



## Preface

## Corporate finance theory: Introduction to special issue

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## ABSTRACT

Theoretical research in corporate finance is critical for our understanding of real-world phenomena, for interpreting empirical results, and for deriving policy implications. We discuss the benefits and limitations of research in corporate finance theory and link them to the nine articles in this special issue on “Corporate Finance Theory.” We provide a perspective on the nine articles in this special issue, and outline our perception of how future research may evolve. We also review several themes that emerge out of the articles, which we think deserve more attention from theorists going forward: interactions between financial markets and corporate finance and dynamic models of corporate decisions, such as capital structure and managerial compensation.

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## 1. Introduction

Modern research in economics and finance can be generally classified into one of two categories: empirical research and theoretical research. In empirical research, researchers analyze data to understand actual connections between different variables, and try to reach conclusions on underlying mechanisms in the real world. The tests and conclusions are typically guided by economic intuition, which in turn is directly or indirectly provided by prior theoretical research. In theoretical research, researchers build mathematical models, which are meant to be simplified versions of the real world, and analyze them to understand potential mechanisms that may be operating in the real world. That is, these mechanisms may be the driving forces behind observable variables in the data. <sup>1</sup>

Over the last two decades or so, the volume of empirical research has grown significantly relative to that of theoretical research. This is perhaps not so surprising. The proliferation of new datasets has made it possible to analyze things empirically, which researchers could only dream of before. Moreover, a growing awareness to empirical methods and understanding of identification techniques led researchers to write many papers taking a more careful look at the data to understand causal mechanisms rather than just correlations. Finally, a common view is that there are already many theories out there and it will take a long time for empiricists to test them all, and so it is justified to have a large volume of empirical research relative to theoretical research for empiricists to catch up with the existing theories.

Corporate finance, which is an important field of economics and finance, is part of this trend towards relatively more empirical research, and perhaps represents it even more strongly than other fields of economics and finance. As researchers in corporate finance, we see many more empirical papers than theoretical ones presented in conferences and published in journals. Many departments are composed primarily of empirical researchers, and as a result many Ph.D. students are trained primarily to do empirical research. It is thus possible that the share of theoretical research in corporate finance will decrease even further over time.

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The two of us have done both empirical and theoretical work in corporate finance, but have a strong tilt to theory. In our view, the path of research in corporate finance going forward needs to exhibit more balance between empirical work and theoretical work. This does not imply that the volumes of the two types of research have to be equal. It may well be that in a steady-state equilibrium there is room for more empirical research than there is for theoretical research. Yet, we think it is important to make sure that the share of theory in overall research in corporate finance does not decline further; in fact, it should probably increase relative to where it is today. For these reasons, we were very happy for the opportunity to edit this special issue of the *Journal of Corporate Finance*, which is dedicated fully to “Corporate Finance Theory.”

Why is it important to continue and explore theories in corporate finance further instead of devoting all resources to data analysis and empirical work? The main reason is that empirical work, which is not guided by theory, can be largely uninformative about the underlying mechanisms, because simple intuitions and ad-hoc hypotheses can be very misleading. Good theories are critical to help us understand the patterns we see in the data, and without them we might come to very adverse conclusions and policy implications. After all, without any good theories we would not be where we are today in the field of corporate finance.

Take, for example, one paper that appears in this special issue “The Prevention of Excess Managerial Risk Taking.” The authors of this paper provide an explanation rooted in optimal contracts for why severance pay is so pervasive in the real world. Another example is “Stock-Based Managerial Compensation, Price Informativeness, and the Incentive to Overinvest,” where the author provides an explanation, based on optimal compensation contracts and learning from prices, for the tendency of corporations to overinvest.<sup>2</sup> Based on data alone, many have observed such phenomena and thought that they indicate the presence of corruption or managerial empire building, and so suggesting the need for tighter regulation to control managerial behavior and prevent managers from capturing value at the expense of small shareholders. Theoretical papers of the kind mentioned here are thus important in telling us that there may be something else going on behind these phenomena and maybe there is no need for a policy response or the policy response should be completely different.

Of course, a skeptic may ask how important these observations are. After all, one can come up with many potential models that will explain and rationalize observed phenomena. Some of these models might be completely implausible. This point is made very strongly in a recent paper by [Pfleiderer \(2014\)](#). He argues that not all models rationalizing a given phenomenon actually contribute to our understanding of it. Models should be judged to a large extent on the reasonableness of their assumptions and whether they fit basic premises we have about the real world. We indeed agree with this point. One has to be careful when writing a model that claims to explain phenomena like severance pay or overinvestment, as there is a high likelihood that these phenomena do stem from bad managerial behavior that needs to be discouraged by appropriate regulation. Only models that rationalize such behavior based on reasonable assumptions should be taken seriously in the overall debate.

Indeed, the two models mentioned above strike us as bringing something useful to the table. They are based on forces like curtailing excessive managerial risk taking and basing managerial contracts on informative prices, which we think are important and should be explored further in the form of new theoretical models so that we better understand their implications and their ability to explain observed real-world phenomena. It is this kind of theory work that we hope will be produced more in the profession, and we hope that the papers in this special issue, carefully selected in a competitive review and editorial process, bring the flavor of such theory work. We do not argue that any such model should be taken immediately to shape public policy. There needs to be a careful examination via a long string of papers and follow-up papers before researchers and policy-makers develop a good understanding of what is important and what should shape policy. This is why the continuous active production of work in corporate finance theory is important.

Furthermore, theoretical research is sometimes necessary in cases where data is simply not available, and so the only answers we can obtain for important questions are based on careful analysis of theoretical models. Many such examples have emerged in the recent financial crisis. The events that happened and the policy responses considered for them were so new and unusual that one could not rely on empirical analysis to help policymakers decide what should be done. One of the papers in this special issue “Bank stability and market discipline: The effect of contingent capital on risk taking and default probability” provides a great example. One proposed policy response to the crisis is to have banks hold contingent capital: debt that automatically converts to equity in bad states. There is a lot of debate on whether this will work and some of the operational challenges around the implementation of contingent capital. Unfortunately, there is not much data to help us make the judgment. The paper published here is one of the first theoretical analyses of this important problem. Hence, one needs extensive theoretical research to analyze the various implications and, in addition to complementing empirical research, theoretical research may at times offer the only way to uncover mechanisms that help us better understand the real world.

Another question that a skeptic may ask is why we need so many models. There are already many existing models, which are hard to test. Is it possible that we already have enough material from the theory side and we need to devote more attention to the empirical side? We disagree with this view. We think that our understanding of the theories of corporate finance is still at a very early stage. There is still a lot that we do not know and do not understand. There are still many plausible settings and assumptions that have not been explored. We agree with the arguments made by [Caballero \(2010\)](#) and think that a lot of what he says in the context of macroeconomics also applies to corporate finance. He uses the recent financial crisis to argue that the basic paradigm in macroeconomics is not capable of explaining important real world events. Hence, he says, people should think of research as being more exploratory, where different directions are attempted, so that we have better success going forward converging on a track that is closer to what we see in the real world. While corporate finance is a very different field, there are still basic issues that we do not understand. The crisis

<sup>2</sup> We provide a more detailed description of all the papers in this special issue below.

indeed exposed how real world events can surprise researchers and bring them to believe that new models have to be explored so that we understand various mechanisms. This is certainly true for corporate finance.

The special issue provided here is of course limited in scope and cannot cover all the range of topics and modeling approaches that one would like to see progress on in corporate finance theory. Still, certain themes come out of the papers in this special issue that we think deserve more attention by theorists going forward. These include the interaction between financial markets and corporate finance – in particular how information in prices interacts and guides corporate decisions – and dynamic models of corporate decisions such as capital structure and managerial compensation. A unifying feature of both these themes is that they link motives from corporate finance with motives from asset pricing and market microstructure research. Such interactions are important in our view for the future of corporate finance research.

The first theme is based on the idea that market prices are efficient in aggregating information from different market participants. This idea goes back to Hayek (1945). Indeed, one of the main interests in the finance literature is in the efficiency of financial markets and the degree to which prices are informative about firms' values and cash flows. Linking this to corporate finance, one would expect that if market prices are doing such a good job at aggregating and providing information, then such information should be useful for decisions in the real side of the economy, such as those made by managers, capital providers, directors, or for contracts set to affect the behavior of such decision makers. As a result there is a feedback loop between the financial markets and the real economy, whereby prices both reflect and affect firms' cash flows and values. The literature that features such a feedback loop has recently developed empirically and theoretically, and was reviewed by Bond et al. (2012). It shows that considering the feedback effect explicitly generates many new implications concerning the price formation process and the determination of firms' investments and other decisions. Two papers contributing to this literature appear in this special issue.

Concerning the second theme, a number of articles in this special issue cover recent research that explores dynamic corporate finance models. This class of models resides at the intersection of asset pricing and corporate finance, given that the contingent claims approach to valuation in asset pricing provides some important and useful tools for dynamic corporate finance.<sup>3</sup> That is, using valuation tools from continuous-time asset pricing has enabled researchers to study a myriad of corporate finance questions in interesting and new ways (when, for example, compared to two-period and two-state models). In part these models' remarkable success is attributable to the fact that much of corporate finance theory centers on firms maximizing (equity) value to make decisions, such as whether and also when to declare bankruptcy or to undertake a major capital expenditure. In essence, the contingent claims approach offers potential for more precise and realistic answers by analyzing truly firm dynamics as well as by obtaining closed form solutions that would be difficult to derive in discrete-time dynamic models. This has enabled theorists to make much progress in recent years.

One unique aspect of the contingent claims approach to research in corporate finance is that it can provide quantitative guidance and predictions instead of largely qualitative implications that are commonly seen in other areas of corporate finance theory. Another unique aspect is that these models lend themselves to structural estimation, which has become an important and insightful technique for corporate finance researchers in recent years. Both of these aspects enable researchers to examine whether calibrated corporate finance models can closely match economic magnitudes (such as leverage ratios or investment rates) observed in the data and even whether empirical regularities in real data sets (such as correlation or regression coefficients) can be generated in simulated data sets of potentially heterogeneous but strictly model-implied firms. As a result, dynamic corporate models have been increasingly regarded as one of the state-of-the-art modeling approaches in corporate finance. This special issue confirms this trend, which leads us also to expect that their merits and uses will continue to grow further in the future.

Earlier contributions to the field of dynamic corporate finance focus largely on capital structure and on investment under uncertainty (real options). More recently, financing and investment decisions have been integrated into these models to study them jointly. As a result, dynamic corporate finance has developed novel insights on more complex capital structure choices as well as interactions between financing and investment decisions. Moreover, the dynamic modeling approach has also been fruitful in other areas of corporate finance theory, leading to more rapid growth of these areas of corporate finance theory in recent years. As underlined by this special issue, this class of models can deliver answers (and thereby open up new research questions) to a broad set of topics, such as the effects bank stability, executive compensation, managerial incentives and performance, optimal contracting, and last but not least optimal capital structure with debt renegotiation.

In emphasizing the importance of theoretical research we do not wish to diminish the importance of empirical research. We think that both types of research are important and should be encouraged by the field and by the profession at large. Indeed, the interaction between theoretical and empirical research is key, and we think there should be more refined and thorough interactions going forward. Theorists should be tuned to empirical findings to inform them about which models are worth writing and empiricists should be tuned to new theories to better understand the data and think about new angles from which to analyze the data. The dialogue between theorists and empiricists is quite productive in a few other fields of economics – asset pricing may be one example – but not yet as productive as it probably could be in corporate finance. The articles in this special issue, we hope, provide examples of such a dialogue and also promote the idea of engaging further in such a dialogue between empiricists and theorists.

Furthermore, the development of better tools to deal with identification challenges in empirical research – in particular distinguishing between alternative channels that can explain an observable effect in the data or treating endogenous selection issues – enables us to better assess the merits of different theories. As a result of sharper identification tools, researchers can provide an increasingly tighter link between empirical and theoretical corporate finance going forward. The accomplishment of empirical researchers to provide a much more nuanced assessment of theoretical predictions itself should invite a growing number of competing, plausible explanations

<sup>3</sup> The contingent claims approach to valuation is a generalization of the option pricing techniques developed by Black and Scholes (1973) and Merton (1973).

for empirical regularities. This interactive process between empirical and theoretical corporate finance research will lead to the broadest possible and probably fastest discovery of knowledge as we have witnessed it, for example, in the medical or physical sciences.

## 2. The papers

The special issue on corporate finance theory of the *Journal of Corporate Finance* contains nine articles. We believe that several themes emerge and that they deserve more attention by theorists going forward, i.e., interactions between financial markets and corporate finance and dynamic models of corporate decisions, such as capital structure and managerial compensation. The remainder of this article briefly discusses each of the articles in this special issue, places each in the broader context of the related literature, and points out open research questions related to each.

In “The prevention of excess managerial risk taking,” Van Wesep and Wang develop a model that rationalizes the common practice to pay large severance fees to departing CEOs. This practice has drawn a lot of criticism, as it seems at first to be inconsistent with optimal incentive contracts. Why would firms pay large amounts of money to CEOs in those cases where they did not succeed and hence have to leave the firm? It might seem that this would act against incentivizing managers to work hard and do well, and so many have attributed it to corruption or conflict of interests.

The authors propose a model where severance fees emerge as the optimal solution in a case where shareholders want to curtail excess managerial risk taking. Suppose that managers are subject to performance targets, whereby they will gain in case their performance exceeds some predetermined threshold. In this case, if they think they are likely to miss the target, they will be tempted to take significant negative-NPV risks due to the potential upside that will bring them above the threshold. Providing severance fees in case the manager does not achieve the threshold then acts to mitigate the incentive for the manager to take excess risks, and so may be part of an optimal contract.

There have been other models in the literature that derived the severance payment as part of an optimal contract, but they are not driven by the attempt to reduce managerial risk taking. Hence, the model adds an interesting new rationale for this widely observed practice, which is of great relevance if one indeed thinks that managerial risk taking is an important problem in some settings. The authors argue in the paper why this is the case. The paper delivers empirical implications which are different from those of the other models. For example, severance pay should be observed when the manager's performance is mediocre but not horrendous and severance pay will interact with firm size, as increasing the size of the firm can act to deter bad managers from taking excess risks even for low levels of severance pay. The authors provide some discussion on why these predictions are reasonable and match the data, but ultimately there is room for careful empirical work to shed light on whether these predictions indeed hold in the data and make the model a better explanation for severance payments than those provided by other models.

In “Stock-based managerial compensation, price informativeness, and the incentive to overinvest,” Strobl provides an optimal-contracting based explanation to another typical corporate behavior that is often attributed to bad managerial behavior: overinvestment. In the model, shareholders have to incentivize managers to exert effort by tying their compensation to signals about the success of the firm's projects. An important such signal is the stock price, which is formed based on speculators' information acquisition and trading in financial markets. The amount of information in the stock price, and hence the usefulness of the price as a signal about managerial effort, depend on speculators' incentives to produce the information.

The author endogenizes the firm's incentive contract, managerial effort and investment decisions, the decision of a speculator on information acquisition and trading, and the price formation process. He shows that the firm may sometimes choose to commit to overinvest in its projects, so as to induce more information acquisition, making the stock price more informative, and the incentive contract more efficient in generating managerial effort. The idea is that when the firm does not undertake investments, then speculators' information is less useful to them in predicting firm value, and hence they acquire less information. Committing to overinvest – even though it leads to inefficient resource allocation *ex post* – provides more incentives for information acquisition and leads to a better solution of the incentive provision problem between the shareholders and the manager.

The paper proposes a very different explanation to the overinvestment observation than the one that is usually provided. If one believes that information in prices is important in determining managerial actions and compensation contracts, then the rest of the analysis, including the overinvestment conclusion, follows through quite naturally. Hence, the mechanism in the paper seems like an important insight to take into account when interpreting corporate overinvestment behavior and thinking about corporate governance conclusions. The paper belongs to a line of literature that highlights the feedback effect from financial markets to firms' decisions, which was recently reviewed by Bond et al. (2012). As we see in this review paper, there is indeed evidence for the importance of price information for corporate actions, and there are other theoretical papers studying the implications for price formation and firms' decisions. We would expect that overinvestment will occur more due to the mechanism in this paper in those cases where shareholders indeed need to rely on stock prices to determine how hard the manager has worked. This opens a path for empirical testing of the ideas in the paper.

Another paper in the special issue which contributes to the feedback-effect literature is “Market efficiency, managerial compensation, and real efficiency” by Singh and Yerramilli. The authors explore the connection between market efficiency and real efficiency, which has been of large interest in the finance literature for a long time (see again the review by Bond et al. (2012)). In the model, as in the previous paper discussed above, shareholders tie managers' compensation to the share price in order to provide them incentives to exert effort. The share price is based on two signals about the firm: a ‘performance’ signal, which is affected by managerial effort, and a ‘productivity’ signal, which is unaffected by managerial effort. As in Paul (1992), an improvement in the precision of the productivity signal increases market efficiency – as it makes the price more informative about firm value – but reduces real

efficiency – as it makes the price less connected to managerial effort and so leads to a reduction in the ability to compensate the manager based on performance.

The authors extend this framework by assuming that the market does not observe the compensation contract between the shareholders and the manager. This assumption is not typical in corporate-finance models, but as the authors argue, it is quite plausible, and fits real-world settings, and so it has the potential to generate new relevant implications. They also assume that shareholders benefit exogenously from a higher share price (this is a more typical assumption). In such a setting, shareholders may choose to induce excessive effort from the manager by providing a very sensitive contract, as this can signal higher intrinsic value to the market and increase share price. Then, an increase in market efficiency, via an improvement in the precision of the productivity signal, may actually increase real efficiency, as it can act to correct this problem of excessive effort.

One of the strengths of this paper is the wide array of empirical implications it offers. Whether the increase in market efficiency leads to an increase in real efficiency depends on how strong the problem of excessive effort is relative to the benefit from incentivizing managers based on a performance-sensitive signal. As the authors explain, this in turn depends on the importance of the performance signals relative to the productivity signal for firm value which can be linked to empirical measures like the market-to-book (M/B) ratio. Of course, all these predictions also depend on the assumption that the compensation contract is not observable, which will be more relevant in some cases than in others, and this leads to another layer of time-series and cross-sectional variation, which can lead to more avenues for empirical testing.

Garcia provides an extension of a typical agency problem in “Optimal contracts with privately informed agents and active principals.” Typically, in such problem, an agent has better information (adverse selection) or takes a hidden action (moral hazard), and a principal just sets a compensation contract to affect the choice of action of the agent or elicit more information from him. In this paper, the author considers a case where the principal is more active and takes an additional action that affects the outcome of the interaction between the principal and the agent. Models of this sort have been analyzed before, as is acknowledged in the paper, but the author here takes a unifying approach and presents a framework that nests many of the existing models and applications. Interestingly, doing so generates new insights, showing when the old results hold and when new results are expected. Along the way, empirical implications are presented.

A leading example in the paper is the setting studied in [Bernardo et al. \(2001\)](#), where headquarters provides performance sensitive compensation to division managers but also decides on capital allocation to them. It is this latter action that makes the principal active. Garcia provides a generalization of the results in [Bernardo et al. \(2001\)](#) regarding which divisions will get more capital and what is the relation between the amount of capital and the sensitivity of pay to performance. Incentives and capital investment are generally shown to be substitutes, and the predictions in [Bernardo et al. \(2001\)](#) are shown to depend on specific assumptions, such that different predictions concerning capital allocation and performance structure are generated for different assumptions on the underlying structure. Garcia also studies an application to a multitasking problem where the principal chooses the range of tasks to allocate to different agents.

During the recent financial crisis most financial institutions were severely undercapitalized, which lead to a series of costly bail outs. Hence one of the central themes for researchers following the financial crisis has been how to design market-based and regulatory mechanism to make the financial system less vulnerable. In particular, the capital structure of banks and their ability or willingness to obtain additional equity capital in bad times has been at the core of this literature. Contributing to this literature, the article by Hilscher and Raviv titled “Bank stability and market discipline: The effect of contingent capital on risk taking and default probability” analyzes the effect of including contingent convertible bonds in the capital structure of financial institutions. Contingent convertible bonds (often simply referred to as “contingent capital” or “coco”) are debt securities that automatically convert into equity if assets fall below a predetermined threshold. In the business press and ongoing debate, these hybrid securities are indeed one of the most prominent suggested solutions for coping with capital shortfalls in bad times. Based on their bank capital structure analysis, the authors establish that a specific type of contingent capital bonds has very helpful features in the management of a financial institution's default probability and its incentives to shift into a higher-risk portfolio. In particular, an automatic debt-to-equity swap or “bail-in” is potentially valuable because it is executed in times of distress when there would otherwise be little or no incentive for such a conversion or swap.

Applying the contingent claims approach, Hilscher and Raviv develop a novel and unique decomposition of bank liabilities into sets of barrier options and present closed-form valuation equations. This enables the authors to quantify the reduction in default probability associated with issuing contingent capital instead of subordinated debt. The article provides a nice set of comparative static-type illustrations of how coco, debt, and equity values are affected by changes in asset portfolio volatility. For example, the value of the bank's equity is nearly insensitive to its asset risk when the conversion ratio is equal to 50%. In addition, the authors show that risk-taking incentives continue to be weak during times of financial distress. Overall, their analysis provides a convincing theoretical argument for the intuition that contingent capital may be effective in reducing bank failure rates and bank bail outs. As such, contingent capital stabilizes the financial sector.

More broadly, the relevance of bank capital structure and especially healthy leverage levels for the banking sector will remain an important subject for academics and practitioners. As long as banks act as (socially useful) liquidity providers and operate assets that are relatively safer than the assets of non-financial firms, then it might be optimal for banks to have relatively high leverage ratios despite of the associated agency problems as well as deposit insurance and too-big-to-fail distortions. Clearly, the insights developed by Hilscher and Raviv help to better manage and understand the risk of financial institutions. It seems, however, necessary to develop dynamic corporate finance models even further to realistically reflect the unique features of banks, so that one can assess the benefits and costs of potentially high (or low) bank leverage in a calibrated, quantitative framework.



The study of Pinto and Widdicks, “Do compensation plans with performance targets provide better incentives?” develops a dynamic option valuation model for executive compensation plans. The model is flexible enough to consider various types of stock and option plans as well as various types of exercise and vesting conditions that may be contingent on stock price performance. In particular, the authors consider many realistic features of these plans, such as calendar vesting periods, exercise conditional on stock price performance, and vesting on stock price performance. In addition, the risk-averse manager can exercise the option or stock holdings in stages. Very different from most papers which consider the incentives at the issuance date, this paper considers the expected total incentives over the lifetime of the plan. For plans that result in the same cost and compensation, the optimal plan is the one that provides the largest (expected) lifetime delta (i.e., incentive to increase firm value).

Pinto and Widdicks find that the use of options is consistent with maximizing total expected lifetime pay for performance incentives. While it is often argued that restricted stocks provide stronger incentives, even though executives sell their restricted stock holdings sooner, resulting in lower lifetime incentives, Pinto and Widdicks show that performance vesting targets provide the least cost effective pay-for-performance expected lifetime incentives, performance exercise targets provide the largest expected lifetime risk incentives, option plans are generally superior to restricted stock plans, and calendar vesting is only efficient up to a maximum of three years. The authors also find that performance exercise targets can increase the expected total lifetime incentives provided by compensation plans. One possible way to increase expected lifetime incentives is to increase the vesting period but doing so reduces the value of compensation to the risk-averse executive. This leads to the result that vanilla options are more effective forms for executive compensation. However, for vanilla options, increasing vesting period beyond three years results in lower expected lifetime incentives. This is contradictory to the intention of adopting long vesting period compensation plans. Furthermore, longer vesting periods prevent optimal exercise, which leads to lower executive valuation and is also more costly to the firm. Overall, standard options with short vesting periods provide, in general, the most cost effective expected lifetime pay-for-performance incentives.

Notwithstanding the large literature on real options pioneered by McDonald and Siegel (1986) and Dixit and Pindyck (1994), there has so far been little work on how investment timing decisions are affected by corporate governance and, in particular by conflicts of interests between managers and shareholders, as well as other real frictions, such as compensation packages or project liquidations. Hori and Osano examine in the article titled “Investment Timing Decisions of Managers under Endogenous Contracts” what kind of managerial compensation contract might be optimal for providing suitable investment timing incentives in the presence of effort costs, imperfect information, and, importantly, compensation constraints. In particular, moral hazard on the part of the manager leads to the manager's objectives not being aligned with the ones of shareholders. An additional investment distortion in the model stems from the fact that in reality most managers also have an option to liquidate projects.

Hori and Osano show that restricted stock is optimal relative to stock options under certain condition. However, the authors also suggest that stock options are more likely to be used instead of, or in addition to, restricted stock in firms with new debt financing and more impatient managers, diversified firms involving more complicated business activities, and firms with weaker corporate governance. The latter findings are not necessarily in line with standard compensation practice and hence suggest a review in the light of the importance of investment timing distortions, which are likely larger in certain industries. Hori and Osano also establish that project start-up is more likely to be deterred by the greater likelihood of project liquidation, because it effectively lowers the intrinsic value of the investment option, and larger managerial effort cost, because it also reduces the net gain from delegating investment decisions to managers. Consistent with economic intuition, the optimal amount of stock-based managerial compensation rises with managerial effort costs, but perhaps surprisingly it is insensitive to the probability of liquidation.

Another notable aspect of their work is that it aims at developing empirically testable implications, which is an important way for theoretical corporate finance to influence empirical corporate finance and vice versa. The authors begin by discussing earlier empirical research and then they summarize succinctly the predictions that are unique to their model. We generally believe that careful empirical tests of real option models with a number of realistic frictions, such as compensation, corporate governance, or project liquidation, are rare and hence bear the promise for fruitful future research. For example, the authors predict that stock options can be more likely to be used instead of, or in addition to, restricted stock in firms with new debt financing and more impatient managers, diversified firms involving more complicated business activities, and firms with weaker corporate governance. We hope that as a result of this special issue both empirical and theoretical research will be spurred.

In “Options, option repricing in managerial compensation: Their effects on corporate investment risk,” Ju, Leland, and Senbet contribute to the literature on the optimal design of managerial incentive contracts and, in particular, their role for influencing corporate investment risk policies. Even though stock options have undoubtedly some merits for the optimal provision of incentives, they can result in deviations from the optimal (firm-value maximizing) risk policy. More specifically, the authors develop a structural model to derive optimal contracts when the manager can initially select the firm's asset risk based on a parsimonious, single-peaked value-volatility function. Thus, this corporate investment risk choice directly affects the initial value of the firm and also indirectly the present value of the manager's expected future compensation. To influence the manager's risk choice, the firm's owners can offer incentive contracts, which feature, for example, base pay (cash), restricted stock, and stock options. For such realistic pay-for-performance compensation packages, the authors calibrate their model, derive optimal contracts, risk levels, and firm values, and examine agency costs associated with deviations from optimal risk policies as well as ways to ameliorate these costs via better compensation structures.

As pointed out by Ross (2004), the literature's commonly held view that granting stock options to risk-averse agents induces more risk-taking is not robust. Consistent with this paradigm shift, Ju, Leland, and Senbet show that, relative to the optimal risk level, call options can induce too much or too little corporate risk-taking, depending on managerial risk aversion and the underlying investment technology. Another novel feature of the article's analysis is that it also allows for lookback options in the optimal contract. The authors establish that inclusion of lookback call options in compensation packages has desirable countervailing effects on managerial risk-taking and can induce risk levels that increase shareholder value. The main reason is that, while standard call options may provide

little incentives when the probability for call options to finish in the money is very small, lookback call options are more likely to be in the money and hence provide better incentives for the manager to increase the firm's stock price. Finally, they argue that lookback call options are analogous to the observed practice of option repricing. Hence the inclusion of option repricing features has desirable countervailing effects on corporate investment risk policies and they are therefore very effective in reducing both agency costs and compensation costs.

In the paper titled "Dynamic Capital Structure with Callable Debt and Debt Renegotiation," Christensen, Flor, Lando, and Miltersen use the contingent claims approach to value debt claims. Their dynamic model features an upper and a lower boundary, in which the optimal capital structure decision is homogeneous of degree one in the state variable (earnings before interest and taxes). One novel feature of the model is that it permits repeated renegotiation between debt and equity holders. In particular, if the firm does well, equity holders recapitalize to increase leverage, and if the firm does poorly, they restructure via a renegotiation process between debt and equity holders.

Unlike earlier strategic debt service models, Christensen, Flor, Lando, and Miltersen consider that equity holders' threat to default after a renegotiation offer might not be credible. That is, if it is not credible, they will carry on and not default. The authors consider four different settings: no renegotiation, repeated dynamic renegotiation in which the value is split in fixed proportions between equity and debt holders, fully dynamic renegotiation, and finally a set-up in which a finite number of renegotiation offers can be made prior to bankruptcy. The core of the paper is the analysis of the last case and its comparative statics, such as the analysis of the dynamics of renegotiation offers as the number of remaining renegotiation options dwindles.

The authors show that in a dynamic capital structure model where taxes and bankruptcy costs are the only frictions, violations of the absolute priority rule can be optimal. This is in line with the evidence of financially distressed firms in private workouts and Chapter 11 reorganizations. They also study the behavior of firm value, equity value, debt value, par coupon rates, leverage, and yield spreads. They show optimal leverage is inversely related to growth options and earnings risk, and optimal financing decisions mitigate the effects of direct bankruptcy costs.

### 3. Final remarks

As final remarks, we note that a large number of excellent papers were submitted for publication in this special issue on corporate finance theory of the *Journal of Corporate Finance*. We believe that the nine articles in this special issue underline the remarkable potential for doing more theoretical research on the intersection of corporate finance and financial markets (or asset pricing). We hope that the ideas outlined in this introductory article and the articles themselves provide directions and guidance for future research on corporate finance theory and, in particular, promote and reinforce a fruitful dialogue between empiricists and theorists in the field of corporate finance.

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