cut statement with an external or internal attribution. Finally, we measure our dependent and process variables, demographics and control variables. Participants receive their show-up fee in cash, before they leave the lab.

Figure 4.2: Timeline of Experimental Study



The study takes place at the experimental lab of a major Western European business school and lasts approximately 40 minutes. The average pay amounts to EUR 12.-. Participants are 119 graduate students in Business-related Master programs and have attended on average (at least) 5.68 (2) courses in Accounting or Finance.[61] The average age is 24 (s.d.= 2.23) and 56.3% identify as male. Eighty-nine percent of participants have prior investment experience or plan to invest in the future.[62] These demographics suggest that our sample is representative of nonprofessional investors in financial markets and suitable for our experiment since our design is relatively low in integrative complexity (Elliot, Hodge, Kennedy and Pronk 2007).

4.3.2 Independent and Dependent Variables

We first discuss our manipulation of *company identification*. Social Identity Theory holds that individuals tend to identify more strongly with a group that they choose to join (Burger 1999). Hence, we present participants with a choice to invest in one of two hypothetical airline companies, LowCo Air or CheapFly Air.[63] For each company, we present three different financial ratios

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[61] 120 participants took part in our study, but we exclude the responses from one participant who gave an unreasonable estimate of future earnings of only EUR 0.07 while the range of the forecast was EUR 1.17 – 1.19. This is the only observation that is more than three standard deviations away from the mean of EUR 1.14. If we include this observation, our inferences remain identical, including those for our credibility analysis and our SEM model analysis. The only difference is that the contrast analysis for company valuation (see Appendix) would become insignificant (p=0.25) if we kept this extreme outlier in our sample.

[62] Controlling for investment experience and intentions does not qualitatively change the conclusions we draw throughout the paper.

[63] Controlling for the company choice (LowCo Air or CheapFly Air) leads to qualitatively similar inferences, so we do not control for this variable in our reported analyses. Additionally, we pre-test the investment choice on undergraduate business students (n = 57) in their final year at the same university as our participants. Our pre-test reveals that one company is not selected more often than the other (χ^2 (1)=.16, p=0.69, two-tailed). Furthermore, we vary the order of company presentation between left and right, which does not matter either for the investment choice (χ^2 (1)=0.45, p=0.50, two-tailed). Additionally, when rating the attractiveness of both companies on a 7-point Likert scale, participants' answers do not

and four short statements about the company's strategy and operations. To ensure that both companies are highly comparable and presented in the same manner, we derive all financial ratios from the actual financial statements of the same real company and carefully select the company descriptions.[64] This design element follows prior studies in experimental accounting research (Elliott, Rennekamp, and White 2018; Fanning, Agoglia, and Piercey 2015; Thayer 2011).

After choosing a company, participants are randomly allocated to the lower or higher company identification condition. All subsequently received information disclosures, which form the basis for participants' credibility judgements, are identical for both groups. We vary the company name in the disclosures, so that the disclosed information either pertains to the chosen company (higher identification condition; 'shareholders') or the non-chosen company (lower identification condition; 'outside investors'). For ease of exhibition, in *our paper* (not the instrument) we label the company to which the disclosures pertain and for which all participants judge management credibility the *focal company*. Half of our participants are invested in this focal company (higher identification).

deviate significantly from the mid-point labelled 'indifferent' (t_{55}=0.71, p=0.48, two-tailed). Further, we cannot reject the null hypotheses that the observed company choice frequencies equal the levels of chance (χ^2 (1)=1.02, p=0.31, two-tailed). Similarly, we find in our experiment that the decision to invest in a certain company is neither related to our dependent variable management credibility (t_{117}=0.37, p=0.71, two-tailed), nor to its components trustworthiness (t_{117}=0.27, p=0.79, two-tailed) and competence (t_{117}=0.59, p=0.56, two-tailed). We discuss measurement of these variables in a later section.

64 In the exit survey, we ask participants whether they can guess the name of the actual low-cost airline. Controlling for the 23 (19.33%) participants who correctly answered does not change our main results. Therefore, we do not exclude these participants and report results without this control variable.

Additionally, we make it very salient to participants which company they are invested in by repeatedly referring to their chosen company as "your company", and to the non-chosen company as "the other company". We employ further identification-triggering cues, which are held constant across lower and higher company identification groups, to further strengthen our manipulation of company identification. Based on Social Identity Theory, we expect that only investors who cognitively accept to be part of the focal company pick up certain identification-triggering cues that foster the bond between shareholders and their company. The identification triggering cues we employ involve (a) mentioning of positive aspects about the focal company in four consecutive press releases (e.g., profits, traffic growth, shareholder approval), and (b) ending each release with a quote by the CEO, mentioning high satisfaction with shareholders and using the pronoun "we" (e.g. "*I am confident that, together with our shareholders, we will build <Focal Company> into an even more efficient, long-term oriented company*").[65] In sum, in line with identification as a process (Postmes, Haslam, and Swaab 2005), we employ cues that should further strengthen the company identification of those participants who have invested in the focal company (shareholders).

We manipulate our second independent variable, *attribution*, as follows. In the guidance cut statement, the CEO indicates that the company is reducing its earnings per share (EPS) guidance by 5 percent, to a lower range, and explains this by referring to the "*recent sharp decline in Pound Sterling*". We manipulate whether the focal company's CEO explains the guidance cut with

[65] Social identity theory is based on individuals' inclusion of other in-group members into the self-concept and a distinction from out-group members. Following prior literature (Haslam et al. 2006), we also ask shareholders (outside investors) to note one thing they would have done similarly to (differently from) management after reading a CEO statement on current company performance.

an internal or external attribution. For the internal (external) attribution group, we formulate this text by using more active (passive) speech, e.g. "*more than we had expected*" (vs. "*more than was expected*"), and by stating that the CEO assumes (no) responsibility for the decrease in results and speaks of an inaccurate estimation (unpredictable macroeconomic event). Furthermore, the CEO either claims that "*precautionary measures were not taken*" (internal attribution) or mentions that "*the decline was out of our control*" (external attribution). The difference therefore lies in whether the text emphasizes dispositional (i.e. internal) or situational (i.e. external) causes. Our manipulation is in line with prior research (Asay, Libby, and Rennekamp 2018; Henley, Miller, and Beazley 1995).[66]

We measure our dependent variable *management credibility* with several items (see Table 3.1, Panel A), using an instrument adapted from Mercer (2005). We add an item directly addressing credibility (item 8) as well as a statement about integrity (item 5), since the literature identifies integrity as an important indicator of trustworthiness (Mayer et al. 1995). The factor structure is verified by a confirmatory factor analysis, where all items load on the same construct.[67] We retain all items and use the factor loadings to

[66] Similar to prior research in accounting (Kelly and Presslee 2017; Rennekamp 2012; Towry 2003), we employ a compound manipulation. The theoretical construct *locus of attribution* varies naturally and inevitably with the choice of language. To illustrate, Asay et al. (2018) show that negative news induce managers to use more attributions, complex language and less personal pronouns. Similarly, communications research demonstrates that external attributions are combined with the use of passive voice in newspaper articles (Henley, Miller, and Beazley 1995).

[67] For this analysis, we co-vary the error terms of items 6 and 7 since they both refer to honesty, just reverse-coded. The global fit is good with $\chi^2(19)=24.08$, $p=0.19$ two-tailed, $\chi^2/df = 1.27$, Standardized Root Mean Squared Residual (SRMR) = .05, and Comparative Fit Index (CFI) = 0.99. Acceptable fit is indicated by the insignificance of χ^2, χ^2/df below 2:1, SRMR <.11 and CFI > .90 (Kline 2015; Tabachnick and Fidell 2007).

compute a factor score that assigns values of management credibility to each observation.

4.3.3 Incentive Structure

A show-up fee of EUR 5.- is paid out in cash to every participant at the end of the experimental session. In addition, we provide participants with incentives similar to those of actual investors. We adapt the incentive structure from Hales (2007) to fit our research purpose. Participants can collect up to 100 bonus points in our experiment, divided among three bonus components. First, they receive 30 bonus points for participating in the study (participation bonus).

Second, participants can earn up to 30 bonus points depending on the earnings performance of their *chosen company*. This *earnings stake* goes up or down by one bonus point for every cent that the earnings of participants' chosen company deviate from the market consensus for the current reporting period. This design element, together with the company choice, reflects experimental realism that increases the strength of our company identification manipulation (Kachelmeier and King 2002; Swieringa and Weick 1982). It ensures that participants share a common fate with managers (Cameron 2004) since both their payoffs depend on the future uncertain performance of a real company.

Finally, participants can earn an *accuracy bonus* of up to 40 bonus points for an accurate forecast of the *focal company's* earnings per share (EPS). After the issuance of the guidance cut, we provide subjects with an overview of the company's quarterly EPS realizations over the past 4 years and reveal that the company "is currently forecasting EPS in the range of EUR 1.17 – 1.21". We then ask them to give an EPS estimate for the current period. The accuracy bonus decreases by four bonus points for every cent of forecast error, with a lower bound of zero.

To translate bonus points into monetary payoffs, we draw a random number out of 100 for each participant once the entire study is completed. If this random number is lower than or equal to a participant's bonus points total, the participant wins an additional EUR 20.- We emphasize in advance that participants must wait about two months before we can establish their *earnings stake* and *accuracy bonus*, based on the actual EPS at the end of the real company's current reporting period.

4.4 Results

4.4.1 Comprehension Checks

To check participants' understanding of our manipulation of company identification, making participants a shareholder in the focal company or an outside investor, we ask participants: "*In which company are you a shareholder?"*. Of all participants, 95.8% indicate their company (LowCo or CheapFly) correctly. Excluding the five participants who do not answer this comprehension check correctly does not change our inferences, so we retain all observations in our analyses. We conclude that participants are sufficiently aware whether they evaluate the focal company from the perspective of a shareholder or that of an outside investor.

To check whether participants took note of our manipulation of attribution, we ask participants to rate their agreement on a 7-point Likert scale with statements (abbreviated here) relating that management (1) "…*blamed themselves*…" (internal attribution) or (2) "…*blamed …exclusively an external event out of their control.*" (external attribution) for the lower earnings in the prior fiscal year. There is a significant negative correlation between responses to the two items ($r=0.81$, $p<0.01$).[68] On the second item, participants in the external attribution condition (mean=5.92) score higher than those in the internal attribution condition (mean=3.71) and the difference is significant ($t=9.00$, $p<0.01$). Similarly, participants in the internal attribution condition score higher (mean=4.59) on the first item than participants in the external attribution condition (mean=2.10) and the difference in means is significant ($t=9.62$, $p<0.01$). We conclude that attribution is salient as intended.

4.4.2 Hypothesis Testing

Table 4.1 reports the mean scores per condition for *management credibility* in Panel B, which shows that participants perceive management as least credible in the lower identification-external attribution condition (-0.35). Their credibility assessment is much higher when given an internal attribution (0.02). In the higher identification condition, the difference in credibility perception is much smaller: shareholders seem slightly more positive with external (0.23) than with internal attribution (0.11). This data pattern is consistent with our expected pattern, as can be seen in Figure 4.1, Panels A and B.

[68] All p-values are one-tailed for directional testing, unless specified otherwise.

Table 4.1: Management Credibility

Panel A: Measurement Items for Management Credibility

1. < *Focal Company* >'s management is competent at providing financial disclosures.
2. < *Focal Company* >'s management has little knowledge of the factors involved in providing useful disclosures. [reversed]
3. Few people are as qualified as < *Focal Company* >'s management to provide useful disclosure about < *Focal Company* >.
4. < *Focal Company* >'s management is trustworthy.
5. < *Focal Company* >'s management behaves with integrity.
6. < *Focal Company* >'s management is honest.
7. < *Focal Company* >'s management may **not** be truthful towards shareholders in their financial disclosures. [reversed]
8. *I find* < *Focal Company* >'s management credible.

Panel B: Descriptive Statistics for Management Credibility; Mean (s.e.)

	Company Identification		
Attribution	Lower	Higher	Mean (s.e.)
Internal	0.02	0.11	0.06
	(0.13)	(0.14)	(0.10)
	n = 30	*n* = 29	*n* = 59
External	-0.35	0.23	-0.06
	(0.13)	(0.13)	(0.09)
	n = 30	*n* = 30	*n* = 60
Mean (s.e.)	-0.17	0.17	
	(0.09)	(0.10)	
	n = 60	*n* = 59	

Panel C: Analysis of Variance on Management Credibility

Source	Sum of Squares	*df*	Mean Square	*F*-Statistic	*p*-Value
Company Identification	3.33	1	3.33	6.16	<0.01
Attribution	0.48	1	0.48	0.89	0.35 [a]
Company Identification x Attribution	1.80	1	1.80	3.34	0.07 [a]
Error	62.16	115	0.54		

Panel D: Planned Contrast and Simple Effects Tests for Management Credibility

Source	*df*	*F*-Statistic	*p*-Value
Planned Contrast			
Weight -3 for Lower Company Identification - External Attribution, and +1 for the other three conditions.	1/115	9.25	<0.01
Simple Effects			
Effect of Company Identification given External Attribution	1/115	9.36	<0.01
Effect of Company Identification given Internal Attribution	1/115	0.21	0.65 [a]
Effect of Attribution given Lower Company Identification	1/115	3.87	0.03
Effect of Attribution given Higher Company Identification	1/115	0.39	0.53 [a]

[a] indicates a two-tailed test.

Management credibility is measured by items 1-8 from Panel A, using factor loadings. For participants in the higher (lower) company identification manipulation, the name of participants' (non-)chosen company was inserted in the field <*Focal Company*>. *Company identification* is a dummy variable equal to one for participants who were in the higher company identification condition and zero otherwise. *Attribution* is a dummy variable that equals zero for internal attribution and one for external attribution. All p-values are one-tailed (unless otherwise specified) given our directional predictions.

A full-factorial ANOVA as reported in Table 4.1, Panel C, shows a significant main effect of *company identification* (F=6.16, p=0.01), no significant main effect of *attribution* (F=0.89, p=0.35), and a marginally significant interaction effect between our two independent variables (F=3.34, p=0.07, two-tailed).

To formally test the ordinal interaction predicted in hypothesis H1, we must use contrast coding and simple effects (Buckless and Ravenscroft 1990; Guggenmos, Piercey, and Agoglia 2018). In line with our predicted pattern (see Figure 4.1, Panel A), we employ weights of -3 for the lower identification-external attribution condition, and +1 for the other three experimental conditions. Table 4.1, Panel D, shows that the contrast is significant (F=9.25,

$p<0.01$). Importantly, the residual between-cells variance does not reach significance ($F=0.60$, $p=0.55$ two-tailed), while q^2, the proportion of between-cells variance not explained by the contrast, equals 0.12 (Guggenmos et al. 2018). These results indicate that the contrast fits the data well and support our hypothesis.

Results of the simple effect analyses, as outlined in Panels B and D of Table 4.1, show that with external attribution, lower company identification leads to lower management credibility (mean=-0.35) than higher company identification (mean=0.23). The difference is significant ($F=9.36$, $p<0.01$), confirming a positive effect of company identification given external attribution. In contrast, given internal attribution, there is no significant effect of company identification on investors' perception of managerial credibility ($F=0.21$, $p=0.65$, two-tailed). These results are in line with our reasoning and prediction. When managers frame adverse events in terms of external attributions, differences in company identification lead to varying interpretations of disclosure, leading to disagreement about management's credibility.

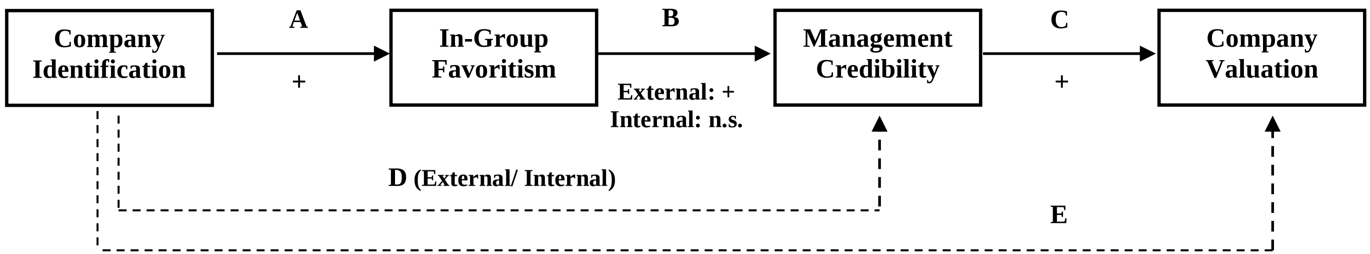
We also find that investors with lower company identification find management significantly less credible if the adverse event is attributed to external as opposed to internal factors (means of -0.35 and 0.02, respectively; $F=3.87$, $p=0.03$). When company identification is higher, perceptions of management credibility are roughly equal for both types of attribution ($F=0.39$, $p=0.54$, two-tailed).

4.4.3 Structural Equation Model

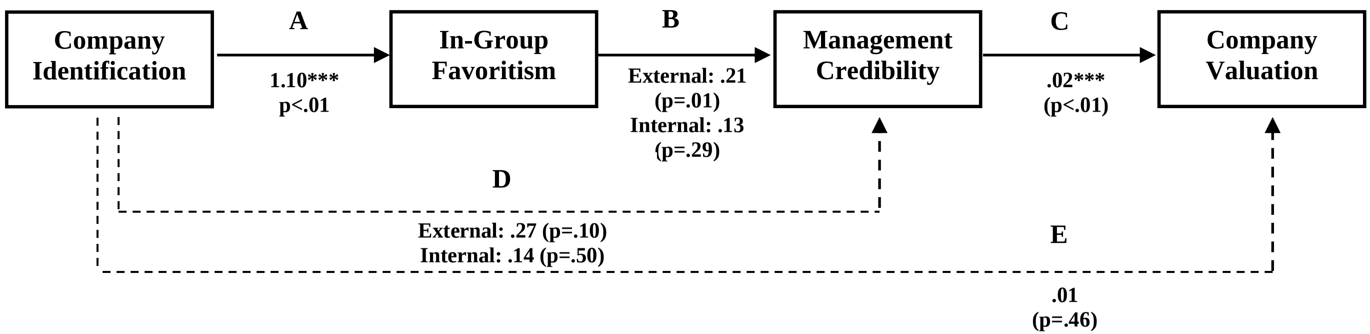
We employ a Structural Equation Model (SEM) to analyze the underlying process through which company identification affects credibility and valuation judgments. To analyze the effect of our second independent variable, *attribution*, we run a multi-group analysis with the external and internal attribution subgroups. This approach has the advantage of minimizing the problem of multicollinearity that potentially arises with interaction terms (Aiken, West, and Reno 1991, p.35; Rigdon, Schumacker, and Wothke 1998). In addition, it reduces the complexity of the model substantially and is therefore also preferable in terms of statistical power (Kline 2015, p.15).

Figure 4.3: Multi-Group SEM Analysis

Panel A: Theoretical Model



Panel B: Results



Note: *p < .10, **p < .05, ***p < .01, one-tailed.

This figure reports the theoretical model and empirical results for a Structural Equation Model (SEM) multi-group analysis, using external and internal locus of attribution conditions as subgroups. Reported path coefficients are unstandardized. We restrict the path coefficients and intercept terms to be equal across the two subgroups on all links except links B and D, as our theory predicts only the effect of Company Identification on Management Credibility to differ dependent on Locus of Attribution. Dotted lines represent control paths. The model has an acceptable global fit as indicated by insignificance of $\chi^2(8)=13.28$, $p=0.103$, $\chi^2/df = 1.66$, Standardized Root Mean Squared Residual (SRMR) = .05 and Comparative Fit Index (CFI)=0.92 (Kline 2015; Tabachnik and Fidell, 2007). *Company identification* is a dummy variable equal to one for the higher company identification condition and zero otherwise. *In-group favoritism* is based on three 7-point Likert scale items, weighted by their factor loadings: "*I would describe an affiliation with <Focal Company> as fortunate*", "*I feel that I am a part of <Focal Company>*" and "*I like <Focal Company>*" (Kelly and Presslee 2017). *Management credibility* is a variable based on items 1 - 8 from Table 4.1 Panel A. *Company valuation* is measured by participants' estimate of the focal company's EPS at the endof the current reporting period.

To corroborate our theory, we analyze whether the effect of *company identification* on *management credibility* is mediated by *in-group favoritism*. We expect a significantly positive path from *company identification* to *in-group favoritism* (Link A), which is our process measure of positive affect (Kelly and Presslee 2017). Notably, we expect a positive path coefficient between *in-group favoritism* and *credibility* for the external attribution group, but no significant relation between these two variables in the internal attribution group (Link B). These expectations are consistent with our main prediction that company identification affects credibility, through in-group favoritism, only under external attribution (see Figure 4.1, Panel A).

We further expect that the path from *management credibility* to *company valuation* (Link C) is positive and significant, consistent with prior literature on investor decision making documenting that credibility judgments positively relate to investors' company valuations (Graham et al. 2005; Grant et al. 2018; Mercer 2005). We also incorporate two control paths. Link D connects *company identification* to *management credibility* directly, allowing for both partial and full mediation. The direct link from *company identification* to *company valuation* (Link E) controls for any potential direct effects of our manipulation involving stock ownership, which are unrelated to an identification process through in-group favoritism (e.g., Bernard et al. 2018). Since we expect only Link B (and potentially Link D) to differ among both attribution groups, we constrain the coefficients and intercepts of the remaining links to be equal (for a similar analysis see Elliott et al. 2012). A likelihood ratio test indicates that our model fits the data better than a model that restricts all coefficients to be equal across groups (χ^2 (5)=16.58, p<.01; Kline 2015, p. 281).

We measure the variables in the model as follows. *Management credibility* is a composite measure based on the same eight indicators as used in our analysis of H1, and all other variables are measured as specified and described before. To measure the process variable *in-group favoritism*, we present participants with items regarding their affective attachment to the focal company, right before the issuance of the guidance cut statement.[69] On a 7-point Likert scale, participants indicated to what extent they (a) like the focal company, (b) feel that they are a part of it, and (c) would describe an affiliation with the focal company as fortunate. We adapt these three items from prior studies (Kelly and Presslee 2017). In a principal component analysis with varimax rotation of these three items, a single factor emerges (eigenvalue = 1.81, variance explained: 60.3%), so we retain all three items and use the factor loadings to construct our measure of in-group favoritism.[70] We measure *company valuation* using the EPS estimate, for which each participant receives an accuracy bonus.

We use a maximum likelihood estimation to analyze our path model. The insignificant chi-square ($\chi^2(8)=13.28$, $p=0.103$, two-tailed), the normed chi-square ($\chi^2/df = 1.66$), the Standardized Root Mean Squared Residual (0.05) and the Comparative Fit Index (0.92) all indicate acceptable model fit (Kline, 2015). Figure 4.3, Panel B visualizes the results and details regression weights. *Company identification* positively affects *in-group favoritism* (Link A; $\beta=1.10$, $p<.01$), which in turn increases investor perceptions of *management credibility,*

[69] We measure *in-group favoritism* again after the measurement of our dependent variable (i.e. after the issuance of the guidance cut). The three items of the measurement scale for this variable again load on the same factor in a principal component analysis with varimax rotation (eigenvalue=1.87, variance explained=62.46%). We find that shareholders still experience higher levels of in-group favoritism (mean=0.51) than outside investors (mean=-0.51) after the guidance cut is issued ($t=6.44$, $p<0.01$).

[70] None of the inferences reported throughout the paper changes in nature or significance if we instead use an oblique rotation method (oblimin) in our factor analyses.

but only under external attribution (Link B; β=0.21, z=2.22, p=0.01). Link B is not significant for the internal attribution group (β=0.13, z=1.05, p=0.29, two-tailed). We also test the significance of the indirect effect of *company identification* on *management credibility* through *in-group favoritism*. We find that this indirect effect, the chain consisting of links A and B together, is significant for the external attribution (β=0.23, z=2.11, p=0.02) but not for internal attribution group (β=0.14, z=1.04, p=0.30, two-tailed). *Company identification* does not have a strong direct effect on *management credibility* under internal attribution (Link D - Internal; β=0.14, z=0.67, p=0.50, two-tailed), yet has a marginally significant effect under external attribution (Link D – External; β=0.27, z=1.67, p=0.10, two-tailed). We conclude that *in-group favoritism* partially mediates the effect of *company identification* on credibility. [71] [72] The

[71] Directly regressing *management credibility* on *company identification* for each attribution group separately results in a significant regression coefficient for the external attribution group (β=0.58, t_{59}=3.50, p<0.01,), but not for the internal attribution group (β=0.09, t=0.41, p=0.68, two-tailed). Hence, a significant Link D indicates partial mediation.

[72] Hence, we establish company identification as an important process underlying effects on credibility. Additionally, we measure variables to address potential alternative explanations. Prior research argues that investment choices may create cognitive dissonance, which is the negative uncomfortable emotion that individuals experience when they prefer a particular outcome, but expect a different one (Festinger 1957; Hobson, Mayew, Peecher, and Venkatachalam 2017). More specifically, Brehm and Cohen (1962) note that cognitive dissonance leads to the experience of inner arousal. Investors reduce such emotions by seeking out and using information that confirms their preferred beliefs (Fanning, Agoglia, and Piercey 2015; Hales 2007; Thayer 2011). We do not expect a similar effect in our setting since, unlike prior papers using a similar investment choice manipulation (Elliott, Rennekamp et al. 2018; Fanning et al. 2015), we do not contrast the two companies by incentivizing shareholders to pick the better performing one. To measure participants' inner arousal before and after the guidance cut, we use Self-Assessment manikins, an established and validated measure of emotions (Bradley and Lang 1994). Results indicate no difference between differently identified investors' feelings of inner arousal before (t=1.16, p=0.25, two-tailed) or after (t=0.96, p=0.34, two-tailed) the guidance cut, which alleviates the concern that participants experience cognitive dissonance. Self-Assessment manikins standardly also measure happiness and dominance, two positive emotions that refer to our construct company identification (Aspara and Tikkanen 2008). We find that identified investors are initially happier (mean=3.81) than non-identified investors (mean=3.53) and the difference is significant (t=2.25, p=0.01). They also score higher on our measure of dominance

positive and highly significant coefficient for Link E (β=0.02, z=3.11, p<0.01) indicates that an increase of one unit on the 7-point Likert scale measuring credibility induces investors to give an EPS estimate that is on average 2 cents higher.[73] The effect is substantial considering that the focal company's earnings guidance ranged from EUR 1.17 – 1.21.

In sum, these results are in line with our theory predicting that, in conjunction with locus of attribution, company identification is an important driver of management credibility assessments. Our findings show why investors occasionally disagree on the prospects of a company despite observing the same company disclosure.

4.4.4 Additional Analyses

We analyze the constituent elements of credibility to explore whether our results are driven by competence and/or trustworthiness. We run a principal component analysis with varimax rotation on items 1-7 from our credibility measurement scale (i.e., we exclude the item directly addressing 'credibility'; see Table 4.1, Panel A). Two factors emerge, consisting of items 1-3 (eigenvalue = 1.73) and items 4-7 (eigenvalue = 2.68), respectively. Both factors together explain 63.0% of the total variance. The first factor precisely

(mean=3.41) than non-identified investors (mean=3.22) and the difference is marginally significant (t=1.40, p=0.08). We establish that these differences between shareholders and outside investors disappear when we measure these items a second time (p>0.22, two-tailed), as one would expect when shareholders share a common fate with managers and experience a guidance cut as a truly adverse event. Importantly, and as reported in footnote 69, investors' in-group favoritism is not affected by the guidance cut and this drop in mood.

[73] Because *company valuation* is significantly positively correlated with *management credibility* (r=0.27, p<0.01), we find results that are inferentially identical to those for the dependent variable management credibility if we run a similar ANOVA analysis with *company valuation* as the dependent variable. We report this analysis in the Appendix. More specifically, we again find a significant contrast (p<0.01), as well as two marginally significant simple effects (both p<0.10), representing a data pattern similar to that for *management credibility* as seen in Figure 4.1. Note that our theory specifies that the independent variables interactively affect credibility, as our model shows, and company valuations only statistically and by extension.

aligns with *trustworthiness*, and the second factor with *competence*, as defined and shown in prior literature (Cuddy et al. 2011; Mercer 2004). We construct separate measures of competence and trustworthiness based on the corresponding items and their factor loadings.

Table 4.2 reports our analyses for *competence*. Panel A displays descriptive statistics; Panel B shows results of an ANOVA analysis, which finds a significant main effect of *company identification* only (F=3.79, p=0.03). Participants in the higher company identification condition perceive managers as more competent (mean = 0.18) than those in the lower company identification condition (mean = -0.17). Figure 4.4, Panel A visualizes the data pattern for competence.

Table 4.2: Management Competence

Panel A: Descriptive Statistics for Management Competence; Mean (s.e.)

	Company Identification		
Attribution	Lower	Higher	Total
Internal	-0.12	0.29	0.08
	(0.18)	(0.18)	(0.13)
	n = 30	*n* = 29	*n* = 59
External	-0.23	0.07	-0.08
	(0.18)	(0.18)	(0.13)
	n = 30	*n* = 30	*n* = 60
Total	-0.18	0.18	0.00
	(0.13)	(0.13)	(0.09)
	n = 60	*n* = 59	*n* = 119

Panel B: Analysis of Variance on Management Competence

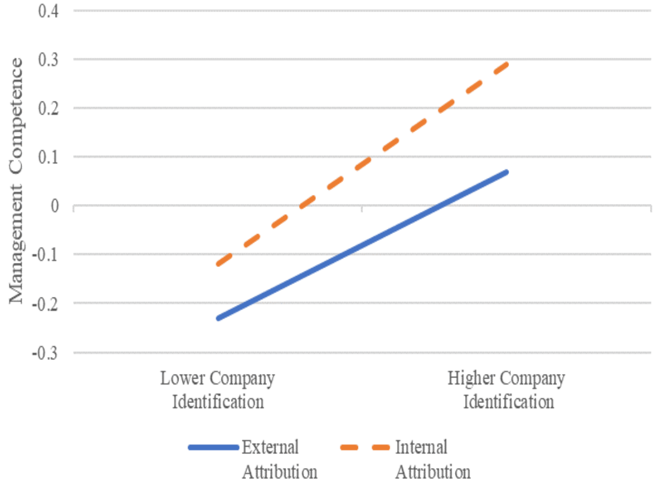
Source	Sum of Squares	*df*	Mean Square	*F*-Statistic	*p*-Value
Company Identification	3.74	1	3.74	3.79	0.03
Attribution	0.79	1	0.79	0.80	0.37 [a]
Company Identification x Attribution	0.11	1	0.11	0.11	0.74 [a]
Error	113.40	115	0.99		

[a] indicates a two-tailed test.

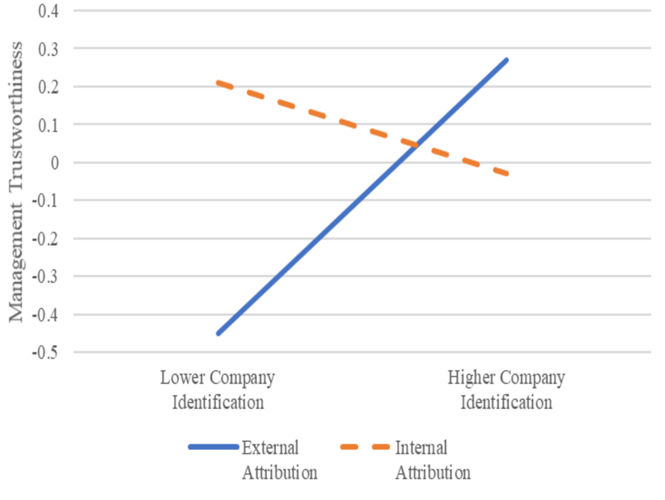
Management competence is measured by items 1-3 from Table 4.1, Panel A, using factor loadings. For participants in the higher (lower) company identification manipulation, the name of participants' (non-)chosen company was inserted in the field <*Focal Company*>. *Company identification* is a dummy variable equal to one for participants who were in the higher company identification condition and zero otherwise. *Attribution* is a dummy variable that equals zero for internal attribution and one for external attribution. All p-values are one-tailed (unless otherwise specified) given our directional predictions.

Figure 4.4: Result Plots for Competence and Trustworthiness

Panel A: Management Competence



Panel B: Management Trustworthiness



This figure visualizes the means for participants' perception of managerial competence and trustworthiness by experimental condition. Both competence and trustworthiness are derived from a principal component analysis on items 1-7 from Table 4.1, Panel A.

While our findings regarding management competence perceptions are in line with our argumentation, they may seem counterintuitive. Survey research indicates that managers are highly concerned about missing an earnings guidance as this may send an adverse signal about their competence as executives and forecasters (Graham et al. 2005, p. 28). Similarly, Asay et al. (2018) report that managers have self-enhancement motives causing them to frame adverse event disclosure in a positive light. Why, then, does locus of attribution not affect perceptions of competence in our setting? Psychology research reports that judgments of others' competence are made less intuitively and more slowly than judgments of trustworthiness (Cuddy et al. 2011; Willis and Todorov 2006). In line with this, and with prior accounting research showing that attributions do not affect competence perceptions (Chen et al. 2016), our results suggest that executives' effort to frame disclosure through locus of attribution in order to influence perceptions of competence is indeed a rather ineffective strategy.

Next, we analyze *trustworthiness*. Table 4.3, Panel A reports the means per experimental cell, and Figure 4.4, Panel B visualizes the data. Notably, the data pattern for trustworthiness appears very similar to that of credibility, which is in line with our expectation that trustworthiness is the main determinant of credibility. Panel B of Table 4.3 shows ANOVA results, and Panel C reports the results of a contrast analysis similar to the one we performed on credibility, but now with *trustworthiness* as dependent variable. We find that the contrast is significant ($F=8.69$, $p<0.01$), while the between-cells residual variance is insignificant ($F=0.84$, $p=0.43$, two-tailed) and the variance not explained by the contrast (q^2) equals 0.16. The simple effects analyses, also reported in Panel C, show that non-identified investors perceive managers who use internal attributions as more trustworthy than those using external attributions and that the difference is significant (means of 0.21 and -0.03, resp.; $F=7.08$, $p<0.01$). Yet, identified investors perceive a manager as equally trustworthy independent of the attribution used ($F=1.45$, $p=0.23$, two-tailed). We also find that managers who use external attribution are perceived as more trustworthy when company identification is higher, and that this difference is also significant (means of 0.27 and -0.45, resp.; $F=8.36$, $p<0.01$). If management decides to attribute the guidance cut internally, there is no significant difference in the perception of their trustworthiness across higher and lower levels of company identification (means of -0.03 and 0.21, resp.; $F=0.95$, $p=0.33$, two-tailed).

Table 4.3: Management Trustworthiness

Panel A: Descriptive Statistics for Management Trustworthiness; Mean (s.e.)

	Company Identification		
Attribution	Lower	Higher	Total
Internal	0.21	-0.03	0.09
	(0.18)	(0.18)	(0.13)
	n = 30	*n* = 29	*n* = 59
External	-0.45	0.27	-0.09
	(0.18)	(0.18)	(0.13)
	n = 30	*n* = 30	*n* = 60
Total	-0.12	0.12	0.00
	(0.13)	(0.13)	(0.09)
	n = 60	*n* = 59	*n* = 119

Panel B: Analysis of Variance on Management Trustworthiness

Source	Sum of Squares	*df*	Mean Square	*F*-Statistic	*p*-Value
Company Identification	1.70	1	1.70	1.81	0.09
Attribution	0.98	1	0.98	1.04	0.31 [a]
Company Identification x Attribution	7.00	1	7.00	7.44	<0.01[a]
Error	108.24	115	0.94		

Panel C: Planned Contrast and Simple Effects Tests for Management Trustworthiness

Source	*df*	*F*-Statistic	*p*-Value
Planned Contrast			
Weight -3 for Lower Company Identification - External Attribution, and +1 for the other three conditions.	1/115	8.69	<0.01
Simple Effects			
Effect of Company Identification given External Attribution	1/115	8.36	<0.01
Effect of Company Identification given Internal Attribution	1/115	0.95	0.33 [a]
Effect of Attribution given Lower Company Identification	1/115	7.08	<0.01
Effect of Attribution given Higher Company Identification	1/115	1.45	0.23 [a]

[a] indicates a two-tailed test.

Management trustworthiness is measured by items 4-7 from Table 4.1, panel A, using factor loadings. For participants in the higher (lower) company identification manipulation, the name of participants' (non-)chosen company was inserted in the field <*Focal Company*>. *Company* *identification* is a dummy variable equal to one for participants who were in the higher company identification condition and zero otherwise. *Attribution* is a dummy variable that equals zero for internal attribution and one for external attribution. All p-values are one-tailed (unless otherwise specified) given our directional predictions.

In conclusion, we find very similar results for trustworthiness as for credibility. Managers' attribution matters in the first place for their communication with investors who do not identify with the company. As a result, managers who decline responsibility for adverse outcomes create uncertainty by inducing identified and non-identified investors to disagree on a managers' trustworthiness and credibility, and in their interpretation of forward-looking disclosures.

4.5 Conclusion and Discussion

While prior research has broadened our understanding how disclosure characteristics drive market reactions to corporate disclosures (Leuz and Wysocki 2016), the role of investor characteristics has received much less attention. Our main objective in this study is to assess how investors' degree of identification with the companies they are invested in, causes differences in their perceptions of management credibility.

We predict and find support for an ordinal interaction between company identification and attribution on judgments of managerial credibility. Our findings indicate that company identification positively affects credibility, but only when managers resort to external attribution. We also find that locus of attribution does not materially affect credibility judgments of identified investors (shareholders), whereas non-identified (outside) investors find management explanations employing external attributions much less believable than explanations using internal attributions. This suggests that shareholders' affective attachment to their companies is so strong that it makes them turn a blind eye to managerial framing of the adverse event. As a result, identified shareholders and non-identified investors disagree on management's credibility when external attribution is employed in disclosures. Results from a structural equation model show that in-group favoritism mediates these differences in management credibility, and that these differences affect investors' valuation judgments. Furthermore, in line with our reasoning, our findings strongly suggest that perceptions of trustworthiness are the main driver of credibility judgments (Cuddy et al. 2011; Willis and Todorov 2006).

Our study has implications for practice and future research. The disclosure literature in accounting generally finds that disclosure increases transparency, which has beneficial effects on market outcomes such as companies' liquidity and cost of capital (Leuz and Verrecchia 2000). Our experiment shows that company disclosure can also induce disagreement among investors regarding managers' credibility and company valuation. This systematic disclosure-induced disagreement may have consequences for trading volumes, as it widens the gap between shareholders' and outside investors' company valuations. More specifically, adding to the literature that finds strong price reactions to managers' use of external attributions (Baginski and Hassell 2004), our findings imply that trading volumes decrease when companies use external attribution in their disclosure of adverse events and shareholders strongly identify with the company. We leave the examination of this corollary to future research.

Moreover, we show that management's choice regarding locus of attribution has consequences for investors' credibility judgments and company valuations. From a practical perspective, our results suggest that external attributions should be avoided when communication is aimed at prospective, non-identified investors who likely view such attributions as deceptive, leading to lower credibility judgments and company valuation. This is an important implication, considering that management's natural inclination may be the exact opposite – that is, to rely on outside factors, rather than admitting their own inadequacies in terms of their (in)actions when explaining adverse results. Our findings also illustrate that company identification is a valuable asset to companies with real effects on the market's interpretation of managerial reporting.

Our study also has its limitations. In line with the growing literature showing shareholders' emotional attachment to their invested-in firms (Aspara and Tikkanen 2011; Bernard et al. 2018; Usul et al. 2017), we assume that shareholders are more highly identified with their company than outside investors and we manipulate our independent variable company identification accordingly. Nevertheless, we acknowledge that there are many factors potentially determining how strongly investors identify with companies, such as the geographic location of the company, and similarities between the CEO and investors. Future research could investigate how these factors influence company identification, how it develops over time, as well as examine what effect company identification has in settings other than a guidance cut. Further, our experimental lab study allows us to draw strong causal inferences about the interaction effect of company identification and locus of attribution on investors' perceived management credibility after an adverse event disclosure. Experimental studies cannot speak to the exact magnitude of this effect, however, and quantifying it presents an interesting challenge for future research.

Appendix: Company Valuation

Panel A: Descriptive Statistics for Company Valuation; Mean (s.e.)

	Company Identification		
Attribution	Lower	Higher	Total
Internal	1.16	1.17	1.17
	(0.01)	(0.01)	(0.01)
	$n = 30$	$n = 29$	$n = 59$
External	1.13	1.16	1.15
	(0.01)	(0.01)	(0.01)
	$n = 30$	$n = 30$	$n = 60$
Total	1.15	1.16	1.16
	(0.01)	(0.01)	(0.01)
	$n = 60$	$n = 59$	$n = 119$

Panel B: Analysis of Variance on Company Valuation

Source	Sum of Squares	*df*	Mean Square	*F*-Statistic	*p*-Value
Company Identification	0.01	1	0.01	1.94	0.08
Attribution	0.01	1	0.01	3.89	0.05 [a]
Company Identification x Attribution	0.00	1	0.00	0.57	0.45 [a]
Error	0.37	115	0.00		

Panel C: Planned Contrast and Simple Effects Tests for Company Valuation

Source	*df*	*F*-Statistic	*p*-Value
Planned Contrast			
Weight -3 for Lower Company Identification - External Attribution, and +1 for the other three conditions.	1/115	5.70	<0.01
Simple Effects			
Effect of Company Identification given External Attribution	1/115	2.33	0.06
Effect of Company Identification given Internal Attribution	1/115	0.20	0.65 [a]
Effect of Attribution given Lower Company Identification	1/115	3.75	0.03
Effect of Attribution given Higher Company Identification	1/115	0.73	0.39 [a]

[a] indicates a two-tailed test.

This table summarizes the effects of our manipulations on participants' valuation of the focal company. *Company valuation* is measured by participants' estimate of the focal company's EPS at the end of the current reporting period. *Company identification* is a dummy variable equal to one for participants who were in the higher company identification condition and zero otherwise. *Attribution* is a dummy variable that equals zero for internal attribution and one for external attribution. All p-values are one-tailed (unless otherwise specified) given our directional predictions.

Conclusion

The purpose of this dissertation is to examine how changes in the information landscape affect the quality of information reported by companies, its dissemination in capital markets and investors' processing of the information. In particular, I investigate experimentally how public scrutinization of companies, changes to the structure of transaction costs and the framing of company disclosure affect capital market communications.

In chapter 2, Jessen Hobson and I contemplate the financial reporting process as a strategic game between managers and investors. Using a decontextualized experimental design, we predict and find that managers avoid exactly meeting benchmarks, such as the analyst earnings consensus, when they report under increased public scrutiny. Consistent with the notion that managers misreport for self-presentational goals (i.e. they lie to appear truthful), we find that this effect is stronger for individuals who score at the lower end of the Dark Triad scale. Thus, it appears that Non-Dark Triad managers, the managers we usually assume to be the good actors, are unable to resist market scrutiny and take welfare-enhancing choices.

Chapter 3 explores how information is disseminated in competitive markets. A recent European regulation requires broker-dealers to charge their clients' separately for the execution of their trades and the provision of investment research. Using experimental markets, I predict and find that investors rely more strongly on forecasted information when transaction costs are unbundled (vs. bundled) and sufficiently high to provoke feelings of regret. Importantly, this bias is reflected at the aggregate market level in the form of impoverished investor learnings. Thus, my results document an unintended consequence of this new regulation. In additional tests, I find that an alternative policy, transaction cost disclosure, does not lead to similar effects while also creating full cost transparency.

Chapter 4, which is joint work with Erik Peek and Marcel van Rinsum, investigates one of the origins of investor disagreement. We posit that shareholders are more likely than outside investors to identify with their selected companies and experience affect-based in-group favoritism. In a fully contextualized experiment, we first manipulate whether participants evaluate a company's management and disclosure from the perspective of a shareholder or an outside investor. Second, we vary whether managers use internal or external attributions when they disclose bad news. Our results suggest that framing adverse events in terms of external attributions leads to different perceptions of a manager's credibility and a company's value. That is, we document a behavioral factor that causes heterogeneous beliefs in financial markets.

Taken together, the three studies of this dissertation illustrate how changes in the information landscape affect communication in capital markets. Companies operate in a constantly changing environment that is shaped by technological innovations, regulatory initiatives and events that are outside of management's direct control. Not only is it crucial to understand how these developments and events directly affect the availability and transparency of information, but it is also important to examine what behavioral forces drive investor-manager communications. Isolating and explaining these forces can shed light on some surprising capital market findings, such as why even conscientious managers manage earnings, why investor protection initiatives do not achieve the desired benefits or why disclosures might induce rather than resolve disagreement among investors.

Samenvatting (Summary in Dutch)

Het doel van dit proefschrift is te onderzoeken hoe veranderingen in het informatielandschap invloed hebben op de kwaliteit van door bedrijven verstrekte informatie, de verspreiding van die informatie op kapitaalmarkten en de verwerking ervan door beleggers. Ik doe voornamelijk experimenteel onderzoek naar hoe overheidstoezicht op bedrijven, veranderingen in de transactiekostenstructuur en framing van bekendmakingen door bedrijven de kapitaalmarktcommunicatie beïnvloeden.

In hoofdstuk 2 bestuderen Jessen Hobson en ik het proces van financiële verslaglegging als een strategisch spel tussen managers en beleggers. Met behulp van een gedecontextualiseerd experimenteel ontwerp doen we de voorspelling, en stellen we vast, dat managers liever vermijden precies te voldoen aan de ijkpunten, zoals de analistenconcensus over de winst, wanneer ze verslaglegging doen onder verhoogd overheidstoezicht. In overeenstemming met het idee dat managers onjuist rapporteren voor zelfpresentatiedoeleinden (d.w.z. ze liegen om waarheidsgetrouw over te komen), stellen we vast dat dit effect sterker is voor personen die laag scoren op de *Dark Triad*-schaal. Het lijkt er dus op dat managers die niet tot de *Dark Triad* behoren – de managers van wie wij gewoonlijk aannemen dat zij oprecht handelen – niet in staat zijn weerstand te bieden tegen het markttoezicht en welvaartsverhogende keuzes te maken.

In hoofdstuk 3 wordt onderzocht hoe informatie wordt verspreid binnen competitieve markten. Een recente Europese verordening verplicht broker-dealers hun klanten afzonderlijk kosten aan te rekenen voor het uitvoeren van hun transacties en het doen van beleggingsonderzoek. Door gebruik te maken van experimentele markten, doe ik de voorspelling, en ontdek ik, dat beleggers meer vertrouwen op aangekondigde informatie wanneer de transactiekosten ongebundeld (vs. gebundeld) en hoog genoeg zijn om spijtgevoelens op te wekken. Belangrijk is dat deze tendens tot uiting komt op het totale marktniveau in een verarming van de kennis die beleggers hebben. Mijn resultaten laten dus een onbedoeld gevolg van deze nieuwe verordening zien. In aanvullende tests ontdek ik dat een alternatief beleid, bekendmaking van transactiekosten, niet tot soortgelijke effecten leidt. Tegelijkertijd zorgt het ook voor volledige kostentransparantie.

Hoofdstuk 4 is in samenwerking met Erik Peek en Marcel van Rinsum tot stand gekomen en hierin wordt een van de oorzaken van de onenigheid tussen beleggers (*investor disagreement*) onderzocht. Wij stellen dat aandeelhouders zich eerder dan externe beleggers identificeren met de door hen geselecteerde ondernemingen en affect-gebaseerde groepsbevoordeling ervaren. Een volledig gecontextualiseerd experiment manipuleren we eerst op zo'n manier dat deelnemers het management en de informatieverstrekking van een onderneming beoordelen vanuit het perspectief van een aandeelhouder of vanuit het perspectief van een externe belegger. Daarna variëren we met het feit of managers interne of externe attributies gebruiken wanneer ze slecht nieuws bekendmaken. Onze resultaten suggereren dat het framen van negatieve gebeurtenissen in termen van externe attributies leidt tot verschillende percepties van de geloofwaardigheid van een manager en de waarde van een bedrijf. Dat wil zeggen, wij documenteren een gedragsfactor die heterogene overtuigingen op de financiële markten veroorzaakt.

Tezamen illustreren de drie studies van dit proefschrift hoe veranderingen in het informatielandschap de communicatie op de kapitaalmarkten beïnvloeden. Ondernemingen opereren in een voortdurend veranderende omgeving die wordt bepaald door technologische innovaties, initiatieven voor regelgeving en gebeurtenissen die buiten de directe controle van het management vallen. Het is niet alleen van cruciaal belang te begrijpen hoe deze ontwikkelingen en gebeurtenissen rechtstreeks van invloed zijn op de beschikbaarheid en transparantie van informatie, maar het is ook belangrijk na te gaan welke gedragsfactoren de communicatie tussen beleggers en managers sturen. Het afzonderlijk onderzoeken en verklaren van deze factoren kan licht werpen op een aantal verrassende bevindingen binnen kapitaalmarkten, zoals waarom zelfs gewetensvolle managers winsten beheren, waarom initiatieven ter bescherming van de beleggers niet de gewenste voordelen opleveren of waarom informatieverstrekking onenigheid tussen beleggers kan uitlokken in plaats van oplossen.

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About the Author

Sebastian Stirnkorb was born on June 27th, 1989 in Künzelsau, Germany. He obtained his Bachelor's degree in International Management from the European School of Business Reutlingen in 2013. In 2016, Sebastian completed his Master's studies in Economics and Management at Humboldt University Berlin. He subsequently joined Erasmus University Rotterdam to begin his doctoral studies at the Rotterdam School of Management's department of Accounting and Control. His research uses experimental methods to examine how changes to firms' information environment, spurred by advances in technology and regulation, affect the judgment and decision making of investors, managers and auditors. During fall semester 2018, Sebastian visited the University of Illinois at Urbana-Champaign. Since September 2021, he has been appointed as Assistant Professor of Accounting at the University of Amsterdam.

Portfolio

Working Papers

- "Managing Earnings To Appear Truthful: The Effect of Public Scrutiny on Exactly Meeting a Threshold"
  - with Jessen Hobson (University of Illinois at Urbana-Champaign)

- "The Effect of Transaction Cost Unbundling on Investors' Reliance on Investment Research: Evidence from Experimental Asset Markets"

- "Not Seeing Eye-to-Eye: Differences between Identifed and Non-Identified Investors' Reactions to Adverse Event Disclosure"
  - with Erik Peek and Marcel van Rinsum (both Erasmus University Rotterdam)

Work in Progress

- "Assessing and Addressing Fraud Risk Based on Earnings Calls: Effects of Focusing Auditors on Fraud vs. on Management's Dissonance"
  - with Jessen Hobson, Mark Peecher, and Devin Williams (all University of Illinois at Urbana-Champaign)

Course Work during PhD

Erasmus Research Institute of Management

- Mathematics and Statistics (Prof. Alex Koning)
- Applied Econometrics (Prof. Marno Verbeek)
- Introduction to Data Visualization, Web Scraping, and Text Analysis in R (Dr. Jason Roos)
- Panel Data Econometrics (Prof. Marno Verbeek)
- Testing and Interpreting Moderation and Mediation with SPSS (Prof. Jeremy Dawson)
- Scientific Integrity (multiple professors)
- Publishing Strategy (multiple professors)
- Cambridge Proficiency English (Ashley Fillingham)

External courses

- Experiments in Accounting (Prof. Rob Bloomfield, Limperg Institute)
- Behavioral Audit Research (Prof. Anna Gold and Prof. Rick Hatfield, NHH)
- Methodology of Experimental Economics (Prof. Jessen Hobson, UIUC)
- Programming in z-tree (Urs Fischbacher, University of Konstanz)
- Capital Markets I (Prof. Peter Easton, Limperg Institute)
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The purpose of this dissertation is to examine how changes in the information landscape affect the quality of information reported by companies, its dissemination in capital markets and investors' processing of the information. In particular, I investigate experimentally how public scrutinization of managers, changes to the structure of transaction costs and the framing of company disclosure affect capital market communications.

Chapter 2 predicts and finds that managers avoid exactly meeting benchmarks, such as the analyst earnings consensus, when they report under increased public scrutiny. Consistent with the notion that managers misreport for self-presentational goals, this effect is strongest for individuals who score at the lower end of the Dark Triad scale. Thus, it appears that Non-Dark Triad managers, the managers we usually assume to be the good actors, are unable to resist market scrutiny and take welfare-enhancing choices. Chapter 3 examines how a recent investor protection regulation (MIFID II) that requires broker-dealers to unbundle their charges to investors affects investors' information processing. Consistent with theory, the results of an experimental market indicate that investors rely more strongly on forecasted information when transaction costs are sufficiently high to provoke feelings of regret. Chapter 4 offers an explanation for why investors sometimes disagree when interpreting a company's disclosure. The results of an experiment show that shareholders who identify with their invested-in companies perceive managers as more credible than non-identified outside investors do, but only when managers frame adverse event disclosures in terms of external attributions. In sum, the study results document how developments in companies' information landscape can have a profound impact on preparers of financial statements and their intended users.

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