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BEHAVIORAL CORPORATE FINANCE

Ulrike Malmendier

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ABSTRACT

Behavioral Corporate Finance provides new and testable explanations for long-standing corporate-finance puzzles by applying insights from psychology to the behavior of investors, managers, and third parties (e. g., analysts or bankers). This chapter gives an overview of the three leading streams of research and quantifies publication output and trends in the field. It emphasizes how Behavioral Corporate Finance has contributed to the broader field of Behavioral Economics. One contribution arises from the identification of biased behavior (also) in successful professionals, such as CEOs, entrepreneurs, or analysts. This evidence constitutes a significant departure from the prior focus on individual investors and consumers, where biases could be interpreted as `low ability,' and it implies much broader applicability and implications of behavioral biases. A related contribution is the emphasis on individual heterogeneity, i. e., the careful consideration of the type of biases that are plausible for which type of individual and situation.

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1 Introduction

The field of *Corporate Finance* might well be the area of economic research with the most misleading name (followed by *Behavioral Economics* as a close second). Many of the research papers identified as "Corporate Finance" deal neither with corporations nor with financing decisions. In this chapter of the Handbook, I first conceptualize the breadth and boundaries of Corporate Finance research, and then present the advances that have resulted from applying insights from psychology. I illustrate how the behavioral toolbox has allowed for progress on long-standing puzzles regarding corporate investment, mergers and acquisitions, and corporate financing choices.

Naturally, this enterprise entails discussing the key research questions and developments in the field of Behavioral Corporate Finance. However, the most important contribution of Behavioral Corporate Finance might well go beyond the concrete applications of insights from psychology to corporate-finance puzzles. Research in Behavioral Corporate has been critical to the development of Behavioral Economics in that it was the first to apply behavioral assumptions not just to individual consumers or small investors, but show that the behavioral framework is crucial for our understanding of the decision-making of smart and highly trained professionals who lead large organizations. Even corporate leaders systematically deviate from our standard neoclassical model of rational decision-making and exhibit, for example, anchoring bias, loss aversion, and overconfidence when they make far-reaching corporate decisions.

This step constituted a sharp departure from the emphasis in much of the prior behavioral research, which had focused on individuals outside the realm of their professional lives and training. Bad consumption choices, ill-informed personal investment choices, biased expectations about variables the individual is not educated to assess (such as future interest rates), and similar applications tended to be the focus of the existing theoretical and empirical research.[1] Corporate Finance researchers have been among the first to argue theoretically and show empirically that top managers and professionals are subject to systematic biases. As such, they have altered the view on what the behavioral toolbox is able to do and why it is important to add psychological realism also to our models of top-level decision making.

Two more general insights have emerged from Behavioral Corporate Finance research on high-level decision-makers. First, the evidence on biased behavior of smart and talented professionals implies that successful "fixes" of biased decision-making will need to be of a different nature than

[1] A notable exception is the study of professional baseball executives, as discussed in Lewis' intriguing book "Moneyball" (Lewis 2004) and analyzed more rigorously by Thaler and Sunstein (2003). They conclude that "the blunders of many [baseball executives] suggests the persistence of boundedly rational behavior in a domain in which market pressures might well have been expected to eliminate them." Relatedly, Romer (2006) analyzes the choice on fourth down in the National Football League, and provides evidence of systematic departures from the decisions that would maximize the chances of winning. Massey and Thaler (2013) study the annual player draft in the NFL and show that the professional scouts persistently overvalue top draft picks.

implied by the earlier emphasis on education and financial literacy. For widespread deviations from the standard rational model, such as overconfidence, for example, cognitive limitations are unlikely to be the root and explanations unlikely to be the remedy.

Second, behavioral researchers should consider carefully which biases are plausible for which individual in which setting, rather than testing them uniformly in their "convenience sample." Being confronted with the objection that "successful CEOs surely won't be biased," or concerns about the seeming inconsistency of considering investor biases in one paper and managerial biases in another, researchers in Behavioral Corporate Finance had to think hard about the type of biases that are plausible for decision-makers in a corporate setting and how they differ from those considered for the untrained individual. For example, psychological research provides ample motivating evidence to test for managerial overconfidence, but less for underconfidence or cognitive limitations that might be relevant for research in household finance. This focus on specific biases for specific settings is a perspective that is now percolating into other fields of Behavioral Economics.[2]

This handbook article presents the existing research and open questions in the field of Behavioral Corporate Finance with the intention of fostering its development and influence on the broader field, as well as inspiring further research along these lines.

In the following pages, I first present a general introduction to research in Behavioral Corporate Finance (Section 2). I distinguish between two main "perspectives:" research on individual investor biases (and managers' response), and research on managerial biases (and investors' response). I give a first indication of what either perspective contributes to answer Corporate Finance questions. I also discuss how the two perspectives might interact, as they have been falsely viewed as contradictory in the past, and add a possible third perspective (biases among other players). The section concludes with a quantitative overview of the research output in the subfields and graphic illustration of its growth, also in comparison to Behavioral Finance and Finance more broadly.

In Section 3, I use one of the core applications in corporate finance, mergers and acquisitions, to work through the insights gained by assuming either of the main two "perspectives" – biases of investors providing financing for stock- or cash-financed acquisitions, and biases of managers pursuing various types of acquisitions – as well as their interaction.

Section 4 complements the discussion with a presentation of the theory and applications developed in some of the most innovative and influential research in Behavioral Corporate Finance. I first present several studies on how firms exploit investors' biased beliefs and non-standard preferences (Perspective 1) for their financing and investment decisions, I then turn to the impact of managerial biases (Perspective 2), starting with a review of the ample evidence on managerial overconfidence.

[2] In Industrial Organization, for example, researchers argue that not only consumer behavior but also firms' choices might be better understood if we allow for biases (e. g., Bloom and Van Reenen (2007) and Goldfarb and Xiao (2011)). And in Macroeconomics, research has shown that not only individual expectations of future inflation might be distorted by personal experiences but even those of central bankers (Malmendier, Nagel, and Yan (2017)).

I move to other managerial biases and characteristics, most of them in the realm of biased beliefs, and fewer on nonstandard preferences or cognitive fallacies. Finally, I discuss behavioral research on network effects, e. g., on how social connections and personal ties affect corporate outcomes. The latter includes both Perspective 1 and Perspective 2 approaches.

Section 5 concludes with a topic-based organization and summary of the wide-ranging research output that exists in the field of Behavioral Corporate Finance. The main areas of research span from investment (including innovation and entrepreneurship) to financing (including capital structure, internal capital markets, and payout policy), and from corporate governance (including compensation, CEO selection and turnover) to venture capital and financial intermediation. I point to some more recent developments in the literature and some of the open issues and questions.

2 Three Perspectives

2.1 Corporate Finance and Behavioral Corporate Finance

As indicated in the introduction, Corporate Finance seems a misnomer for the type of research presented at modern corporate finance conferences, or at least it is far too narrow. While the finances of corporations were originally at the center of the field,[3] and the Modigliani and Miller (1958) theorem still constitutes the typical "Lecture 1 material" in graduate Corporate Finance classes, current research is much broader. It covers firms that are not incorporated, entrepreneurs, analysts, and households, all making decisions far beyond the "financing" aspects.

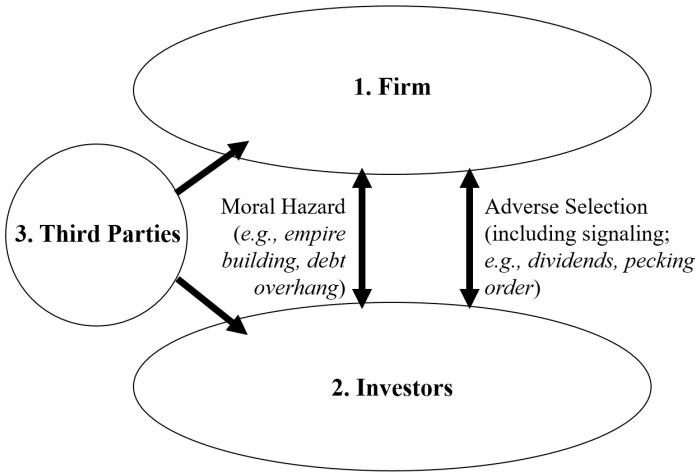


Figure 1: **Corporate Finance in a Nutshell**

Figure 1 illustrates the types of interactions analyzed in traditional Corporate Finance. A

[3]As Jensen and Smith (1984) write in their historical overview of the theory of corporate finance, "[t]he major concerns of the field were optimal investment, financing, and dividend policies."

firm seeks financing from outside investors, and has to overcome two hurdles: moral hazard and adverse selection. Moral hazard concerns incentive misalignment between managers and investors. For example, a manager may choose to expand the firm due to private benefits, even when such expansion is not profitable. This incentive conflict affects the firm's ability to obtain financing when investors cannot observe and control managers' behavior. Adverse selection concerns a different type of asymmetric information, namely, that investors cannot distinguish promising and less promising investment opportunities. As a result, a firm can fail to obtain financing for an investment project even when it would be profitable to the investors. The firm may resort to signaling via dividend payments or to a pecking order of financing choices in order to overcome these frictions.

Figure 1 also indicates potential interactions with third parties, which may affect financing opportunities and choices. As the more detailed depiction in Figure 2 reveals, these include analysts who forecast the firm's future earnings, investment banks who offer assistance with equity issues, rating agencies who rate the firm's debt, regulators who require the firm to reveal financial information, and central bankers whose rate setting affects the firm's cost of debt. Figure 2 also acknowledges moral hazard issues *within* the firm, which constitute part of the research in corporate finance, in particular the large area of corporate governance.

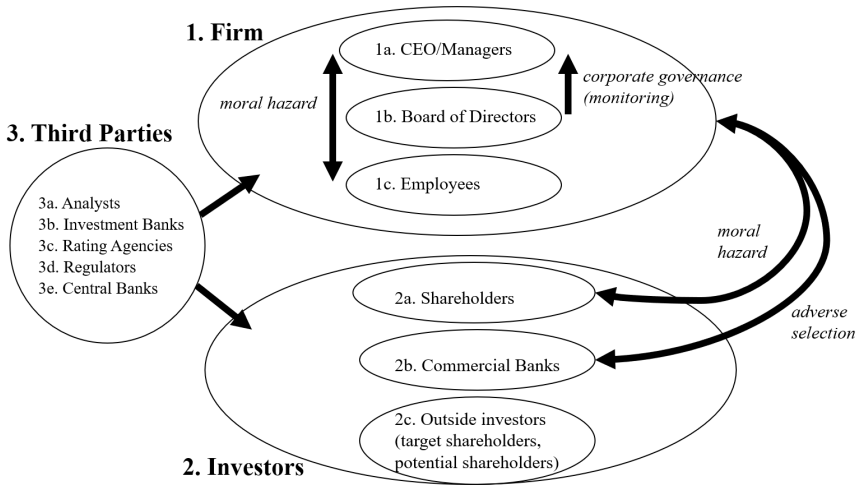


Figure 2: **Corporate Finance—Zooming in**

The two figures convey an idea of the (stereo-)typical research topics in corporate finance, but, as acknowledged earlier, fail to capture where the field stands today, with its much broader set of actors and actions, research questions, and methodologies. Examples of research closely tied to non-finance fields include contracting in micro-finance (development economics), corruption and its detection in the stock market (political economy), the allocation of human capital within firms

(labor and organizational economics), and the incentives and biases of stock analysts (accounting).[4]

So what, then, distinguishes Corporate Finance from other areas of applied microeconomics? First, while the set of actors and actions in corporate finance models might be broad, it still has to feature some elements of the set "firm, manager, investor, analyst, entrepreneur" as they are involved in mergers, equity issuance, and other corporate decisions. Second, there continue to be differences in empirical methodology, such as standard-error calculations using the Fama and MacBeth (1973) approach, and event study methodology to evaluate the net value creation in, say, earnings news or merger announcements.[5] At the same time, we also see convergence from both sides. Petersen (2009) clarifies the differences between the Fama-MacBeth approach and clustering, and anticipated the move to clustering as Fama-MacBeth standard errors will frequently be too small.[6] Vice versa, applied microeconomists outside corporate finance are now embracing the event study methodology and aggregate difference-in-differences approach.

With these definitions and caveats in mind, I turn to *Behavioral* Corporate Finance, which applies tools and insights from Behavioral Economics to corporate finance settings. Let's define Behavioral Economics following Rabin (2002) as an approach that allows for

1. deviations from rational belief formation,

2. non-standard utility maximization, and

3. imperfect maximization processes due to cognitive limitations.

Non-standard beliefs in (1) include all deviations from Bayesian belief, such as overconfidence (Svenson 1981, De Bondt and Thaler 1995), overextrapolation (Cagan 1956, Cutler, Poterba, and Summers 1990, De Long, Shleifer, Summers, and Waldmann 1990b, Barberis and Shleifer 2003),

[4] A good indicator of the breadth of topics are the Corporate Finance programs at the NBER meetings. For example, the 2017 NBER Summer Institute in Corporate Finance featured papers on the labor costs of financial distress (Baghai, Silva, Thell, and Vig 2017) and on social networks (Bailey, Cao, Kuchler, and Stroebel 2017), and the 2015 NBER Summer Institute included work on student loans (Lucca, Nadauld, and Chen 2016).

[5] See MacKinlay (1997) for a detailed overview of the methodology. Event studies calculate returns around an event, e. g., +/- 1 day, relative to a benchmark, typically market returns, CAPM returns, industry-specific returns, or book-to-market, size, and momentum-matched returns. Short horizons are ideal for identification purposes. Long-run studies are more sensitive to the modeling of the counterfactual (expected) returns. Further discussion on the difficulties and a new strategy to estimate long-run abnormal returns in contested M&A deals, are on p. 28.

[6] In finance panels, OLS standard errors can be biased because of unobserved firm effects or time effects. Clustered standard errors allow for correlated residuals within a cluster (e. g., a firm or a year), and assume that residuals across clusters are uncorrelated. The Fama-MacBeth (FM) approach entails two steps. First, estimate T cross-sectional regressions, separately for each year $t = 1, ..., T$. Second, calculate the coefficient $\hat{\beta}_{FM}$ as the average of the T cross-sectional coefficient estimates $\hat{\beta}_t$, and the estimated variance as $\frac{1}{T}\sum_{t=1}^{T}(\hat{\beta}_t - \hat{\beta}_{FM})^2/(T-1)$. FM standard errors are unbiased if the year-by-year estimates $\hat{\beta}_t$ are independent, i. e., there are (only) unobserved time effects. (Standard errors clustered by time are also unbiased given a sufficient number of clusters.) FM standard errors are too small if there are unobserved firm effects, while standard errors clustered by firm are unbiased. Examples of corporate finance publications that use clustering approach, and reference Petersen (2009), include Ferreira and Matos (2008), Leary and Roberts (2014), Fang, Tian, and Tice (2014), Falato and Liang (2016), and Ho, Huang, Lin, and Yen (2016).

which can in turn be motivated by the representativeness heuristic (Tversky and Kahneman 1974), and experience-based learning (Malmendier, Pouzo, and Vanasco 2017). Non-standard preferences in (2) include, for example, reference dependence (Baker, Pan, and Wurgler 2012) and other social preferences (Charness and Rabin 2002, Malmendier, te Velde, and Weber 2014). Imperfect maximization processes in (3) include limited attention and mental accounting (Thaler 1985, 1999).

Researchers have applied features (1) to (3) to the main players in corporate finance settings. Originally, the emphasis was on investor biases, often labeled (somewhat non-specifically) "investor sentiment," and rational managers exploiting these biases. A second wave of research established that also the firm side, including CEOs, fund managers, and bankers, exhibits systematic biases that affect corporate outcomes. In the next subsection, I explore both perspectives,[7] and present a potential third one, which applies the behavioral features to the third party in Figures 1 and 2.

2.2 Perspective 1: Biased Investors

Perspective 1 analyzes the interaction between investors that exhibit non-standard behavior ("investor sentiment") and rational managers. It explains the corporate-finance policies that have been hard to reconcile with standard neo-classical models as the managerial response to investor biases, akin to the Behavioral Industrial Organization literature on firm responses to biased consumers in (DellaVigna and Malmendier (2004), Ellison (2006), Spiegler (2011), Grubb (2015), and Heidhues and Koszegi, ch. XXX, in this Handbook).

Investor biases in this literature have mostly been characterized as systematic mis-valuation of stocks, either overall (stock market) or for specific subgroups of stocks. That is, rather than modeling concrete, known investor biases, such as loss aversion, overconfidence, or experience effects, earlier research tended to refer to the general label of "investor sentiment." The notion of investor sentiment goes back at least to Keynes (1936). Key contributions are Shiller (1981) on excess volatility of stock indexes, De Long, Shleifer, Summers, and Waldmann (1990a) on noise trader risk, Lee, Shleifer, and Thaler (1991) on the closed-end fund puzzle, and, in corporate finance, Morck, Shleifer, and Vishny (1990) on the influence of sentiment on firm investment.

Behavioral corporate finance adds the rational managerial response to the picture. Baker and Wurgler (2000) and (2002) got this literature off the ground with their research on the timing of security issuances. As discussed in Section 4.1, they posit that, whenever investors are too optimistic about the intrinsic value of a firm, equity financing is a cheap way to fund investment, and managers tilt their external financing towards stock. Shleifer and Vishny (2003) apply the same idea to the

[7] Earlier surveys of the literature make similar distinctions. Baker, Ruback, and Wurgler (2002) distinguish between the "irrational investors approach" and the "irrational managers approach," with a clear emphasis on the former. Baker and Wurgler (2012) also put most weight on the first theme. They distinguish between "market timing and catering," "managerial biases," and "behavioral signaling." The latter category features investors with prospect-type preferences for dividends; the authors argue that it falls in between the first two perspectives.

timing of stock-financed mergers, as discussed in Section 3.2. In the same vein, Baker and Wurgler (2004b) consider how managers cater to investor fads for dividends. Rational managers may also cater to biases within their organizations, and issue stock-based compensation to employees with "high sentiment" towards their firm, as shown by Bergman and Jenter (2007). Recent research in area argues that the wealth transfer arising from equity transactions that exploit investor sentiment are large (Sloan and You 2015) and that the shareholder value implications of managerial responses to sentiment-induced misvaluations are positive (Warusawitharana and Whited 2016).

The biased-investor perspective was a natural starting point of the field of Behavioral Corporate: To allow for "smart managers facing dumb investors" is a departure from the standard rationality assumption that was initially easier to digest than positing that successful CEOs and other managers may behave in a biased manner. At the same time, the approach has faced two conceptual hurdles.

A first shortcoming is the assumed homogeneity of investors, and the empirical lack thereof. Not all investors are "dumb." More precisely, different groups are subject to different behavioral biases. The question is whether, to a first approximation, we can ignore this heterogeneity in corporate finance settings and make progress using a simple "representative biased agent" model, in the tradition of standard representative-agent models. Or, in the same way some traditional models allow for differences in opinion between investors, do behavioral models need to allow for differences in non-traditional determinants of beliefs and other non-standard features? Do we need to account for systematically different biases across different generations, between male and female investors, between day traders and other types of investors?

One reason why heterogeneity in biases is important empirically is self-selection. Consider the following example from outside finance: In DellaVigna and Malmendier (2006), we show that the vast majority of gym members attend too little to justify their flat-fee membership relative to a pay-as-you-go option. In the overall population, it might easily be the case that the average person does not harbor such overconfidence about future work-out frequencies. In fact, many might be underconfident about their ability to attend a health club consistently and shy away from enrolling. However, those who self-select into flat-fee contracts display significantly biased expectations.

In practice, one reason for the homogeneity assumption has simply been the lack of individual-level proxies for bias. As such data has become available, refined behavioral analyses have started to emerge. A common starting point is the differentiation between firms with and without institutional stock ownership. Researchers test whether the posited catering to biased investors is less prevalent in firms with large institutional ownership. For example, Baker, Greenwood, and Wurgler (2009) find stronger support managers catering to investor demand for low-priced securities in firms with low institutional ownership.[8] Other researchers, however, find the opposite. Hoberg and Prabhala

[8] In the realm of analysts' interactions with investors, Malmendier and Shanthikumar (2007) show that small investors, but not large institutional investors are naive about biased analyst recommendations.

(2009) report that dividend catering is more prevalent towards institutional investors: Institutional rather than retail investors increase their ownership positions after firms' dividend initiations, even in periods of high dividend fads.[9] Both types of findings are important to the development of Behavioral Corporate Finance. On the one hand, it is crucial to acknowledge investor heterogeneity and identify instances where biases might be more prevalent among non-professional investors. On the other hand, it is an key contribution of the Behavioral Corporate Finance literature to emphasize the presence of behavioral biases among professional actors.

Outside corporate finance, behavioral finance researchers have started to distinguish even more finely. Consider Barber and Odean (2001): Not all investors in their data "trade too much," but the authors are able to show that young males do.[10] Using the same individual-investors data, Kumar (2009) finds that stocks with lottery-type payoff functions are especially popular among "[p]oor, young, less educated single men who live in urban areas, undertake non-professional jobs, and belong to specific minority groups (African-American and Hispanic)," as well as "investors who live in regions with a higher concentration of Catholics." This research goes further in accounting for investor heterogeneity in biases, but also reveals the risks: Researchers need to make sure that they avoid checking all possible dimensions of heterogeneity without much theory guidance. It would not be surprising to find about every tenth demographic to matter. Such research needs a solid theoretical framework, building on robust insights from psychology that suggest why a specific characteristic would predict more trading or other financial decisions for which type of agent.

The second issue tainting some of the Perspective-1 research in Behavioral Corporate has been the initial focus on a rather unspecific bias, dubbed "investor sentiment." Behavioral research is at its best when it builds on a specific model of a bias, allowing the researcher to leverage the strength of the psychological foundation and to derive specific predictions. Instead of dealing with "over-valuation at some times, and under-valuation at some other times," we would like to know what triggers which deviations, which stocks are predicted to be the object of this bias, and whether certain investors are more likely to be subject to this bias than others. Only if we pin down a concrete (psychological or cognitive) mechanism we can test and falsify whether the proposed bias is at work. Moreover, it becomes easier to distinguish the bias from alternative explanations, many of which could fall under the ominous and omnipresent "informational frictions" label.

Here, too, researchers have made significant progress relative to the early literature. Corporate finance research has started to move to more specific investor biases, such as the implications of reference dependence for merger pricing (Baker, Pan, and Wurgler 2012)[11] and dividend payments

[9] They relate this finding to the "prudent man" investment motives of institutional investors (cf. Brav and Heaton (1998)), and a theoretical model by Allen, Bernardo, and Welch (2000), in which firms pay dividends to appeal to (tax-advantaged) institutional investors who have superior monitoring capacities.

[10] Note that males differ from females in numerous ways, and the paper presents only tentative proxies for bias.

[11] Li (2013) and Betton, Eckbo, Thompson, and Thorburn (2014) corroborate Baker, Pan, and Wurgler (2012).

(Baker, Mendel, and Wurgler 2016). These models feature investors with prospect-theory-like preferences who use past prices or dividends as a reference point and, thus, are averse to lower prices or dividends cuts. A good next step might be an even more precise modeling approach, and clearer distinction from related mechanisms, if empirically testable. For example, Baker, Pan, and Wurgler (2012) treat reference points and anchoring almost interchangeably, stating that "Parties ... appear to use recent peaks as reference points or anchors" in the abstract, and listing both reference dependence and anchoring as the psychological underpinning in the main text. Similarly, Baker, Mendel, and Wurgler (2016) use the two concepts interchangeably when writing that an ADR is "unable to create a reference point in two different currencies at once. What this means is that the anchor of past dividends can be relevant only in one currency, not both."

In summary, Perspective 1 of Behavioral Corporate explains several important stylized facts in corporate finance, most of them revolving around the type of financing chosen by managers. Other puzzles of seemingly non-standard managerial decisions remained unexplained, such as patterns of investment-cash flow sensitivity, the strong path-dependence of capital structure, and the heterogeneity in financing patterns among otherwise similar firms. This observation was the starting point of the Behavioral Corporate research performed from the perspective of biased managers.

2.3 Perspective 2: Biased Managers

Perspective 2 of Behavioral Corporate Finance considers biases on the side of the manager. Here the question is whether non-standard managerial behavior, and the market's response to it, helps explain existing puzzles in corporate finance. The managerial biases considered include overconfidence, reference-dependence, experience effects, and more generally "traits" that are not relevant in traditional models. The response of the market is generally assumed to be rational.

Examples include attempts to explain the "urge to merge" and its link to managerial overconfidence (Malmendier and Tate 2008); debt aversion and its link to past lifetime experiences of the CEO such as economic depressions or military service (Malmendier, Tate, and Yan 2011; Benmelech and Frydman 2015; Schoar and Zuo 2017); leverage choices and their link to CEOs' personal leverage choices in home purchases (Cronqvist, Makhija, and Yonker 2012); or firm performance and its link to behavioral characteristics of CEOs (Kaplan, Klebanov, and Sorensen 2012).

The applications have been wide-ranging, and continue to expand, moving from the more traditional areas of investment, financing, capital structure, and mergers, to the role of the board and corporate governance (e. g., options vs. debt overhang), internal labor market (the role of tournaments, design of compensation contracts), and "corporate repairs." The last category, corporate repairs, describes organizational fixes of issues arising from biased managerial decisions. Examples include executive training to eliminate biases, different selection criteria for CEOs than in a world without biases, or re-structuring of the board or organization (cf. Camerer and Malmendier 2007).

Corporate repairs have not been researched as widely as one might expect, in light of the practical importance, and provide opportunities for researchers who obtain access to relevant firm data.

In this line of research, the promises and challenges are almost reversed relative to the first perspective. First, in terms of strengths, research on behavioral managers has benefited from the relative homogeneity of the subjects. CEOs and other top-level executives are bound to be more similar in terms of their socio-economic status, cognitive abilities, and some other background characteristics than the whole market of investors. This (relative) homogeneity is even more plausible for subgroups of managers such as CEOs of Forbes 500 companies or entrepreneurs in certain industries, which are often the subject of research studies. Selection works in the same direction—many unobserved traits will be correlated, especially if they tend to foster the career of a manager.

These similarities also help identify *plausible* biases to consider in a specific corporate setting – biases that are unlikely to hinder a manager's rise to a the top, and that might even be beneficial to or a by-product of such a career. For example, in research on Forbes 500 CEOs, overconfidence might seem like a natural bias to consider, but cognitive limitations or under-confidence less so.[12] More generally, researchers may ask for which type of person and career path psychological phenomena such as, say, "mental accounting" or "sunk cost fallacy" seem more or less plausible.

In addition, it is actually more easily feasible to account for remaining heterogeneity in the "Biased Manager" strand of research than it is oftentimes under the "Biased Investor" approach, due to the more detailed data sets on the smaller number of managers. ExecuComp, BoardEx, Who's Who, or the Million-Dollar-Directory, to name only a few of the data sets, tend to be available at many research institutions. Moreover, given the information disclosure requirements for publicly traded companies, researchers are also able to control for incentives set by compensation contracts, governance structure, and other features of the firm manager is running.

Another appeal of research focusing on top managers is that their (biased) decisions tend to have far-reaching consequences. Acquisitions, hiring, down-sizing, or investment programs affect the wealth of shareholders, the lives of employees, the retirement savings of mutual fund investors, etc. Hence, while other research in behavioral finance often has to face the "but-what's-the-alpha" criticism (say, studies of small investors being naive about analyst distortions), behavioral CEOs and other top-level executives are of clear economic significance. As much as the early research on "Behavioral Managers" was faced with skepticism of how successful top managers could plausibly be subject to behavioral biases—or even if they were, how the advisors and governance bodies would allow these biases to affect outcomes—the existing body of research has provided overwhelming evidence that this skepticism did not reflect reality, and that the impact of these biases is large.

[12] Goel and Thakor (2008) develop a model showing that an overconfident manager, who sometimes makes value-destroying investments, has a higher chance than a rational manager of being promoted to the position of the CEO under value-maximizing corporate governance.

At the same time, the self-selection of a certain type of person into a managerial career path poses different issues. These individuals are unlikely to be representative of the population and, as a result, prior knowledge about the distribution of traits and biases might be misleading. A good example is the role of gender. When pursuing research questions about overconfidence, loss aversion, or limited attention in corporate decisions, researchers are commonly asked whether there are differences by gender. In fact, behavioral finance research has explicitly shown such differences in portfolio holding (Barber and Odean 2001). Applied to managers, however, the results might be very different. The subsamples of females in such studies tend to be both minuscule and highly selected. If, say, women were on average less likely to be overconfident in their own abilities than men, we might not find the same among those who achieve a top-level corporate position.

A second issue in the analysis of managerial biases is that the outcome variables of interest tend to be of lower frequency, e. g., merger announcements or equity issuances, and requires longer panel data. To clarify, the outcome variables are similar to studies on "Biased Investors" (Perspective 1), but those studies can identify out of higher-frequency variation in the measured investor bias, e. g., monthly variation in closed-end fund discounts to measure variation in investor sentiment.[13] At the same time, this challenge for Perspective 2 research is an opportunity for researchers who obtain access to higher-frequency within-firm data on managerial decisions.

In summary, Perspective 2 is a significant departure from standard modeling in that it allows for behavioral biases to affect top-level, far-reaching managerial decision-making. It also sheds new light on the welfare implications of these decisions. In a traditional modeling framework, the manager running a firm is either assumed to maximize the welfare of the owners (shareholders), or her own private benefits. Under Perspective 1 of Behavioral Corporate models, this is still the case, with the added wrinkle that the manager does so by exploiting the biases of investors. Under Perspective 2, the manager *aims* to maximize own or existing shareholders' wealth, but fails: Due to the manager's biased perspective, she ends up maximizing "perceived" wealth. She chooses actions that seem optimal under her biased beliefs, but might not be optimal given the true probability distribution. As a result she will not maximize her true objectives. These welfare considerations are a key reason why researchers should aim to go beyond rational "as if" models for reasons of modeling discipline when, in reality, behavioral biases may be at work.

2.4 Perspective 3: Biased Third Parties

The dichotomy of "managers versus investors" is of course an incomplete representation of corporate finance models. Many corporate finance settings feature a third group of players, most frequently financial intermediaries or analysts, who may also display non-standard behavior. This could in

[13] Cf.; Lee, Shleifer, and Thaler (1991). Baker and Wurgler (2006) use the closed-end data for their sentiment index, but employ a cruder version, based on annual data.

turn affect corporate decision making. For example, in the context of analyst biases, we may consider systematic "representativeness" (stereotyping firms, e. g., as "losers" and "winners") and then derive the implications for corporate decisions, such as earnings manipulation or budgeting decisions that aim to exceed thresholds ("meet or beat analyst forecasts"). Indeed, there is a large literature in accounting and finance on analyst biases, including overoptimism, overconfidence, confirmatory bias, stickiness in beliefs and expectations, (anti-)herding in forecasts, over-weighting of private information, and credulity about accounting accruals, to name just the more prominent ones.[14] However, as of now there is much less research on the question how analyst biases affect corporate finance decisions.[15] One exception is the research of Fracassi, Petry, and Tate (2016) on credit analysts. They provide evidence that credit analysts are often biased in their assessment of borrowers and that these differences in assessment carry through debt prices.

Other promising applications are financial intermediaries, rating agencies, regulators, law makers, or central bankers. Cortés, Duchin, and Sosyura (2016) provide evidence of mood-induced biases in the decision of loan officers, using exposure to sunshine as an instrument. Relatedly, the literature on venture capital financing features some work on trust and friendship networks affecting outcomes in a non-standard manner. Gompers, Mukharlyamov, and Xuan (2016) find that venture capitalists who share the same ethnic, educational, or career background are more likely to syndicate with each other, at the expense of the probability of investment success. On the macro level, we have evidence in Malmendier, Nagel, and Yan (2017) that central bankers' inflation expectations are affected by their personal lifetime experiences of inflation, with immediate implications for the funding of firms (via the feds funds rate). Turning to the role of governments, Dinc and Erel (2013) show an effect of economic nationalism on M&A activities. Nationalist interventions block foreign acquirers and help create domestic companies that are too big to be acquired by foreigners.

Generally, this "third perspective" is in its infancy. Even papers just on biased decisions without considering corporate-finance implications, are rare and seem an interesting avenue to pursue.

2.5 Which Perspective is Right?

The juxtaposition of Perspectives 1 to 3 may leave the reader with the impression that the different approaches are inconsistent. In fact, the typical set of assumptions in the underlying models lend

[14] Cf. Malmendier and Shanthikumar (2014), Pouget, Sauvagnat, and Villeneuve (2017), Bouchaud, Krueger, Landier, and Thesmar (2016), Bernhardt, Campello, and Kutsoati (2006), Chen and Jiang (2006), and Teoh and Wong (2002), among others. For a more general recent survey of the literature on analysts see also Bradshaw (2011)'s unpublished but much-cited paper "Analysts Forecasts: What Do We Know after Decades of Work?"

[15] There is some work on the connection between analysts and firm decisions more generally. For example, McNichols and Stubben (2008) present evidence that firms overinvest during times when they manipulate earnings, and hypothesize that "decision-makers within the firm believe the misreported growth trend." Matsumoto (2002) finds that firms not only want to avoid negative earnings surprises, but take deliberate action to guide analysts' forecasts downward to avoid falling short of expectations.

themselves to such concerns: In the literature on biased investors, the manager is modeled as rationally optimizing a standard utility function, and investors display non-standard utility or form non-Bayesian beliefs. In the literature on biased managers, it is the reverse. Who is right?

This impression is misguided, and "Who is right?" is not a useful question. Let me return to a non-finance example, the much-studied case of gyms, to illustrate why. DellaVigna and Malmendier (2006) show that individuals enrolling in a health club frequently overestimate their future attendance and, as a result, may choose a flat-fee membership that is suboptimal given their expected behavior. Health clubs still stir them towards the monthly membership to the firm's financial benefit. Hence, we have a Perspective-1 type setting – biased consumers, rational firms. However, this does not imply that health club managers are not subject to behavioral biases themselves. A large literature documents the high failure rates and poor returns to entrepreneurial ventures (Dunne, Roberts, and Samuelson 1988; Camerer and Lovallo 1999; Hamilton 2000; Moskowitz and Vissing-Jørgensen 2002) and attributes them to overconfidence and other biases (Cooper, Woo, and Dunkelberg 1988, Camerer and Lovallo 1999, Bernardo and Welch 2001, Moskowitz and Vissing-Jørgensen 2002). In fact, many health clubs that were interested in collaborating in the above-mentioned DellaVigna and Malmendier (2006) study, had to shut their doors before our study was completed! The insight here is that both the biases of consumers (self-control problems) and the biases on the business side (overconfidence) are important features to understand the health club industry, and the different biases are important for different aspects of the industry—understanding the predominant contract design versus the optimality of personal investment and start-up decisions.

The example illustrates that the seemingly contradictory set of assumptions simply reflects the usual focus of our models on the essential ingredients to derive the predicted behavior. When analyzing the implications of managerial biases, it is not essential or useful to also model out behavioral biases of investors unless they interact with those of the managers, and vice versa. I will illustrate this argument in the context of the merger example in the next section. There, I will also discuss potential interaction effects, and the question whether a correlation between the biases might help to generate interesting results or more distinctive predictions.

2.6 Where Do We Stand?—Quantifying Behavioral Corporate Research

Before diving into the actual research findings in Behavioral Corporate Finance, I would like to give a brief indication of where the field stands in terms of the research output. What volume of research has been published up to now, overall and separately for Perspectives 1, 2, and 3? This brief quantitative overview allows us to identify some trends, but also gaps and opportunities. We will further see the Corporate Finance applications that have been of most interest to behavioral researchers so far, and consider those that may merit further investigation.

I restrict this brief overview to articles published in a top finance journal (Journal of Finance,

Review of Financial Studies, Journal of Financial Economics) or one of two top economics journals that have published some behavioral finance research, the Quarterly Journal of Economics and the American Economic Review. Starting from a complete download of all abstracts published in these journals since 2000, I ask (i) whether a paper falls into the area of Behavioral Finance, and if yes, (ii) whether it is Behavioral Corporate Finance, and if yes again, (iii) whether from "Perspective 1" (biased investors), "Perspective 2" (biased managers), or "Perspective 3" (biases of other agents).[16]

The decisions about (i) and (ii) are based on the title and abstract, and the final decision about the categorization under (iii) is based on the entire article. As detailed in Appendix A.1, key requirements of the categorization are a true psychological underpinning, rather than mere talk of "frictions" or mention of possibly non-standard explanations. In instances where the decision is more challenging, I use language such as the words "cater," "exploit," "bias," or "psychological" as indicators. Finally, I require a corporate interaction, which is especially relevant for research of investor biases as it may otherwise fall into behavioral asset pricing.

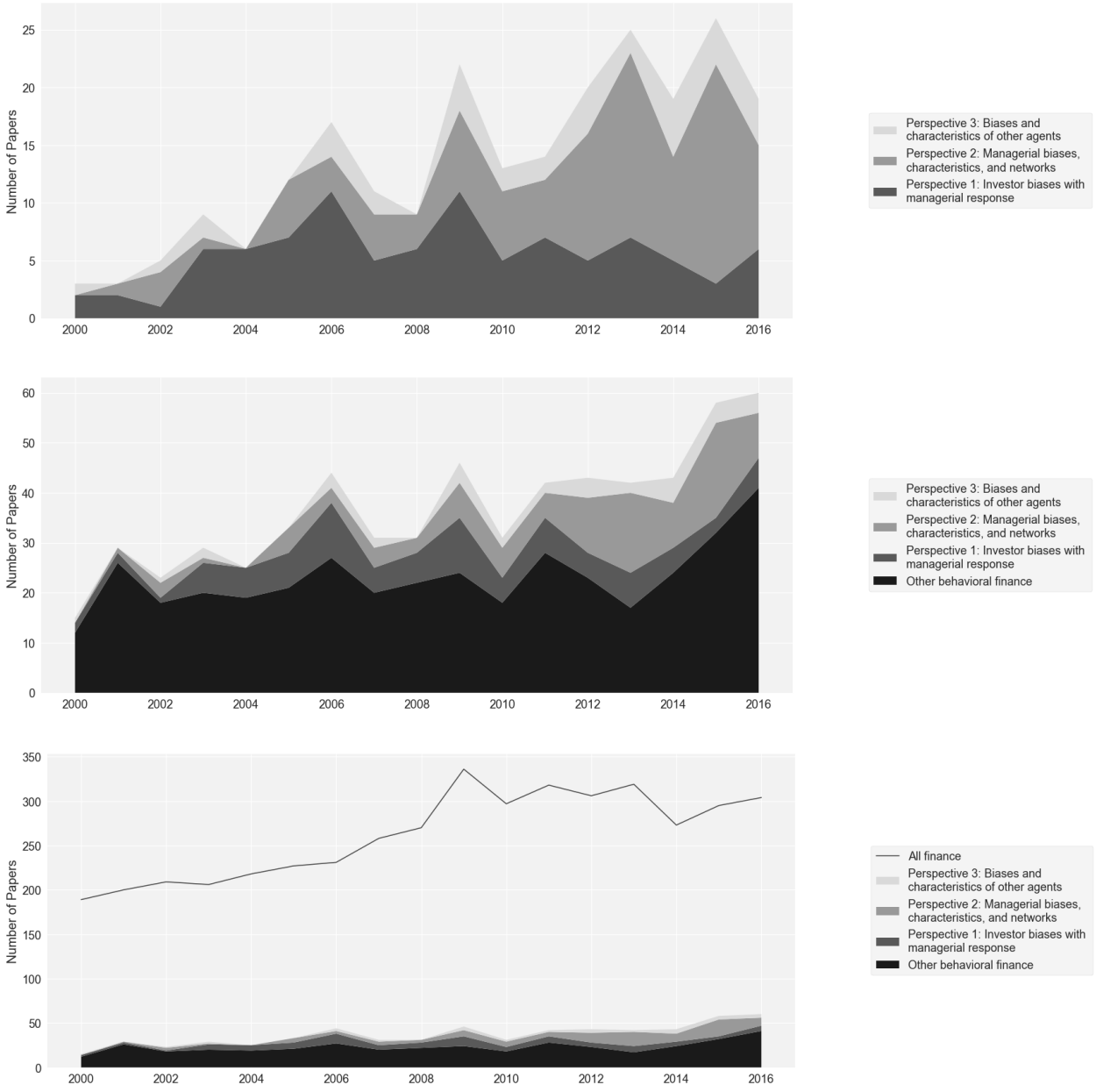
My classification as Behavioral Corporate Finance also includes papers that provide evidence against a behavioral explanation, as long as they address these non-standard approaches in detail. For example, I include the pseudo market timing paper by Schultz (2003) as its main purpose is to argue against a behavioral explanation of the long-run underperformance of equity issuances. In contrast, I do not include Biais, Rochet, and Woolley (2015), whose model of an innovative industry frames confidence as a feature of rational agents, and the authors only briefly mention towards the end of the paper that psychological biases might amplify their findings. In fact, I identify several research strands that appear to have a Behavioral Corporate "flavor" at first glance, but whose findings are not rooted in investor or managerial psychology upon closer inspection. Examples include papers on catering to investor needs explained by rational motives, papers on managerial risk-taking incentives, and those exploring peer effects and herding. (See Appendix A.1 for a complete list of these research areas). These papers are then classified as "Other finance."

Figure 3 visualizes the year-by-year research output in these categories. (Appendix A.2 contains the figures for finance journals only, as well as results for the three finance journals individually.) The top graph captures the evolution of the three main perspectives of research in Behavioral Corporate. We see that, early on, the bulk of Behavioral Corporate research focused on investor biases. Starting in the late 2000s, the managerial perspective gained momentum, and now produces the majority of papers in the field. Research exploring biases of other agents is still in its infancy, and has never been given as much attention in the literature as the other two perspectives.

[16] For the two economics journals, I also tag all papers that could have been published in a finance journal, and calculate the fraction of behavioral-finance research relative to that baseline for comparability. Such a categorization is subjective, and journals have changed their openness to finance research over time. I use the following criteria: (i) Which type of research does the paper cite as related literature? (ii) Does the paper cite any papers published in a finance journal? (iii) Do subsequent finance papers cite the paper? (iv) Who are the authors of the paper? Using this procedure, the baseline mostly consists of macro finance, public finance, and behavioral finance papers.

Figure 3: Research in Behavioral Corporate Finance

All graphs show the year-by-year number of finance papers published in the journals JF, RFS, JFE, QJE, and AER under the denoted categories. The categories in the top graph are the three main perspectives of research in Behavioral Corporate Finance as delineated in Sections 2.2-2.4. The middle graph adds the general Behavioral Finance category, and the bottom graph also includes the total research output in Finance.



The middle graph in Figure 3 compares the output in Behavioral Corporate Finance to that in Behavioral Finance more generally. We see that Behavioral Corporate represented only a minuscule fraction of behavioral finance research in the early 2000s, and has now become a strong subfield and occasionally reaches an equal volume to other behavioral finance research, e. g., in 2013.

The bottom graph in Figure 3 provides a comparison with the total research output in finance. Over the years, an average of 14% of papers have featured behavioral research, with a slight increase in recent years, e. g., between 16% and 20% of published finance research in 2014 to 2016.

When looking only at the top three finance journals (see Appendix Figure A.2.1), the picture is very similar. The Journal of Financial Economics is by far the most open to behavioral and behavioral corporate research, with about 20 behavioral finance papers per year in recent years, more than half of which are in the behavioral corporate area (see Figure A.2.4). In the Journal of Finance and the Review of Financial Studies, these numbers are significantly smaller and more volatile, around 13 per year overall and approximately half of those in behavioral corporate. Some of the early milestone papers for the two main perspectives were published in the Journal of Finance (e. g., Baker and Wurgler 2000; Baker and Wurgler 2002; and Malmendier and Tate 2005).

In summary, behavioral finance research in general and behavioral corporate research in particular continue to be on the rise. Behavioral research makes up about 15-20% of top publications in finance, with Behavioral Corporate starting from virtually zero around 2000 and now reaching a third to a half of the behavioral finance research. As we move forward, it may become harder to disentangle behavioral and non-behavioral approaches. One vision for behavioral finance is that it will simply be submerged into mainstream approaches as it will be a matter of course to feature a realistic discussion of the underlying individual decision-making.

What has the impact of these papers been so far? Table 1 contains summary statistics on the number of papers published in the main categories, the years of publication, and the number of citations.[17] A remarkable number of 233 behavioral corporate papers have been published since 2000 in the five journals analyzed here (see Panel A). The two main perspectives have received similar attention in the literature: 95 out of the 233 papers examine "investor biases with managerial response," and 102 papers analyze "managerial biases, characteristics, and networks." As we saw already in the time-series graphs, fewer papers are devoted to the biases and characteristics of other agents, such as board members or analysts. Turning to these papers' research impact, the mean and total number of citations are slightly higher for papers on managerial biases than on investor biases, while the median number of citations instead, is higher for papers on investor biases.

These numbers paint a somewhat biased picture since Perspective 1 constitutes the older of the two main streams of the literature. The median year of publication is 2009, compared to 2013 for papers on managerial biases and characteristics. If we increase comparability of the statistics by

[17] Citations are the number of Google Scholar citations as of 3/26/2017.

Table 1: Summary Statistics on Research in Behavioral Corporate Finance

This table provides summary statistics on the number of papers published in the main categories ("Perspectives"), years of publication, and Google Scholar citations, pooling together Finance and Economics journals. Appendix-Table A.3.1 shows a detailed version, differentiating all six behavioral categories from above.

Panel A: All papers

Category		Year of publication			Citations		
	N	median	first	last	total	mean	median
Perspective 1: Investor biases with managerial response	95	2009	2000	2016	34,331	361	182
Perspective 2: Managerial biases, characteristics, and networks	102	2013	2001	2016	37,433	367	112
Perspective 3: Biases and characteristics of other agents	36	2012	2000	2016	10,210	284	180
Total	233						

Panel B: Papers published since 2010

Category		Year of publication			Citations		
	N	median	first	last	total	mean	median
Perspective 1: Investor biases with managerial response	38	2013	2010	2016	3,705	98	57
Perspective 2: Managerial biases, characteristics, and networks	75	2013	2010	2016	9,058	121	87
Perspective 3: Biases and characteristics of other agents	23	2014	2010	2016	3,171	138	83
Total	136						

including only papers published since 2010, Google Scholar citations of papers on the "managerial perspective" outnumber of the two other perspectives by more than 5,000, as shown in Panel B. The average (median) paper in the managerial biases category receives 23 (30) more citations than those falling into the "investor biases" category. Interestingly the small third category tops both of those means (and is above or close to both medians). The latter finding emphasizes, again, that Perspective 3 appears to be underdeveloped relative to its potential.

Finally, a word on methodology. Both of the main streams of Behavioral Corporate research heavily lean towards empirical work. In the Perspective-1 literature, only 13 of the 95 papers included in Figure 3 are mostly or purely theoretical. Most of those papers are published more recently (e. g., Bolton, Chen, and Wang (2013) on external financing and payout decisions with market timing), possibly making head against the empirical leanings of the overall Corporate Finance literature. The majority of the empirical papers focus on U.S. data, though some follow-up

papers use international data to speak to the breadth of a documented phenomenon or highlight differences across countries (e. g., Henderson, Jegadeesh, and Weisbach (2006) on the importance of market-timing motives in many countries). The share of theory work in the Perspective-2 research on managerial biases and characteristics is about the same, with 10 papers out of the 102 being mostly or purely theoretical. Almost all (9 out of 10) of the theoretical papers are focused on managerial biases rather than managerial characteristics or networks. One area that has attracted significant interest from theory is managerial overconfidence, as for example Gervais, Heaton, and Odean (2011) on endogenous compensation contracts and capital budgeting. Still, empirical work dominates, mostly following the style of Malmendier and Tate (2005, 2008) in that they build on a concrete psychological heuristic or bias, which is ideally modeled and then tested for empirically.

In terms of other methodology, I would emphasize the frequent inclusion of survey data in recent papers, which has also helped improve the psychological realism. Researchers recognize that it is worthwhile checking agents' stated beliefs and motives, before imposing them, whether behavioral or otherwise. For example, among Perspective-1 studies, Brau and Fawcett (2006) document market-timing motives in a survey of 336 CFOs about their IPO decisions, as do Brav, Graham, Harvey, and Michaely (2005), surveying 384 financial executives, for payout policies. An example from Perspective-2 research is Graham, Harvey, and Puri (2015), who survey over 1,000 CEOs and CFOs around the globe about their views and practices regarding capital allocation and delegation of decision-making to lower-level management. A relatively new trend is the use of individual-level, psychological analyses of managers, as for example psychometric tests on more than 1,500 U.S. and 800 non-U.S. CEOs and CFOs in Graham, Harvey, and Puri (2013), or the "detailed assessments of 316 candidates considered for CEO positions in firms involved in PE transactions", which are based on "4-hour structured interviews," in Kaplan, Klebanov, and Sorensen (2012).

Two tools that may been under-used in the literature so far are simulations and structural estimations. A few exceptions among the investor-biases papers are: Schultz (2003) and Baker, Taliaferro, and Wurgler (2006), who use simulations to gauge whether returns are predictable with managerial variables in small samples; Warusawitharana and Whited (2016), who use structural estimations to assess wealth transfers between selling and long-term shareholders in equity transactions and to show that managers' rational responses to misvaluation increase long-term shareholders' value by up to 4%; and Alti and Tetlock (2014) who pin down specific investor biases and structurally estimate the investment inefficiencies that result from firms adapting their investment decisions to investor overconfidence paired with trend extrapolation. Simulations and structural estimations have the potential to improve our understanding of the economic magnitudes and welfare implications arising from investor biases. Examples from the manager-biases research include Giat, Hackman, and Subramanian (2010), who develop a dynamic structural model in which optimism affects contracts and investment and calibrate the model to R&D investment data from the phar-

maceutical industry, concluding that "the average R&D manager is significantly optimistic about the value of an R&D project." This work illustrates that structural approaches will also help to identify the specific bias at work, similar in spirit to Alti and Tetlock (2014) for investor biases. Ma, Sraer, and Thesmar (2018) find aggregate TFP and output losses from systematic managerial biases in forecasting, compared to a counterfactual economy where managers have rational expectations.

I will argue below, most progress comes from papers that formulate a precise (if simple) theoretical model of a specific psychological phenomenon, citing the respective psychology or cognitive-science evidence, and that derives specific predictions allowing to test and possibly reject the theory.

3 An Illustration: Theory and Empirics of M&A

In this section, I focus on one of the largest areas of Corporate Finance research, the analysis of mergers and acquisitions, to illustrate how behavioral economics adds value by explaining the most important stylized facts, both from Perspective 1 (Biased Investors) and from Perspective 2 (Biased Managers).

3.1 Stylized Facts

The large volume of research on mergers and acquisitions reflects the enormous practical importance of these corporate decisions. Takeovers are among the largest investments firms make, and include multi-billion dollar deals (Vodafone's acquisition of Mannesmann for $202bn in 1999). We can measure their economic significance in terms of deal value, value of firms involved, shareholder value created or destroyed, and also jobs created, lost, or changed as a result of mergers and acquisitions.

The key observation that has puzzled researchers for a long time is that the value implications of mergers for the existing owners appear to be often negative. While empirical analyses generally estimate positive announcement returns to *target* shareholders, this is not the case for *acquirer* shareholders, at least not for a large portion of transactions and especially when the transaction is stock-financed. Below, I show these stylized facts in Tables 2 and 3, estimated on the most recent data available from the SDC Mergers and Acquisitions database.

To construct my data set, I start from all available data on transactions involving U.S acquirers since 1980.[18] To ensure comparability with existing M&A studies, I exclude government-owned entities or joint ventures, i. e., require the target type to be "Public," "Private," or "Subsidiary," following Fuller, Netter, and Stegemoller (2002). In addition, the deal status has to be "Completed" and, in order to exclude repurchases, self tenders, and stake purchases, the deal type has to be "Disclosed Dollar Value" or "Undisclosed Dollar Value," both as in Netter, Stegemoller, and

[18] SDC has only 66 observations before 1980. Cf. Betton, Eckbo, and Thorburn (2008).

Wintoki (2011). I also follow the latter paper in requiring that the acquirer held between 0 and 49 percent of target shares six months prior to the announcement, and acquired between 50 and 100 percent in the transaction.[19] I delete any duplicate observations, and, in a final step, restrict the sample to U.S. *public* acquirers that are included in CRSP and traded on NYSE, NASDAQ, or AMEX.[20] The final sample includes 4,698 acquisitions of public targets with available return information from CRSP. Of these, about 27% are cash mergers, 39% are stock mergers, and 14% are undertaken using mixed financing; for about 20% of acquisitions, the payment type is undisclosed.

In Table 2, I show the return to publicly traded targets over a $+/-1$ day event window. I calculate abnormal returns around merger announcements as the difference between the actual realized return and the return on the CRSP value-weighted index (including distributions). Note that, while I could use other models to calculate abnormal returns (CAPM-style, Fama-French 3-factor model, matching firms), the choice of benchmark tends to have little effect on the results for short event-windows, reflecting the small risk component over short horizons. Moreover, it is unclear whether other methods would even be preferable from a theoretical perspective since the estimation of parameters (e. g., estimates of α and β in a CAPM-style model) in the pre-event window might be confounded due to other firm events occurring in the pre-event estimation window (e. g. dividend changes). Table 2 shows that, for all types of payment, returns are significantly positive at the 1% level. On average, the abnormal return is 21.5%, which is a sizable gain for the shareholders of public targets.

In Table 3, however, we observe a different pattern for the shareholders of the bidding companies. The acquirers of public target companies, represented in the second and third rows of Table 3, experience negative returns on average, which contrast with the generally positive returns of the targeted U.S. public companies as seen above. Even for private targets in the fourth row, whose acquisitions are generally deemed more profitable (Fuller, Netter, and Stegemoller (2002), Betton, Eckbo, and Thorburn (2008)), a puzzlingly large fraction—over 25%—still generates negative abnormal returns, especially when stock-financed.

Overall, the effects of mergers appear to be rather volatile and sometimes highly negative, as further illustrated in the event-window representation of Figure 4.

A second set of puzzling stylized facts concerns the clustering of mergers. Mergers tend to occur in waves. That is, merger activity tends to be high during some time windows and low during others, as illustrated in Figure 5. The figure plots the number of U.S. publicly traded firms

[19] This requirement excludes deals where the total after the transaction amounts to 50-100% but less than 50% have been acquired, e. g., an acquirer going 30% percent before to 70% after the transaction. The argument for this is to focus on deals where the transaction is significant for both the bidder and target. However, in contrast to Fuller, Netter, and Stegemoller (2002), I do *not* require that the deal value be at least one million dollars.

[20] To match SDC with CRSP, I follow Malmendier, Opp, and Saidi (2016) and (i) transform 8-digit CUSIPs in CRSP into 6-digit CUSIPs (first six digits); (ii) remove observations with the higher 7th digit when 6-digit CUSIPs are not unique; (iii) match SDC and CRSP based on the 6-digit CUSIPs.

Table 2: Cumulative abnormal returns for public targets. Event window = [-1, 1].

Data on mergers and acquisitions is obtained from SDC. The sample period covers 1980-2016. Return data is obtained from CRSP. Abnormal returns are calculated as the difference between realized return and the return on the CRSP value-weighted index (including distributions). Returns are displayed as fractions. * and ** indicate whether the mean and median are different from zero at the 5%- and 1%-level, respectively, according to a two-sided t-test for the mean and Wilcoxon signed-rank test for the median.

	N	Mean	Median	p25	p75	S.D.	Min	Max
Full sample	4,698	0.215**	0.170**	0.058	0.320	0.257	-0.992	2.998
Cash merger	1,293	0.302**	0.246**	0.117	0.418	0.299	-0.896	2.574
Stock merger	1,814	0.174**	0.138**	0.036	0.274	0.222	-0.659	2.998
Mixed	660	0.208**	0.172**	0.075	0.305	0.211	-0.992	1.381
Unknown	931	0.181**	0.134**	0.027	0.284	0.256	-0.567	2.364

Table 3: Cumulative abnormal returns for acquirers. Event window = [-1, 1].

Data on mergers and acquisitions is obtained from SDC. The sample period covers 1980-2016. Return data is obtained from CRSP. Abnormal returns are calculated as the difference between realized return and the return on the CRSP value-weighted index (including distributions). Returns are displayed as fractions. * and ** indicate whether the mean and median are different from zero at the 5%- and 1%-level, respectively, according to a two-sided t-test for the mean and Wilcoxon signed-rank test for the median.

	N	Mean	Median	p25	p75	S.D.	Min	Max
Full sample	70,575	0.011**	0.003**	-0.018	0.029	0.095	-0.669	6.006
Public targets	6,960	-0.003**	-0.004**	-0.032	0.022	0.086	-0.514	2.141
U.S. public targets [(1)]	4,687	-0.009**	-0.007**	-0.038	0.019	0.083	-0.514	2.141
Private targets	41,966	0.012**	0.003**	-0.017	0.029	0.100	-0.666	6.006
Other and unknown	21,649	0.014**	0.004**	-0.015	0.031	0.086	-0.669	3.942
Cash merger	9,886	0.013**	0.005**	-0.015	0.031	0.068	-0.460	0.876
Stock merger	6,782	0.014**	-0.001*	-0.031	0.036	0.141	-0.516	4.496
Mixed	4,551	0.020**	0.007**	-0.026	0.053	0.114	-0.551	3.942
Unknown	49,356	0.010**	0.002**	-0.016	0.026	0.089	-0.669	6.006

[(1)] This restricts the sample to acquirers of target firms whose stock price reaction I examine in Table 2. The return data for the acquirer is unavailable in CRSP in 11 cases, which explains the slightly smaller number of observations compared to Table 2 (4,687 vs. 4,698).

in CRSP that delist in each year between 1926 and 2016 because of a merger or takeover as a fraction of all firms included in the CRSP database.

We can discern the conglomerate merger wave of the 1960s, the wave of acquisitions that helped to undo the very same conglomerates in the 1980s, and the global and strategic merger wave of the 1990s. Generally speaking, the windows of high merger activities are times of economic expansion.

Within a wave, mergers appear to occur in industry clusters. They are often a central channel

Figure 4: Aggregate change in market capitalization for successful acquirers.

This plot shows the aggregate dollar abnormal returns, in 2000 $, across deals in each year from 1980 to 2016. Aggregate dollar abnormal returns are obtained by first multiplying the market capitalization of the acquirer on the day before the start of the event window with the cumulative abnormal return over the event window, and then summing across deals within a given year. The Narrow event window is [-1, 1] (solid line), and the wide event window is [-15, 15] (dashed line). Sources: SDC Mergers and Acquisitions Database and CRSP, data retrieved in March 2017.

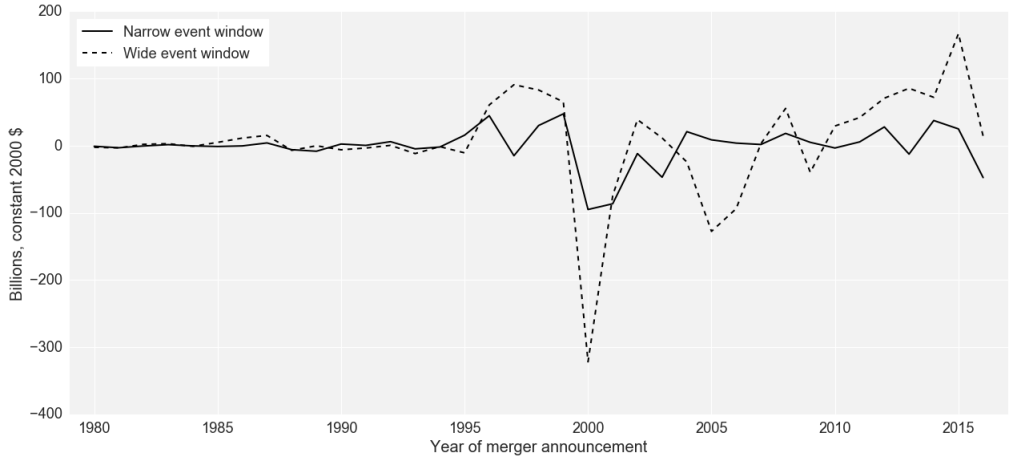
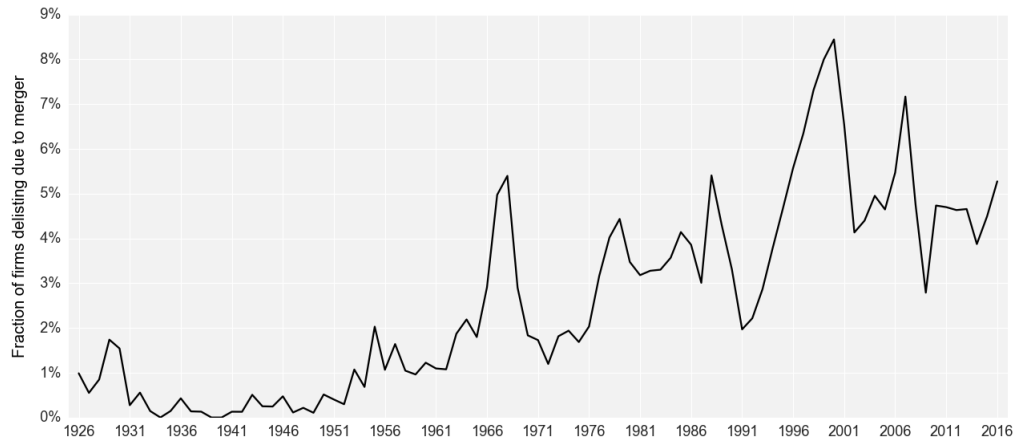


Figure 5: Merger waves.

This plot shows the number of firms delisted in each year in CRSP due to a merger or acquisition, as a fraction of the total number of firms in CRSP with share codes 10 or 11 and exchange codes 1, 2, or 3. Source: CRSP, data retrieved in December 2017.



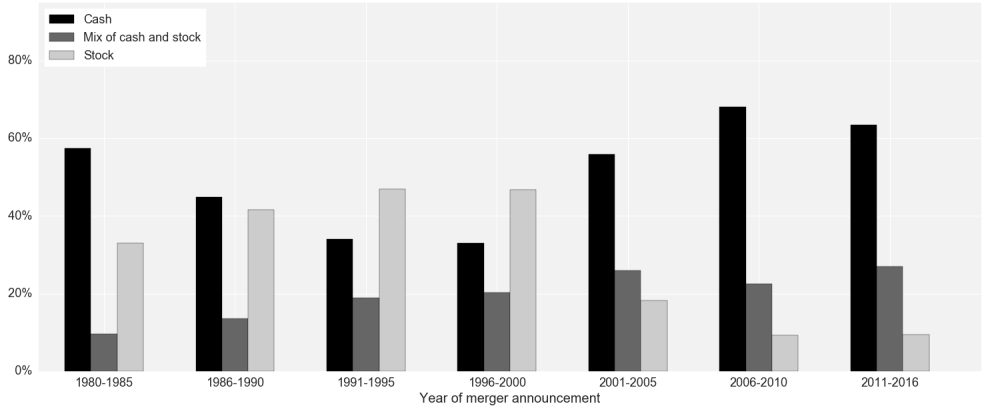
of industry restructurings, including both expansions and consolidations.

A third stylized fact concerns merger financing. The historical pattern and variation in merger financing over time is quite striking and has triggered much of the research discussed below. As indicated in Figure 6, the popularity of different payment methods has varied substantially over time, with stock payments peaking in the mid-1990s, and cash payments before (early 1980s) and

after (late 2000s).[21]

Figure 6: Payment method.

This plot shows the popularity of different payment methods between 1980 and 2016. Payment methods include cash (black), mixed (dark gray), and stock (light gray). Source: SDC Mergers and Acquisitions Database, data retrieved in March 2017.



These three sets of stylized facts are at the core of the huge literature on mergers and acquisitions. As argued by Betton, Eckbo, and Thorburn (2008), the observation that merger waves are correlated with economic expansions and high stock-market valuations, in particular, has been central in spurring the development of models in which merger waves result from market overvaluation and managerial timing, which I will discuss in the next section. Note that both behavioral and non-behavioral models have leveraged this fact. I will contrast this approach with the assumption of behavioral managers in the subsequent subsection.

3.2 Biased Investors

I now illustrate how a model of investor biases and managerial catering to such biases may help to better understand stylized facts about mergers and acquisitions. I use a variant of the model of Shleifer and Vishny (2003), which was motivated by the third stylized fact, about financing choice. It aims at explaining why, in the late 1990s, most deals were stock-financed. As this medium of financing has become less popular, even in times of high market valuation, the modeling approach naturally reveals some limitations. Nevertheless it serves to illustrate the basic insight—that managers might be able to detect mis-valuations of individual investors and cater to them in order to maximize their objectives. I will then discuss the empirical evidence in Rhodes-Kropf, Robinson, and Viswanathan (2005), which supports several of the model predictions.

[21] The figure leaves outs acquisitions with unknown form of payment as of the SDC database. For a complete picture see Table B.1.1 in Appendix B.1.

Table 4: **Model Notation**

	Capital stock	Current market value	Fundamental value
A-firm	K_A	$\widetilde{V}_A = S_A K_A$	$V_A = qK_A$
T-firm	K_T	$\widetilde{V}_T = S_T K_T$	$V_T = qK_T$
Combined firm	$K_A + K_T$	$\widetilde{V} = S(K_A + K_T)$ $= S_A K_A + S_T K_T + \widetilde{e}$	$V = q(K_A + K_T)$

3.2.1 Model and Predictions

Consider the following setting. The manager of an acquiring company A aims to acquire a target company T. I denote the 'fundamental value' (or long-run value) of any firm, per unit of capital, as q. Managers know the fundamental value of both their own firm and the potential merger partner, while investors might over- or underestimate them. As indicated in Table 4, the value of an acquiring company with K_A units of capital is thus $V_A = qK_A$; the value of a target company with K_T units of capital is $V_T = qK_T$; and if A acquires T, the value of the merged company is $V = q(K_A + K_T)$. The latter also implies that, in the long-run, there are no synergies from the merger. This simplification merely serves to illustrate the catering motivation. (I will generalize and include synergies below, again allowing managers to be fully informed about them.)

Investors believe the values of acquirer- and target-capital units to be S_A and S_T, and hence the current market values of acquirer firm and target firm are $\widetilde{V}_A = S_A K_A$ and $\widetilde{V}_T = S_T K_T$. In addition, they may misperceive the value of the merged company, and its market value will be $\widetilde{V} = S(K_A + K_T)$. We can separate out investors' misperception of the value created by the merger, $\widetilde{e} = \widetilde{V} - \widetilde{V}_A - \widetilde{V}_T$, and rewrite $\widetilde{V} = S_A K_A + S_T K_T + \widetilde{e}$, including the case that the perceived synergies are zero, $\widetilde{e} = 0$, and hence $\widetilde{V} = S(K_A + K_T) = S_A K_A + S_T K_T$.[22]

In the long-run, firm values converge to their fundamental value. In the short-run, rational managers of the acquiring company exploit the discrepancy between (short-run) market values and (long-run) fundamental values in the interest of their (existing) shareholders. Importantly, investors draw no inferences about the long-run (fundamental) value of their companies from acquisition announcements.[23]

Both target and acquirer managers are maximizing existing shareholders' wealth, though they assume different horizons: The A manager has a long-run perspective and is thus maximizing the

[22] I change the notation from Shleifer and Vishny (2003) to mirror Malmendier and Tate (2008), which allows for easy juxtaposition and ultimately nesting of the investor-biases and manager-biases perspectives in Section 3.4.

[23] This shortcoming of the myopic setting of Shleifer and Vishny (2003) is remedied in Rhodes-Kropf and Viswanathan's (2004) rational representation of a similar model, discussed in the next subsection. There, investors misvalue firms, relative to the private information of acquirer and target management. They rationally adjust to the announcement and announced financing of an acquisition, but might not fully adjust given their limited information. Partial market reaction can also be incorporated into the Shleifer and Vishny (2003)-setting and is excluded only for algebraic simplicity.

fundamental value, and the T manager is maximizing the short-term payoff.[24]

Let's now return to the main question and consider under which conditions the manager of company A would consider a cash-financed versus a stock-financed acquisition. In a cash-financed acquisition, denote the price paid per capital unit of the target firm as P, and hence the total cash payment c is $c = PK_T$.[25] In the short run, the announcement of A acquiring T will generate the following abnormal returns (announcement effect) to acquiring company shareholders:

$$S(K_A + K_T) - PK_T - S_A K_A = (S - S_A)K_A + (S - P)K_T.$$

Target shareholders, instead, will experience an announcement effect of

$$(P - S_T)K_T.$$

Hence, acquiring company shareholders gain from perceived synergies or perceived higher value of target capital (both of which feed into $S > S_A$) and from perceived underpayment relative to the market value of the merged company ($S - P > 0$). Vice versa, they lose from perceived dilution ($S - S_A < 0$) and perceived overpayment ($S - P < 0$). For target shareholders, instead, only (perceived) over- or underpayment relative to the market value, $P \gtrless S_T$, matters in the short-run.

In the long-run, the comparisons of P with S and P with S_T as well as the comparison between S and S_A turn out to be misguided. By assumption, the acquisition is a zero-sum game, $q(K_A + K_T) - qK_A - qK_T = 0$. Nevertheless, acquiring-company shareholders may benefit (or suffer) from the transaction, with a change in shareholder wealth (i. e., long-run abnormal returns) of

$$q(K_A + K_T) - PK_T - qK_A = (q - P)K_T,$$

and T-shareholders experiencing a change in shareholder wealth of

$$(P - q)K_T.$$

In other words, all that matters for shareholders in the long-run is the price paid relative to the fundamental value of the firm. Acquiring-company shareholders gain from underpayment ($P < q$) and target shareholders gain from overpayment ($P > q$) relative to the long-run value.[26]

[24] As Shleifer and Visny discuss, the different horizons may reflect true differences between target shareholders who "want to sell out" and acquirer shareholders who are locked in; or we can consider the horizon an outcome variable.

[25] Shleifer and Vishny (2003) do not spell out how the cash is generated. Company A may have cash available as part of K_A, or may need to sell some capital units to obtain cash. This matters because of the discrepancy between short-term and long-term valuations. To keep the algebra as simple as possible I propose the interpretation that A raises the cash via a loan, which the firm later repays at its nominal value PK_T.

[26] As anticipated in fn. 25, the precise formula for the change in shareholder wealth depends on how the cash is generated. If c available as part of the assets, then the transaction lowers the number of capital units K_A, and it might

Thus, even for the case of cash-financed acquisitions, the model framework illustrates how seemingly value-destroying acquisitions may actually create value to acquirer shareholders. The negative announcement effect merely reflects a low assessment of the merged company in current terms ($S < S_A$ and/or $S < P$), and acquirer shareholders will experience value creation due to the high long-run assessment q relative to the payment, $P < q$. Vice versa, mergers that seem to be value-creating to the acquirer and value-destroying to the target in the short-run may in reality benefit target shareholders due to a low long-run realization of synergy relative to price ($q - P < 0$).

These effects are exacerbated in stock-financed acquisitions. Let's denote the fraction of the merged company that target shareholders obtain as x and, for comparability, assume that the short-run value of this fraction is identical to the payment in a cash-financed acquisition, $x = \frac{PK_T}{\widetilde{V}}$. Thus, if target shareholders choose to sell their shares in the stock market, they obtain the same amount P per unit of capital. In this case, short-run abnormal returns (announcement effects) will be the same as before: $(S - S_A)K_A + (S - P)K_T$ for acquiring-company shareholders and $(P - S_T)K_T$ for target shareholders.

The long-run abnormal returns, however, are different. Even though we continue to assume that the value of the combined firm is identical to the sum of the stand-alone companies, the long-run abnormal returns experienced by acquiring-company shareholders now depend on the relative value of P and S rather than P and q. Specifically, the value A-Shareholders gain from the merger transaction now amounts to

$$\begin{aligned} & q(1-x)(K_A + K_T) - qK_A \\ = \; & q(1 - \frac{PK_T}{S(K_A + K_T)})(K_A + K_T) - qK_A \\ = \; & q(K_A + K_T - \frac{PK_T}{S}) - qK_A = q(1 - \frac{P}{S})K_T. \end{aligned}$$

And the value generated for T-Shareholders is the negative of this amount, $q(\frac{P}{S} - 1)K_T$.

Hence, under a stock-financed acquisition, A-shareholders gain from high valuation of the merged company relative to payment, $(S - P > 0)$, in the long-run, and the opposite is the case for T-shareholders $(P - S > 0)$.

The key insight here is that a stock-financed acquisition allow the A-manager to exploit the differences in misvaluation between target and acquirer ($S \neq S_A \neq S_T$) for value creation in the long run. Without the acquisition, A-shareholders would have experienced a long-run mean reversion

alter the long-term value q per average unit of capital as the NPV of cash and other assets differ. If, instead, A has no cash available and sells a fraction α of the K_A capital units in order to generate cash $c = PK_T$, the number of capital units decreases and the implied long-term value V_A becomes $(1-\alpha)qK_A + qK_T = (1 - \frac{PK_T}{S_A K_A})qK_A + qK_T$ instead of qK_A without the merger. (In this scenario, we also need to spell out whether former shareholders are included in the objective function.) Finally, as discussed above, a third possibility, which generates the simple formula in the text, is that A finances the cash transaction with a loan that it pays back at its nominal value PK_T later.

of $(q - S_A)K_A$, which is positive if A is initially underpriced, and negative if A initially overpriced. The *incremental* long-run returns that A-shareholders experience from the acquisition, instead, is positive if A-managers are able to set a price $P < S$. The most important implication of the model, then, is that A-shareholders gain from high short-term valuations of A as they allow them to set a price $P < S$. This holds even if the overall long-run returns are negative, and a naive observer might want to classify the merger as value-destroying. In that case, the returns are still not "as negative as they would have been without the acquisition."

More generally, the model sketched here features the key ingredients of a typical corporate-finance model in the "biased investor" camp: Investors misvalue an asset; managers (CEOs) realize the misvaluation; they then cater to investor biases by selling the asset when it is overvalued.

3.2.2 Empirical Evidence

Providing empirical evidence for this line of argument is not easy. There are two main hurdles.

First, to show that the above catering mechanism is at work, the empirical test has to identify overvaluation and relate it to the corporate decision to sell the overvalued asset, either directly in a stock issuance as in Baker and Wurgler (2000, 2002), or indirectly in a stock-financed acquisition as in Shleifer and Vishny (2003). But how can we find a good benchmark for over- and under-valuation? Prior attempts compare market-to-book ratios to those of other firms in the same industry or to the overall market (Ang and Cheng 2006). However, market-to-book ratios are also often used as proxies for Tobin's q.[27] Hence, empirical result using this proxy might be evidence for a neo-classical Q-driven theory of corporate decisions rather than behavioral determinants.

Second, the empirical test should assess the long-run abnormal returns caused by the catering decision, e. g., the issuance or takeover. In the model framework above, a short-term negative revaluation does not indicate that the merger destroyed shareholder value. To the contrary, it is consistent with managers exploiting the current overvaluation to create value for their shareholders. However, it is difficult to calculate long-run abnormal returns and attribute them to a specific corporate event, i. e., to separate out the return effects of other events in the corporate life of the firm. As discussed in Malmendier, Moretti, and Peters (2017), even observing a long-term decrease in the market valuation of the acquirer following an acquisition cannot serve as evidence that the acquisition destroyed firm value. The acquirer might have been overvalued to begin with, and the

[27] Tobin's *marginal* q captures the market value of an *additional* unit of capital relative to its replacement value. It is generally different from the *average* Q, the ratio of the market value of *existing* assets to their replacement value. While q is the correct measure that should govern a firm's (investment) decisions, it is not directly observable. Researchers oftentimes use the market-to-book ratio to proxy for q, even though this ratio is more of a proxy for Q. (Hayashi (1982) derives the conditions under which the two concepts coincide.) However, market-to-book ratios will not even capture average Q when current stock market valuations are excessively high or low, i. e., in times of over- or undervaluation. In light of these considerations, Lang and Stulz (1994) argue that the market-to-book ratio at best serves as a "coarse proxy" for q, and might be a proxy for misvaluation.

market value would have dropped even more without the acquisition.[28]

In addition to these two empirical hurdles – how to identify misvaluation and how to identify long-run abnormal returns – a further challenge is that a (quasi-)rational model of misvaluation generates very similar predictions. For example, Rhodes-Kropf and Viswanathan (2004) also aim to explain the stock-financed merger wave of the 1990s, but fully adhere to rational belief formation and to both acquirer and target maximizing the long-run (fundamental) value of their shareholders' wealth, rather than assuming a different, short-run horizon for the target management.

Instead of the behavioral assumptions, Rhodes-Kropf and Viswanathan (2004) introduce two different frictions to generate the empirical stylized facts they aim to explain. The first is informational asymmetry: The acquirer has private information about the stand-alone value of their firm and the synergies generated by the potential merger, and target management has private information about the stand-alone value of their firm. Second, Rhodes-Kropf and Viswanathan (2004) distinguish between two components of misvaluation, market-wide and firm-specific misvaluation, and assume that managers cannot distinguish between them. That is, as in Shleifer and Vishny (2003), firms can be over- or undervalued; but differently from Shleifer and Vishny (2003), only managers know whether their firm (but only their firm) is mispriced, and even they cannot determine whether the mispricing is market-level or firm-level mispricing. For example, managers might have superior information about sales prospects relative to investors, but cannot judge how much of this information is firm-specific and how much is relevant for other firms as well.

In this setting, the target tries to assess whether the long-run value of the offered shares lies above the long-run stand-alone value of the firm. Target management knows the latter, its own fundamental value, but does not know the former. While it draws some inferences from its own misvaluation, this inference is limited because it cannot distinguish between market-wide and firm-specific misvaluation. When the market-wide overvaluation is particularly high, the target underestimates the market-wide component (i. e., attributes too much of its own misvaluation to firm-specific overvaluation and filter out too little of the market-wide effect) and thus overestimate the fundamental value of the offer. The opposite holds when the target overvaluation is mostly firm-specific.

Hence, the model framework generates a link between misvaluation and merger waves similar to Shleifer and Vishny (2003). The key difference to the Shleifer and Vishny (2003) interpretation is that the target management is not irrational or short-term oriented when accepting overvalued "currency" (acquirer/merged company stock) to merge; it is the best they can do given their information set. Moreover, informational constraints might also be at work in the market's misvaluations.

[28] Malmendier, Moretti, and Peters (2017) propose a novel identification strategy to estimate long-run abnormal returns. They compare the performance of bidders who acquire a company to that of bidders whose acquisition attempt for the same firm fails. They focus on contested deals where either bidder was ex ante a plausible acquirer, and find that, in these contests, losers outperform winners by 24 percent over the following three years. The underperformance of winners is especially pronounced in all-cash acquisitions.

The comparison illustrates that it is hard to distinguish whether limited updating reflects informational constraints or biases. Indeed, Rhodes-Kropf, Robinson, and Viswanathan (2005), who empirically test the prediction that misvaluations trigger merger waves, argue that the theories put forth by Shleifer and Vishny (2003) and Rhodes-Kropf and Viswanathan (2004) "although economically very different, [...] yield parallel empirical predictions." Developing direct measures of biased belief formation, as done in the Perspective 2 (Biased Manager) literature (e. g., the *Longholder* measure for CEO overconfidence) would thus be an important additional step in the literature, and the lack thereof in the current literature is a third hurdle to providing empirical evidence.

With these caveats in mind, it is still interesting to review the empirical evidence Rhodes-Kropf, Robinson, and Viswanathan (2005), especially as their research design is able to address to some extent the two hurdles mentioned at the beginning – how to identify misvaluation and how to identify long-run abnormal returns. Below, I replicate their main results regarding the relative valuations in mergers and extend the sample period to December 2016.

To test the link between (mis-)valuations and merger activity, Rhodes-Kropf, Robinson, and Viswanathan (2005) make one further distinction, relative to Shleifer and Vishny (2003) and Rhodes-Kropf and Viswanathan (2004): Rather than distinguishing only between a fundamental value V and a possibly different current market value $\widetilde{V}$, they also allow for time variation in the fundamental value. Specifically, they distinguish between current and long-run fundamental values, denoted V^C and V^{LR} respectively. The current measure represents the true value of a firm at a given time, which may include temporary sector-wide booms or busts, whereas the long-run measure averages out any time-varying sector-wide effects. The long-run fundamental value V^{LR} essentially captures the fundamental value V introduced in the original setup. However, the empirical estimates are a function of (time-varying) book value, and hence vary over time.

Using this distinction, Rhodes-Kropf, Robinson, and Viswanathan (2005) decompose the market-to-book ratio $\frac{\widetilde{V}}{B}$ into $\frac{\widetilde{V}}{V^C} \times \frac{V^C}{V^{LR}} \times \frac{V^{LR}}{B}$, where B is the book value of a firm, and $\widetilde{V}$ is the firm's current market value, as in our previous notation. Taking logs, this equation becomes $\widetilde{v} - b = (\widetilde{v} - v^C) + (v^C - v^{LR}) + (v^{LR} - b)$, with lowercase letters representing logged values.

Thus, the market-to-book ratio is decomposed into three parts:

1. firm-specific error $(\widetilde{v} - v^C)$, i. e., the difference between the current market value and to the current fundamental value;

2. time-series sector error $(v^C - v^{LR})$, i. e., the difference between the current fundamental value and the long-run fundamental value;

3. the difference between the long-run fundamental value and the book value $(v^{LR} - b)$.

These three components help to disentangle true long-run growth opportunities from over- or under-valuation. The firm-specific error measures the valuation of a firm to those of other firms in the same industry and thus captures idiosyncratic misvaluation. The time-series sector error compares valuations in an industry to their long-run average, and thus captures misvaluation affecting a whole industry, or even the entire market. And the last piece, the long-run value relative to book value, is meant to separate out the neoclassical q-theory element, i. e., long-run growth opportunities. From the perspective of Shleifer and Vishny (2003)'s model, a two-fold distinction between the price-to-fundamental-value ratio and the fundamental-value-to-book-value ratio would have sufficed. The further distinction between firm- and industry- or market-specific misvaluation is inspired by the simultaneous test of the more rational approach of Rhodes-Kropf and Viswanathan (2004); here, I focus on the behavioral interpretation.

Using these distinctions, the authors derive three testable predictions.[29]

Prediction 1. Overvalued firms use stock to buy relatively undervalued firms when both firms are overvalued.

While our discussion of Shleifer and Vishny (2003)'s model has emphasized the overvaluation of the acquirer as motivation, the authors also consider the target, who seeks out an acquisition when their own stock is overvalued. The acquirer, of course, is even more overvalued so that acquisition is still profitable in the long-run, despite the overpayment for the overvalued target.

The same argument directly implies the next prediction:

Prediction 2. Overall merger activity is higher in overvalued markets. On average, firms in overvalued sectors use stock to buy firms in relatively less overvalued sectors.

And finally, merging the consideration of stock- and cash-financed mergers, the authors formalize the insight that cash mergers are driven by undervaluation of the target, while stock mergers are driven by overvaluation of the acquirer:

Prediction 3. Cash targets are more undervalued than stock targets. Cash acquirers are less overvalued than stock acquirers.

How do the authors implement a credible empirical test? How do they proxy for the three ingredients current market value, current fundamental value, and long-run value (and thereby overcome the challenge to identify misvaluation and long-run abnormal returns)? Only current market value is immediately available from stock-market data.

The authors utilize three different valuation models to proxy for the two fundamental values, all of which impose a linear structure between market value and book value and other accounting variables (net income and leverage), but which differ in their identifying restrictions. The fitted

[29] They further derive two predictions regarding the frequency of mergers.

values from these regressions produce estimates of a firm's fundamental values, where the firm's current fundamental value is book value of assets plus residual (i. e., plus the residual income produced by those assets) and the firm's long-run fundamental value is the average of the current fundamental value over time.

For example, their simplest model takes the form $v^C_{ijt} = \alpha_{0jt} + \alpha_{1jt} b_{ijt} + \varepsilon_{ijt}$, where i indexes firms, j industries, t time, and ε is the error term. For each industry-year cross-section of firms, they regress log market value on log book value, using the Fama-MacBeth procedure (Fama and MacBeth (1973)). The current fundamental value of a firm v^C_{ijt} is then estimated as $\hat{v}^C_{ijt} = \hat{\alpha}_{0jt} + \hat{\alpha}_{1jt} b_{ijt}$, where $\hat{\alpha}_{0jt}$ and $\hat{\alpha}_{1jt}$ are the industry-year specific coefficient estimates, and the current market value (from stock-market data) minus this estimated current fundamental value of a firm, i. e., $\widetilde{v}_{ijt} - \hat{v}^C_{ijt}$, is thus the firm-specific error. The long-run fundamental value is calculated as $\hat{v}^{LR}_{ijt} = \bar{\alpha}_{0j} + \bar{\alpha}_{1j} b_{ijt}$, where the $\bar{\alpha}$'s are averaged within industry across time, and hence the time-series error is $\hat{v}^C_{ijt} - \hat{v}^{LR}_{ijt}$. Lastly, the long-run value to book is measured as $\hat{v}^{LR}_{ijt} - b_{ijt}$.

As acknowledged in the paper, it is easy to challenge many aspects of these approaches—the linearity assumption, the interpretation of the accounting variables, the estimation procedure, etc. However, both from the perspective of the accounting and the perspective of the existing finance literature, the details of the analysis and the broad array of robustness checks put the approach to the forefront of credible evidence.

Table 5: Decomposition of Market-to-Book Ratio (Extension)

This table replicates Table 6 in Rhodes-Kropf, Robinson, and Viswanathan (2005) for an extended sample period ending 12/2016. The estimation is based on 205,170 nonmerger firm-level observations and 11,692 (completed) merger observations in which participating firms are included in CRSP and Compustat. The data contain 1,559 all-cash deals, 2,090 all-stock deals, and 812 mixed deals. See Rhodes-Kropf, Robinson, and Viswanathan (2005) and Figure B.2.2 for a more detailed description of the three models used for the firm-level decomposition of the market-to-book ratio.

Valuation component	Overall comparison			Only mergers			Only all cash			Only mixed			Only all stock		
	No Merg.	Merg.	t(diff)	Tar.	Acq.	t(diff)	Tar.	Acq.	t(diff)	Tar.	Acq.	t(diff)	n Tar.	Acq.	t(diff)
Market-to-book (log)	0.47	0.74	-24.62	0.65	0.82	-10.63	0.63	0.79	-5.41	0.65	0.82	-4.23	0.71	0.94	-8.56
Model 1:															
Firm-specific error	-0.02	0.32	-34.17	0.10	0.53	-29.10	0.02	0.51	-18.52	0.18	0.52	-8.74	0.15	0.60	-18.40
Time-series sector error	0.01	0.06	-17.93	0.07	0.06	1.04	0.08	0.09	-0.57	0.11	0.11	-0.18	0.09	0.09	-0.15
Long-run value to book	0.48	0.35	33.58	0.48	0.23	39.03	0.53	0.19	31.86	0.35	0.19	10.04	0.47	0.25	19.12
Model 2:															
Firm-specific error	-0.01	0.28	-31.62	0.12	0.43	-23.57	0.06	0.37	-13.07	0.18	0.43	-7.53	0.16	0.52	-15.60
Time-series sector error	0.01	0.07	-19.41	0.07	0.07	-0.51	0.08	0.09	-1.21	0.13	0.13	0.19	0.09	0.10	-1.19
Long-run value to book	0.48	0.39	17.86	0.47	0.31	17.70	0.49	0.32	11.17	0.34	0.26	3.67	0.45	0.32	9.68
Model 3:															
Firm-specific error	0.00	0.23	-27.78	0.11	0.34	-16.77	0.01	0.30	-11.80	0.20	0.35	-3.81	0.07	0.39	-12.23
Time-series sector error	0.01	0.04	-12.76	0.04	0.05	-2.50	0.04	0.07	-3.40	0.07	0.09	-1.02	0.05	0.05	-0.33
Long-run value to book	0.49	0.53	-5.63	0.57	0.50	5.34	0.62	0.47	6.22	0.45	0.44	0.37	0.70	0.64	2.34

Here, I replicate their calculations, both for the original sample period and for an extended sample until December 2016. I provide a detailed comparison between the original estimates, the replication, and the replication with the extended sample in Appendix B.2. Figures B.2.1 and B.2.2 provide detailed plots of the estimated "factor loadings" under the different valuation models employed. In all three models, the estimates from the replication exercise are qualitatively and quantitatively very similar across industries, with an average correlation of 0.88. The loadings remain nearly unaltered when using the extended sample until December 2016.

Equipped with these estimates, I implement the market-to-book ratio decomposition and re-test the main predictions of valuation-driven merger wave theories. Table 5 shows the results for the extended sample period. (Table B.2.1 in Appendix B.2 shows the results when replicating for the same period.[30]) The results are largely very similar to the original ones in Rhodes-Kropf, Robinson, and Viswanathan (2005), and generally support the main empirical predictions tested in their paper. Consistent with Prediction 1, the firm-specific error $\widetilde{v} - v^C$ is smaller for targets than for acquirers in all three models, implying relatively higher overvaluation of the acquirer, while the sum of firm-specific and time-series error, $\widetilde{v} - v^{LR}$, is larger for all firms involved in mergers than for non-merger firms, implying overvaluation of all firms participating in mergers. Moreover, the latter result is more pronounced in stock-financed than cash-financed takeovers. For example, focusing on Model 3 in Table 5, the firm-specific error is 0.11 for targets and 0.34 for acquirers; the combined firm-specific and time-specific error is 0.01 for non-merger firms, but 0.27 for merger firms, and is, with a value of 0.39, especially large for acquiring firms. In addition, the firm-specific error is lower for targets and acquirers in cash deals (0.01 and 0.30) compared to stock deals (0.07 and 0.39).

Consistent with Prediction 2, the time-series error $v^C - v^{LR}$ is also larger for acquirers than targets for most models and methods of payments, confirming that firms in overvalued sectors buy firms in less overvalued sectors. I note, though, that the differences are less strong in my extended-sample replication than in the original study. For example, under Model 3, the difference in the time-series error between acquirers and targets ranges from 0.02 in stock mergers to 0.05 in cash mergers when I replicate the estimates for the original sample period (see Table B.2.1), and becomes slightly smaller, though directionally unchanged, for the extended sample, amounting to 0.01 when pooling across payment types (see Table 5).

Finally, in support of Prediction 3, the firm-specific errors $\widetilde{v} - v^C$ and the time-series errors $v^C - v^{LR}$ are larger for stock than cash targets. For example, in Model 3, the firm-specific and time-specific errors are 0.07 and 0.05 for stock targets, and 0.01 and 0.04 for cash targets. The evidence is somewhat mixed under the other specifications, though strong under Model 2.

[30] Figures B.2.3 to B.2.6 provide a visual comparison between results of the replication and extension analysis and those of the original paper.

The key insights here is that, as far as we can disentangle, the high valuation of stock acquirers largely reflects overvaluation, or firm-specific error and time-series error in the language of Rhodes-Kropf, Robinson, and Vishnawathan.

An alternative approach, and attempt to differentiate between high valuation consistent with long-run growth factors and over-valuation, is Dong, Hirshleifer, Richardson, and Teoh (2006)'s focus on residual-income value. They calculate a firm's fundamental value as the sum of its book value and the discounted value of all future expected residual incomes, following Ohlson (1995). The ratio of market price to residual-income value functions as the measure of overvaluation. To implement this approach, they need a measure of expected return on equity, and use earnings-per-share forecasts of analysts contained in the I/B/E/S database for this. Dong, Hirshleifer, Richardson, and Teoh (2006) confirm that acquirers have substantially higher valuations than targets, and that highly valued bidders strongly prefer stock as the means of payment. They also conclude that the evidence in favor of the misvaluation theory of mergers is particularly strong during the second half of their sample period (1990-2000).

In summary, the most successful attempts to provide evidence of the catering-to-behavioral-investors approach to corporate finance go beyond simple market-to-book measures as proxies of overvaluation and attempt to exploit accounting variables as well as within- and across-sector comparisons of financial variables to tease out elements of overvaluation. The key findings confirm that managerial catering is consistent with a broad range of stylized facts and offers a new interpretation, in particular for the 1990's wave of stock financed mergers.

3.3 Biased Managers

A different behavioral approach, 'Perspective 2' from above, focuses on potential biases on the side of the manager. In the case of mergers and acquisitions, this literature is also, like Perspective 1, motivated by the puzzling negative abnormal announcement returns to acquisitions and acquirers' seeming long-term underperformance. However, instead of re-interpreting these observations as a hard-to-detect value creation, this second approach argues that managers engage in truly value-destroying acquisitions, and aims to show the reasons.

3.3.1 Model and Predictions

Before I return to and expand the model set-up from the previous section to account for managerial biases, let's step back from considering just one given acquirer A. Instead, let's follow the earliest paper in this literature, Roll (1986), and consider N potential acquirers $A_1, A_2, ..., A_n, ..., A_N$ of a given target T. Their current market values are $V_{A_1}, V_{A_2}, ..., V_{A_N}$, and V_T. Each of them goes through a valuation process and comes up with an expected value of the merger, which I denote

by $E_n[V_n] - V_{A_n}$ for company A_n. In this setting, the question is how much company A_n should bid (at most) for target T.

Under full rationality, we can derive the answer following Vickrey (1961) if we assume a private-values setting, and following Milgrom and Weber (1982) for a common- or affiliated-values setting. For example, if expectations are based on a signal drawn from a common distribution, the bid b_n of company n has to be strictly lower than the expected value generated by the acquisition, $b_n < E_n[V_n] - V_{A_n}$. This setting includes the case of a buy-out firms acquiring T, and hence $E_n[V_n] - V_{A_n} = E_n[V_T]$, the scenarios that signals about the future value of T are drawn from a common distribution and hence $b_n < E_n[V_T]$. If bidders bid above this lower bound, instead, we have a case of winner's curse, as first discussed in Capen, Clapp, and Campbell (1971) (cf. also Thaler 1988).

Within this set-up, Roll (1986) was the first to propose a "hubris hypothesis" of overbidding. That is, he put forward the notion that acquirers bid more because they overestimate the value they are able to generate in a merger.

We can motivate and formulate the hubris hypothesis in two manners. The version advanced by Roll (1986) is that bidders bid above the theoretical upper bound because they do not account for the winner's curse. In that scenario, bidders fail to shade their bid and are willing to pay up to $E_n[V_T]$. An alternative interpretation of high bids is that bidders account for winner's curse, and shade their bids appropriately, but over-estimate the private-value element of the auction. That is, they think that the acquisition will generate more value just when their own company is the acquirer, while that is not the case.

As Roll (1986) points out, overbidding is quite plausible for a number of reasons. First, we observe bids above the current market value of the target, $b_n > V_T$, but rarely bids below, $b_n < V_T$. Thus we observe upward but not downward biased errors. Second, managers and firms have little opportunity to learn from past mistakes. Typically, managers make relatively few acquisitions over their managerial lifetimes (tenures), and even those they undertake have rather noisy outcomes. It is hard to distinguish abnormal returns caused by the merger from returns due to other corporate events. Third, executives appear particularly prone to display overconfidence, both in the form of excessive optimism and overestimation of the precision of signals, in experiments, psychometric tests, and predictions of financial and macroeconomic series (Kidd (1970), Larwood and Whittaker (1977), Moore (1977), Ben-David, Graham, and Harvey (2013), and Graham, Harvey, and Puri (2013)). This might be due to selection; but it might also reflect three main factors that social psychologists have found to foster overconfidence: (1) being in control, including the illusion of control, (2) high commitment to good outcomes, and (3) ambiguous benchmarks for success and failure (Weinstein (1980), Weinstein and Lachendro (1982), and Alicke, Klotz, Breitenbecher, Yurak, and Vredenburg (1995)). All of these criteria directly apply to the situation top-level executives

find themselves in: Their high position inside the company puts them in control, and additionally they may experience the illusion of having more control over noisy outcome variables than they actually do. Having pursued a successful corporate career they are also highly committed to good corporate outcomes, and stock-based compensation tends to reinforce such incentives. And finally, it is notoriously difficult to assess the causal effect of mergers as discussed in a large literature on mergers, including in Savor and Lu (2009) and Malmendier, Opp, and Saidi (2016).

Roll's (1986) work provided an important starting point for the literature on managerial biases and their impact on corporate outcomes. As in the behavioral-investors literature, however, its empirical validation faced the obstacle that it is hard to distinguish behavioral biases from asymmetric information, i. e., over-valuation relative to the rational benchmark (due to biases) from over-valuation relative to the full-information setting (due to informational asymmetries). Moreover, Roll focuses on the decisions of manager or firm, and does not consider their market interaction with investors who do not suffer from hubris.

An early theoretical example that is Heaton (2002), who was the first one to argue that commonly observed investment distortions could stem from managerial overconfidence, i. e., managers overestimating the returns to their projects. The first papers to model out the application to investment, mergers and capital structure *and* test these predictions empirically are Malmendier and Tate (2005), Malmendier and Tate (2008) and Malmendier, Tate, and Yan (2011). Here, I present a variant of Malmendier and Tate (2008), to continue with mergers and acquisitions illustrations.

Returning to the model set-up of the previous section, consider again a single acquirer and a single potential. In order to analyze managerial biases, I introduce a few extensions as well as simplifications, which are indicated in Table 6.

First, I generalize the previous definition of fundamental values to allow for non-zero synergies, or other merger-induced changes in valuation. That is, I generalize V to be the sum of the merging firms' stand-alone values, $V_A + V_T$, plus value e created by the merger, $V = V_A + V_T + e$, or, using the prior notation, $V = q(K_A + K_T) + e$. We can think of e as the true synergies generated by the merger or, more generally, any increase in market value beyond the sum of the stand-alone values, e. g., due to merger-induced increases in market power. If the merger is value-destroying, e can be negative. In the previous subsection, we had streamlined the model and set $e = 0$ as such effects were irrelevant to the main insight of the Shleifer and Vishny (2003)-model. In this section, e and its potential misperception by the A-manager will play a more central role.

Second, I introduce a separate notation for the A-manager's valuations. As shown in the last column of Table 6, I denote the A-manager's perception of the own firm's value as $\widehat{V}_A$, the A-manager's perception of the target firm as $\widehat{V}_T$, and his perception of the potentially merged firm as $\widehat{V}$. Mirroring the expression for the current value of the merged firm ($\widetilde{V}$) and the now-generalized expression for the fundamental value of the merged firm (V), we can spell out that $\widehat{V}$ might differ

Table 6: **Extended Model Notation**

	Capital stock	Fundamental value	**Valuation of A-manager**
A-firm	K_A	$V_A = qK_A$	$\boldsymbol{\widehat{V}_A}$
T-firm	K_T	$V_T = qK_T$	$\boldsymbol{\widehat{V}_T}$
Combined firm	$K_A + K_T$	$V = V_A + V_T + \boldsymbol{e}$ $= q(K_A + K_T) + \boldsymbol{e}$	$\boldsymbol{\widehat{V} = \widehat{V}_A + \widehat{V}_T + \widehat{e}}$

from the sum of the merged firms' perceived values, $\widehat{V} = \widehat{V}_A + \widehat{V}_T + \widehat{e}$, where $\widehat{e}$ is the additional value the manager believes will be created by the merger. We can think of $\widehat{e}$ as the perceived synergies, or other increases in perceived value attributed to the merger.

Finally, I will for now abstract from deviations of the current market valuations from firms' fundamental values. (This is the reason why I am omitting the column on "Current market value" in Table 6. I will reintroduce it in the next subsection, where I consider both managerial and investor biases.) In other words, I equate

- $\widetilde{V}_A = V_A$ or, equivalently, $S_A = q$,
- $\widetilde{V}_T = V_T$ or, equivalently, $S_T = q$, and
- $\widetilde{V} = V$ or, equivalently, $S = q$ and $\widetilde{e} = e$.

Our focus is on managerial overconfidence. The A-manager overestimates the value his own company ($\widehat{V}_A > V_A$), and further overestimates the value of the merged company ($\widehat{V} - \widehat{V}_A > V - V_A$), at least under some financing scenarios, i. e., for some ranges of cash payments $c \geq 0$. (As will become clear, the misperception of the manager will be affected by the chosen form of merger financing.) For simplicity, I assume $\widehat{V}_T = V_T$, i. e., abstract from biased beliefs about the target company, and I also do not consider potential biases on the side of the target management.

How much is the A-manager willing to pay for T, and how does his willingness to pay for the target depend on cash- versus stock-financing? I start from considering the case of a single acquirer with full bargaining power. In this case, target shareholders require $P = S_T = q$, i. e., the acquirer has to pay V_T for the target for the merger to go through, independent of the CEO's overconfidence.

If the CEO is rational ($\widehat{V}_A = V_A$ and $\widehat{V} = V$), he conducts the takeover if and only if he can find a combination of cash payment c and stock payment x such that $(1-x)(V_A + V_T + e - c) - V_A > 0$. Since full bargaining power implies $x = \frac{V_T - c}{V - c}$, this condition simply amounts to $e > 0$. That is, as expected, rational CEOs make the first-best acquisition decision: They acquire the other company if and only if the acquisition is value generating ($e > 0$), and this decision is independent of the amount of cash c used in the transaction, ranging from $c = 0$ in a fully stock-financed acquisition to $c = V_T$ in a fully cash-financed acquisition. Since the capital market is fully efficient, there is no

extra cost of raising external capital to finance the merger, and the CEO is indifferent among cash, equity, or a combination.

This decision rule changes if the manager is overconfident. Now, the decision solves the following trade-off: On the one hand, the overconfident manager overestimates the returns to undertaking the merger ($\widehat{e} > e$), making him more inclined to pursue the acquisition than the rational manager from the vantage point of perceived value creation. On the other hand, the overconfident manager perceives the required (stock) financing as too costly. Target shareholders require a fraction $x = \frac{V_T - c}{V - c}$ in order to tender their shares. The overconfident manager expects the value of the merged company to be $\widehat{V}$ rather than V and thus believes that ownership of a fraction $\widehat{x} = \frac{V_T - c}{\widehat{V} - c}$ would be appropriate. In the spirit of models where agents "agree to disagree," target shareholders insist on x as their reservation price, while the A-manager believes that the correctly calculated reservation price is $\widehat{x}$, but the merger only takes place if the payment is at least x.

As a result, an overconfident manager chooses to undertake the acquisition if and only if $(1 - x)(\widehat{V}_A + V_T + \widehat{e} - c) > \widehat{V}_A$, or equivalently $\widehat{e} > (x - \widehat{x})(\widehat{V} - c)$. That is, the perceived merger synergies $\widehat{e}$ have to exceed the perceived loss due to dilution, $(x - \widehat{x})(\widehat{V} - c)$.[31] The perceived loss from dilution decreases in the amount of cash financing c as only the (partial) equity financing entails a loss to current shareholders.

These insights generalize with slight modifications to a setting where the acquirer has less than full bargaining power and hence the cash-only price per capital unit P is larger than the current market value, $P > S_T = q$. For example, if target shareholders are able to extract a fraction $\alpha \in (0, 1)$ of the surplus e, the A-manager needs to offer ownership $x = \frac{V_T + \alpha \cdot e - c}{V - c}$ (in addition to cash financing c). The decision of the rational manager remains unaltered. As long as the merger creates value, a rational CEO will pursue it. An overconfident A-manager believes, however, that the target's ownership share should be $\widehat{x}$ as defined by $\widehat{x} \cdot (\widehat{V} - c) = V_T - c + \alpha \cdot \widehat{e}$, or $\widehat{x} = \frac{V_T + \alpha \cdot \widehat{e} - c}{\widehat{V} - c}$, and hence acquires if and only if $(1 - x)(\widehat{V} - c) > \widehat{V}_A$, i. e., if $(1 - \alpha)\widehat{e} > (x - \widehat{x})(\widehat{V} - c)$. Thus, the threshold for conducting the merger (the inequality) becomes harder to meet, but the basic insight remains unaltered.[32]

The different thresholds, or conditions, for rational and overconfident CEOs to conduct a merger turn the perspective of neoclassical economics on its head. Previously, economists had argued that

[31] Another formulation of the inequality is $\widehat{e} > x(\widehat{V} - V)$. That is, we can express the perceived loss as the ownership fraction target shareholders obtain times the difference in perceived and actual value of the merged firm.

[32] The A-manager believes both that the target shareholders are underestimating the value of the merged firm by $\widehat{V} - V$ and therefore asking for too high an ownership share, and that the target shareholders are underestimating the surplus generated (by $\widehat{e} - e$) and are therefore implicitly asking for too low an amount as part of their share of the surplus. Sharing the (perceived) surplus lowers the perceived benefit (on the LHS of the inequality) by $\alpha\widehat{e}$, but also changes the perceived dilution as it affects the difference $x - \widehat{x}$, and thus the RHS changes by $\alpha(\frac{e}{V - c} - \frac{\widehat{e}}{\widehat{V} - c})(\widehat{V} - c)$, or $\alpha e \frac{\widehat{V} - c}{V - c} - \alpha\widehat{e}$.

seemingly value-destroying mergers either (1) are not truly value-destroying, and we are simply mismeasuring the returns to mergers, or (2) are value-destroying from shareholders' perspective but optimal from the CEO's perspective, i. e., reflect misaligned incentives between principal and agent. Behavioral Corporate Finance research on biased managers, instead, proposes that even without incentive misalignment CEOs may choose to conduct value-destroying mergers (or abstain from value-increasing ones). In other words, the theory sketched above implies that mergers might neither be optimal for shareholders nor for managers.

3.3.2 Empirical Evidence

How can we make this claim testable in the data? The model points to the tradeoff between heightened acquisitiveness due to overestimated merger returns ($\widehat{e}$) and perceived dilution or financing frictions. We can leverage this insight to derive several hypotheses that can be rejected empirically.

Let's first consider merger frequency. As the above model illustrates, the common intuition of overconfident CEOs pursuing more mergers, which is at the heart of Roll (1986)'s hubris hypothesis, does not always hold. It depends on the availability of financing. If a firm has sufficient liquidity to fully cash-finance a merger, then it does hold: The overconfident CEO's decision rule to pursue a merger if and only if $\widehat{e} > (x - \widehat{x})(\widehat{V} - c)$ simplifies to $\widehat{e} > 0$, compared to the rational CEO's decision rule $e > 0$.[33] That is, only the overestimation of the value the CEO expects to create would matter, and we observe heightened acquisitiveness. Moreover, an overconfident CEO prefers to fully cash-finance an acquisition whenever possible.

Lemma 1. *An overconfident CEO exhausts his supply of internal (non-diluting) assets before issuing equity to finance a merger.*

Proof. An overconfident CEO perceives the post-acquisition value to current shareholders as $G = (1-x)(\widehat{V} - c) = \frac{V-c+V_T+c}{V-c}(\widehat{V} - c) = \frac{(V_A+e)\left(\widehat{V}_A+V_T+\widehat{e}-c\right)}{V_A+V_T+e-c}$. Then $\frac{\partial G}{\partial c} = \frac{V_A+e}{(V-c)^2}(\widehat{V}_A - V_A + \widehat{e} - e) > 0$ (as $\widehat{V}_A > V_A$ and $\widehat{e} > 0$ by assumption). Post-merger value is maximized on $c \in [0, V_T]$ by setting c as high as possible. Q.E.D.

Hence, overconfident CEOs are predicted to maximize cash financing (or, more generally, financing with non-diluting assets). When such financing is sufficiently available, an overconfident CEO will indeed pursue a higher number of acquisitions. That is, defining "abundant internal resources" as cash availability of $c \geq V_T$, the overconfidence model implies the following testable predictions (Malmendier and Tate 2008).

Prediction 1. *In firms with abundant internal resources, overconfident CEOs are more likely to conduct acquisitions than non-overconfident CEOs.*

[33] In the case of shared suprlus, the new threshold is $\widehat{e} > \alpha e \frac{\widehat{V}-c}{V-c} = \alpha e \frac{\widehat{V}_A+\widehat{e}}{V_A+e}$.

Under this scenario, i. e., if internal, non-diluting assets are sufficient to finance mergers that a rational CEO would not pursue but seem worthwhile to the overconfident CEO, we can also draw conclusions about the implied value creation. In general, comparing the decision-rule for rational CEOs, $e > 0$, and that for overconfident CEOs, $\widehat{e} > (x - \widehat{x})(\widehat{V} - c)$, reveals that overconfident CEOs pursue some mergers that rational CEOs forgo, namely, those driven by their overestimation $\widehat{e} > e$, and some of those might be value destroying, i. e., if $e < 0$. On the other hand, the comparison also reveals that the overconfident CEO will forego some value creating mergers where $e > 0$ because of the perceived loss from dilution, $(x - \widehat{x})(\widehat{V} - c)$. As discussed, this perceived dilution goes to zero as the portion of cash financing increases and disappears under full cash financing (implying $x = \widehat{x} = 0$).

Hence, we can conclude that, in cases of full cash financing, the value created by mergers of rational CEOs must on average be weakly higher than the value created by the mergers of overconfident CEOs. More generally, as long as the CEO pursues more mergers, he must be adding value-destroying transactions (while possibly also dropping some value creating opportunities.) Hence we can conclude:

Prediction 2. *If overconfident CEOs do more mergers than rational CEOs, then the average value created in mergers is lower for overconfident than for rational CEOs.*

The extreme case is, as in Prediction 1, the scenario of purely cash-financed mergers, where overconfident CEOs pursue all the mergers rational CEOs pursue and, in addition, some value-destroying ones. As we are increasing the fraction of stock financing above zero, overconfident CEOs will start to forgo value-creating as well as value-destroying mergers. As long as the overconfident CEO pursues more deals, we know that some value-destroying ones are added and hence the average value creation is lower and can even be negative.

Before turning to the empirical approach to test these predictions, it is worth noting that the predictions differ from those derived under Perspective 1 in their focus on cash-financing as a key feature of mergers the overconfident CEO is leaning to. The rational CEO's catering to overly optimistic investors, instead, leans on stock financing to maximize value creation. Relatedly, merger activities of overconfident CEOs should respond to cash-flow availability, while the merger activities under catering respond to fluctuations in the market's valuation of firms. Both differences are a first indication that both sets of explanations can be at work. I will return to the question of "compatibility" between Perspective 1 and Perspective 2 below in Section 3.4.

As in the case of Perspective 1 (the Biased Investor approach), the empirical test of these hypotheses faces a few hurdles. First, researchers have to find an empirical measure of managerial overconfidence. This parallels the difficulties in finding a clean measure of market misvaluation under Perspective 1. Second, the empirical analysis would ideally rely on exogenous variation in a

firm's exposure to overconfidence versus rational decision-making, i. e., variation in the assignment of CEOs. This hurdle parallels the endogeneity of periods of over- and undervaluation in the market, even if they were measured cleanly and disentangled from growth factors. Third, in the case of mergers, it is unclear whether a researcher has enough power to identify the causal effect due to the limited empirical variation—mergers are not everyday business, and CEO turnover also does not happen with high frequency.

Regarding the first challenge, the most common empirical approach to measuring CEO overconfidence in the behavioral finance literature, introduced in Malmendier and Tate (2005), relies on "revealed beliefs." The basic idea is to exploit managers' personal portfolio decisions and classify those as overconfident who invest an overly high proportion of their personal funds in their own firms. Specifically, the construction of the overconfidence proxy relies on the prediction that managers who overestimate their firms' future cash flows tend to overinvest their own wealth in their companies and expect to personally benefit from future stock-price increases. In particular, they do not diversify their stock-based compensation and delay the exercise of executive stock options.[34]

The construction of the measure relies on the historical development of managerial compensation. Since the 1980s, and particularly in the 1990s, top US executives have received increasingly large stock and option grants as part of their compensation (Hall and Murphy (2003)). As result, top-level executives are typically under-diversified w. r. t. company-specific risk. Moreover, CEOs have only a limited ability to address this issue. For example, the stock-linked compensation might be restricted stock, which is subject to a time-based vesting or performance-based vesting scheme. Stock options are not even tradeable and typically also take years to vest. In addition, executives are contractually prohibited from taking short positions in the company's stock.

Given this compensation design, rational, risk-averse executive should seek to exercise stock options (once vested) in order to diversify. The exact timing of optimal option exercise depends on "moneyness" of the options, risk aversion, and extent of under-diversification (Lambert, Larcker, and Verrecchia (1991), Hall and Murphy (2002)). An overconfident CEO, instead, who overestimates the future performance of his firm, will be more willing to hold options, as he expects to profit from expected stock price appreciation. Hence, overconfidence is predicted to correlate with a systematic tendency to hold options longer before exercise.

Concretely, researchers have constructed the so-called *Longholder* measure, typically as a binary variable which indicates if a CEO has held options that were at least 40% in the money, all the way to expiration. Such behavior indicates that the CEO has taken a long-term bet on the future performance of their company's stock, despite their under-diversification. The exact calibration is

[34] Another way to overinvest in the own company is to delay the sale of stock. Overconfident managers exhibit such behavior as well, and even buy additional stock of their firms. Empirical research has relied more on option-based measures than on stock purchases and sales as they raise fewer concerns about signaling to the market; cf. Malmendier and Tate (2008).

based on typical executive stock option with a ten-year duration and a four-year vesting period.

Malmendier and Tate (2005) construct the original *Longholder* measure from the data of Hall and Liebman (1998), which documents CEOs' stock and option holdings in Forbes 500 companies from 1980 to 1994. Updated versions have been created from Thomson Reuters' Insider Filings with the data available since 1996, and from the Compustat's ExecuComp database with the more detailed data available after 2006 (cf. Malmendier and Tate (2015)).

Perspective 2 research, then, links the decisions that the executive makes on his or her **personal portfolio** of company stock options to **corporate decisions**. The *Longholder* measure has been linked to investment decisions, investment financing, merger decisions and merger financing, borrowing decisions, leverage choices, dividend payments and other corporate decisions. I will present a selection of this work in Section 4.2.1 below, and focus here on testing the predictions regarding mergers.

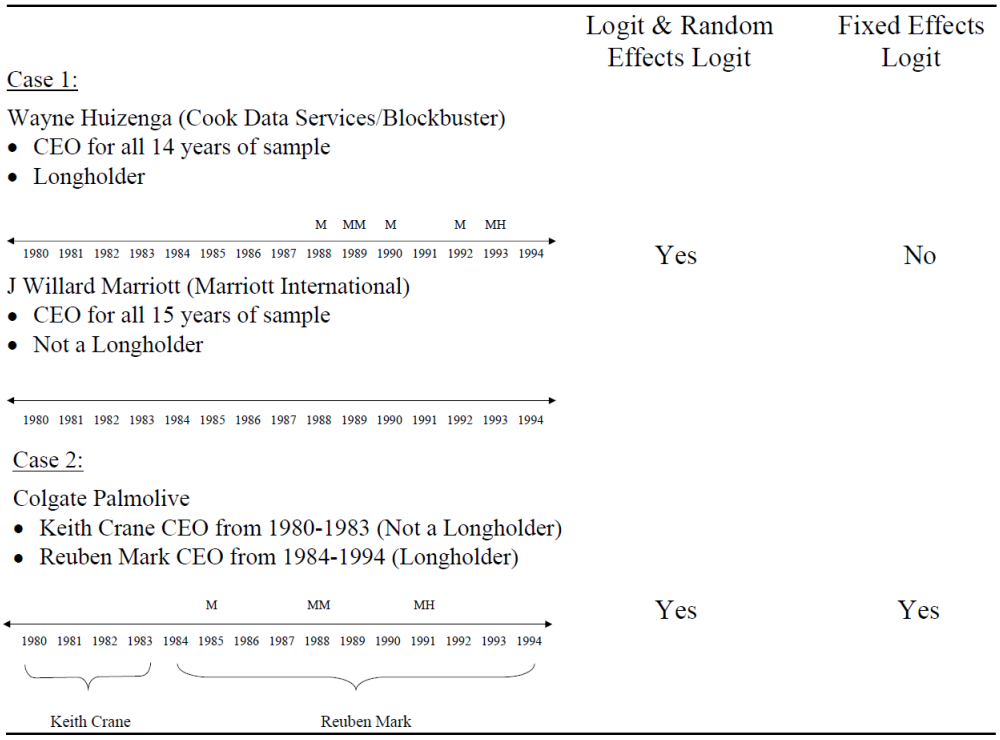
Before turning to the empirical analysis, let's consider the second challenge—the lack of exogenous variation in the CEO's overconfidence. Here it is noteworthy that many of the existing studies are able to include not only year- but also firm-fixed effects into their estimating equations. Consider the estimating equation

$$\Pr(Y_{it} = 1|X, O_{it}) = \mathrm{G}(\beta_1 + \beta_2 O_{it} + X^T\gamma)$$

where i indicates the company, t the year, Y is an acquisition dummy, O the Longholder measures of overconfidence, and X is a set of controls. The null hypothesis H_0 would be that $\beta_2 = 0$, i. e., that overconfidence does not matter, while the alternative "overconfidence" hypothesis H_1 is that $\beta_2 > 0$, i. e., overconfidence does matter for merger frequency, for the sample under consideration. We can estimate this relationship in, say, a logit regression (with G as the logistic distribution) without accounting for time-invariant firm-specific factors. In that case, identification comes both from between-firm differences and from within-firm variation. As indicated in Figure 7, both the comparison between Wayne Huizenga running Cook Data Services/Blockbuster and J Willard Marriott running Mariott would contribute to the identification, and the within-firm turnover from Keith Craen to Reuben Mark as the CEO of Colgate Palmolive. In the first case, we observe Huizenga being classified as a *Longholder* in 1993 and him repeatedly acquiring other companies, while Marriott never held is options excessively long, nor did he pursue mergers. In the second case, we observe that Crane was neither a *Longholder* nor an acquirer, while Reuben Mark was both. Both sets of comparisons would contribute to the identification of β_2. If we include instead firm fixed effects, and estimate a conditional logit regression to avoid concerns about incidental parameter issues, only the latter variation contributes to the estimation of the overconfidence effect.

Hence, the inclusion of fixed effects ensures that any unobserved firm-specific or time-specific factors that might influence the presence of an overconfident CEO in a given firm in a given year

Figure 7: Identification Strategy



and that might also predict merger activities are non-parametrically accounted for.[35] Nevertheless, time-variant firm characteristics might challenge the identification and cannot be ruled out. Instead, the existing research devotes significant portions of their papers to the leading alternative interpretations. Here, I discuss some of the most frequently mentioned concerns about the interpretation of the Longholder measure and how the existing research has ruled them out. In addition, the alternative measures of overconfidence discussed in Section 4.2.1 below and their high correlation with the *Longholder* measure has helped to ascertain its leading interpretation as a measure of managerial overconfidence.

Inside information. One alternative interpretation is that Longholder CEOs might hold on to company stock because they have private information about firm prospects. However, inside information differs from overconfidence in terms of persistence of the behavior. Under inside information, we would not predict that the a given CEO keeps holding on to his stock over time as inside information is of transitory nature by definition. The observed behavior of Longholders, instead, is better described by a trait than by transitory superior information. As will be shown in Table 8, Malmendier and Tate (2005) and Malmendier and Tate (2008) do not find evidence that CEOs

[35] A disadvantage of the fixed-effects model is that it reduces the sample to those firms that undertake at least one merger, and that had at least one overconfident and one non-overconfident CEO, during the sample period.

who do not sell their stock earn abnormal returns (compared to, e. g., investment in the S&P 500 index). Hence, an ex-post performance comparison also does not support the inside information story.

Signaling. CEOs might use their stock-holding as a (costly) signal to the market about the value of their firm. However, if this were the case, the Longholder measure should be positively related to merger returns since signaling ameliorates information asymmetries; the opposite is the case. Moreover, it is unclear how powerful signals from executive-option exercises are. In general, the financial press views vesting and expiration times as the primary reasons for the exercise of managerial stock options (see fn. 15 in Malmendier and Tate (2005)). Most importantly, the timing is disjoint: The data reveals heightened acquisitiveness of *Longholder* CEOs after the ultimate option exercise. These instances are inconsistent with the signaling story as the information causing late exercise cannot be information about the value created in the merger.

Risk tolerance. Low risk aversion could explain why CEOs do not decrease their exposure to company risk and hold on to options. Risk tolerant CEOs may also be more willing to undertake risky acquisitions, explaining the positive correlation. However, in that case, the market should value the more risk-neutral decision making of the CEO positively, rather than responding with (even more) negative abnormal announcement returns. Moreover, low risk aversion does not predict that a CEO is averse to stock financing or more prone to undertake diversifying mergers, as we will see below.

Tax considerations. Similar to the arguments above, any postponement of option exercises due to tax reasons (e. g., in order to delay tax obligations) would not imply heightened merger activities and their dependence on internal funding among *Longholders*. One additional concern might be that, if firms were less likely to pay dividends around mergers, option holders may delay option exercise that are motivated by reaping dividend payments on the underlying shares. However, Malmendier and Tate (2008) show that the relationship between having a *Longholder* CEO and merger activities is robust to controling for dividend payments.

Procrastination. CEOs could hold on to company stock for reasons related to inertia or procrastination. However, the very fact that *Longholder* CEOs conduct more acquisitions contradicts the notion that they would "enjoy the quiet life" in their role as corporate leaders. Moreover, Malmendier and Tate (2005) find that approximately 8% of the CEOs classified as Longholders *do* make changes to their personal portfolios in the two years prior to the expiration of the "long-held" option, which is inconsistent with CEOs postposing investment decisions.

In summary, the main "defense" against these plausible alternative interpretations is two-fold—sometimes more careful consideration of their implications (such as more positive abnormal returns) helps to address them; and sometimes the large body of existing empirical results and their incon-

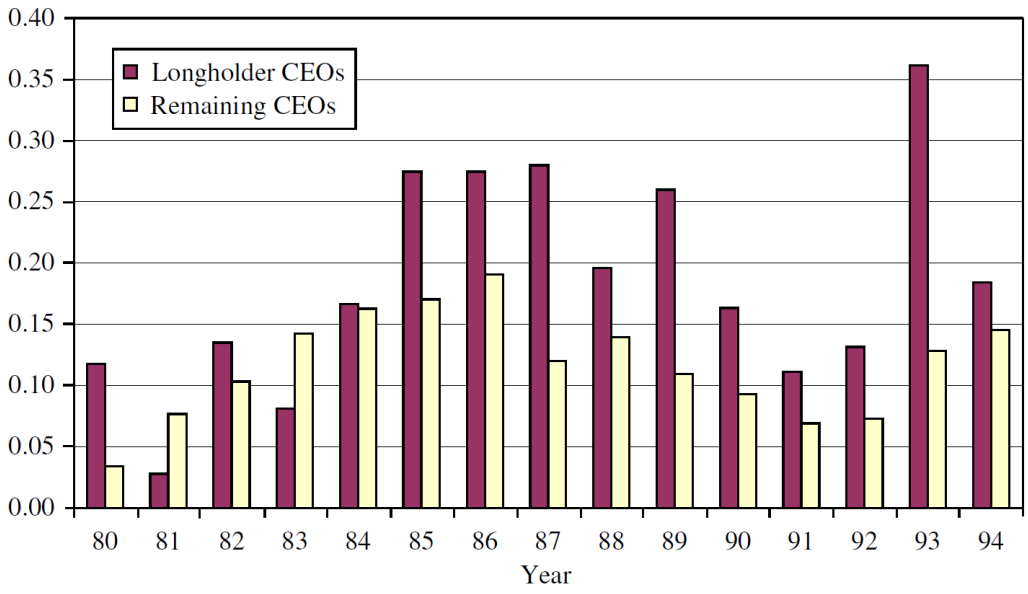
sistency with the alternative story moves the odds in favor of the original overconfidence interpretation. Nevertheless, as in the case of Perspective 1, the estimation results are bound to remain less convincing than RCT-type evidence.

Finally, regarding the third challenge—the potential lack of power—it is probably fair to say that relatively large effects of overconfidence on corporate decisions were needed to generate significant results, but that such a presumption was also not unrealistic in the case of C-level executives and the prominence of the hubris hypothesis in practice.

With these caveats and reassurances in mind, I turn to the results. As Figure 8 shows, even in the raw data, overconfident CEOs undertake more acquisitions in almost all years, and sometimes by a very large margin, which aligns with Prediction 1.

Figure 8: Merger Frequency: Average Number of Mergers

This figure shows Figure 2 from Malmendier and Tate (2008). It displays the year-by-year merger frequencies, separately for Longholder CEOs and the remaining sample of CEOs. For each subgroup, the frequency is calculated as the number of mergers divided by the number of CEOs in that subgroup in a given year. Years are fiscal years.

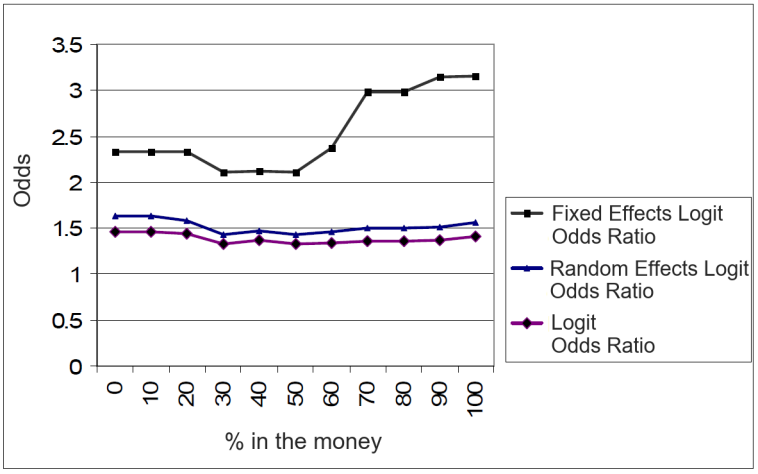


Malmendier and Tate (2008) provide a more robust test of Prediction 1, with their key findings presented in Table 7. As stated in the Prediction, managerial overconfidence implies heightened acquisitiveness only among firms that have ample non-diluting assets. Nevertheless, columns (1) to (4) first provide the estimates for the full sampled as a baseline. The specifications shown here include both year- and firm-fixed effects. The overconfidence proxy is significantly related to merger frequencies, as are the variants of the Longholder measure in column 2 (splitting the dummy into two separate Pre- and Post-Longholder dummies for the periods prior to and after the

first Longholder behavior) and in column 3 (holding on to vested options with five years remaining duration that are at least 67% in the money[36]). Note that the Holder 67 measure is designed to vary even within CEO and hence adds an additional dimension of identification. Finally, the estimation in column (4) replicates the baseline from column (1) in a random-effects version, with very similar results. Note that the logit regression with random effects uses all sources of variation, but explicitly models the effect of the firm on acquisitiveness. If the estimated effects of overconfidence in the pooled specification (without firm fixed effects) were due to firm effects, the estimates should have declined after including random effects. This is not the case. Moreover, the estimated effect does not depend on the exact model specifications for the rational option-exercise benchmark. If one varies the percentage in the money required to identify option exercise as delayed, anywhere between 0 and 100% the estimated odds ratio either remains virtually identical (logit and random-effects logit) or increases (fixed-effects logit for higher values), as shown in Figure 9.

Figure 9: Robustness Check (Different %-Moneyness)

This figure shows the odds ratio of acquisition for various logit specifications and thresholds of Longholder classification. The odds ratio is the odds of acquisition for a Longholder CEO divided by the odds of acquisition for a non-Longholder CEO as given by the logit model. The threshold for Longholder classification is specified along the horizontal axis. CEOs who hold options with a percentage in the money greater than or equal to the specified threshold are classified as Longholder CEOs.



The estimations in columns (5) to (6) add the distinction between cash-rich and cash-poor firms,

[36] Holder 67 relaxes the requirement that CEOs hold their options all the way until expiration. It focuses on the earliest point when options can be fully exercised, year 5, based on the typical ten-year duration and vesting schedule over the first four years. The corresponding exercise threshold in the framework by Hall and Murphy (2002) is 67%: Holder 67 is set equal to 1 if a CEO fails to exercise options with five years remaining duration despite a 67% increase in stock price (or more) since the grant date. When using this proxy, the sample includes only those CEOs who could have been classified as Holder 67 to control for past performance. That is, a CEO enters the sample once he has an option with five years remaining duration that is at least 67% in the money. Once he postpones the exercise of such options he is classified as Holder 67 for his remaining sample years.

based on Harford (1999)'s model.[37] Firm-years are classified as "cash rich" if beginning-of-the-year cash reserves are at least 1.5 standard deviations above the value predicted by the baseline model, where the standard deviation is the time series deviation in firm cash reserves. Columns (5) and (6) reveal that the odds ratio is indeed significant in cash-rich firm. It is also larger than in the overall sample (or the sample of cash-poor firms in column (6)). A Wald test fails to reject equality of the estimates in the two subsamples. The latter finding neither confirms nor rejects the overconfidence hypothesis: the CEOs decision in constrained firms depends on the unobservable relation between overestimated gains to merging and perceived own-company undervaluation. Hence, overconfidence may or may not have a significant impact on acquisitiveness in cash-poor firms. The results are similar using Holder 67 (cash-rich odds ratio = 2.14, significant at 5%; cash-poor odds ratio = 1.39, insignificant).

Returning to the importance of availability of cash and other non-diluting assets, Malmendier and Tate (2008) explore additional measures of cash availability including a quintile split using the (twice-lagged) Kaplan-Zingales index rather than the Harford model. The index, based on logit estimates from Kaplan and Zingales (1997), is defined as

$$KZ_{it} = -1.001909 \cdot \frac{CF_{it}}{K_{it-1}} + 0.2826389 \cdot Q_{it} + 3.139193 \cdot Lev_{it} - 39.3678 \cdot \frac{Div_{it}}{K_{it-1}} - 1.314759 \cdot \frac{C_{it}}{K_{it-1}},$$

where CF/K is cash flow to capital, Lev is debt to total capital, Div/K is dividends to capital, and C/K is cash holdings to capital.[38] Here, the Longholder coefficient in (random effects) logit regression estimated on the least constraint (lowest) quintile is positive and significant (odds ratio = 2.03, significant at 5%), and the coefficient estimated on the most constraint (highest) quintile is insignificant (odds ratio = 1.07). Again, the cross-quintile difference is not significant at conventional levels (p-value = 0.133).

One strength of these findings is that it allows differentiation from traditional, empire-building explanations of merger activities. While misaligned incentives can also explain why CEOs may undertake value destroying acquisitions, they would not personally invest in their company above and beyond what the compensation contract induces. The Longholder measure identifies the active decision of the CEO to deviate from rational behavior and not divest company risk, in contrast to the predictions of agency theory.

[37] The approach consists of two stages. The first stage is a regression of cash reserves (cash stock normalized by sales) on the change in the risk premium between AAA and Baa bonds, a dummy for NBER recession years, the ratio of market to book value of assets, the coefficient of variation of operating cash flows, firm size (market value), and the level of and changes from year t to $t+1$ and $t+1$ to $t+2$ in operating cash flow net of investment (normalized by sales). The model explains 84% of the variation in cash reserves in the sample.

[38] To provide some intuition for the logistic regression, note that leverage captures debt capacity; deflated cash flow, cash, and dividends capture cash on hand; and Q captures market value equity.

Table 7: Do Late Exercisers Complete More Mergers?

This table presents Table 3 in Malmendier and Tate (2008). See their paper for a detailed description. All coefficients are shown as odds ratios. z-statistics are included in parentheses. *, **, and *** denote significance at the 10%-, 5%-, and 1%-level, respectively.

	Fixed-effects logit			Random-effects logit		
				Baseline	Cash rich	Cash poor
	(1)	(2)	(3)	(4)	(5)	(6)
Size	0.6537	0.6600	0.3278	0.9022	0.9480	0.9177
	(2.50)***	(2.42)***	(3.42)***	(1.49)	(0.50)	(1.03)
Q	0.7135	0.7154	0.9062	0.7019	0.7686	0.6839
	(2.20)***	(2.18)**	(0.45)	(2.96)***	(1.25)	(2.70)***
Cash flow	2.0231	2.0377	1.6607	1.5427	0.9948	1.8719
	(1.72)*	(1.72)*	(0.67)	(2.07)**	(0.01)	(2.35)**
Stock ownership	0.3840	0.3813	0.0418	1.4084	21.4335	0.7232
	(0.95)	(0.96)	(0.70)	(0.36)	(1.80)*	(0.29)
Vested options	0.4566	0.4595	0.6384	1.2165	4.2168	1.3186
	(3.97)***	(3.93)***	(0.51)	(0.46)	(0.91)	(0.63)
Efficient board size	1.0817	1.0811	1.8488	0.8012	0.5750	0.9184
	(0.40)	(0.40)	(2.10)**	(1.55)	(2.44)**	(0.48)
Longholder	2.1891			1.7447	1.9728	1.5471
	(2.70)***			(3.21)***	(2.53)**	(2.10)**
Post-Longholder		1.8642				
		(1.91)*				
Pre-Longholder		2.3305				
		(2.72)***				
Holder 67			2.5159			
			(2.49)**			
Firm fixed effects	Yes	Yes	Yes	No	No	No
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,568	2,568	853	3,540	1,227	2,313
Number of firms	225	225	124	322	282	314

Malmendier and Tate (2008) also provide some evidence consistent with the financing preferences of overconfident CEOs according to Lemma 1. They document that Longholder CEOs are more likely to use cash to finance their transactions, with the odds ratio of using cash versus other means of financing being 1.10 for the Longholder measure, and 1.38 for the Holder 67 measure.

As already indicated in the discussion of alternative explanations, the authors perform a slew of robustness checks. Turning to Prediction 2, Malmendier and Tate (2008) utilize two main approaches to show that overconfident CEOs are more likely to pursue value destroying mergers. First, they employ the diversifying (versus intra-industry) nature of mergers as an ex-ante indicator.

A large number of papers in corporate finance has provided evidence of a so-called diversification discount, and while there is some debate about it indicating risk-adjustment versus value destruction outside the case of mergers, the correct interpretation in the case of mergers appears to be value destruction (see, e. g., Lang and Stulz (1994), Berger and Ofek (1995), Graham, Lemmon, and Wolf (2002), Schoar (2002), and Villalonga (2004) for research on the diversification discount). Using the Fama-French 48 industry groups, the authors re-estimate their model separately for diversifying and non-diversifying mergers. Longholder CEOs are found to be significantly more likely than other CEOs to pursue a diversifying merger (odds ratio = 2.54, significant at the 1% level) but display no significant differences regarding intra-industry mergers (odds ratio = 1.66, insignificant), with similar results for the Holder 67 measure.

More precisely, however, Prediction 2 states that overconfident CEOs are more likely to undertake value-destroying transactions only within the subset of cash-rich firms; in the sample of cash-poor firms the prediction is ambiguous. Hence, the authors also replicate the random-effect split-sample estimations. The effect remains strong in the cash-rich subsample (with odds ratios of 2.50 for Longholder versus 1.32 for other CEOs) and is somewhat weaker in the cash-poor subsample (with odds ratios of 1.78 for Longholder versus 1.15 for other CEOs).

In summary, the estimates confirm directly that overconfident CEOs, as proxied by the Longholder dummy, display a significantly higher inclination to pursue diversifying mergers when they have cash at hand.

The second approach exploits the immediate market reaction: Comparing the announcement returns of mergers by overconfident and other CEOs, is the market's assessment of the value implications more negative for mergers pursued by the overconfident CEO? As before this prediction should hold in the subsample of firms with abundant internal resources.

The raw data strongly confirms this prediction for the full sample: the market reaction to a merger announcement by a Longholder CEO is -90 basis points and highly significant, but only -12 basis points (and insignificant) for other CEOs.

The results are even stronger when estimated in a controlled regression environment,

$$CAR_i = \gamma_1 + \gamma_2 O_i + X_i' G + \epsilon_i,$$

where CAR_i are the cumulative abnormal return to acquirer i, $CAR_i = \sum_{t=-1}^{1} (r_{it} - \mathbb{E}[r_{it}])$, using the daily S&P 500 return as the proxy for the expected returns $\mathbb{E}[r_{it}]$. O is the overconfidence proxy, and the set of controls X includes the standard controls in announcement return regressions, an indicator for intra-industry versus diversifying mergers and controls for the type of financing, as well as the controls for corporate governance and year controls from above. The estimated difference in announcement returns increases to -115 basis points and is highly significant.

The economic magnitudes implied in these findings are quite large. If one calculates the value creation or destruction to acquiring-company shareholders as announcement effect times market capitalization before announcement, we find that the 10.8% of CEOs identified as overconfident with the Longholder measure cause 44% of value destruction around merger bids, $2.15 billion of acquiring shareholders' wealth (out of $4.39 billion total). Per bid, Longholders destroy on average $7.7 million more value than other CEOs.

As indicated above, a successful empirical test of the overconfidence hypothesis in mergers and acquisitions, and more generally much of the empirical analyses of the Perspective 2 literature in Behavioral Corporate, relies on two main ingredients: Carefully deriving predictions that go beyond the baseline effect (here, for example, the conditioning on abundant internal resources or preference for cash financed merger), and devising a battery of additional empirical tests to address alternative interpretations.

For example, returning to the concerns about inside information or signaling, Malmendier and Tate (2008) argue that, if those asymmetric-information based explanations were to explain the empirical findings, then merger activities and the delay of option exercises should correlate, i. e., merger activities should "cluster" in the final years of the option terms. They show that this is not the case. Second, as already stated, a simple signaling model would predict that the market reacts more favorably to the merger announcements of Longholder CEOs, and we just saw that this is not the case. A last interesting additional test, which targets the inside information aspect, is that CEOs should "win" by holding. That is, compared to exercising in-the-money options at the earliest time possible (in order to diversify) the extended period of non-exercise should generate additional returns to the personal portfolio of the CEO, who is benefiting from inside information.

As shown in Table 8, this is not always the case, at least from an ex post perspective. If we compare the returns to holding the option over the last year to those the CEO would have earned had he exercised the option just one year earlier and invested the proceeds in the S&P 500,[39] we see that (slightly more than) 50% of executives are losing money ex post. The average return to exercising a year earlier is positive, though statistically insignificant, and the results become even more negative when replicating the test for hypothetical exercise two, three, four, and five years before expiration. In other words, Longholder CEOs do not appear to earn positive abnormal returns from those instances when they delay option exercise, as an insider story would imply—at least as far as we can measure ex post. Moreover, if one zooms in on the subset of Longholders who did not earn positive returns (again, with the caveat that these are ex-post calculations), and includes separate dummies for those ("did OK") and those that "should have exercised," one sees that the opposite is the case: the coefficient on the "should have exercised" Longholders is significant

[39] The calculation allows for maximum insider knowledge and assumes exercise at the peak price of the respective year, both in the hypothetical scenario and in the year of actual exercise.

and large. Hence even the consideration that some late option exercise might be motivated by inside information and those could be driving the findings on merger activities is rejected in the data.

Other relevant considerations are concerns about stock-price bubbles, heterogeneity in stock volatility, and the role of finance training. The first of these comes from a biased-investors perspective: Might Longholder CEOs exploit an overly high valuation of their company stock when pursuing mergers (and holding on to their options)? Of course, the preference of Longholder CEOs for cash- rather than for stock-financed acquisitions is already in conflict with this notion. Moreover, both year effects and, in the conditional logit analyses, firm effects are already removed. To go one step further, Malmendier and Tate (2008) re-estimate the odds ratios after including lagged stock returns for the past five years, which would capture the hypothesized "bubble effect." They find that the results are not affected.

Similarly, the concern about differences in volatility—CEOs of higher-volatility firms delay option exercise since the volatility makes their options more valuable, and pursue diversifying mergers in order to reduce their firm's volatility—can be addressed by including measures of firm volatility into the regression. Again the results are unaffected.

Finally, an interesting question is whether Longholder CEOs are simply less "financially educated" than other CEOs. Do they hold on to their executive stock options since they do not understand the concept of diversification, and do they pursue value-destroying mergers for related reasons? In unreported results, the authors find again that the results are unaffected if controlling for a finance related (undergraduate or graduate) degree.

In summary, the empirical strategy to address alternative explanations is three-fold. First, one can rule out many potential explanations for the delay in option exercise since they do not predict increased merger activity. That is, they might be correlated with the proxy for overconfidence but not with the outcome variable. Second, one can rule out other theories because they imply additional predictions that are rejected in the data. Third, some alternative determinants can directly be controlled for in the regression analysis, such as cash flow, stock returns, dividends, or volatility. Overconfidence, then, remains the only interpretation that predicts all of the estimated results and does not generate other predictions that are refuted by the data. To be clear, the alternative determinants of option exercise are not "refuted" in and of themselves; rather, the robustness checks and wide array of control variables indicated that the residual relation between late option exercise and mergers is most consistent with overconfidence. Finally, it is important to note that the results have been corroborated using alternative measures of overconfidence, such as the media-based measure, which I will discuss in Section 4.2.1 and which is unlikely to be affected by tax motivations or board pressure. The corroboration further supports the interpretation put forward by Malmendier and Tate (2008).

Table 8: Are Overconfident CEOs Right to Hold Their Options?

This table presents Table 7 in Malmendier and Tate (2008). The calculation in Panel A subtracts the return the CEO would have earned from exercising the option one year earlier and investing in the S&P 500 from the return earned by holding until expiration for each option that was held until expiration and was at least 40% in-the-money at the beginning of its final year. Exercise is assumed to occur, both in the final year and in the hypothetical year, at the maximum stock price during that year. In Panel B, all coefficients are shown as odds ratios. z-statistics are included in parentheses. See Malmendier and Tate (2008) for a detailed description of the variables. *, **, and *** denote significance at the 10%-, 5%-, and 1%-level, respectively.

Panel A: Returns to diversifying

Percentile	Return
10th	-0.24
20th	-0.15
30th	-0.10
40th	-0.05
50th	-0.03
60th	0.03
70th	0.10
80th	0.19
90th	0.39

Panel B: Do "mistaken" holders drive the acquisitveness result?

	Fixed-effects logit
Size	0.6757
	(2.20)**
Q	0.7147
	(2.14)**
Cash flow	2.0520
	(1.71)*
Stock ownership	0.3502
	(0.97)
Vested options	0.3026
	(1.03)
Efficient board size	1.111
	(0.54)
Longholder: did OK	1.4259
	(0.76)
Longholder: should have exercised	3.4042
	(3.47)***
Year fixed effects	Yes
Obervations	2,515
Number of firms	221

3.4 Biased Investors *and* Biased Managers

In Subsection 2.5, I have emphasized that Perspective 1 (Biased Investors) and Perspective 2 (Biased Managers) may come across as contradictory. Managers are modeled as rational in the Biased Investors literature, and investors are modeled as rational in the Biased Managers literature. However, these rationality assumptions merely keep the model simple and focus the analysis on the bias of interest. Both sets of biases are easy to nest, and there is no contradiction.

We can make this claim more concrete in the context at hand, CEOs conducting mergers and acquisitions. Consider the basic model ingredients of the Perspective 1 approach in Table 4, and those of the Perspective 2 approach in Table 6. Let's now merge the two approaches in the sense that we allow both for investors and managers to be biased. That is, as summarized in Table 9, I allow the current market value of stock to deviate from its fundamental value, and I allow managers' valuation of their firm as well as their valuation of the merged firm to deviate from the respective fundamental values. (I continue to consider the more general case of possible synergies $e \geq 0$ in the fundamental value of the merged firm, as introduced in Subsection 3.3, and to abstract from misvaluation of the target firm, $\widehat{V}_T = V_T$. I focus on A having full bargaining power.)

Table 9: **Merged Model Notation**

	Capital stock	Current market value	Fundamental value	Valuation of A-manager
A-firm	K_A	$\widetilde{V}_A = S_A K_A$	$V_A = qK_A$	$\widehat{V}_A$
T-firm	K_T	$\widetilde{V}_T = S_T K_T$	$V_T = qK_T$	$\widehat{V}_T$
Combined firm	$K_A + K_T$	$\widetilde{V} = S(K_A + K_T)$ $= S_A K_A + S_T K_T + \widetilde{e}$	$V = V_A + V_T + e$ $= q(K_A + K_T) + e$	$\widehat{V} = \widehat{V}_A + \widehat{V}_T + \widehat{e}$

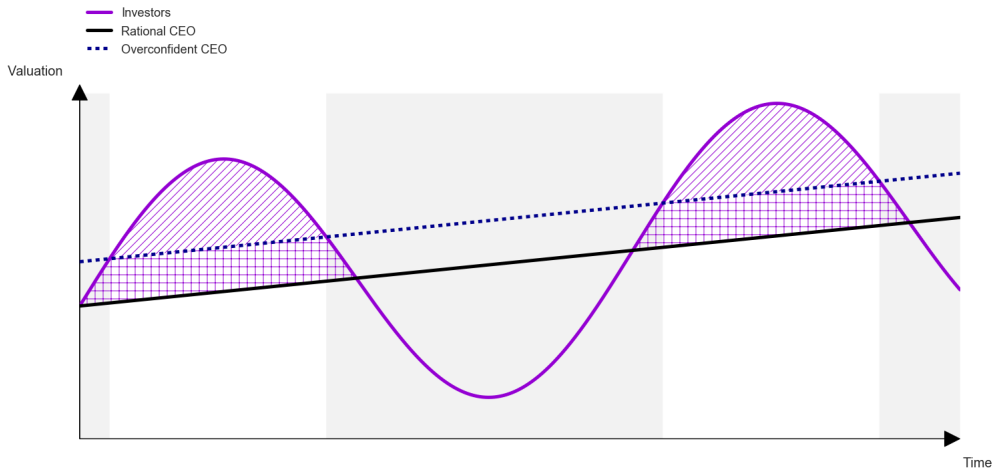
Rather than considering the choice between cash- and stock-financed acquisitions, let's zoom into the case of cash acquisitions first. In the subsection on biased managers I derived the decision rules $e > 0$ for rational managers and $\widehat{e} > 0$ for overconfident managers. With the addition of investor sentiment, target shareholders will now require a reservation value of $\widetilde{V}_T$, and the new decision rule becomes $e > \widetilde{V}_T - V_T$ for the rational manager and $\widehat{e} > \widetilde{V}_T - V_T$ for the overconfident manager. We see that both the misvaluation of the market and the misvaluation of the manager drives a wedge between the shareholder-value maximizing and the socially efficient acquisition decisions.

The fundamental mechanism, however, remains unchanged. As illustrated in Figure 10, a mean upward shift in perceived mean value of the firm on the side of the CEO and time-varying over- and undervaluation on the side of investors, still allow for periods when the CEO overvalues the firm more than the market (grey shaded region); those periods are simply somewhat shorter. Hence, the baseline inclination to do more mergers is still at work and will be acted upon if cash is available,

albeit for narrower time windows. If we also introduce catering to investors' undervaluation (e. g., with stock repurchases), those actions will be observed for longer time windows.

Figure 10: Illustration of Differences in Firm Valuation

A similar figure is also included in Malmendier and Tate (2015).



The theoretical discussion of stock mergers is more involved. The rational A-managers acquires if and only if $(1-x)(V_A+V_T+e) > V_A$ (cf. Section 3.2 for the case $e=0$), and hence $e > x(V-\widetilde{V})+(\widetilde{V}_T-V_T)$, and the overconfident acquirer if and only if $\widehat{e} > x(\widehat{V}-\widetilde{V})+(\widetilde{V}_T-V_T)$.[40] Hence, in this case, also the *relative* misvaluation of the merged company, in addition to disagreement about the target value, will govern stock acquisitions.

We could go further into the case of stock financing and, more generally, the question of what financing choices which type of CEO prefers. However, the real question, beyond showing the consistency of both approaches, is whether there are interesting testable predictions to be derived from combining both sets of biases. Could there be interesting interaction effects of these biases? What if biases of managers and of investors are correlated? For example, in unpublished results in the working paper version of Malmendier and Tate (2005), the authors show that CEO overconfidence appears to by pro-cyclical. Hence, to the extent that, say, market-wide investor sentiment is pro-cyclical, it is easy to see that the interaction could give rise to amplified booms and busts; but such implications will be hard to test empirically.

Kuchler (2017), and more recently Guenzel (2017), focus exactly on this question. They merge the biased-manager and biased-investor frameworks and derive some such testable implications. For example, starting from the context of cash acquisitions, Guenzel (2017) turns the question of whether overconfident CEOs pursue higher-frequency and lower-quality mergers on its head and

[40] As before, x represents the fraction of the combined firm target shareholders are asking for in a stock merger, i. e., $x = \frac{\widetilde{V}_T}{\widetilde{V}}$, and $\widehat{x}$ denotes that "appropriate" fraction as perceived by an overconfident A-manager, i. e., $\widehat{x} = \frac{\widetilde{V}_T}{\widehat{V}}$.

show the following: Suppose we want to *identify* whether an acquiring CEO is overconfident about the value implications of the transaction. Then we can leverage measures of investor sentiment to form a more precise estimate: The likelihood that the A-manager in a cash acquisition is overconfident is higher when target shareholders have high sentiment. As Guenzel (2017) spells out, while the cash-acquisition probability of an overconfident manager is always higher than that of a rational manager, the difference increases in investor sentiment. While this argument ignores selection into cash versus stock acquisitions, note that selection aspects might strengthen the above prediction as rational A-managers will switch from cash to stock acquisitions at lower levels of target shareholder sentiment, when overconfident A-managers still prefer cash as the means of payment.

Guenzel (2017) confirms his prediction empirically in an SDC sample of all-cash acquisitions. He estimates a significant positive relationship between the Longholder-based overconfidence measure for acquiring CEOs and high (target investor) sentiment,[41] controlling for a broad array of merger characteristics, such as the transaction value, indicator variables for public targets and horizontal mergers, and Fama-French 12-industries fixed effect for the target and the acquirer industry.

Kuchler (2017) further examines how managerial overconfidence and *market-wide* misvaluations affect acquisition outcomes, and generalizes to include the choice of cash as an outcome identifying overconfident CEOs during marketwide (moderate) overvaluation, rather than conditioning on the choice of cash. In her main specification, she identifies market-wide misvaluations based on deviations of the price-to-earnings ratio of the S&P 500 from its long-term trend. Another interesting implication she derives are scenarios of "extreme enough" overvaluation that both rational and overconfident CEOs would choose stock financing. In those cases, she argues that stock acquisitions undertaken by overconfident CEOs will outperform those of rational CEOs.

The above-mentioned results are both interesting and leave room for further investigation of the interaction and possibly correlation of investor and manager biases. At a minimum, they illustrate that the two perspectives are neither "contradictory" nor inconsistent.

4 Key Areas of Research

Building on our discussion of mergers and acquisitions, I will now discuss other existing behavioral research over a range of corporate finance topics. This review is not meant to be exhaustive or even representative of the overall field. Most of the areas I highlight have been especially active; in some others more research is needed.

I proceed with the general framework of the three Perspectives in mind, though the section is

[41] Drawing on Baker and Wurgler (2006)'s index components and the definition of high and low sentiment months in Stambaugh, Yu, and Yuan (2012), high investor sentiment of target shareholders is identified with an indicator variable for the cyclical component of the monthly industry-level turnover ratio exceeding its long-run median.

organized more by topics and methodology. I start from papers that focus on corporate responses to biased investors (Perspective 1) and consider work on catering to biased beliefs versus catering to non-standard preferences. Some of those papers will also cover managerial responses to biased analyst behavior (Perspective 3). I then move to papers focused on managerial biases (Perspective 2). I first build on the earlier discussion of managerial overconfidence, and fill in details about the exact definition, measurement, and applications, reflecting the prominent role this particular bias takes in the field. I then review how other managerial biases and characteristics, including experience effects, extrapolation, reference dependence, and cognitive fallacies, affect corporate outcomes. I also discuss papers that emphasize networks between these agents and other promising areas of research.

4.1 Corporate Response to Biased Investors and Analysts

In this section, I will first discuss two of the key topics in this area of corporate responses to biased agents: market-timing in response to investor sentiment, and catering to non-standard investor demand. The literature on market-timing focuses on biased investor beliefs that generate periods of over- or under-valuation, which managers exploit. Our discussion of stock-market driven acquisitions in the previous section was one example. The literature on catering to non-standard investor demand, instead, focuses on investor fads, or demand for characteristics that should be irrelevant to the fundamental value. Applications include equity issuance, repurchases, and dividends, among others. In addition to these direct interactions between rational managers and biased investors, I will also discuss the role played by the media and limited investor attention.

4.1.1 Timing non-rational investor beliefs

Baker and Wurgler's (2000) and (2002) papers explore the implications of investor over- or under-valuation for a rational manager's external financing decisions: When a firm's stock price is too high, the firm should issue relatively more equity, and when the price is too low, the firm should issue more debt.[42] If rational managers are able to time the market in this fashion and investors are unaware of the managers' motives, then the extent to which they issue equity versus debt becomes an indication of how overvalued the market is.

The difficulty in showing such market timing and its aggregate effects empirically is, of course, that we do not have a direct measure of overvaluation to prove the theoretical argument. Baker and Wurgler (2000) resort to the following indirect argument: Assuming that over- or under-valuation are transitory, periods of over-valuation should, on average, be followed by low returns, and periods

[42] Similarly, rational intermediaries might time their decisions to respond to changing investor sentiment. Shleifer and Vishny (2010) present such a model: Banks invest in newly securitized loans when asset prices are relatively high, and buy or hold distressed securities when asset prices are low.

of under-valuation should be followed by high returns. Hence, if managers are engaged in market-timing, periods of high-volume equity issues should, on average, be followed by low returns as the market corrects. This relation between the equity share (in the total volume of new equity and debt issues) and subsequent market returns is precisely the key result in Baker and Wurgler (2000). As predicted by the theory, returns are highest in the years following low equity issuance and lowest in the years following high equity issuance.

Figure 11 illustrates their main findings graphically with both a replication (Panel A) and an extended analysis (Panel B). The replication in Panel A mirrors Figure 2 of Baker and Wurgler (2000), plotting average annual real returns from 1928 to 1997 against equity issuance activity in the previous year, i. e., the prior year's equity share of new issues separated into quartiles. Annual returns are the CRSP NYSE/AMEX/NASDAQ equal-weighted and value-weighted returns including dividends and converted to real returns using the BLS Consumer Price Index inflation rate from Global Financial Data. The annual equity share of new issues, along with the total new debt and equity issues, are from Jeffrey Wurgler's website.[43] We see a clear negative relationship between returns and equity share, especially for value-weighted returns. In other words, building on earlier work that shows that firms' equity issuances predict idiosyncratic returns (e. g., Ritter (1991) and Loughran and Ritter (1995)), Baker and Wurgler (2000) are able to show that the relation aggregates to the market level.

Panel A also reveals that the negative relationship is driven by the negative returns in the highest quartile of equity share. The concentration in the last quartile increases further when I expand the analysis to more recent years, through 2008. As we can see in Panel B, the result for Q4 persists, but the relationship between returns and the prior year's equity share of new issues is attenuated. In fact, the relationship between equity share and returns is flat (or even increasing) in the lower-equity share quartiles, with the negative effect driven exclusively by the difference in returns in the highest quartile. In fact, when I plot the relationship between returns and prior-year equity shares over just the latest 20 years, I find a positive instead of a negative relationship. However, this 'reversal' is somewhat misleading as the equity issuances in the last 20 years all fall in the lowest equity-share quartile. As I illustrate in Panel C, the sharp decline in the equity share of new issues since the mid-1980s largely reflects a strong increase in new debt issues. In fact, all of the years in the highest quartile of equity share, which drive the negative relationship with future returns, occur before the mid-1980s. Assuming that motives other than market-timing explain the increase in corporate debt issues, the equity share may not be the best measure of market timing (any more), leaving this an open area for future work.[44]

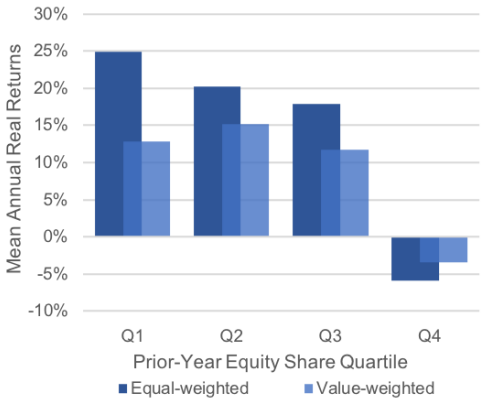
[43] The data is available at `http://people.stern.nyu.edu/jwurgler/`. Slight differences in my replication relative to Baker and Wurgler's original graph are likely due to using of different inflation data.

[44] Another approach to measuring market timing, employed by Baker, Taliaferro, and Wurgler (2006), is the log difference of aggregate gross equity issuance and the average annual gross equity issuances over the previous five

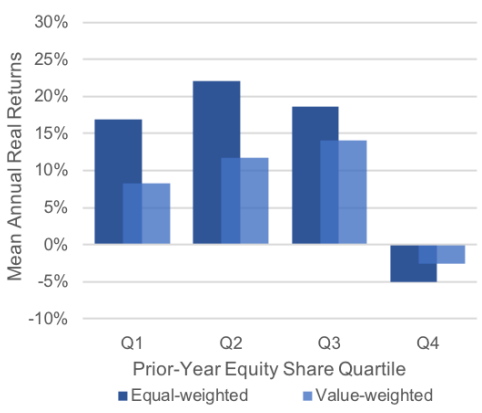
Figure 11: Mean Equity Returns by Prior-Year Equity Share in New Issues, 1928-1997

Panels A and B plot the average of annual real returns by quartile of prior-year's equity share in new issues (where quartile 4 is the highest equity share). Panel A replicates Figure 2 of Baker and Wurgler (2000), plotting returns from 1928-1997. Panel B extends the analysis to include data to 2008. Panel C plots new equity issues, new debt issues, and the equity share from 1927-2007. I obtain annual nominal new equity issues, new debt issues, and the equity share in new issues from Jeffrey Wurgler's website, `http://people.stern.nyu.edu/jwurgler/`. Annual returns are CRSP NYSE/AMEX/NASDAQ equal-weighted and value-weighted returns including dividends, which I convert to real returns using the BLS Consumer Price Index inflation rate from Global Financial Data.

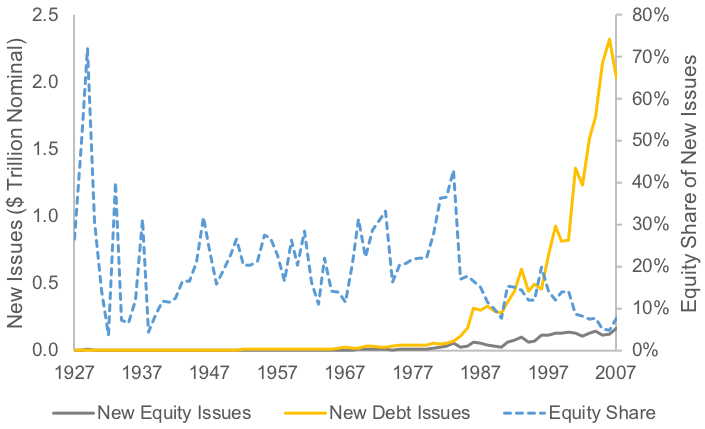
(a) Mean Equity Returns by Prior-Year Equity Share in New Issues, 1928-1997



(b) Mean Equity Returns by Prior-Year Equity Share in New Issues, 1928-2008



(c) New Equity Issues, New Debt Issues, and the Equity Share, 1927-2007



Baker and Wurgler carefully consider alternative explanations for the negative relationship between equity issuance and future returns. First, they ask whether the positive correlation between

years. This detrended equity issuance measure captures changes in equity issue that are less tied to changes in debt issuance. In yet another approach, Butler, Cornaggia, Grullon, and Weston (2011) use net equity as a share of net equity and net debt flows.

leverage and expected returns, described by Modigliani and Miller (1958), could explain the results, as leverage tends to be higher when the equity share is low. Baker and Wurgler use the analytical expression for the derivative of expected returns with respect to the equity share of new issues from Modigliani and Miller (1958) to show that, when plugging in historical averages, the implied relationship is far too small to explain their results; the leverage mechanism can explain only one-twentieth of the estimated relation between equity share and future returns.

Second, they ask whether the return-equity share relationship can be explained by changes in investments. If, for example, firms follow a pecking-order of finance with equity as a funding source of last resort (Myers and Majluf (1984)) and face a debt constraint, then increases in investment mechanically result in a higher equity share. If investment is also related to expected returns, say, because high expected market returns tend to lower investment (due to its increased opportunity costs), we will estimate a spurious relationship between the equity share and subsequent returns. However, Baker and Wurgler are able to show that the return-equity share relationship is not explained by the level of investment, the level of total new issues, or even the level of equity issues.

Third, they address concerns about other unobserved risk factors that might simultaneously affect both the optimal capital structure and future returns. While the reverse relationship is theoretically more likely, the optimal amount of leverage might be positively related to expected returns, generating a spurious negative relationship between expected returns and the share of new issues that reflects the desire to change total leverage rather than a desire to time the market. Baker and Wurgler find no empirical support for this theory. Controlling for leverage does not diminish the relationship between returns and the equity share of new issues. Hence, Baker and Wurgler argue that the most plausible explanation of their findings is that managers believe that they can time the market.

In a similar vein of research, DeAngelo, DeAngelo, and Stulz (2010) provide empirical evidence that firms conduct SEOs at times when market valuations are relatively high, and Lowry (2003) finds that investor sentiment, proxied by the discount on closed-end funds and post-IPO market returns, predicts the volume of IPOs, though both papers also emphasize the role of standard motives (such as demand for capital). Emphasizing repurchase prices rather than subsequent returns, Dittmar and Field (2015) find that repurchases occur at a price below the average closing price in the months around a repurchase. Baker, Greenwood, and Wurgler (2003) focus on market timing in the debt market and find that firms tend to borrow long-term when future bond returns are relatively low.

One short-coming of the market-timing literature has been that it does not address the underlying psychology or specific biases that lead to investor over- or under-optimism. One paper that takes a step in this direction is Peyer and Vermaelen (2009), who focus on firm repurchases as a response to investors over-reacting to bad news. The authors show that the long-run abnormal

returns are highest for firms that repurchase after large negative returns and for those that cite "undervaluation" or "best use of money" as the motivation in the press release. As they argue, these firms are more likely to be repurchasing in response to investor over-reaction to bad news, giving us a first indication of a potential trigger of mis-valuation.

Several papers, including Schultz (2003), Butler, Grullon, and Weston (2005), and Butler, Grullon, and Weston (2006a) have challenged the view that managers can time the market, accurately recognizing under- or over-valuation. They argue instead that managers are more likely to issue equity when prices have risen, but cannot actually predict future returns ("pseudo market-timing"). As a result, there are more issuances at market peaks, generating what looks like a negative relationship between issuances and subsequent returns. While the point is theoretically well taken, Baker, Taliaferro, and Wurgler (2006) use simulations to illustrate that such pseudo-market timing can generate only a quantitatively small fraction of the market-timing estimate.[45]

Market-timing has implications for the distribution of wealth between existing, exiting, and new shareholders. Warusawitharana and Whited (2016) embed market-timing in a dynamic model of firm investment and financing decisions with equity misvaluation. Estimates of the model imply that the presence of misvaluation shocks increases the equity value for buy-and-hold shareholders by 2-4% relative to a benchmark of no equity misvaluation. Sloan and You (2015) find that equity issuances and repurchases transfer 1-2% of the pre-transaction market value from transacting to ongoing shareholders, measured as of five years after the transaction. These findings suggest that the continued debate about the biases, or rationality, of the parties involved in financing transactions is important from a welfare perspective.

4.1.2 Catering to non-standard investor demand

The literature and debate on the market timing of equity issuance and repurchases was the focus of the early "Perspective 1" research in Behavioral Corporate Finance. However, by focusing on biased beliefs, researchers left out non-standard preferences and imperfect "optimization" processes as the other two key features of behavioral research (cf. Section 2.1). Later installments of the literature aimed to remedy this omission and started to focus more on non-standard investor preferences, e. g., for dividends or for stocks with a low price-per-share, much of it dubbed managerial "catering" to biased investors (cf. Baker, Greenwood, and Wurgler (2009)).

The early work in this area focuses on the corporate response to (irrational) investor demand

[45] In a follow-up note, Butler, Grullon, and Weston (2006b) argue instead that the bias can be of a meaningful magnitude. Their key disagreement is about "regime changes" in returns. Butler et al. argue that not adjusting for structural breaks or large shocks can result in spurious correlations, while Baker et al. argue that the ability to time these shocks should be a part of the test for market-timing. Other papers have made different arguments against the market-timing hypothesis. For example, market timing also predicts a negative relationship between repurchases and equity issuances or stock-financed mergers, and Dittmar and Dittmar (2008) find that these events are positively correlated.

for dividend-paying stocks. Baker and Wurgler (2004a, 2004b) describe time-varying investor preferences for such stocks, resulting in a time-varying dividend premium. Firms respond, and in times when the dividend premium is high, firms have a higher propensity to pay dividends.[46]

Like the literature on investor sentiment, the early catering literature conveys little about the underlying psychological mechanism that is at the root of such non-standard preferences. Rather, the emphasis lies on ruling out alternative explanations, such as tax motives, transaction costs, and institutional investment constraints, and on identifying the circumstances when demand for dividends arises (see Baker and Wurgler (2004b)). For example, Baker and Wurgler (2004a) argue that the dividend premium is low in times of high investor sentiment for extreme-growth stocks, which typically do not pay dividends, and it is high when investors flock instead to stocks with "safer" characteristics after a crash.

A step forward is Baker, Mendel, and Wurgler's (2016) model of reference-dependent investors who care not only about the level of dividends, but also the change in dividends from the previous period. As in Kahneman and Tversky (1979), investors exhibit loss aversion, captured by a kink in the utility function. Their marginal utility is thus discretely higher in the loss domain than in the gain domain.[47] This results in an aversion to dividend cuts, and allows managers to use dividends as a credible signal of earnings. Managers choose how much to pay in dividends, balancing a desire for a high stock price today (the signaling motive) with a desire to avoid cutting dividends tomorrow. This behavioral signaling model is consistent with many stylized facts while not operating through the same implausible mechanism assumed in traditional signaling models.[48]

The model generates two key testable predictions about the distribution of dividend changes. First, reference dependence predicts bunching at the reference point of zero dividend change. This includes both low-earnings firms, who make just enough to pay the same dividends as in the prior period, and some higher-earnings firms, who could pay a higher dividend, but choose zero change to keep a lower reference point moving forward. Second, the distribution to the right and the left of zero will be asymmetric, with little mass to the left. Since firms aim to avoid the loss region, only the lowest performing firms, who cannot afford to pay the reference level of dividends, will cut dividends. To the right, instead, the model predicts dividend increases of high-performing firms, who can credibly signal higher anticipated future earnings.

Both predictions match the empirical distribution of dividend changes, shown in Figure 12. In

[46] Several papers have extended the theory, e. g., Li and Lie (2006) and Becker, Ivković, and Weisbenner (2011). The catering theory of dividends has also been challenged. Denis and Osobov (2008) fail to find evidence for the dividend premium story outside the United States. Hoberg and Prabhala (2009) argue that the results on catering are better explained by changing risk and managers' conservative attitude towards paying dividends.

[47] See chapter XXX by O'Donoghue and Sprenger (2018) in this Handbook for a detailed discussion of reference-dependent preferences.

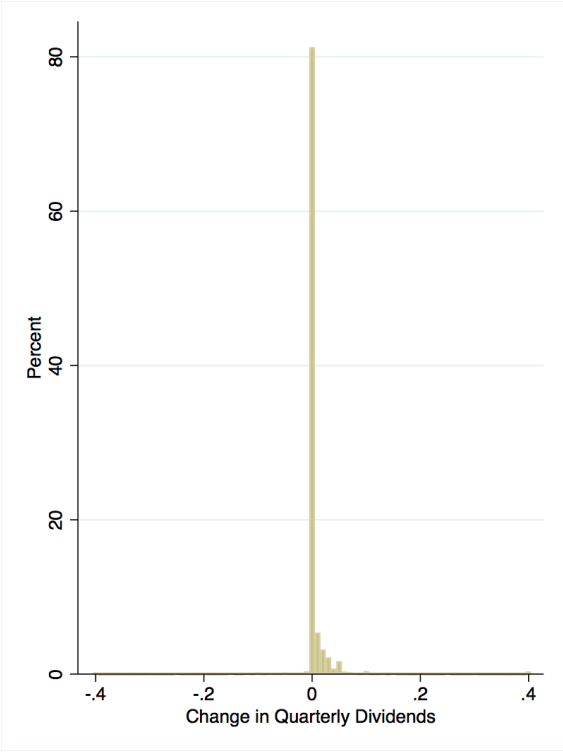
[48] Standard signal models assume that managers use dividends as a costly device to signal strength. Brav, Graham, Harvey, and Michaely (2005), however, present survey evidence that only 4.4% of financial executives subscribe to this notion.

a replication of Figure 4 from Baker, Mendel, and Wurgler (2016), I plot the quarterly changes in dividends per share as recorded in the CRSP event files, both for the full sample (Panel A) and excluding changes of zero (in Panel B). In the full sample, more than 80% of observations have exactly zero change in dividends. When I zoom into the dividend changes excluding zero, we can also see the predicted asymmetry around zero. Less than 3% of observations are decreases in dividends, while about 15% of quarterly changes are increases.[49]

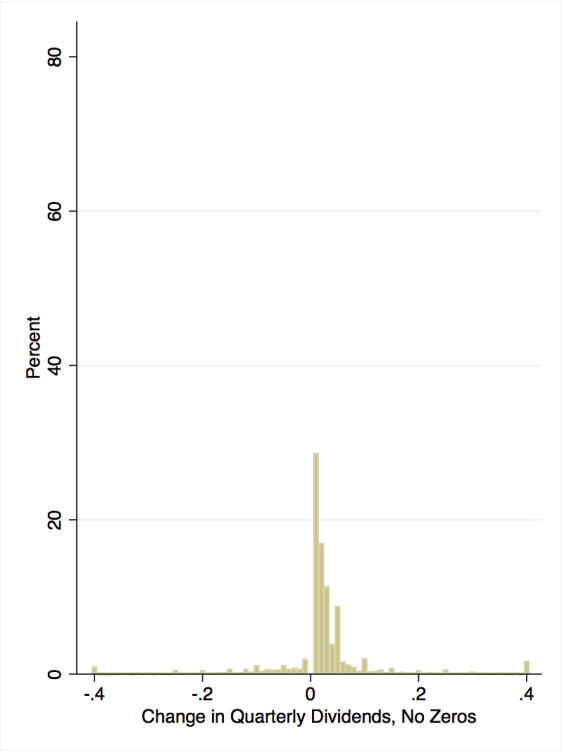
Figure 12: Histogram of Quarterly Changes in Dividends

This figure plots the histogram of quarterly changes in dividends per share, replicating Figure 4 of Baker, Mendel, and Wurgler (2016). The sample includes all records of dividends, taxed normally (distribution code of 1232) from the CRSP event file for US ordinary common shares (always having a share code of 10 or 11). Dividends per share are summed over the quarter of the declaration date. Data excludes any quarter with a stock split (distribution code of 5523). Quarterly change in the dividend per share is winsorized at $+/-0.4$. Panel A includes all changes. Panel B excludes observations with zero change in dividends.
This figure based on figures in Malcolm Baker, Brock Mendel, and Jeffrey Wurgler, "Dividends as Reference Points: A Behavioral Signaling Approach," The Review of Financial Studies, 2016, Volume 29, Issue 3, Pages 697-738, by permission of Oxford University Press and the Society for Financial Studies.

(a) All Changes



(b) Excluding Zeros



The methodological progress from the precise specification of investors' non-standard preferences in Baker, Mendel, and Wurgler (2016) is twofold. By referring to a specific psychological model,

[49] Applying the reference dependence model to price-setting in mergers and acquisitions, Baker, Pan, and Wurgler (2012) find a similar distribution for offer prices.

their hypotheses are both more plausible ex ante and also directly testable and rejectable, compared to non-specific "frictions" or "investor sentiment." The authors are able to generate more specific predictions about which companies are most likely to pay dividends of what size given which earnings.

Regarding both points, it is also worth pointing to the value of direct survey evidence that can corroborate the proposed interpretation. Here, Baker, Mendel, and Wurgler (2016) are able to cite survey of 384 financial executives conducted by Brav, Graham, Harvey, and Michaely (2005), that indicates that their model not only matches stylized facts from the data, but also managers' self-proclaimed dividend strategies. This and other managerial surveys, for example Graham and Harvey (2001) and Bloom and Van Reenen (2007), have proved to be a valuable tool for understanding behavior. We will see in Section 4.2 how survey evidence is useful not only to inform whether and how rational managers react to biased investors, but also to better understand managerial biases (see also chapter XXX by Barberis in this Handbook).

My discussion of the catering approach has focused on explaining dividend payments, reflecting the emphasis in the current literature. There are a few exceptions. Baker, Greenwood, and Wurgler (2009) test a catering theory of nominal (per-share) stock prices. They find that stock splits, which result in a lower price per share without any change to market value, are more frequent when investors appear to pay a premium for low-priced securities. Gennaioli, Shleifer, and Vishny (2012) present a model of financial intermediaries, rather than managers, catering to investor preferences for stable cash flow by engineering new securities.[50] Overall, however, the applications are limited, and catering appears to be an understudied and promising topic for future research.

4.1.3 Media, Attention, and Information

In the previous sections, I have discussed how rational managers respond to investors' biased beliefs or non-standard preferences. In this section, I will dive deeper into one particular way in which investors can be non-rational, namely by misinterpreting or not using all of the available information. Specifically, if investors have limited attention, or a limited ability to process information, it opens the door for the media to play a larger role in markets and for rational managers to strategically manage their media coverage.[51]

A necessary building block to understand managers' incentives to manipulate media coverage is the evidence that the media influences market outcomes. For example, Grullon, Kanatas, and Weston (2004) find that firms with higher advertising expenditure have an increased number of

[50] In their model, investors with infinite risk aversion generate a demand for safe assets. Financial intermediaries generate new securities to match demand. However, both intermediaries and investors ignoring improbable risks of these new securities, rather than intermediaries taking advantage of investor biases.

[51] Note that models of investors with rational inattention can also provide such an incentive for managers to manipulate the media.

shareholders, especially retail investors, and increased liquidity in their stocks, consistent with the hypothesis that these additional investors are more likely to be uninformed noise traders. One channel might be that advertising increases investors' familiarity with a firm and therefore their desire to invest, similar to the mechanism underlying home bias (e. g., French and Poterba 1991). To generate *over*-valuation, and as a result in short-term positive returns, followed by longer-run lower returns, we can embed these considerations in a model with short-sale constraints and heterogeneous investor beliefs, so that assets will be priced by the most optimistic investors (Miller 1977). The effect is strengthened if advertising specifically attracts uninformed or optimistic traders. For example, as Lou (2014) argues, product advertising may not only boost investor awareness of a stock, but also lead to over-optimism as investors take the advertisement at face value and view the product more favorably.[52]

Turning to the firm response, Lou (2014) argues that managers strategically manipulate advertising to benefit from biased investors' reactions. He finds that advertising is high prior to insider equity sales and subsequently lower just after the sale, which is suggestive of strategic manipulation. Moreover, the patterns of advertising spending are similar around SEOs and stock-financed acquisitions, but not around debt issues or cash-financed acquisitions.

In addition to product advertising, a series of papers identify strategic manipulation of media in other corporate settings. Ahern and Sosyura (2014) focus on acquirers in stock-financed mergers who would benefit from a short-term boost in their share prices. Their identification exploits the differing incentives to influence stock prices in different types of stock-financed mergers. In fixed-exchange ratio mergers, parties negotiate over a fixed number of shares as payment for target stock. Here, acquirers benefit from a high price during the negotiations, which allows them to offer fewer shares for the same overall takeover price. In floating-exchange ratio mergers, instead, parties negotiate over a price per target share and at the close of the merger, the acquirer issues new shares to match the price. Here, acquirers benefit from having a high price at the merger close so that they can issue fewer shares to reach the agreed-upon takeover price. Ahern and Sosyura (2014) exploit these differences, comparing acquirer newswire coverage during fixed and floating exchange ratio mergers.

The authors plot newswire coverage (a) over the 100 days prior to the public announcement (the negotiation period) and (b) over the 100 days prior to the merger completion (the transaction period), separately for fixed-exchange ratio and floating-exchange ratio mergers.[53] Figure 13, reproduced from Figure 2 of Ahern and Sosyura (2014), reveals the predicted patterns: fixed-exchange

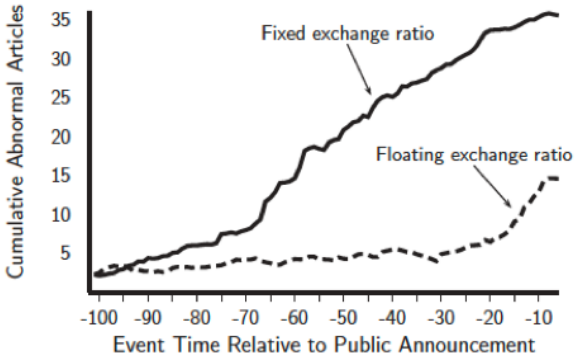
[52] An alternative mechanism, discussed by Barber and Odean (2008), is that investors need to search through thousands of stocks when deciding to buy, but only consider the stocks they already hold when deciding to sell. This makes investors more likely to buy stocks that grab their attention than to sell them.

[53] The average merger negotiations begin 65 days prior ot the public announcement, and the average merger is completed 65 days after the public announcement. Daily abnormal articles are calculated as the ratio of the number of articles to the firm's average number of daily articles during the pre-negotiation period.

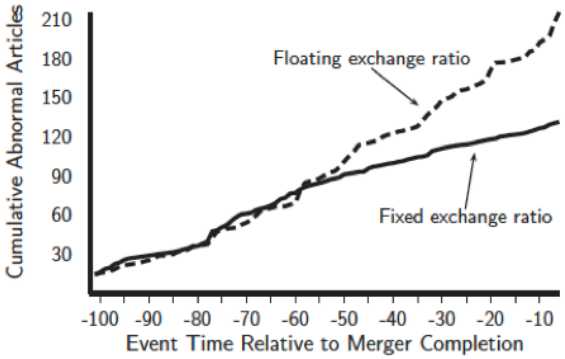
Figure 13: Abnormal Acquirer Newswire Coverage During Mergers

This figure shows Figure 2 from Ahern and Sosyura (2014). This figure displays the average acquirer's cumulative number of abnormal newswire articles in time relative to public merger announcement in (a) and relative to the close of the merger in (b). Data are from 507 acquisitions during 2000 to 2008. See Ahern and Sosyura (2014) for more detailed information.

(a) Negotiation Period



(b) Transaction Period



acquirers have significantly more news articles during the negotiation period while floating-exchange acquirers have relatively more news in the transaction period. Focusing on the negotiation period, the authors further demonstrate that the strategy appears to work: the increase in coverage in fixed-exchange acquisitions has a positive effect on short-term returns, saving acquirers between 5 and 12% on the takeover price.

While the press releases studied by Ahern and Sosyura (2014) represent a direct connection between firms and the media, related literature has also explored the role of third parties. For example, Cook, Kieschnick, and Van Ness (2006) argue that issuers in IPOs benefit from investment banks' marketing to attract more uninformed investors. Indeed, they find evidence that an investment bank's promotional activities, measured by the number of pre-IPO news articles about the firm, result in positive offer price revisions and higher valuations. Further, banks are rewarded for these activities; pre-IPO news coverage predicts higher levels of investment bank compensation and a lower likelihood that the firm switches to another investment bank for SEOs. In another example, Solomon (2012) focuses on the use of investor relation firms to "spin" the news, documenting the use of these firms to generate more media coverage of positive press releases and less coverage of negative releases.

Another mechanism through which firms can control their publicity is earnings announcements. Consider the headlines following an earnings announcement just above vs. just under analyst expectations. Though the true information gap may be negligible, the tone of the media coverage of these two events is likely to be very different. Bhojraj, Hribar, Picconi, and Mcinnis (2009) find evidence that firms adjust expenditures to beat expectations and, as with other media effects, subsequently

experience short-term positive but long-run lower return. The authors also show that managers issue more equity and engage in more insider selling after marginally beating expectations, suggesting that the authors understand the myopic nature of the distortion. Acharya and Lambrecht (2015) develop a theoretical model to demonstrate the long-run costs arising from managers' desire to meet analyst expectations. First, managers may cut investment to meet earnings expectations. Second, managers will have an incentive to underproduce in order to manage future expectations. Finally, firms can influence *when* they make earnings announcement. If investor attention varies, managers seeking to maximize short-term value should release good news when investors are attentive and bad news when they are not. Specifically, DellaVigna and Pollet (2009) hypothesize that investors are less attentive on Fridays when the upcoming weekend might be distracting. They find evidence that the immediate response to earnings surprises is less pronounced on Fridays than on other weekdays, resulting in a larger post-earnings announcement drift.

With the rise of other online media and networks, it will be interesting to explore to what extent managers are able to leverage biased investor responses more, or instead are less able to do so because of faster dissemination of information.

4.2 Biased Managers

Turning back to Perspective 2 of Behavioral Corporate Finance, we can easily see that managerial overconfidence has been at the core of many papers exploring the managerial side of Behavioral Corporate Finance, and constitutes the dominant bias in the field. 51% of the papers on managerial biases that are included in systematic graphical overview of the field in Figure 3 explore managerial overconfidence. Even if we include papers on broader managerial characteristics in our count, the share of papers on overconfidence is still at a remarkable 26%. These papers address a broad array of corporate-finance applications, including investment and financing decisions; they discuss the "bright side" of managerial overconfidence, and connect it to corporate governance. I will review a selection of papers on overconfidence in Section 4.2.1, before turning to other managerial biases and characteristics in Section 4.2.2. As will become apparent there, while researchers have isolated an array of other biases and characteristics that influence managers' decision making and economic outcomes, most of the work has been done in the realm of beliefs, and the evidence on non-standard preferences and cognitive fallacies is still less wide-ranging.

4.2.1 Overconfidence

Before delving into research on managerial overconfidence, the by far most frequently analyzed bias in the literature, let me be more specific about the exact definition(s) of this concept used in the literature. In the section on M&A above, I have modeled overconfidence as the overvaluation of

different valuation components, such as the stand-alone value or expected synergies. The interpretation of overconfidence underlying this modeling approach is an upward shift in the mean of the underlying distribution. An alternative definition, also referred to as overconfidence, is instead that managers systematically underestimate the range of possible outcomes and provide too narrow confidence intervals, or overestimate the precision of signals. The latter is also dubbed "miscalibration" (see, e. g., Ben-David, Graham, and Harvey (2013)) and "overprecision" (see, e. g., Moore and Healy (2008)). In what follows, I will use the term overconfidence to refer to both a shift in mean and an underestimation of variance, and use the other terms to add clarity if needed.

Overconfidence Measures. Given the popularity of research on overconfidence, how do researchers identify overconfident managers?

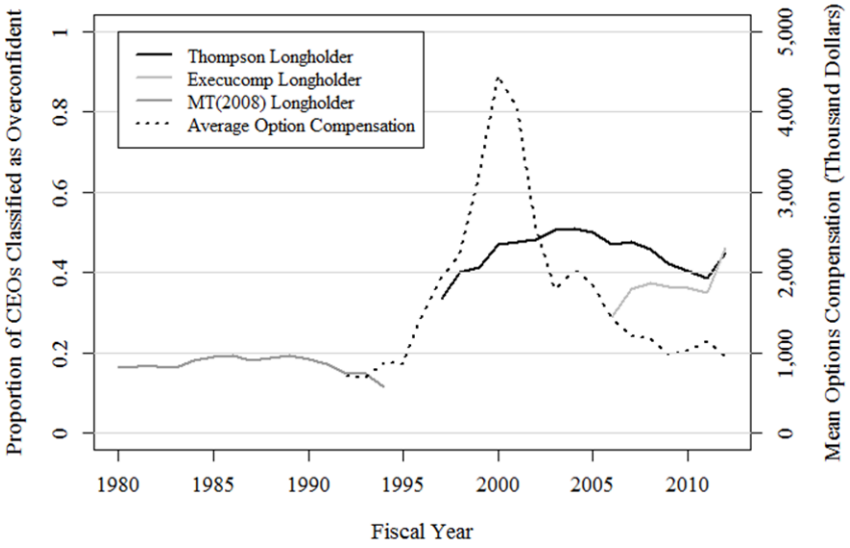
For overconfidence in the sense of overestimating mean outcomes, we can broadly discern four different measurement approaches: the option-based approach, the earnings-forecast-based approach, the survey-based approach, and the press-based approach. Researchers typically employ more than one approach and provide evidence of the positive correlation of their measures.

The option-based approach, introduced by Malmendier and Tate (2005), has been a popular approach in the literature (see, e. g., Galasso and Simcoe (2011), Hirshleifer, Low, and Teoh (2012), Humphery-Jenner, Lisic, Nanda, and Silveri (2016)). As explained in detail in Section 3.3.2, this measure leverages "revealed beliefs." It infers managers' beliefs about the future performance of their firm from their exercising behavior of vested own-firm stock options. Since its original construction in Malmendier and Tate (2005) and Malmendier and Tate (2008), the option-based measure has been updated to use the expanded and refined data on option holdings and exercises from the Thomson Reuters' Insider Filings and Compustat's ExecuComp databases. Malmendier and Tate (2015) provide a comprehensive description and comparison of the different option-based measures. Figure 14 from their paper illustrates the proportion of overconfident CEOs over time and across methods. The original *Longholder* measure identifies a relatively small fraction of CEOs from the 1980s and 1990s as overconfident. The more recent classifications identify around 40% of CEOs as overconfident in more recent years, and the different databases seem to provide converging classifications towards the end of the period analyzed. Malmendier and Tate (2015) suggest that one reason for the increase in the share of overconfident CEOs might be that the distribution of option receivers has changed with the rising popularity of pay-for-performance compensation since the 1990s. Nowadays, compensation in the form of stock options is much more common for younger CEOs and in small firms. Alternatively, overconfidence levels might have risen after managers had witnessed long periods of favorable stock market performances.[54]

[54] This explanation suggests that there is some time variation in overconfidence. Indeed, some research has refined the view of overconfidence as a a persistent belief distortion that can be switched on but not switched off. Malmendier and Tate (2008), for example, split the *Longholder* measure into *Pre-Longholder* and *Post-Longholder*, capturing the years before and after the CEO was first identified as overconfident, respectively.

Figure 14: Option-Based Overconfidence Measures

This figure is also included in Malmendier and Tate (2015). Malmendier and Tate (2008) classify a CEO as overconfident if she holds vested options until final year, if these options are at least 40% in the money at the start of the final year (dark gray, dashed). This "Longholder" measure is updated using either the Thomson Reuters' Insider Filings database (black, solid, 1996-2012), or data from Compustat's post-2006-format ExecuComp database (light gray, solid, since 2006). Average option compensation is calculated using data from ExecuComp.



The earnings-forecast-based approach, introduced in Otto (2014), relies on earnings per share forecasts, voluntarily released by a large fraction of firms that are contained in the Execucomp database. Overconfident CEOs are those whose firms issue upward biased earnings forecasts relative to the eventually realized earnings numbers.

The survey-based approach to identifying overconfidence is used, for example, by Graham, Harvey, and Puri (2013). They measure optimism in senior executives with a Life Orientation Test. Specifically, respondents to the psychometric test are asked to indicate their level of agreement to a series of questions capturing optimism or pessimism, such as "If something can go wrong for me, it will." or "I'm always optimistic about my future."

Lastly, the media-based approach, utilized by Malmendier and Tate (2008) as well as Hirshleifer, Low, and Teoh (2012), examines the language used in press articles to describe corporate executives. Most commonly (see Malmendier and Tate (2008)), this measure is derived from counting the number of media articles that describe the CEO as "confident" (identified through a pre-specified list including words like confident, confidence, optimistic, and optimism) or "cautious" (cautious, reliable, practical, conservative, frugal, steady, and negations of words used to describe "confident" people), and classifying those CEOs as overconfident for which the number of "confident" mentions exceeds that of "cautious" mentions.

As mentioned above, researchers typically employ more than one approach and show the robustness of their results, mitigating potential concerns associated with each measure individually. These papers usually find that the different overconfidence measures are positively correlated. For example, Malmendier and Tate (2008) report a correlation between the *Longholder* and the press-based measure of 0.10, and Hirshleifer, Low, and Teoh (2012) write that their press-based measure identifies 6% of CEOs as overconfident, and that this fraction decreases to 3% among *Non-Longholders*.[55] Hence, while the measures clearly do not have a one-to-one correspondence, both their positive correlation and their differences, combined with the robustness of the estimated overconfidence effects, help to corroborate the overconfidence interpretation.

In the "miscalibration" or "overprecision" strand of the overconfidence literature, survey-based methods have been the most popular approach to identify managerial overconfidence. Ben-David, Graham, and Harvey (2013), for example, use stock market predictions of financial executives (mostly CFOs). As an example, one question used by the authors to infer miscalibration asks survey participants to indicate: "There is a 1-in-10 chance the actual return [on the S&P 500 over the next year] will be less than ___ %; I expect the return to be ___ %; There is a 1-in-10 chance the actual return will be greater than ___ %." Figure 15 presents the time-series of managerial miscalibration in Ben-David, Graham, and Harvey (2013), plotting the fraction of surveyed CFOs for whom the realized return on the S&P 500 is within the 80% confidence interval they specified during the survey. We see that in almost all survey quarters, the fraction of CFOs with sufficiently large confidence intervals is below 80%, direct evidence for managerial miscalibration.

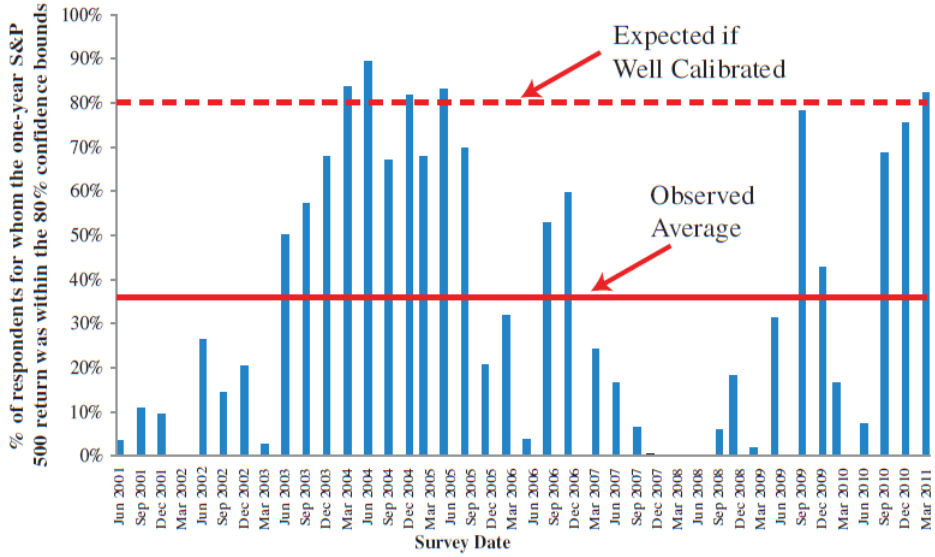
It is worth mentioning that other managerial traits have been linked, explicitly or implicitly, to overconfidence. A common example is gender. For example, Huang and Kisgen (2013) report differences in investment and financing decisions between male and female executives. They find that men undertake more acquisitions and issue more debt relative to women, and that announcement returns are weaker for both sets of corporate events for male executives. Huang and Kisgen (2013) interpret this as suggestive of higher overconfidence in male executives. As noted earlier though, such a conclusion has to be examined carefully as corporate executives—and especially women—are a very selected subgroup of the population. Still, these potential correlates pose an opportunity for researchers to develop theoretical models based on psychological evidence and derive clean theoretical predictions that imply such a correlation, rather than running the risk of p-hunting or ex-post labelling.

Applications. Using these measures, researchers have studied a broad array of corporate outcomes. The earliest empirical study relating an overconfidence measure to a corporate finance

[55] Similarly, Otto (2014) finds a correlation of 0.05, significant at 1%, between his *Longholder* and earnings-based measures. He also reports positive correlations of these two overconfidence measures with an earnings-based measure that captures overconfidence in the sense of overprecision of information.

Figure 15: Time-series of CFO Miscalibration

This figure is taken from Ben-David, Graham, and Harvey (2013). It shows the fraction of surveyed CFOs for whom the realized return on the S&P 500 is within the 80% confidence interval they specified during the survey. The solid red line indicates the average fraction of CEOs who provide wide enough confidence intervals across survey quarters. The dashed red line is the 80% line, corresponding to the case of well-calibrated managers for whom the probability of providing an 80% confidence interval that includes the realized return should be 80%.
Reprinted from Itzhak Ben-David, John R. Graham, and Campbell R. Harvey,"Managerial Miscalibration," The Quarterly Journal of Economics, 2013, Volume 128, Issue 4, pages 1547-1584, by permission of Oxford University Press and the president and fellows of Harvard University.



puzzle is the study of investment-cash flow sensitivity in Malmendier and Tate (2005), which first proposed the option-based approach. It was the first to illustrate how managerial overconfidence can be understood as perceived asymmetric information: Managers believe to have superior (and persistently positive) information about the true value of the firm, relative to the market. As overconfident CEOs overestimate the returns to their projects, they tend to overinvest whenever they have sufficient internal resources available. Whenever cash is scarce, however, overconfident managers cut back on their investment, possibly even below the level of their rational counterparts, since they view external financing via equity as too costly. They believe that new shares would dilute existing shareholders' claims as the offer price will be "too low" relative to the fundamental value.

This explanation for the observed sensitivity of investment to cash flow is closely related to theories of "actual" information asymmetries between managers and capital market participants (Myers and Majluf (1984)): Managers act in the best interest of shareholders, but are hesitant to access the capital market because capital providers require a discount given the informational frictions. Another standard economic approach relies on the misalignment in the interests of man-

agers and shareholders (Jensen and Meckling (1976), Jensen (1986)). Agency theories reason that managers overinvest to garner perks and for empire-building motives.

How have Malmendier and Tate (2005) been able to disentangle the overconfidence hypothesis from these and other alternative interpretations? Their approach, and that of similar subsequent papers, benefits from three ingredients. The first is a theoretical foundation. A simple but careful theoretical characterization of the psychological bias in question allows the researcher to clarify the underlying behavioral mechanism and to derive more subtle, differential predictions, which, in this case, distinguish the overconfidence hypothesis from alternative interpretations. For example, while a superficial treatment of the impact of managerial overconfidence on investment may lead to the inference that overconfident managers tend to overinvest, the theoretical model reveals that overconfidence does not necessarily predict too much investment. Instead, it predicts investment-cash flow sensitivity and does so predominantly among cash-constrained firms. Second, it is crucial to provide a detailed discussion of all plausible alternative explanations, including the derivation of contradictory implications of overconfidence versus the alternative explanations. In comparison to, say, a typical applied micro paper that exploits (quasi-)experimental variation, these analyses will need to include more tests, tables, and write-ups. And, third, it pays off to work hard on constructing a credible empirical measure that is not just 'any' proxy for the bias in question, but one that aims for situational similarities. In the context here, managers' assessment of their driving skills might be a clean indicator of overconfidence, but whether overconfidence in driving translates into corporate decision-making is unclear. The *Longholder* measure instead captures directly managers' overestimation of future increases in their companies' stock price and hence an overestimation of the value created in their firms. In some sense, rather than getting in the middle of the "personality- versus situation-based psychology" debate (beginning with Mischel (1968) and summarized in Ross and Nisbett (2011)), researchers should maximize their success chances in the construction of a credible proxy by satisfying both camps. These strategies allowed Malmendier and Tate (2005) to show that behavioral research can be useful to explain long-standing unresolved puzzles, above and beyond existing theories.

At the same time, I want to offer a word of caution. Modern standards for identification, applied to this context, would require researchers to find exogenous variation in the occurrence of overconfidence as well as, in the above case, the inflow of cash flow. Such variation is hard to come by in corporate-finance settings, but researchers can make some progress, as illustrated by Malmendier and Tate (2015). For the case of overconfidence and corporate investment, they suggest to borrow the instrument for financial constraints introduced by Almeida, Campello, Laranjeira, and Weisbenner (2012), who study the effect of a shock to financing constraints on corporate investment. Their idea is to exploit the supply shock in corporate credit markets in August 2007. As they document, both investment-grade and high-yield bonds had stable spreads over the prior

three years. In August 2007, however the decline in house prices and wave of subprime mortgage defaults started to affect long-term corporate bond prices, and in early 2008 spreads had risen from 1 to 3 percent for investment-grade bonds, and from 3 to 7-8 percent for high-yield bonds. The idea of Almeida et al. is to exploit differences across firms in the portion of long-term debt that matured just after the credit-market shock hit. They consider firms that had at least 20 percent of their long-term debt maturing in 2008 and match them to firms that had no large amounts of debt maturing in the crisis period, and they repeat the comparison for higher thresholds. The "treated" firms faced the choice of refinancing or raising alternative sources of finance during the credit market disruption in late summer and fall of 2007.

As Malmendier and Tate (2015) lay out, a simpler approach suffices to identify the effect of overconfidence on the investment response to a shock to financing constraints. Here, we can exploit variation within "treated" firms, computing the difference-in-differences in investment between firms run by overconfident and rational CEOs. The analysis confirms the theory prediction that the investment of overconfident CEOs is more sensitive to external financing costs than the investment of rational CEOs. Differently from the original analysis, the new methodology sidesteps the endogeneity of cash flow in standard investment-cash flow regressions.

This improvement comes at some cost—the analysis only uses a small subsample of data to ensure clean variation around the shock, casting doubt on the generalizability and broader validity of the results. Moreover, shocks such as the one exploited here are rare and thus identification opportunities in other samples are unlikely. And, of course, the analysis still employs the standard overconfidence measure, i. e., we do not have exogenous variation in the measure of the bias.

Following the more traditional route, other researchers have linked overconfidence to different areas such as entrepreneurship and financing choices. Landier and Thesmar (2009) develop a theoretical model of debt financing, in which a separating equilibrium allows to distinguish optimists, who favor short-term debt, from realists, who self-select into long-term debt. Short-term debt is optimal for optimists due to the increased upside potential in good states and the added flexibility regarding project continuation in bad states. Long-term debt is optimal for realists as it smooths entrepreneurial wealth across states. Using data on more than 10,000 French entrepreneurs, they confirm that short-term debt is indeed associated with positive expectation errors about start-up development and hiring, used as a proxy for overconfidence.

Malmendier, Tate, and Yan (2011) also analyze the financial decisions made by overconfident CEOs. Consistent with the reasoning in Malmendier and Tate (2005) that overconfident CEOs prefer internal resources and are averse to new equity, they find that overconfident CEOs limit the use of external financing, and conditional on approaching the capital market, prefer debt to equity. Similar behavior, both on the investment and the financing side, has also been documented for managers displaying overconfidence in the sense of "miscalibration." Specifically, Ben-David,

Graham, and Harvey (2013) find that miscalibrated managers choose riskier strategies, including higher investment levels and higher debt financing.

The "bright side" of overconfidence. As often stated in the overconfidence research discussed above, the evidence on biased managerial decisions and the resulting deviations from optimal firm and market outcomes tends to leave out the potential "bright side" of overconfidence. Given the documented prevalence of overconfident C-suite managers, it seems plausible that their biased beliefs confer advantages, either to them personally or to the firm. The most obvious example is probably that overconfidence can counteract individual risk aversion, which is not desirable from the perspective of shareholders. When a manager is risk-averse, overconfidence can induce her to take risky value-creating projects that a rational manager would reject given her risk aversion. For example, Hirshleifer, Low, and Teoh (2012) present evidence that overconfidence (interpreted as a shift in mean) can spur R&D activities. In similar spirit, Campbell, Gallmeyer, Johnson, Rutherford, and Stanley (2011) argue that some level overconfidence can lead to value-maximizing policies, and find that mildly overconfident CEOs have a lower probability of being removed from the top of the corporate ladder by the board.

Governance Responses to Overconfidence. Some of the "bright side" papers also connect to the question of optimal corporate governance and regulatory interventions in the presence of managerial biases. Goel and Thakor (2008), for example, illustrate that value-maximizing CEO selection mechanisms can fuel the prevalence of overconfident leaders. They develop a CEO selection model, in which the ability of potential successors is unknown and can only be inferred from the success or failure of their prior project choices. As a result of this tournament-type process, candidate managers increase the riskiness of their projects. Furthermore, overconfident managers (who are modeled as underestimating the variance of project cash flows) have the highest probability to be appointed as CEOs. In a related model, Gervais, Heaton, and Odean (2011) complement these findings, focusing on the case when firm owners know that the CEO is overconfident and adapts compensation contracts correspondingly.

Several empirical papers confirm that firms respond to managerial overconfidence by adjusting their compensation packages. Otto (2014) reports that overconfident CEOs are paid fewer option grants and bonus payments and receive less total pay, consistent with the idea that overconfident agents require fewer incentives to exert effort. Humphery-Jenner, Lisic, Nanda, and Silveri (2016), instead, find that, relative to non-contingent pay, the compensation of overconfident CEOs is tilted towards *more* incentive claims such as stock and option grants. While the findings in the two papers are not necessarily inconsistent (as they focus on absolute versus relative levels of compensation and slightly different components) and their broad conclusions are the same (principals tailor incentive schemes to biased agents), the emphasis on "less" versus "more" incentive compensation suggests that future research is needed to establish robust stylized facts about executive compensation in

the presence of managerial biases.[56]

In addition, Banerjee, Humphery-Jenner, and Nanda (2015) provide evidence that changes in the regulatory environment can mitigate some of the downsides pertaining to hiring an overconfident CEO. For example, they find that after the enactment of the Sarbanes-Oxley Act, firms with overconfident CEOs—but only those who were not compliant with the Act's provisions prior to its passage—show a decrease in investment-cash flow sensitivity, have improved post-acquisition operating performance, and pay higher dividends.

4.2.2 Other Managerial Biases and Characteristics

As mentioned above, research on overconfidence has dominated the field so far, even though the evidence on managerial biases is much broader. I will discuss research on other biases and managerial characteristics, organized around the categories of belief distortions, non-standard preferences, and cognitive fallacies.

Belief Distortions. A relatively large group of papers provides evidence on the role lifetime experiences. Broadly speaking, these papers argue that personal experiences stick with managers and leave an imprint on the corporate decisions they make years, or even decades, later in their life. To put it in the figurative language used by Dittmar and Duchin (2016), managers are "[l]ooking in the rearview mirror" when deciding on corporate policies. We can group those papers into research that focuses on the role of personal experiences and those pertaining to how managers' professional life shapes their decision-making.

Starting from the former category, Malmendier, Tate, and Yan (2011) find evidence that salient personal experiences from the Great Depression have a long-lasting impact on managers' willingness to take financial risk in their firm. Specifically, they show that CEOs who grew up during the Great Depression tend to shy away from external financing and rather rely on internal cash flow for their investments.[57] Instead, CEOs who in the past served in the military are found to follow more aggressive financial policies, and prefer higher leverage ratios.

[56] Here are two aspects that deserve further investigation. First, Humphery-Jenner, Lisic, Nanda, and Silveri (2016) build on the model of Gervais, Heaton, and Odean (2011), who define as overconfidence as "overestimat[ing] the precision of their information," but use the option-based overconfidence measure of Malmendier and Tate (2005), which captures overconfidence in the sense of a shift in the perceived mean outcome. Second, when contrasting their findings with those by Otto (2014), they write that, in untabulated results, their main variable of interest, *Holder67*, remains significant when including the measure by Otto (2014), *LongHolder*, as a control variable. However, both measures are based on the option-exercising behavior of CEOs, and capture overconfidence in the sense of a shift in the perceived mean, making the inclusion of both measures uninformative if not problematic.

[57] Bell, Chetty, Jaravel, Petkova, and Van Reenen (2017) provide complementary evidence on the influences of childhood experiences on economic decision-making. Using variation in innovative activity across commuting zones, they show that people with higher early-life exposure to innovation are more likely to become inventors later on. Moreover, these people tend to innovative in the same technology categories (e. g., computers or medical devices) they had exposure to as a child.

The type of military experience (e. g., victorious or not) and selection into the military appears to play a role. For a different, more recent sample and somewhat different specification (no firm fixed effects), Benmelech and Frydman (2015) find that military CEOs pursue less risky strategies on average, manifested in lower investment levels and slightly lower leverage. They also add an interesting aspect in that they argue that the military training results in a lower incidence of fraud.

Turning to the influence of prior professional experiences, Dittmar and Duchin (2016) track the complete employment histories of around 5,000 CEOs and 4,000 CFOs. Their evidence suggests that managers who have witnessed salient negative firm outcomes in the past, including bankruptcy and distress, tend to play it safe going forward—they choose lower leverage, higher cash holdings, and lower levels of capital expenditures. The paper is also interesting from the perspective of identification challenges. The analysis is challenged by two types of selection issues, which are common in much of the behavioral-manager literature in Corporate Finance: First, managers who become CEOs after past negative experiences might be different from managers who become CEOs without those past experiences—but not *because* of those experiences. Instead, it is selection: Only certain "types are persistent enough to work their way up the corporate ladder after their career has been tainted by going through bankruptcy or other distress. These "types" might be the careful types, who are inclined towards playing it safe, even before and throughout the negative experience. Second, conservative managers (whether triggered to be conservative because of the experience or inherently conservative) might be likely to join a firm that is looking for a conservative decision-maker at this point in time. Hence, the corporate outcomes might be at least partly firm-specific, rather than manager-specific. While the paper does not discuss much the first challenge, it provides a very helpful discussion of the second issue. First, the paper clarifies that the inclusion of firm fixed effects is insufficient to address the issue. Fixed effects capture time-invariant firm characteristics, and hence address the alternative interpretation that managers who have experienced distress in the past become CEOs of firms that have a *general* tendency to follow cautious strategies. Fixed effects do not capture time-variant firm characteristics, though, i. e., the actual concern that a conservative manager is sought out at a moment when "corporate attitudes" in the firm have changed towards less risk-taking, which drives both the appointment of CEOs and changes in corporate policies. To deal with this issue, Dittmar and Duchin (2016) focus on exogenous CEO turnovers, capturing, e. g., CEO deaths or planned retirements. In other words, the authors exploit cross-sectional variation in past experiences within the subgroup of exogenous CEO changes.

In recently published work, Schoar and Zuo (2017) provide evidence complementary to Dittmar and Duchin (2016) regarding the long-run impact of early-career experiences and conditions. They find that managers who started their careers during recessions adopt more conservative policies, characterized by lower capital and R&D expenditures, as well as lower leverage. To avoid selection issues associated with the starting date of a manager's career, the authors aim to capture the

exogenous component of the economic conditions at the beginning of the manager's career, which they equate to age 24 (the modal age of starting the first full-time employment in their sample) for their analyses.

Relatedly, Xuan (2009) shows that managers' career trajectories affect their strategic considerations regarding the allocation of capital once they are promoted to the position of CEO. Focusing on newly appointed CEOs in firms with multiple business segments, he shows that CEOs who have worked in *some but not all* segments allocate more capital to divisions the CEO has not worked in. This result may be surprising in light of the expanding network literature. The author interprets it as consistent with "bridge-building motives" to garner the support from middle managers of these divisions.

Greenwood and Hanson (2015) also focus on the influence of more recent professional experiences on managerial decision-making. Analyzing the investment behavior of firms in the ship industry, they show that firms boost ship production when current ship earnings are high, but that these investments have low returns in the future. To explain these trends, they develop a model of behavioral firms or managers, who overextrapolate recently experienced demand shocks into the future, modeled as an upward-biased perception in the persistence of demand shocks.[58]

This last set of papers addresses, to some extent, the question of why so much of the behavioral-mananger literature has focused on overconfidence. These recent papers may have stemmed from the availability of a robust measure from Malmendier and Tate that had broader applications, and from increasing openness of the profession towards overconfidence-related studies. Still, from an ex-ante perspective, other belief-based biases such as sunk-cost fallacy and hindsight bias seem very powerful, and the slow pursuit of these avenues remains surprising.

Non-standard Preferences. Turning from belief-based to preference-based theories, the most widely studied approach is prospect theory (Kahneman and Tversky (1979), Tversky and Kahneman (1992)). It posits that people frame outcomes as gains or losses relative to a reference point, react more strongly to losses than gains (loss aversion), and underweight small-probability events but overweight high-probability events. Prospect theory has been extensively applied in asset pricing (e. g., Barberis, Huang, and Santos (2001); see also chapter XXX by Barberis in this Handbook). So far there is less research on the corporate side.

One early exception is Loughran and Ritter (2002)'s work on IPO underpricing. They aim to explain why, in a significant number of IPOs, a lot of money "is left on the table" without much regret on the side of the issuers. Loughran and Ritter show that, in these cases, the final offer price tends to have been revised upward compared to the prospectus. From an ex-post perspective, the

[58] Foreshadowing the research on cognitive limitations, Greenwood and Hanson (2015) also argue that managers do not fully account for the endogenous responses of their competitors to demand shocks, a fallacy frequently one might dub 'competition neglect.'

firms' founders or other issuers could have reached for even higher offer prices and revised even more, but Loughran and Ritter argue that the issuers are "happy" as they are comparing their post-IPO wealth *relative to* the wealth they had anticipated during the road show. In other words, their decision might be influenced by prospect-theoretic thinking, and the losses associated with underpricing are more than offset by wealth gains associated with the higher than expected offer price. Consistent with this, Ljungqvist and Wilhelm (2005) find that when issuers are "satisfied" with the IPO income, as defined based on the relative wealth gains and losses in Loughran and Ritter (2002), they are more likely to stick to the same underwriter for subsequent SEOs.

Loughran and McDonald (2013) go one step further and argue that lead underwriters can exploit the prospect-like preferences of issuers. When issuers choose language expressing increased uncertainty for the initial prospectus, possibly reflecting a weak informational position of the issuer relative to the underwriter, underwriters can set low initial offer prices. These prices become the reference point for issuers, who partially but not fully adjust them upward later on.

Another, very plausible application of reference dependence is compensation setting. Dittmann, Maug, and Spalt (2010) consider a manager who is loss averse and values wage gains and losses relative to a reference wage, rather than in absolute terms. They first consider piecewise linear contracts, comprised of a fixed salary, stock, and options, and calibrate their model to the observed contracts of executives from Execucomp. They find that the model matches the observed data well when reference points are low, but fails to explain the data when references wages are high, in which case the piecewise linear model predicts negative option holdings and salaries. They then derive the optimal non-linear contract for loss averse managers and confirm the same basic insight: Above a cutoff, which is increasing in the reference wage, the optimal compensation is increasing and convex, akin to the empirically observed CEO contracts that feature positive stock and option holdings. Below the cutoff, compensation drops to a lower bound. In their calibration of the optimal contract, the region with increasing and convex contracts dominates only when reference wages are low. Hence, managerial loss aversion might help explain observed compensation packages, and further investigation, possibly with better and more detailed panel data, might prove an excellent avenue for future research.

Reference dependence has also been studied in the context of mergers and acquisitions. I have already discussed the evidence in Baker, Pan, and Wurgler (2012) that the probability of bid acceptance discontinuously jumps up when the offer coincides with a likely reference point, such as a recent peak price, and in particular the 52-week high. While I emphasized the implied investor biases in my prior discussion in Section 2.2, it is an open question whose reference points determine the observed pricing pattern—advisors, boards, investors, or financiers of both the bidder and the target. As such, the paper serves as another example of how the interaction of biases on the part of actors from Perspectives 1 and 2, and even 3, can shape economic outcomes.

A related bias proposed as a driver of acquisitions is "envy." Goel and Thakor (2010) point out that managers comparing their wage to that of their peers can help explain merger waves, especially as managers' wages tend to increase in firm size (in addition to performance). Consider the scenario that a manager makes a value-enhancing (and firm size increasing) acquisition and her wage increases. Other CEOs will feel envy, modeled as higher marginal utility from a wage increase. This in turn, makes it more likely that other managers will pursue acquisitions themselves, resulting in a merger wave.

As a last preference-based theory, I would like to mention hyperbolic discounting. Despite being one of the most popular topics in Behavioral Economics, we still have few successful applications in finance. One notable exception in the realm of entrepreneurship and investment is Grenadier and Wang (2007), who examine investment timing in a real-options framework when entrepreneurs have time-inconsistent preferences. While typical calibrations of real-options models reveal the benefits from waiting, hyperbolic discounting works in the opposite direction. Hence, hyperbolic entrepreneurs would tend to invest earlier than a sophisticated person. Further distinguishing between sophisticated and naïve individuals, Grenadier and Wang (2007) establish that—perhaps in contrast to what one would expect at first—sophisticated entrepreneurs invest even earlier than naïves. Coined the "sophistication effect" in O'Donoghue and Rabin (1999), this arises because "sophisticated entrepreneurs are concerned about the suboptimal timing decisions of future selves [which] further erodes the value of their option to wait relative to that of naive entrepreneurs" (Grenadier and Wang (2007)). One nice aspect of this application is that it may include choices where entrepreneurs are trading off "real effort" and "payoffs under financial constraints," which would help with the usual criticism of applying hyperbolic discounting to financial choices—even a hyperbolic agent would not go for a faster (but lower) payoff if he can benefit from a higher payoff in the future already today (e. g., by borrowing against the future payoff).

In addition to theoretically well-understood non-standard preferences, the behavioral literature has also analyzed managerial "traits." While less grounded in specific psychological constructs and biases, this research nevertheless contributes to the more general point that managers leave a personal imprint on their firms.

Cronqvist, Makhija, and Yonker (2012) study the relation between CEOs' personal and corporate choices. They find that high corporate leverage is associated with high ratios of mortgage value to purchase price in the CEO's most recent home purchase. In terms of mechanisms, they conclude that both endogenous matching—in which high leverage firms match with leverage-seeking managers—and a causal imprinting of preferences on firm policies can account for the documented relationships. Hence, their finding might reflect that the personal willingness to take risk is related to doing so on the corporate level.

Graham, Harvey, and Puri (2013) document the link between managers' (general) risk attitudes

and corporate risk taking directly. Their psychometric tests, which I mentioned before (in Section 4.2.1) regarding the measurement of (over-)optimism, also measure executives' risk aversion using questions about the preferences regarding more or less risky income streams.[59] They find that managers are significantly more risk-tolerant than similar-aged lay people. For example, they classify less than 10% of executives in their sample as very risk averse, compared to 64% of the lay population with a similar age profile found to be very risk averse in Barsky, Juster, Kimball, and Shapiro (1997).[60] They then show that risk tolerance is reflected in corporate policies. For example, more risk tolerant CEOs in their sample make more acquisitions and are more likely to manage growth firms. The authors also point out that their findings are consistent with managers matching with firms based on characteristics, managers altering firm strategies, and even managers "learning a trait on the job."

Another trait that has caught researchers' attention is religious orientation. Hilary and Hui (2009) show that firms headquartered in more religious regions (counties with a high fraction of religious adherents) exhibit less volatile ROE and ROA and invest less in risky projects such as R&D. Schneider and Spalt (2016), instead, use the fraction of Catholics in the county of a company's headquarters as an instrument for the CEO's gambling attitudes, as Catholic teachings are more open towards gambling than Protestant teachings. They find that CEOs with a higher propensity to gamble induced by county-level religious attitudes overinvest in business segments characterized by right-skewed payoff distributions, i. e., "long shot" projects that offer a high payoff albeit with low probability. As in the first paper, the instrument relies, of course, on the assumption that local religious norms influence managerial decisions, even if the manager does not share the religious beliefs that are predominant in the county.[61]

In both papers, the outcome variables may not be the first ones that come to mind when thinking about religiosity. In fact, taking the two papers by Hilary and Hui (2009) and Schneider and Spalt (2016) together, it seems hard to square that higher county-level religiosity would be associated with *lower* risk tolerance, while a higher *share* of certain religions would be associated with a *higher* propensity to gamble. As before, a more detailed theoretical underpinning and investigation of the psychological mechanism would be helpful.

Finally, we can also consider traits that are not innate, but learned or otherwise acquired. For

[59] A typical question is: "Suppose you are the only income earner in your family. Your doctor recommends you move because of allergies. You have to choose between two possible jobs (choose one): (a) 100% chance that the job pays your current income for life; (b) 50% chance that the job pays twice your current incomes for life and 50% chance that the job pays 2/3 of your current income for life."

[60] High risk aversion in both studies is defined as an estimated relative risk aversion coefficient of 3.76 or higher. As Graham, Harvey, and Puri (2013) point out, the original question used by Barsky, Juster, Kimball, and Shapiro (1997) to elicit risk preferences was framed slightly differently (namely, as a new job opportunity as opposed to a necessity of job change), leading to a potential concern of status-quo bias influencing participants' choices.

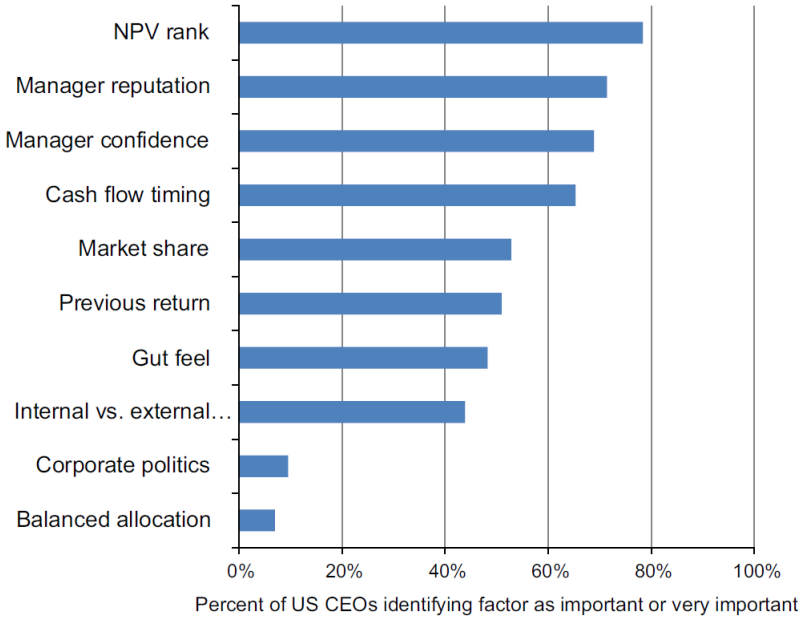
[61] An example Schneider and Spalt (2016) give is that a manager in Salt Lake City should be affected by local Mormon teachings, even if he or she is not a Mormon.

example, Custódio and Metzger (2014) find that firms whose CEOs have a finance education or experience in the financial sector have lower cash holdings, higher leverage, and lower investment-cash flow sensitivities—all consistent with a better financial decision-making. As an example of a dynamically changing feature, consider CEOs who attain "superstar status," which Malmendier and Tate (2009) identify through awards of prominent U.S.-wide magazines and organizations such as *Business Week*, *Forbes*, *Time*, or Ernst & Young. Malmendier and Tate report several adverse effects associated with superstar status: Superstar CEOs significantly underperform subsequent to the award and start to manage earnings. At the same time, they extract higher compensation and engage in potentially distracting activities, such as writing memoirs and taking outside board seats. These and other papers are even further from specific, testable behavioral models, but may still help to improve the psychological realism of our analyses.

Cognitive Fallacies. A few papers speak to managers' cognitive limitations and to the mental shortcuts they take. Graham, Harvey, and Puri (2015) survey more than 1,000 CEOs in the U.S. to see how capital is allocated in their firms. Figure 16 shows the response frequencies for various options. Reassuringly, a project's NPV appears to be the most important driver. However, for more

Figure 16: Decision Criteria for Capital Allocation (U.S. CEOs)

This figure is taken from Graham, Harvey, and Puri (2015). Surveyed CEOs were asked to indicate the importance of each factor on a scale of 1 to 5. The bars represent the fraction of CEOs who identified a factor as important (4) or very important (5). "Internal vs. external..." asked whether the fact that a project requires external funding was deemed important.



than 60% of surveyed CEOs, the reputation and confidence of the overseeing divisional manager

is another key factor for capital allocation, and every second CEO allocates capital based on "gut feel." While such criteria may also indicate successful heuristics, they clearly raise the possibility of biases and suboptimal corporate decision-making. In fact, in related survey evidence from a 392 U.S. and Canadian CFOs, Graham and Harvey (2001) find that executives rely heavily on practical rules of thumb when choosing capital structure, such as choosing debt maturity so that they will not need to refinance in bad times. They also subscribe to the "EPS dilution fallacy," i. e., the concern that issuing new equity dilutes EPS, even though standard textbook finance explains that equity issues do not dilute earnings as long as the firm earns the required return on the new capital (Brealey, Myers, and Allen (2012)). Graham and Harvey (2001) also find evidence for another prominent fallacy discussed in Brealey, Myers, and Allen (2012), namely the WACC fallacy. When asked which discount rate their firm would regularly use to value a new project in an overseas market, the most common response was the company-wide discount rate (approval rate of 59%), rather than a rate that corresponds to the project-specific risk. This method was especially popular among CFOs with shorter tenure (up to 8 years), and less so among managers who have strong exposure to foreign markets operations and work for Fortune 500 firms.

Krüger, Landier, and Thesmar (2015) bring the "WACC fallacy" to firm-level data. They explore the investment behavior of conglomerate firms that have both a *core* division and *non-core* divisions. If firms apply a company-wide discount rate to all projects, they will overestimate the NPV of risky projects and underestimate the NPV for safe projects. Hence, they will overinvest in relatively risky divisions and underinvest in safer divisions. Consistent with the prediction of the WACC fallacy, the authors find that the investment level in *non-core* division increases in the difference between the non-core and core divisions riskiness, measures as the difference between their respective industry-level asset betas.[62] Figure 17 plots this increasing relationship between investment levels and beta spread for three different measures for investment, raw, adjusted for median industry investment, and further adjusted for a firm's weighted average industry adjusted investment.

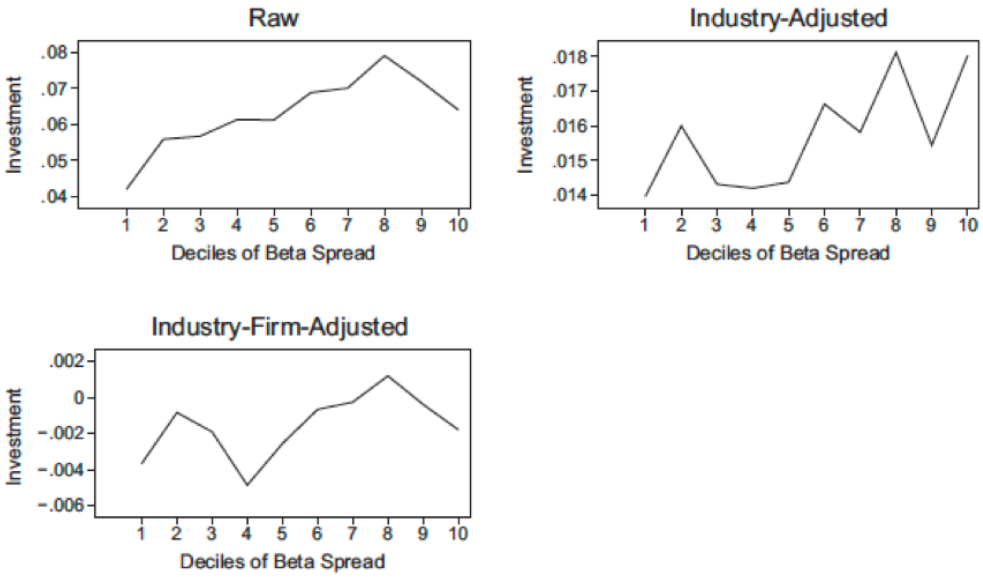
4.3 Networks

The study of network effects has recently become a rather active area of research, spanning both Perspective 1 and Perspective 2. These papers ask whether social connections, past working relationships, and other ties affect corporate decision-making by inducing non-standard preferences and biases. The theoretical predictions of such network effects are often ambiguous. At the stage of the literature, the empirical findings seem to reflect this ambiguity, with empirical support for

[62] To measure divisional risk, industry-level asset betas are constructed by first performing 60-month rolling-window regressions of Fama-French 48-industries portfolio returns on the CRSP Value Weighted Index, and second unlevering these estimated equity betas.

Figure 17: Firms' Non-Core Division Investment Levels

This figure presents Figure 1 in Krüger, Landier, and Thesmar (2015). Observations are grouped into deciles based on the spread between the estimated *non-core* division and *core* division spread. The vertical axis indicates *non-core* investment levels (top left: raw, top right: adjusted for industry investment, or bottom left: further adjusted for a firm's weighted average industry adjusted investment).



both a dark and bright side of social connections.

Across-firm connections. I begin with research on connections across firms. To see that the theoretical predictions of social connections are ambiguous, consider the case of M&A. Personal relationships could result in increased information sharing between the target and acquirer and improve the efficiency of merger outcomes. Alternatively, social connections may bias management towards thinking more positively about the merger, resulting in them over-valuing connected mergers.

Recent empirical evidence provides support for the latter prediction. Using biographical data from BoardEx, Ishii and Xuan (2014) define two individuals as connected if they attended the same undergraduate or graduate institution or worked at the same firm in the past. They show that the more directors and executives of a target and an acquirer are connected, as the percent of all across-firm pairs of directors and executives, the lower are the abnormal returns around the announcement for the acquirer and for the combined entity. In the long-run, mergers with more social connections are also more likely to result in a divestiture for performance-related reasons. El-Khatib, Fogel, and Jandik (2015) generate similar results on negative announcement effects for measures of CEO network centrality, or a CEO's network "power," using the same BoardEx data

on CEOs' employment history.[63]

One potential explanation for these patterns is that people prefer working with people similar to themselves. Gompers, Mukharlyamov, and Xuan (2016) explore this mechanism in the context of selecting co-investment partners in venture capital syndication. They find that the probability of collaboration between two venture capitalists is higher if they attended the same university, are from the same ethnic group, or worked at the same company earlier in their careers. Moreover, such homophily reduces success of the venture. Deals in which venture capital partners share any of these characteristics are less likely to result in an IPO.

In these cases, the negative outcomes cast doubt on the rationality underlying network effects. Rationality would imply that the cost of, for example, taking on a value-destroying merger must be compensated by some other benefit like future career prospects. Especially in the case of large-scale decisions such as mergers, this seems unlikely. Only if the utility gains from working with "similar people" are large can these network patterns be explained in a rational (albeit still behavioral) model with non-standard homophily preferences.

In other cases, instead, the net effect of social connections appears to be positive. For example, Engelberg, Gao, and Parsons (2012) use DealScan data on syndicated bank loans to show that social connections between bankers and firms lower the firm's cost of funding, probability of future credit downgrades, and expected default risk, and increase future stock returns. The higher ex-post performance after connected deals indicates that the lower interest rate may reflect gains in increased information sharing between the firm and the bank.

Within-firm connections. Within firms, network research has focused on CEO-manager and CEO-board connections. One example of the former is the evidence in Glaser, Lopez-de Silanes, and Sautner (2013) that, in detailed data from one large conglomerate, more powerful and more connected division managers are able to obtain a larger share of investment following a cash-windfall. Their measures of power and connectedness utilize some of the familiar proxies, such as going to the same university as the CEO, but also unique measures based on their survey data, such as having frequent lunches with the CEO or listing the CEO as a mentor. In terms of value implications, the additional cash-flow to powerful or connected managers appears to result in over-investment and lower subsequent performance.

Duchin and Sosyura (2013) add an interesting twist to these findings. They argue that the value- and efficiency-decreasing effects of social connections only apply to firms with weaker governance, while social connections appear to increase efficiency and firm value in firms with high information asymmetry (where the potential gains from increased information sharing may be larger). As in the above-mentioned paper, their findings also rely on the increased allocation of capital to connected

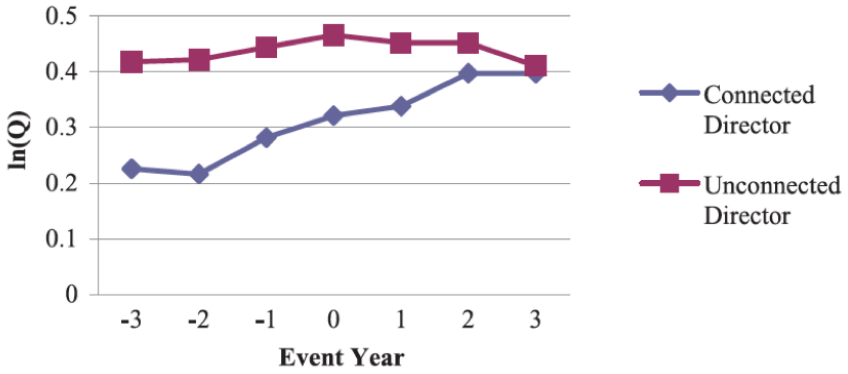
[63] Both papers also find network effects on the frequency of mergers: Connected firms (in Ishii and Xuan (2014)) and more central CEOs (in El-Khatib, Fogel, and Jandik (2015)) are more likely to engage in more M&A activity.

divisional managers, in this case using BoardEx along with other sources to identify connections through non-profit organizations, education, and past employment. Methodologically, it is difficult to draw these conclusions since it is hard to measure investment efficiency.[64]

Turning to CEO-board connections, Fracassi and Tate (2012) use BoardEx data to count the number of connections between outside directors and the CEO, where a connection is either the same current employment (typically overlap in an external directorship), overlapping prior employment, attending the same school and graduating within one year of each other, or active participation in another organization (e. g., a club or charity). First, Fracassi and Tate demonstrate that firms with more powerful CEOs, measured by entrenchment index of Bebchuk, Cohen, and Ferrell (2009), add new directors with more connections to the CEO.[65] They then show that the number of connected directors on a board is negatively related to firm value (log(M/B)). In an attempt to address the endogeneity of board composition, they also collect data on director deaths and mandatory retirements of connected directors, and show that these incidences predict a subsequent increase in firm value.

Figure 18: Firm value around director deaths and retirements

This figure shows Figure 1A from Fracassi and Tate (2012). It displays firm value in fiscal years relative to the death or mandatory retirement of a board member. Firm value is measured by the log of the ratio of market value of assets to book value of assets (Tobin's Q). Connected directors are those who share current employment, prior employment, education, or are involved in another organization with the CEO. See Fracassi and Tate (2012) for more detail.



The authors acknowledge the identification challenges, and in particular that their main findings on the value implications appear to originate several years before the respective changes in board composition. (Cf. the rise in firm value before the exogenous departure of a connected board member in Figure 18.) Nevertheless, or maybe because of these challenges, the paper proves very

[64] Glaser, Lopez-de Silanes, and Sautner (2013) and Duchin and Sosyura (2013) each use various approaches, including investment sensitivity to Tobin's Q, and the market to book ratio.

[65] These results are consistent with earlier work by Hwang and Kim (2009), demonstrating that CEOs with social connections to the board receive more compensation and that, in the years following CEO appointment, the proportion of directors with social ties to the CEO increases.

helpful in discussing the possible origins of the apparent CEO preference for connected directors: better information versus reduced search costs in hiring friends and past colleagues. While the first channel predicts positive effects of CEO-director ties, the latter could harm shareholder value if CEOs select friends who will engage in weaker monitoring. Additional results on connected CEOs engaging in more mergers (which require board approval) and on those mergers having lower announcement returns support the latter channel. Overall, CEO-director connections appear to reduce firm value.

Other research points in the opposite direction. Schmidt (2015) suggests that there is a bright side to having connected board members in the M&A context. He identifies types of mergers where board advice can go beyond monitoring, for example, when a board member has worked in the target industry and the CEO has not. In these mergers, more social ties between CEOs and directors actually predict higher announcement returns.

Exogenous Network Formation. Much of the network literature wrestles with a possible confound of the connectedness measures: It is difficult to distinguish between the causal effect of social connections from outcomes driven by similarity between connected individuals and poosibly a bias towards those more similar to oneself. For example, people who went to the same college at the same time may be more likely to know each other, but may also just have more similar beliefs and preferences and hence interact differently with each other on the corporate level than they would with other managers.

Several recent papers have used one setting with exogenously created peer groups to address this issue, namely, randomly assigned MBA sections at Harvard Business Schools (HBS), to disentangle these effects. These sections define groups of students who take all of their first-year courses together, arguably generating a strong peer group. In the first paper to use this setting, Lerner and Malmendier (2013) find that post-MBA entrepreneurship is lower in peer groups with a higher share of pre-MBA entrepreneurs. They document that the effect is concentrated in a decline of unsuccessful ventures (where success is measured by an IPO, acquisition or revenues above $5M, or at least 50 employees), consistent with learning from peers with entrepreneurial experience. Building on the same design, Shue (2013) finds that post-MBA compensation and acquisition attempts are more correlated among students from the same section. Exploiting yet an additional feature of the HBS setting, she finds that peer effects are strongest in the years following class reunions.

Trust. One potential underpinning of the formation of networks that has received particular attention is trust. Trust might be the basis of network mechanisms such as lower cost of information acquisition, a preference for working with or helping those with social ties or similar backgrounds, or the formation of biased beliefs about connected versus unconnected parties.

At the micro level, a number of outcomes have been related to trust. Guiso, Sapienza, and

Zingales (2015) provide survey evidence that firms where employees perceive management to be more trustworthy and ethical have a higher Tobin's Q, higher profits, and a lower fraction of unionized workers. Focusing more on the mechanisms through which increased trust could affect firm outcomes, Bloom, Sadun, and Van Reenen (2012) develop a model in which trust allows CEOs to delegate more often, allowing them to grow the firm more quickly. The authors conduct interviews with plant managers from thousands of manufacturing firms to determine the level of autonomy plant managers have (e. g., amount of capital investment, hiring decisions). Using regional trust data from the World Values Survey, they find that firms headquartered in high-trust regions are more likely to decentralize, and to have a larger firm size.

At the macro level, Guiso, Sapienza, and Zingales (2009) find that countries with higher levels of bilateral trust, as measured Eurobarometer surveys, also have higher levels of trade and investment. The same measure of bilateral trusts predicts venture capital investments across countries in Bottazzi, Da Rin, and Hellmann (2016). The authors develop a rational model in which high-trust investors are involved in more and higher-risk deals, and have lower success rates than low-trust investors. Empirically, they find that deals are more likely to occur when the VC and portfolio country have a high level of bilateral trust. Consistent with the theory, these deals result in fewer IPOs, fewer acquisitions, and more companies going out of business. Ahern, Daminelli, and Fracassi (2015) focus on the disparity in the perceptions of trust (and other cultural differences). Countries with more disparity, as measured by the World Values Survey, have fewer cross-country mergers and, when mergers do occur, they have lower announcement returns.

The breadth and sometimes contradictory results in this section imply not only a need for more data and better identified analyses, but also for a clearer theoretical foundation. What psychological mechanism do we have in mind when studying the effects of "connectedness"? Might some of the measures be too broad-brush to capture that a plausible (psychological) network effect, or depend on unmodeled circumstances? As the famous example of Zuckerberg and the Winklevoss brothers illustrates, a shared college experience can make friends and foes. Rather than pursuing a vaguely defined concept of "connectedness," the literature would benefit from a clear delineation of the psychological mechanisms, the existing psychological evidence used as the foundation, and the theoretical predictions, including their interaction with variations in the empirical setting. The above-mentioned study by Schmidt (2015) provides for a nice example. Schmidt delineates two specific roles of a board in mergers: monitoring and advice, and specifies that social connections between board members and a CEO lower monitoring but increase advice. Consistent with the theoretical framework, he demonstrates connectedness hurts in mergers where the need for monitoring is high (i. e., free cash-flows, low-powered CEO incentives, high CEO entrenchment) and helps in mergers where the need for advice is high (i. e., when the board member has industry knowledge the CEO does not).

5 Past and Future Developments, Open Questions, and Conclusion

In the previous sections, I have provided an overview of the field, introduced the organizational structure of the two main perspectives (biased investors and biased managers), argued for their internal consistency, and added a third perspective (biased third parties). As my review of the existing work is necessarily incomplete, I conclude the discussion with a topical overview of the published work in Behavioral Corporate Finance to date. To this end, I peruse the data I collected on all top publications in finance and identify the key topics in the Behavioral Corporate literature. I have grouped the literature into bins, such as "Investment and divestment," "Dividend and repurchases," and "Firm performance, firm value, and cost of capital." Table 10 reveals 15 different key topics and the count of papers that fall into the respective bin.[66]

As the table reveals, the topics most frequently addressed under "Perspective 1" papers, which focus on investor biases with managerial response, are financing decisions (e. g. debt-equity mix) and payout policy, followed by papers discussing the market timing of mergers and acquisitions. In contrast, "Perspective 2" papers, which focus on behavioral managers, most frequently cover investment, mergers and acquisitions, and firm performance. These differences in topical emphasis make intuitive sense. Firms' financing and payout policy are corporate decisions with a direct link to investors (so managers can exploit investor biases), while investment decisions are at the core of a manager's jobs (so managerial biases matter). Finally, in topics such as mergers and acquisitions, both perspectives matter, since the valuations of investors and managers, on both the bidder and target side, shape negotiation outcomes such as offer prices (so biases of multiple parties, including the interaction of these biases, are worth analyzing). Finally, some topics are barely affected by one of the perspectives, e. g., compensation or governance and CEO selection and turnover have little to do with investor biases.

Not all of the output distribution appears to follow from the nature of the topic, though. Some of the clustering might be path dependent. In the last-mentioned categories, for example, i. e., compensation, governance, CEO selection and turnover, behavioral biases among parties other than the manager and investors seem to be a promising avenue to pursue, but little has been published so far from the "Perspective 3" angle (with some exceptions in the realm of corporate governance). As individual data on board members, including compensation-committee and hiring-committee members becomes more easily available, filling these gaps seems like an obvious and promising path to pursue.

[66] Here, I allow for multiple topics to be assigned to one paper.

Table 10: Key Topics

This table provides information on the relative importance of key topics found in the literature, pooling together Finance and Economics journals.

	Category		
Topic	Perspective 1: Investor biases with managerial response	Perspective 2: Managerial biases, characterstics, and networks	Perspective 3: Biases and characteristics of other agents
Investment & Divestment	10	24	5
M&A	16	16	5
Innovation	1	1	1
Venture capital	-	1	3
Internal capital markets	-	5	-
IPO	12	5	-
Financial decisions, debt-equity mix, capital structure	37	14	2
Dividends and repurchases	18	3	-
Financial intermediation	4	3	5
Entrepreneurship	-	7	1
Compensation	1	15	2
Governance	-	9	5
CEO selection and turnover	-	6	2
Firm performance, firm value, cost of capital	6	30	3
Other	9	19	17

"Other" includes earnings management and ethical behavior, (corporate) culture, and fraud, as well as topics related to analysts, government, and society and workforce.

I conclude with a word of caution and advice for future researchers. To date, much of the field of behavioral corporate finance has been focused on demonstrating that corporate decisions correlate with "behavioral factors" outside of the standard model. This first wave of evidence has successfully demonstrated that managers, investors, and boards do not always act according to standard beliefs or preferences. This literature is also a step forward, relative to the early empirical work in behavioral *economics*, in that it considers heterogeneity in biases and often attempts to identify a measure of the individual extent of the bias which is then correlated with the non-standard behavior.

At the same time, some of the behavioral corporate research took a step back, relative to the early research in behavioral economics, in that a significant fraction has been model-free. Some of the model-free research has been essential in establishing robust stylized facts, altering our view of

financial decision-making, and uncovering the empirical determinants. But it is now time to think harder about the underlying determinants of the non-standard behavior and test model-based out-of-sample predictions.

To illustrate the argument with yet another example, consider the large literature on the role of weather and seasons on financial decision-making. Most of it has focused on asset-pricing and especially stock trading behavior.[67] Cortés, Duchin, and Sosyura (2016) were the first to demonstrate that also credit decisions are affected by weather: Loan officers approve mortgages at higher rates on sunnier days. They are willing to grant riskier loans and they reject fewer loans for subjective reasons. This evidence is important in that it challenges existing models on the functioning of credit markets. The question I want to pose here is: Can we go deeper and figure out why weather has this effect? Can we provide more direct evidence that weather affects risk-tolerance or subjective evaluations and, if so, describe the mechanisms? Does good weather make optimistic thoughts more vivid or present? Does it increase altruism towards loan applicants? Can we utilize details on the physiological effects of weather to predict which loan officers are likely to be affected, or which type of loans?

As a final example, consider the discussion about the determinants of CEO firing. Jenter and Kanaan (2015) convincingly show that CEOs are often fired for exogenous market reasons, even though these factors should not matter in standard models. The paper is important in that it questions the basic mechanisms underlying the literature on incentives, compensation, and promotion. However, do we understand the precise motivation for such behavior? For starters, a standard economic model with career concerns (of board members) and asymmetric information might suffice to explain the outcomes. Alternatively, the paper unveils evidence for a model of blame and overattribution of bad outcomes, possible a first piece of evidence on hindsight bias in Corporate Finance. These alternatives would be important progress in the literature, especially as they challenge the standard approach in moral hazard (principal agent) models in economics.

A more model-driven approach will also benefit from being much less susceptible to p-hacking, which is a serious topic of concern in the social sciences.[68] I would hope that most researchers would not engage in the most extreme forms, such as looking at the correlation between corporate decision X and 100 different, more or less plausible variables, and then focus on the emerging spurious significant relationships. But even well-intentioned researchers should be concerned. Rather than the scenario above, assume the 100 variables are instead the hypotheses of 100 different researchers. Moving towards more formal models of the mechanisms greatly reduces the likelihood of false

[67] Hirshleifer and Shumway (2003) find that daily market returns at exchanges across cities are higher on days with more sunshine. Goetzmann, Kim, Kumar, and Wang (2014) find that cloud coverage increases the likelihood that institutional investors perceive overpricing in the market, resulting in fewer buys.

[68] See, for example, Simmons, Nelson, and Simonsohn (2011) for a discussion on false-positives in the psychology literature and Camerer, Dreber, Forsell, Ho, Huber, Johannesson, Kirchler, Almenberg, Altmejd, Chan, et al. (2016) for a discussion on replicability of laboratory experiments in economics.

positives if researchers test a cohesive set of predictions.

By moving towards more concrete models of behavioral biases, I hope that future research will refine our understanding of the mechanisms driving corporate decision-making.

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Appendix A Supplementary Material on Quantification of Behavioral Corporate Finance Research

A.1 Identification of Relevant Research Areas

(i) Identification of papers in the area of Behavioral Finance

The first step is to identify all papers that fall into Behavioral Finance, defined as finance research where non-standard beliefs or preferences, or cognitive limitations are central to the analysis, using title and abstract. I use an array of keywords as indicators of a behavioral paper topic, including the words: non-standard, cater, exploit, timing, sentiment, fads, extrapolation, over-reaction, under-reaction, predictability, predict, bias, psychological, bounded rationality, overconfidence, optimism, experience, personal, CEO, CFO, and executive. Importantly, these words are necessary but not sufficient for the classification, as the following examples illustrate:

- Campbell and Thompson (2007), Predicting Excess Returns Out of Sample: Can Anything Beat the Historical Average?
  The title contains a keyword (predict) and, taking a look at the entire paper, it also contains the word bias and uses Baker and Wurgler (2000)'s equity share of new issues in the predictive regressions. However, bias does not refer to a psychological bias here, but instead to bias in the estimated coefficient. Moreover, Baker and Wurgler (2000)'s variable is only one of *many* variables included in the predictive regressions that had previously been proposed in the literature. In summary, this paper focuses on empirical asset pricing but does not take a behavioral perspective. It is therefore not included into the starting sample of (i).

- Barberis, Greenwood, Jin, and Shleifer (2015), X-CAPM: An Extrapolative Capital Asset Pricing Model
  The title contains the word extrapolative, and the abstract explains that they study a model in which "some investors form beliefs ... by extrapolating past price changes, while other investors hold fully rational beliefs." Thus, this paper falls into category (i).

- Jenter (2005), Market Timing and Managerial Portfolio Decisions
  The title specifically mentions market timing and the abstract states that "perceived mispricing seems an important determinant of managers' decision making" and that "managers try to actively time the market both in their private trades and in firm-level decisions." Hence, this paper clearly meets the criteria to be included in (i).

- Dittmann and Maug (2007), Lower Salaries and No Options? On the Optimal Structure of Executive Pay
  This paper contains the keyword executive in the title. While it focuses on CEOs and their compensation, it "calibrate[s] the standard principal-agent model with constant relative risk aversion ... to a sample of 598 U.S. CEOs." Hence, it does not take a behavioral perspective and is not included under (i).

- Dittmann, Maug, and Spalt (2010), Sticks or Carrots? Optimal CEO Compensation when Managers are Loss Averse
  This paper also focuses on executive compensation, and contains the keyword CEO in its title. In contrast to the paper above, it focuses on the case when managers are loss averse

and have reference-point-dependent preferences. Hence, there is a key behavioral topic at the center, and the paper is included in (i).

(ii) Identification of papers in the area of Behavioral Corporate Finance

Next, I take all papers classified as Behavioral Finance under (i) and ask whether they belong in the realm of Behavioral Corporate Finance. A key criterion for this decision is whether there is a clear corporate interaction, i. e., a focus on agents that are relevant to Corporate Finance (see Figures 1 and 2), such as managers, investors, boards, analysts, or banks. I use the papers which were discussed above and included in (i) as illustrative examples for this step.

- Barberis, Greenwood, Jin, and Shleifer (2015): This paper studies an asset pricing model with extrapolative agents. Clearly, there is no corporate interaction, since the paper's focus is not on firms exploiting investors' biased expectations and beliefs. Hence, the paper is classified as "Other behavioral finance" and not as Behavioral Corporate Finance.

- Jenter (2005): This paper has market timing and perceived misvaluation at its core, so it focuses on the interaction between firms and investors from a behavioral perspective. Hence, it is classified as a Behavioral Corporate Finance paper.

- Dittmann, Maug, and Spalt (2010): Since the paper focuses on executives, a key party in Corporate Finance, and makes use of central behavioral concepts (reference dependence, loss aversion), it is classified as a Behavioral Corporate Finance paper.

(iii) Classification of papers into research on Perspective 1, Perspective 2, or Perspective 3

Finally, I take all papers classified under (ii) as Behavioral Corporate Finance papers, and categorize them as Perspective 1 (Investor biases with managerial response), Perspective 2 (Managerial biases, characteristics, and networks), or Perspective 3 (Biases and characteristics of other agents) papers. Here, I explain the categorization for the two remaining papers from above.

- Jenter (2005): Market timing and perceived misvaluation are key elements for Perspective 1 research. Jenter (2005) contributes to that stream by showing that managers take market timing into account for their personal portfolio decisions. Therefore, the paper falls under Perspective 1.

- Dittmann, Maug, and Spalt (2010): Clearly, the focus here is on non-standard CEO preferences. Consequently, the paper falls under Perspective 2.

(iv) Seemingly relevant research areas

Below is a complete list of research areas that are seemingly related to Behavioral Corporate Finance, but whose findings are not rooted in investor or managerial psychology upon closer inspection.

- *Catering to "rational heterogeneity"*: I do not consider papers that explore catering to investor needs if these needs are explained by rational motives. Example: Guibaud, Nosbusch, and Vayanos (2013), who analyze optimal government debt maturity structure in the presence of overlapping generations representing different investor clienteles.

- *Earnings management*: There is a literature that unveils the ways in which firms opportunistically manage components of reported earnings around corporate actions such as equity issuances and buybacks and M&A transactions. I do not include these papers unless a specific paper analyzes earnings management in the context of investor inattention, overreaction to news, etc. Example: The paper by DuCharme, Malatesta, and Sefcik (2004) is not included, since they focus on managers' incentives to inflate earnings to maximize proceeds from new issues, not on the vulnerability of investors resulting from biases and bounded rationality.

- *"Attention management"*: I include papers that explore news manipulation by firms if this is motivated by the idea that firms exploit investor (in)attention or other biases. An example is DellaVigna and Pollet (2009), who find that investors underreact to earnings announcements on Friday. I disregard papers which analyze communication flows between managers and investors in the context of agency considerations and information asymmetries. An example is the cheap talk paper by Almazan, Banerji, and Motta (2008).

- *Managerial risk-taking incentives*: While these papers explore how managers shape their companies, managerial actions are viewed and modeled as a rational response to particular components of executive compensation (pay-performance sensitivity). Example: Coles, Daniel, and Naveen (2006), who investigate the effect of managerial risk-taking incentives on various corporate policies, such as investment and debt policy.

- *Managerial ability*: Some papers introduce models in which there is heterogeneity in CEO ability. I do not include these papers unless a specific paper links managerial ability to personal experiences, social networks, etc. Example: Taylor (2010), who studies forced CEO turnover and models firm profitability as a mean-reverting process around the CEO's ability level α_{CEO}.

- *Managerial entrenchment, tunneling, and free-riding*: As above, these managerial actions affect firm outcomes. However, such behaviors are attributable to agency problems, not behavioral biases or personal preferences for certain firm policies. Example: Cronqvist, Heyman, Nilsson, Svaleryd, and Vlachos (2009), who find evidence that entrenched CEOs increase their employees' pay.

- *Managerial myopia*: Myopia is usually viewed as resulting from short-term incentives (e. g. reputation and career concerns or pay structure), not from a manager's present bias. Example: Edmans (2009), who studies the interplay of managerial myopia and blockholder trading.

- *Peer effects and herding*: There is a literature that explores how and why corporate policies are shaped by the decisions of peer firms. I include papers who attribute the response to peer firm behavior to managerial irrationality (e. g. over-reaction). An example of such a paper is Kaustia and Randala (2015), since they interpret a firm's tendency to follow peer firms in splitting their stock as managers "mistaking noise for a signal." I disregard papers that view peer effects exclusively as a social learning construct or a rational response to competitors' actions. An example is the paper by Foucault and Fresard (2014), in which managers gauge investment opportunities from peer firms' valuations.

- *Political connections*: I include papers if they explore the *personal* ties that managers have with politicians (through, e. g., previous attendance at the same university). An example of such a paper is Faccio, Masulis, and McConnell (2006). In contrast, I do not consider papers if they focus on general connections between firms and the political community (e. g., lobbying or donations). An example is Cooper, Gulen, and Ovtchinnikov (2010).

- *Analyst optimism*: There is a literature that documents that analysts systematically issue overly optimistic forecasts. I only consider papers that attribute this over-optimism to psychological factors. I exclude papers who explain the issuance of optimistic forecasts with career or reputational concerns, as it is the case in Hong and Kubik (2003).

A.2 Quantification of Papers by Field and Journal

The following panels show the year-by-year number of finance papers published in the specified journal(s) under the denoted categories. The categories in the top graph of each panel are the three main perspectives of research in Behavioral Corporate Finance as delineated in Sections 2.2-2.4. The middle graph in each panel adds the general Behavioral Finance category, and the bottom graph also includes the total research output in Finance.

Figure A.2.1: Research in Behavioral Corporate Finance

Finance journals (JF, RFS, and JFE)

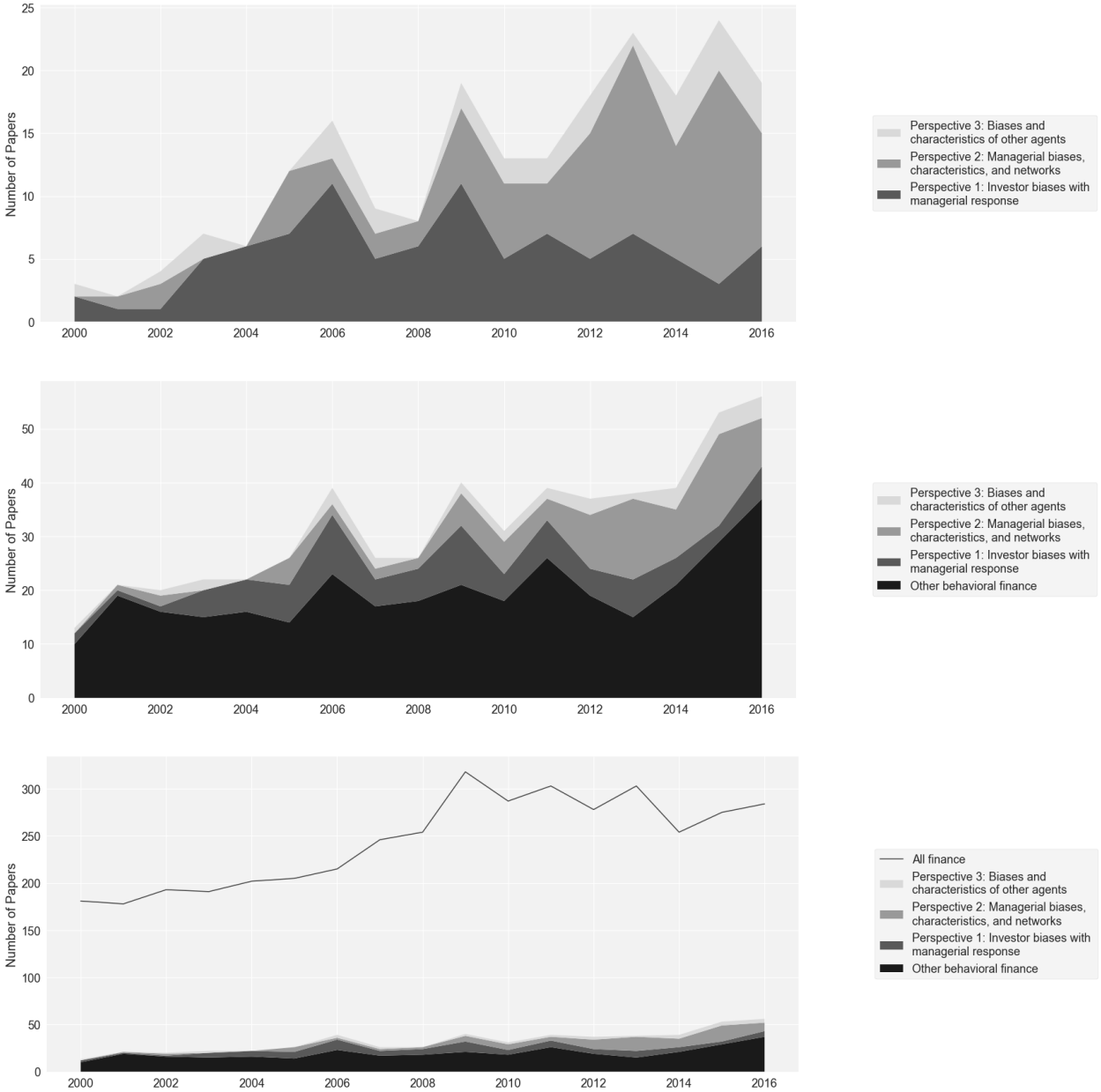


Figure A.2.2: Research in Behavioral Corporate Finance

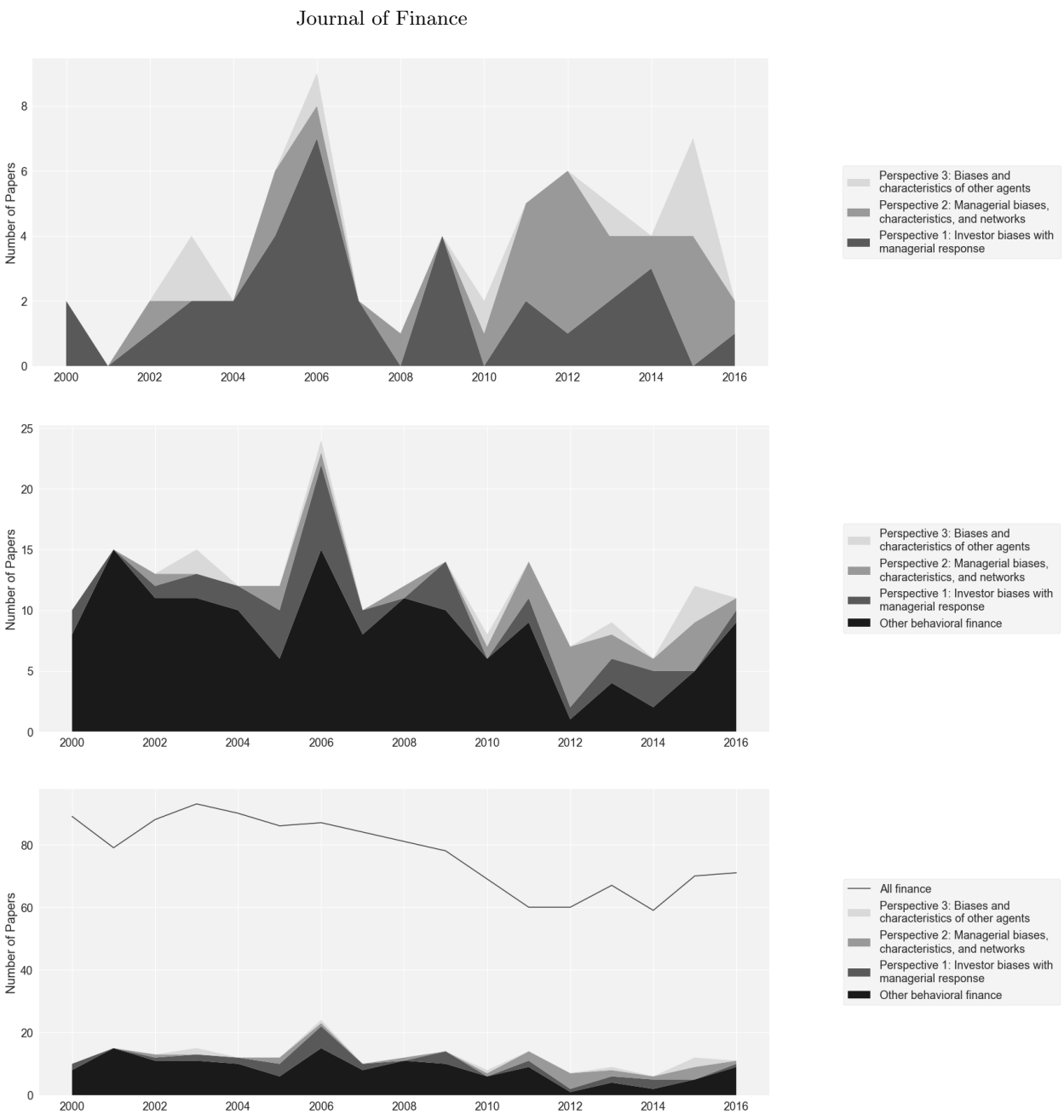


Figure A.2.3: Research in Behavioral Corporate Finance

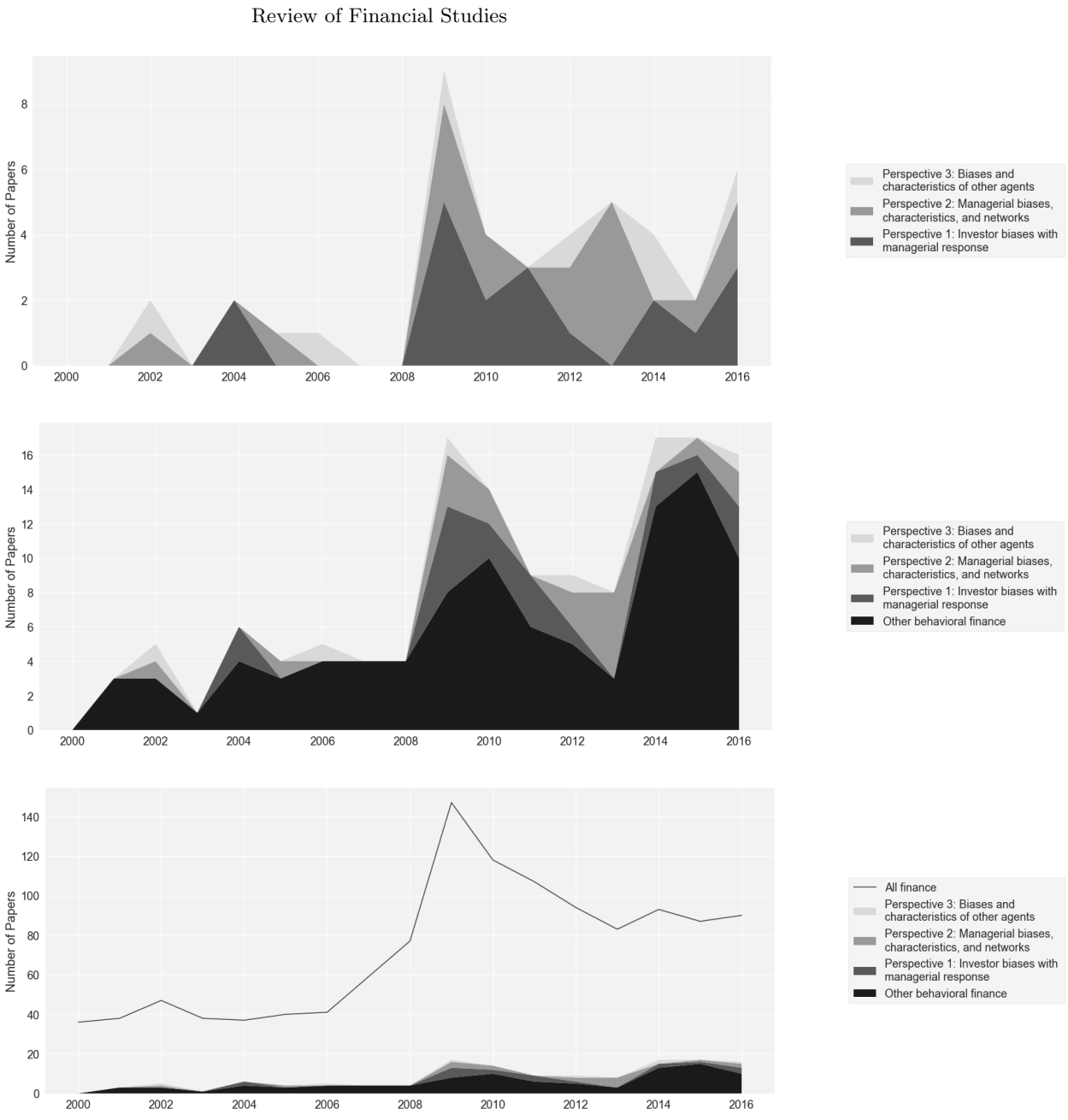
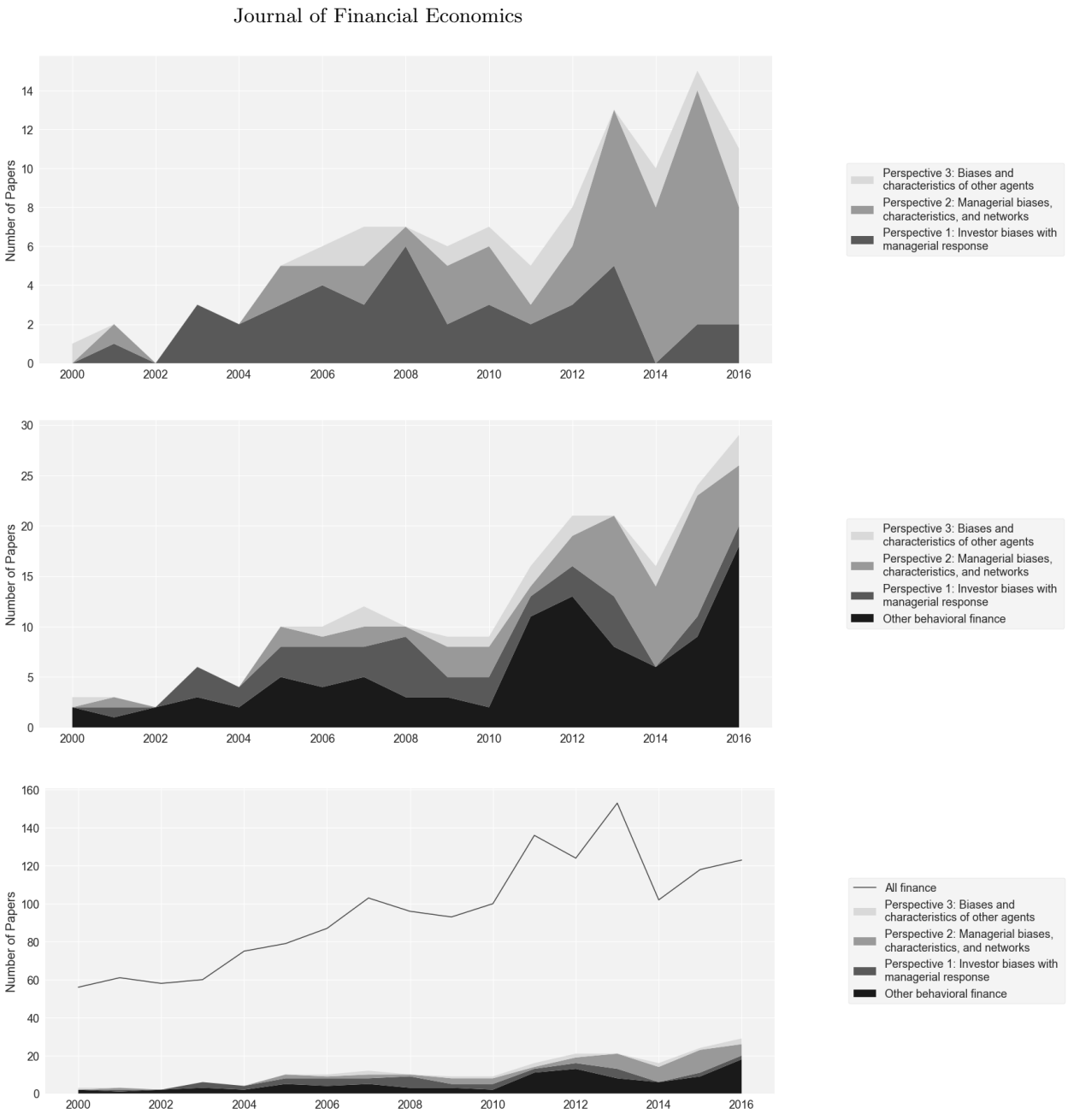


Figure A.2.4: Research in Behavioral Corporate Finance



A.3 Detailed Summary Statistics

Table A.3.1: Detailed Summary Statistics

Panel A: All papers							
Category		Year of publication			Citations		
	N	median	first	last	total	mean	median
Perspective 1: Investor biases with managerial response	95	2009	2000	2016	34,331	361	182
Perspective 2:							
a) Managerial biases	41	2012	2001	2016	18,784	458	107
b) Managerial characteristics	39	2013	2003	2016	10,649	273	104
c) Managerial networks	22	2012.5	2006	2016	8,000	364	139
Perspective 3:							
a) Biases of other agents	16	2008	2000	2016	4,702	294	227
b) Characteristics of other agents	20	2012	2007	2016	5,508	275	163
Total	233						

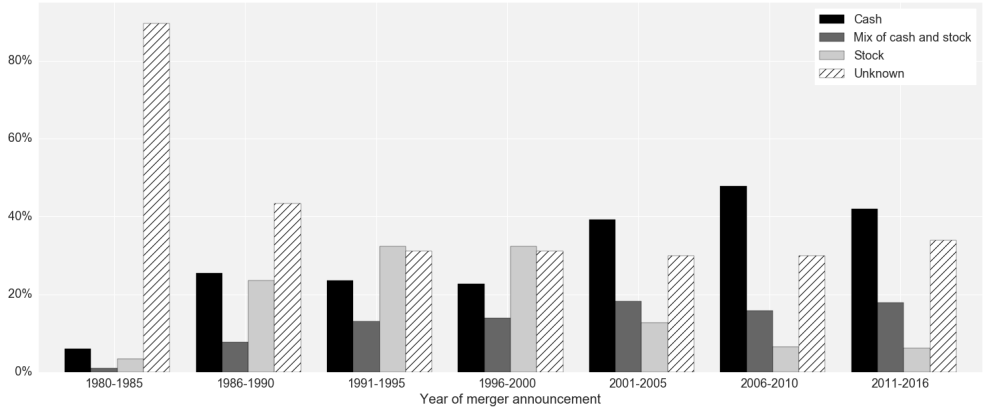
Panel B: Papers published since 2010							
Category		Year of publication			Citations		
	N	median	first	last	total	mean	median
Perspective 1: Investor biases with managerial response	38	2013	2010	2016	3,705	98	57
Perspective 2:							
a) Managerial biases	29	2013	2010	2016	3,746	129	69
b) Managerial characteristics	30	2014	2010	2016	3,405	114	68
c) Managerial networks	16	2013	2010	2016	1,907	119	104
Perspective 3:							
a) Biases of other agents	7	2014	2013	2016	805	115	75
b) Characteristics of other agents	16	2012.5	2010	2016	2,366	148	120
Total	136						

Appendix B Supplementary Material on Theory and Empirics of Mergers and Acquisitions

B.1 Additional Figures on Stylized Facts on M&A

Figure B.1.1: Payment method.

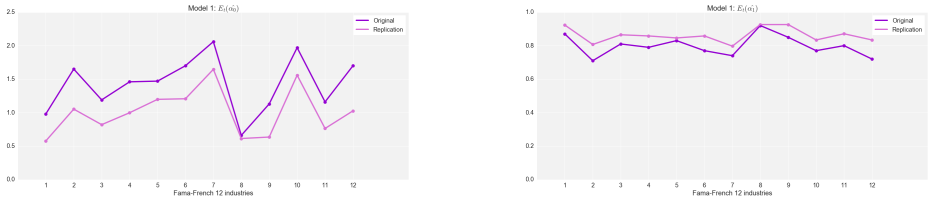
This plot shows the popularity of different payment methods between 1980 and 2016. Payment methods include cash (black), mixed (dark gray), stock (light gray), and other or unknown (white with hatches). Source: SDC Mergers and Acquisitions Database, data retrieved in March 2017.



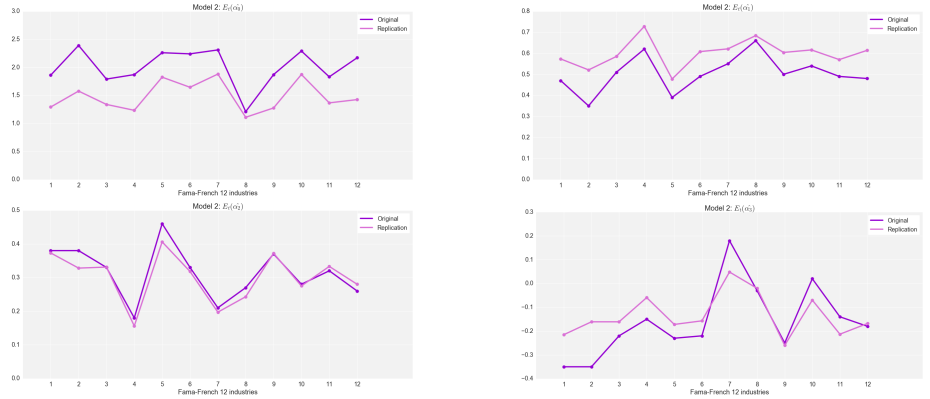
B.2 Additional Figures and Tables on Model and Empirics of Merger Example

Figure B.2.1: Conditional Regression Multiples (Replication)

This figure visualizes the estimated regression multiples (light violet) used in the decomposition of the market-to-book ratio and compares them to those in Table 4 of Rhodes-Kropf, Robinson, and Viswanathan (2005) (dark purple). Each figure corresponds to one multiple. Omitting subscripts, m denotes the natural log of market value, b the natural log of book value, ni^+ the log of the absolute value of net income, $I_{(<0)}$ is an indicator for negative net income, and Lev is leverage.



(a) Model 1: $m_{it} = \alpha_{0jt} + \alpha_{1jt} b_{it} + \varepsilon_{it}$

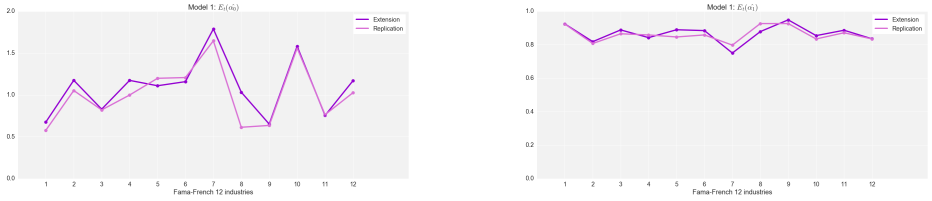


(b) Model 2: $m_{it} = \alpha_{0jt} + \alpha_{1jt} b_{it} + \alpha_{2jt} ni^+_{it} + \alpha_{3jt} \left(I_{(<0)} ni^+\right)_{it} + \varepsilon_{it}$

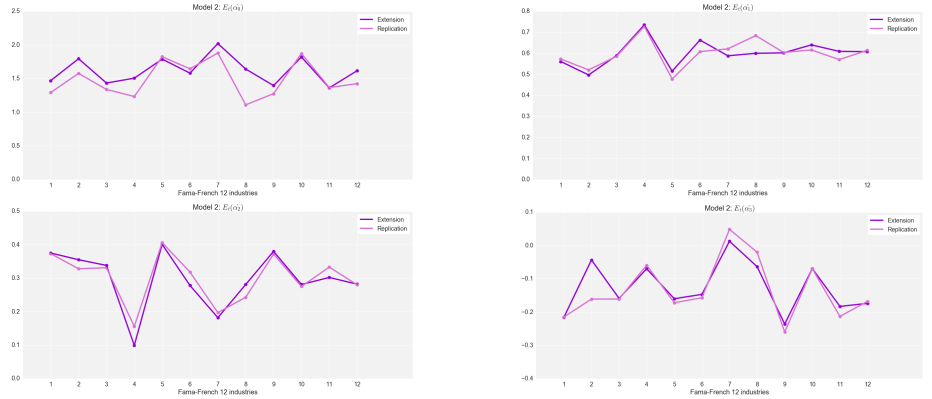
(c) Model 3: $m_{it} = \alpha_{0jt} + \alpha_{1jt} b_{it} + \alpha_{2jt} ni^+_{it} + \alpha_{3jt} \left(I_{(<0)} ni^+\right)_{it} + \alpha_{4jt} Lev_{it} + \varepsilon_{it}$

Figure B.2.2: Conditional Regression Multiples (Extension)

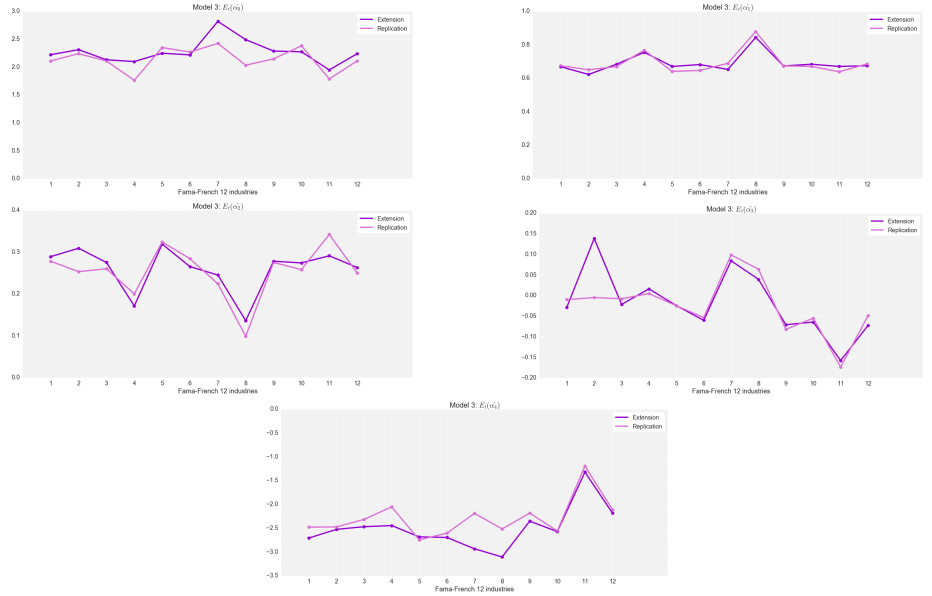
This figure visualizes the regression multiples estimated for the extended sample period ending 12/2016 (dark violet) used in the decomposition of the market-to-book ratio and compares them to those estimated using the original sample period spanning 1977 to 2000. Each figure corresponds to one multiple. Omitting subscripts, m denotes the natural log of market value, b the natural log of book value, ni^+ the log of the absolute value of net income, $I_{(<0)}$ is an indicator for negative net income, and Lev is leverage.



(a) Model 1: $m_{it} = \alpha_{0jt} + \alpha_{1jt} b_{it} + \varepsilon_{it}$



(b) Model 2: $m_{it} = \alpha_{0jt} + \alpha_{1jt} b_{it} + \alpha_{2jt} ni^+_{it} + \alpha_{3jt} \left(I_{(<0)} ni^+\right)_{it} + \varepsilon_{it}$



(c) Model 3: $m_{it} = \alpha_{0jt} + \alpha_{1jt} b_{it} + \alpha_{2jt} ni^+_{it} + \alpha_{3jt} \left(I_{(<0)} ni^+\right)_{it} + \alpha_{4jt} Lev_{it} + \varepsilon_{it}$

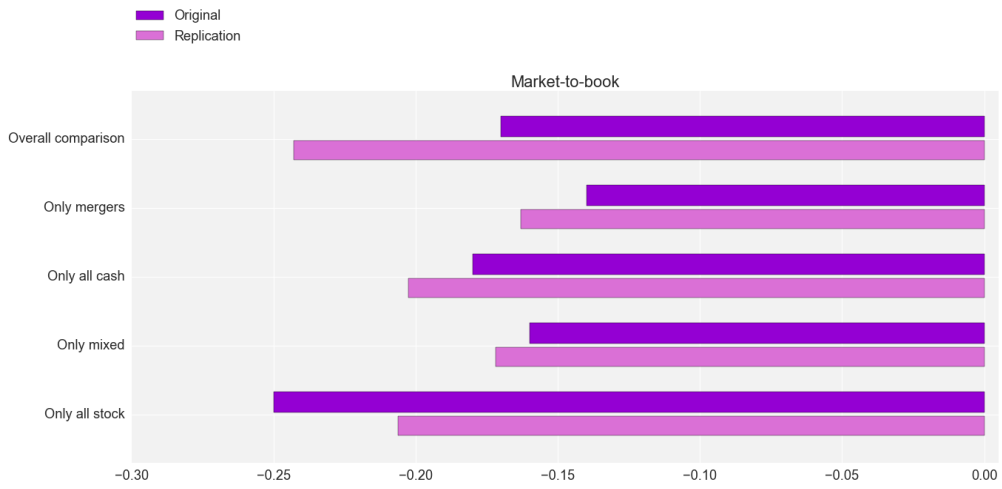
Table B.2.1: Decomposition of Market-to-Book Ratio (Replication)

This table replicates Table 6 in Rhodes-Kropf, Robinson, and Viswanathan (2005) for the original sample period spanning the years 1977 to 2000. The estimation is based on 130,403 nonmerger firm-level observations and 8,196 (completed) merger observations in which participating firms are included in CRSP and Compustat. The data contain 902 all-cash deals, 1,673 all-stock deals, and 360 mixed deals. See Rhodes-Kropf, Robinson, and Viswanathan (2005) and Figure B.2.1 for a more detailed description of the three models used for the firm-level decomposition of the market-to-book ratio.

Valuation component	Overall comparison			Only mergers			Only all cash			Only mixed			Only all stock		
	Nonmerger	Merger	t(diff)	Tar.	Acq.	t(diff)	Tar.	Acq.	t(diff)	Tar.	Acq.	t(diff)	Tar.	Acq.	t(diff)
Market-to-book (log)	0.47	0.71	-19.25	0.63	0.79	-8.66	0.49	0.69	-5.69	0.56	0.73	-2.79	0.77	0.98	-6.81
Model 1:															
Firm-specific error	-0.02	0.31	-28.26	0.09	0.51	-24.94	-0.07	0.45	-15.54	0.11	0.45	-6.11	0.18	0.62	-16.57
Time-series sector error	0.03	0.12	-28.50	0.12	0.12	-0.89	0.11	0.14	-2.22	0.11	0.11	-0.09	0.17	0.18	-1.69
Long-run value to book	0.46	0.29	38.36	0.42	0.16	33.98	0.45	0.10	23.55	0.34	0.17	6.63	0.42	0.18	19.55
Model 2:															
Firm-specific error	-0.01	0.26	-25.98	0.10	0.42	-20.25	-0.02	0.33	-11.73	0.10	0.38	-5.42	0.19	0.53	-13.52
Time-series sector error	0.04	0.14	-28.47	0.13	0.14	-1.64	0.13	0.16	-2.71	0.14	0.14	-0.20	0.18	0.20	-1.73
Long-run value to book	0.45	0.31	22.62	0.40	0.23	16.23	0.39	0.20	8.85	0.32	0.21	3.08	0.40	0.25	9.69
Model 3:															
Firm-specific error	-0.01	0.22	-22.50	0.10	0.33	-14.08	-0.01	0.32	-11.00	0.17	0.27	-1.80	0.08	0.38	-10.04
Time-series sector error	0.02	0.08	-20.48	0.06	0.09	-4.87	0.06	0.11	-4.61	0.07	0.10	-1.71	0.09	0.11	-2.12
Long-run value to book	0.49	0.50	-1.47	0.54	0.46	4.84	0.48	0.34	4.29	0.43	0.45	-0.37	0.75	0.68	2.59

Figure B.2.3: Replication Results (Market-to-Book Ratio)

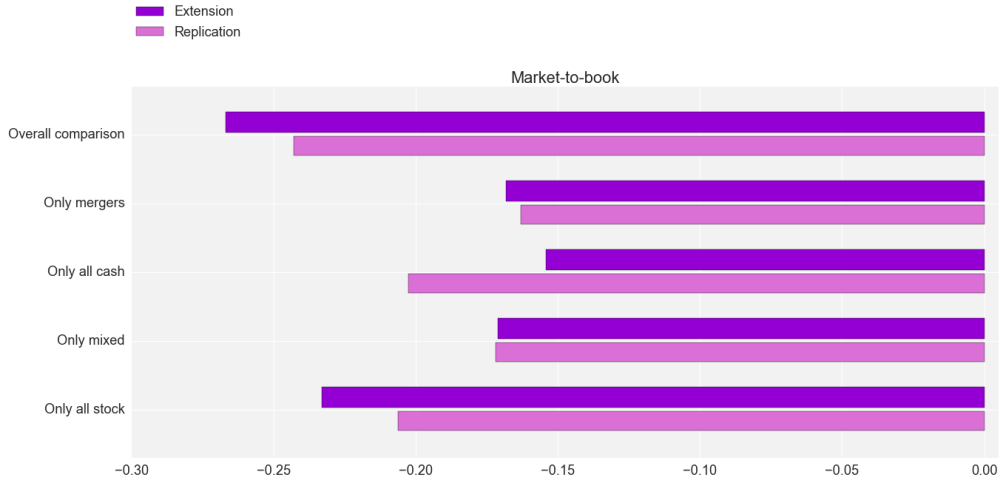
This figure visualizes the difference in means of market-to-book ratio (in logs) across sub-groups presented in Table B.2.1. For the overall analysis, sub-groups are nonmerger and merger firms. For the analysis involving mergers only, sub-groups are target and acquirer firms. The light violet bars present the results shown in Table B.2.1, the dark violet bars those of the original study for comparison.



Market-to-book: $m_{it} - b_{it}$

Figure B.2.4: Extension Results (Market-to-Book Ratio)

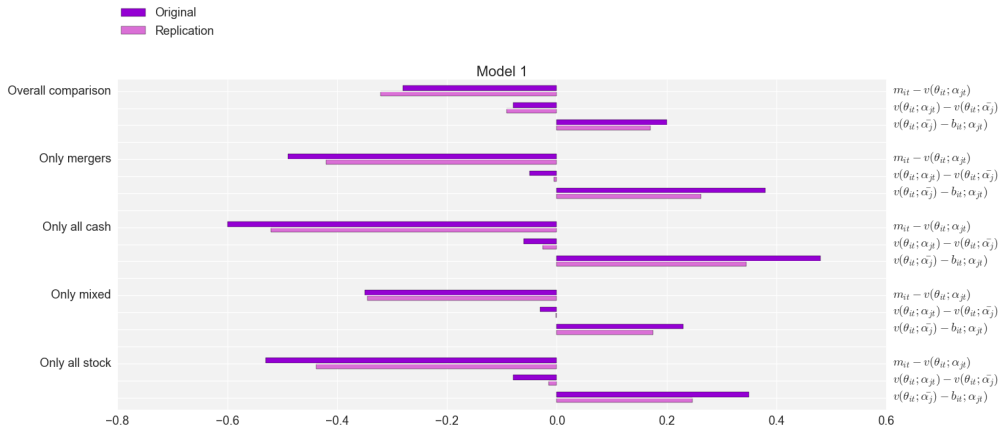
This figure visualizes the difference in means of market-to-book ratio (in logs) across sub-groups presented in Table 5. For the overall analysis, sub-groups are nonmerger and merger firms. For the analysis involving mergers only, sub-groups are target and acquirer firms. The dark violet bars present the results shown in Table 5, the light violet bars those of the replication presented in Table B.2.1.



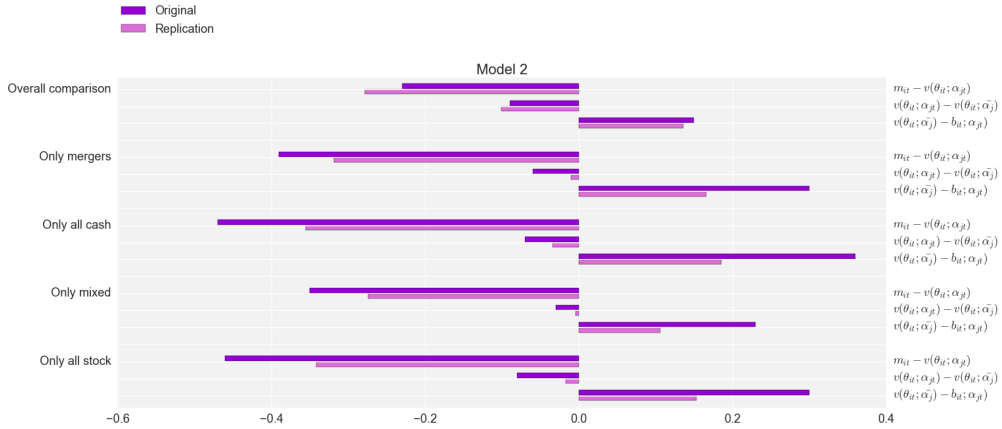
Market-to-book: $m_{it} - b_{it}$

Figure B.2.5: Replication Results (Market-to-Book Ratio Decomposition)

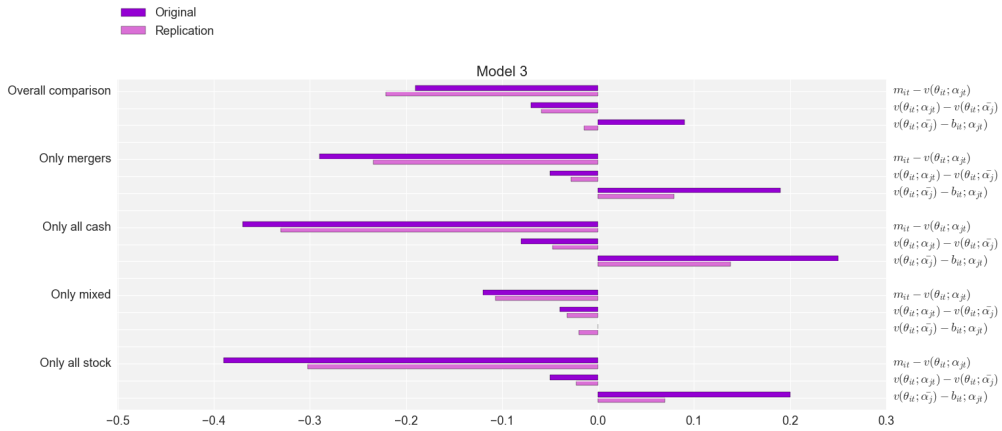
This figure visualizes the difference in means of the individual valuation components across sub-groups presented in Table B.2.1. See Rhodes-Kropf, Robinson, and Viswanathan (2005) and Figure B.2.1 for a more detailed description.



(a) Model 1: $m_{it} = \alpha_{0jt} + \alpha_{1jt} b_{it} + \varepsilon_{it}$



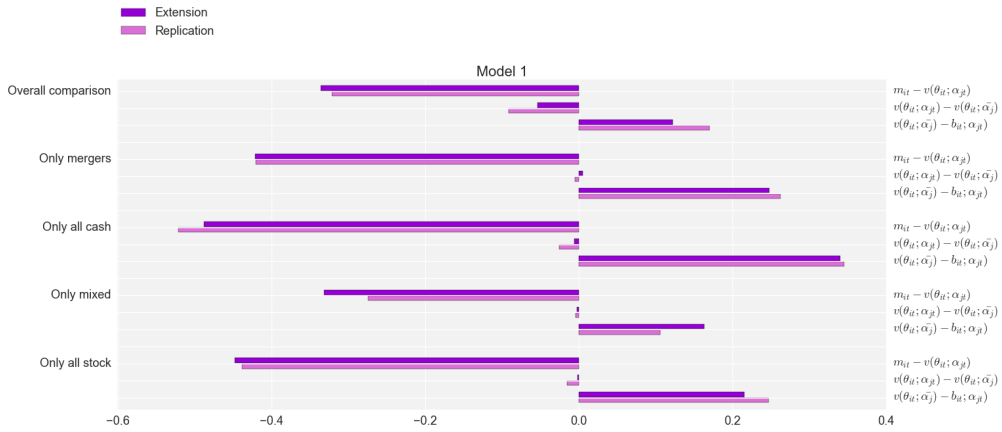
(b) Model 2: $m_{it} = \alpha_{0jt} + \alpha_{1jt} b_{it} + \alpha_{2jt} ni^{+}_{it} + \alpha_{3jt} \left(I_{(<0)} ni^{+} \right)_{it} + \varepsilon_{it}$



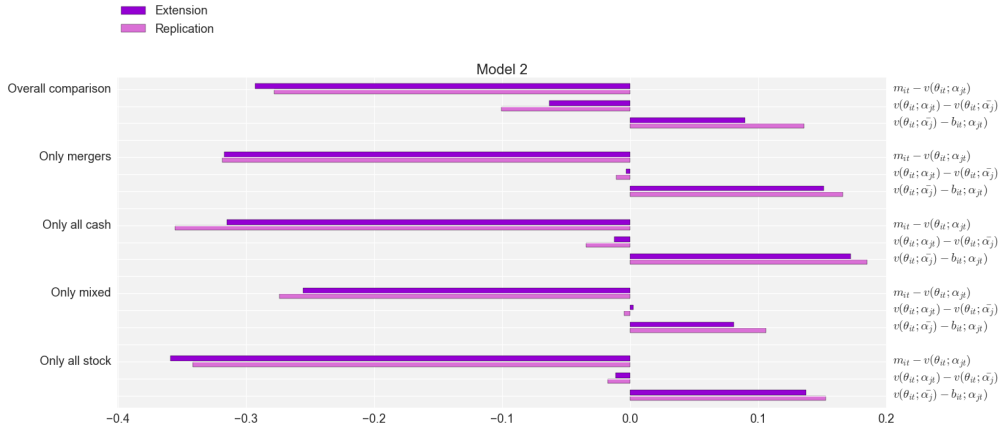
(c) Model 3: $m_{it} = \alpha_{0jt} + \alpha_{1jt} b_{it} + \alpha_{2jt} ni^{+}_{it} + \alpha_{3jt} \left(I_{(<0)} ni^{+} \right)_{it} + \alpha_{4jt} Lev_{it} + \varepsilon_{it}$

Figure B.2.6: Extension Results (Market-to-Book Ratio Decomposition)

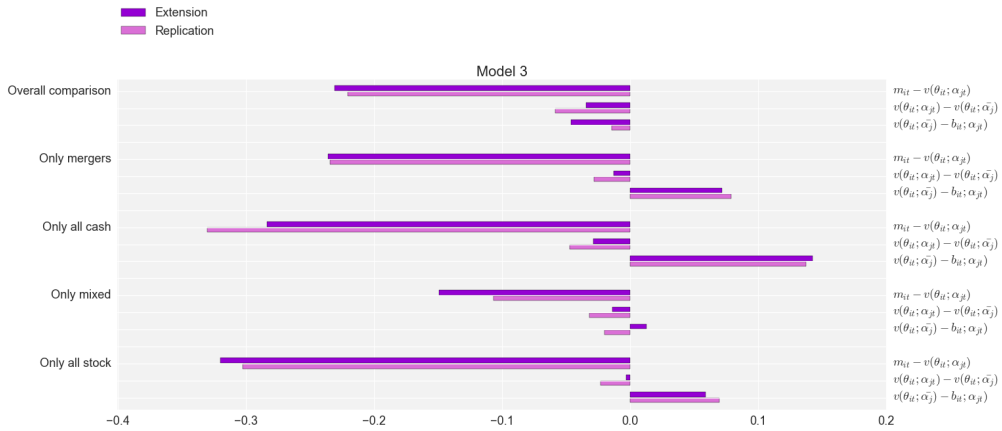
This figure visualizes the difference in means of market-to-book ratio (in logs) across sub-groups presented in Table 5. See Rhodes-Kropf, Robinson, and Viswanathan (2005) and Figure B.2.2 for a more detailed description.



(a) Model 1: $m_{it} = \alpha_{0jt} + \alpha_{1jt} b_{it} + \varepsilon_{it}$



(b) Model 2: $m_{it} = \alpha_{0jt} + \alpha_{1jt} b_{it} + \alpha_{2jt} ni^{+}_{it} + \alpha_{3jt} \left(I_{(<0)} ni^{+} \right)_{it} + \varepsilon_{it}$



(c) Model 3: $m_{it} = \alpha_{0jt} + \alpha_{1jt} b_{it} + \alpha_{2jt} ni^{+}_{it} + \alpha_{3jt} \left(I_{(<0)} ni^{+} \right)_{it} + \alpha_{4jt} Lev_{it} + \varepsilon_{it}$