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CORPORATE FINANCE, FINANCIAL INTERMEDIATION AND BANKING: AN OVERVIEW

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At one time, perhaps before the emergence of market microstructure as a rich field for research, Financial Intermediation was viewed by many as a distinct subfield of Finance, along with Corporate Finance and Asset Pricing/Investments. Although dominated by Banking, Financial Intermediation also includes studies of non-depository institutions like insurance companies, mutual funds, credit rating agencies and the like. Some would even include market microstructure. The distinctiveness of Financial Intermediation as a research area stemmed in part from the fact that it was a highly specialized area, replete with institutional detail, banking practices, and descriptions of regulations, and somewhat disconnected from the mainstream paradigm shifts that were occurring in Corporate Finance and Asset Pricing.

There has been a sea change in this landscape in the past thirty years or so, a period that has not coincidentally also seen the meteoric rise in the popularity of asymmetric information and agency theory as legitimate approaches to research in Economics and Finance. In the absence of an explicit recognition and modeling of these frictions, the fundamental Modigliani-Miller irrelevance theorems relating to capital structure and dividend policy, as well as the separation theorems relating to investment and financing policy, made it difficult to even visualize what economic functions financial intermediaries really served, let alone develop theories that could explain *how* banks and other financial intermediaries were intimately connected with the financial policy choices of firms and thereby highlight the joining at the hip of Financial Intermediation and Corporate Finance research.

Agency theory and asymmetric information kept opening a variety of new doors for research in Corporate Finance, thereby transforming the field. Almost concurrently, asymmetric information and agency theory had an even more profound transformational effect on Financial Intermediation research, seemingly paradoxically engendering two diametrically-opposed phenomena. On the one hand, Financial Intermediation research in the past 20-30 years has allowed the field to develop a somewhat distinct research identity in terms of issues, questions and paradigms. Examples of this abound: financial intermediary existence, credit rationing, collateral, relationship banking, various forms of bank regulation, and deposit insurance are just a few examples. On the other hand, Financial Intermediation research has moved so close to Corporate Finance research that research in these two subfields of Finance is virtually indistinguishable in terms of tools and research methodologies. Consequently, one could legitimately view Financial Intermediation research as a subset of Corporate Finance research. As far as analytical approaches go, examining bank-borrower or bank-regulator moral hazard in Financial Intermediation is really no different from examining firm-bondholders moral hazard in Corporate Finance. Empirically examining whether bank lending has information content by measuring cumulative abnormal returns (CARs) associated with the stock price dynamics of firms around bank-loan-announcement dates is basically the same as exploring the signaling content of dividend changes. Extracting the implications of viewing equity as a call option on total assets is conceptually the same as extracting the implications of viewing deposit insurance as a put option for the bank. The list goes on and on.

The vanishing of the lines of distinction between Corporate Finance and Financial Intermediation research is a natural outcome of the fact that the contemporary research efforts in both subfields have been drawing their intellectual inspiration from the same wells: asymmetric information and agency theory. It is partly for this reason that this readings book is dedicated to

issues that straddle the (vanishing) boundary, if one even exists, between Financial Intermediation and Corporate Finance research. Apart from the fact that the various strands of the research surveyed and synthesized in this book are connected by the commonality of the information-theoretic tools that they rely on, another common element is that "design" issues are prominent in much of the discussion: security and contract design, institutional design, the design of optimal regulation, and the design of trading mechanisms and markets. In this sense, the book's focus is akin to that in Tirole's book, where design issues are discussed using the tools of information economics and contract theory (see Tirole (2006)).

Six topics are covered in this book: design of contracts and securities; market microstructure; credit market implications of bank size, scope and structure; mutual funds; bank regulation; and finally the interaction between interbank competition, regulation and banking stability. We invited leading researchers in these areas to act as "section editors" for these six topics. Each section editor was asked to perform two tasks: select specific issues to be explored within that topic and invite prominent and active researchers on those issues to write original articles that reviewed and synthesized the research done on those issues thus far, with indications for future research. We are very pleased with the outcome: we have twenty outstanding chapters in this book besides this chapter, each representing an original contribution that achieves the twin goals of research review and synthesis of an important topic.

Why should you read this book? There are two simple reasons. First, good review articles are rare. This book has not one but numerous excellent review articles that provide valuable perspectives on a host of issues that are well represented in the research published in the top journals today. Second, on each topic, the section editors have not only provided an overview of the chapters in their sections, but also a succinct description of the important research questions that should be addressed in future research. Thus, there is a wealth of potentially interesting new research ideas here.

The first section, edited by Franklin Allen, is concerned with security design. Rather than taking observed financial securities as given, this literature goes back to first principles and endogenizes these securities as optimal financial contracts under specific circumstances. This is an exciting field of study that has relied heavily on the creative use of agency and asymmetric information to generate striking new insights about why financial securities take the form that they do, and how the endogenously-arising rationale for the existence of these securities can help us understand the security issuance timing and capital structure decisions of firms. A significant advantage of endogenizing security design using only primitives is that it allows us to comprehend more clearly why these securities are deployed even when they generate distortions of various sorts. For example, it is well known that the credit rationing in Stiglitz and Weiss (1981) or the asset-substitution moral hazard in Jensen and Meckling (1977) could be avoided if borrowers were not using debt contracts to raise financing, which raises the obvious question about why firms don't use alternative contracts. The papers in the section on security design confront the following questions: (1) *When is a debt contract optimal?* (2) *What role does asymmetric information play in explaining certain features of debt contracts?* (3) *How is securitization - - which involves pooling together and then the tranching of debt securities - - structured?*

The Fulghieri and Goldman paper provides an insightful synthesis of the literature that seeks to address the first question and explain why debt contracts arise and the circumstances in which it is efficient to use them. Moreover, they also address the second question above by pointing out that, once the debt contract is endogenized, the addition of various sorts of

asymmetric information paves the way for the emergence of subordination and maturity structures, collateral requirements and so on as important endogenous elements of debt contract design.

These issues of debt contract design and innovations in debt contracting are addressed by An, Deng and Sanders who take up the third question and explain how securitization helps ameliorate asymmetric information problems, and also provide an empirical analysis of securitization structures using data on commercial mortgage-backed securities. The evidence here provides a plethora of insights into the mechanics of securitization and the economic functions it serves.

As Allen points out, these two papers reflect what has been the predominant focus in the security design literature, namely explaining debt contracts. But numerous other issues related to security design that are not covered in these papers have begun to attract research attention. These include endogenous justifications for the emergency of equity securities, the interaction between security design and corporate governance, the design of corporate charters, security design in an international context, security design as a component of auction design, and security design with heterogeneous beliefs and learning. We would like to add two more to this list. One is the interaction between security design and corporate information disclosure, which is as yet underexplored (e.g. Boot and Thakor (2001)). In particular, the question of how security design affects information acquisition incentives in financial markets is interesting and relevant. The other is the role of heterogeneous beliefs in determining the design of securities as well as firms' security issuance and capital structure decisions, which is an area that is loaded with potential. These are all exciting topics that should be studied more in future research. There is a significant chance that future research in this area will move the frontiers of Financial Intermediation and Corporate Finance.

As many researchers have explored the design of securities, others have focused on the design of trading mechanisms and markets. A special case is limit order markets, which is the topic of the second section in the book. This section consists of an excellent survey paper by Parlour and Seppi. The topic of limit orders is especially important since most equity and derivative exchanges in the world today are either pure electronic limit order markets or permit limit orders in addition to on-exchange market making. Given two decades of theoretical and empirical research on market microstructure issues, it is an opportune time for Parlour and Seppi to pose the question: *What has been accomplished theoretically in studies of limit order markets?*

In addressing this question, the authors survey a vast literature on the subject and cover both the significant new insights that this literature has generated as well as the questions that remain to be addressed. There are five main issues the authors address in their survey: price formation in dynamic limit order markets, issues of liquidity supply and demand in limit order markets, the dynamics of the limit order book, the process of information aggregation, and inter-market competition involving limit order markets competing with hybrid markets that have both dealers and limit orders.

The key messages that emerge are the following. First, unlike Walrasian markets, there is *no* unique market-clearing price in dynamic limit order markets. Rather, there is a sequence of prices associated with bilateral trades. Second, because of the blurred distinction between liquidity supply and demand in limit order markets, it is *not* possible to unambiguously extract the compensation for liquidity provision that is embedded in limit order quotes. Third, the dynamics of the limit order book are such that trades and prices may exhibit path dependence. Fourth, since limit order books contain prospective information about future price volatility and

order flow, they can be a source of information for those who seek to engage in informed trading. Finally, if the only friction in the market is asymmetric information, then limit order markets can provide the most liquidity and hence implement price schedules that are immune to competition from other trading mechanisms. However, if non-informational frictions exist, then pure limit order markets may be driven out by other market forms, including markets that combine dealers and limit orders.

Despite these impressive insights, Parlour and Seppi note that much remains to be done. They note that the following research questions may hold the most promise: (1) What individual investor order submission strategies aggregate into the observed aggregate order flow? (2) How does the fact that investors trade groups of stocks affect their order submission decisions compared to the situation in which investors trade just one stock? (3) What is the interaction between the characteristics of the limit order book and asset pricing? (4) What are the social welfare implications of limit order markets in a variety of situations? (5) What guidance can limit order theory provide about how observations should be aggregated in empirical research (to deal with the problem of very large order flow data sets) and which exogenous instruments should be used to deal with the thorny endogeneity problem arising from the fact that many observable variables from limit order markets are endogenously determined?

Although the issues of how securities are designed and how trading mechanisms are designed are conceptually related, the extent of overlap between these two streams of research has been quite limited thus far. While the potential connections between market microstructure and asset pricing have already begun to be explored, the linkages between security design and the design of trading mechanisms in markets are also worth thinking about. For example, security design is often predicated on the recognition of certain informational frictions and is aimed at designing specific features of the security to optimize an objective function in light of the assumed informational frictions. But the impact of the design of the security on the issuer of the security often depends on the market mechanism used for trading that security. Hence, the choice of the trading mechanism can impinge on optimal security design.

In designing a financial *system*, there are three main pillars one has to design: the securities/contracts that are used for financial transactions, the market structures and trading mechanisms that are used to execute these transactions, and the financial institutions that enable the execution of *non-market* transactions as well as market transactions (e.g, the role of banks in securitization). In the third section of the book, edited by Mitchell Berlin, there are three chapters that address the design of financial institutions from an Industrial Organization (IO) perspective. The big question addressed collectively by these contributions is: *What factors determine the boundaries, size and internal structure of financial intermediaries?*

In the first chapter in this section, Strahan poses three questions that his contribution seeks to address: (1) *What are the implications of bank size and structure for lending behavior?* (2) *Is relationship banking feasible in a deregulated market?* (3) *Why are deposit-taking and lending combined within a single institution, the bank?* Strahan's overall conclusion from surveying the empirical literature is that larger banks are more efficient and lend more on average, so credit availability is augmented by bank size. The evidence regarding the relationship between bank size and the availability of relationship loans is mixed, but there is no compelling evidence that points to the demise of relationship banking in a deregulated market.. On the issue of the jointedness of deposit-taking and lending, Strahan concludes that little is known that truly establishes the synergies between these activities. Berlin observes that this question can be dealt with merely as a special case of the issue of vertical integration in IO, and

that the banking literature could fruitfully "borrow" more from the well-established IO literature on this topic.

In the second chapter in this section, Mester examines economies of scale and scope in banking. One of the conundrums in this literature is that a vast body of empirical research in this area had established that scale economies in banking were fully exhausted at a relatively small size - - as little as $500 million in assets - - and yet the existence of many very large banks seemed to offer evidence to the contrary. Mester asks: *At what size are scale economies in banking truly exhausted?* Mester observes that more recent work in this area has helped to dramatically revise upward these optimal size estimates, primarily by being explicitly cognizant of the intermediary functions of banks as reflected in risk-taking and financial capital.

In the final chapter in this section, Gande turns to the *scope* of banking, which essentially deals with another kind of joining of activities within a bank: lending and underwriting. Since the dismantling of the Glass-Steagall restrictions on U.S. banks in 1999, commercial and investment banking activities can be conducted under the same corporate roof. Gande surveys the empirical literature on this topic to address the question: *What are the efficiency gains, if any, from permitting banks to offer both lending and securities underwriting services*? Gande's conclusion is that significant efficiency gains might be realized when lending and underwriting are joined, but definitive answers are elusive since competition effects could also explain the lower spreads post-1999.

In his overview of this section, Berlin notes that many interesting research questions remain unanswered or only partially answered. In particular, what is the optimal internal organization of financial intermediaries? Should banks combine transaction and relationship lending (e.g. Boot and Thakor (2000))? What is the role of loan syndication and when is it optimal? What role will credit bureaus play in affecting the informational rents of banks? As is apparent, some of these questions are at the interface between the design of securities and the design of institutions, so "hitch-hiking" on the insights of the security design literature may prove to be of some value in future attempts to address these questions.

In the fourth section, edited by Sudipto Bhattacharya, issues related to mutual funds are taken up. A mutual fund is merely a special kind of financial intermediary, and it is important to place mutual funds in the broader framework of financial intermediaries by carefully delineating the economic functions they serve, which in turn requires good measures of mutual fund performance to assess whether they deliver a risk-return package that dominates what investors can do on their own. There are three chapters in this section that collectively deal with the following questions: (1) *How do we measure mutual fund performance?* (2) *What incentives do mutual fund managers have and how can they be aligned with those of investors?* (3) *How do investors choose between mutual funds, given the issues in alignment of interests between fund managers and investors, and the difficulties in measuring fund performance accurately?*

The first issue is taken up by Lehmann and Timmermann who highlight the econometric challenges in assessing mutual fund performance. They conclude that it is exceedingly difficult to get much power to detect abnormal skills among fund managers using conventional econometric tests on performance evaluation. This exacerbates the principal-agent-issues associated with fund delegation and worsens the problem of asymmetric information between fund managers and investors. Thus, one of the central messages of this paper is closely aligned with the information-theoretic themes of this book.

The second question is addressed by Bhattacharya, Dasgupta, Guembel and Prat, who focus on the potential divergence of interests and the consequent incentives for fund managers to

adopt herding strategies. In a sense, the Lehmann and Timmermann essay and the one by Bhattacharya, Dasgupta, Guembel and Prat are related. While Lehmann and Timmermann point out how difficult it is to use fund performance to extract reliable inferences about (abnormal) skills in fund managers, Bhattacharya, Dasgupta, Guembel and Prat examine the consequences of this for fund manager behavior.

The third question is the focus of Zheng's chapter. While emphasizing that the overarching conclusion emerging from the empirical research on mutual funds is that investors are better off with low-cost index funds than with actively-managed funds, Zheng finds that there is a strong interaction between investor behavior and the strategies of mutual funds, which could either help or hurt investors. And, as suggested by the conclusion of Lehmann and Timmermann, investor behavior may itself be constrained by their inability to reliably infer fund manager skill.

An interesting question that remains to be addressed is the extent to which the existence of mutual funds affects the kinds of securities and contracts that are designed. For example, would a firm design its securities differently when a wide range of mutual-fund alternatives are available to investors? Perhaps not. But the potential interplay between mutual funds and security/contract design is worth contemplating.

The penultimate section in this book, edited by Mark Flannery, deals with bank regulation, including capital requirements and scope and entry restrictions, and how this has influenced the size, scope and design of banks. Some of the questions posed in this section are as follows: (1) *Why did banking consolidation occur when it did in the U.S. and what can we say about the future?* (2) *How did the structure and operations of banks evolve and how are banks likely to position themselves in the future?* (3) *Why has there been so much excess capital in banking?* (4) *How will the Basel II capital regulation be implemented and what will be its likely effect?*

The first question is addressed by Jones and Critchfield. They conclude that the structure of the U.S. banking industry has been significantly restricted by regulation, and yet they note that by the 1990s most regulatory restrictions had been dismantled. Jones and Critchfield offer this changed regulatory landscape along with numerous other reasons to explain the dynamics of the consolidation in the banking industry that has been underway for some time. They predict that the consolidation trend will continue in the U.S, albeit at a slower pace.

DeYoung examines the second question. He studies the evolution of banking over the past two decades and observes, like Jones and Critchfield, that bank specialization has been limited by government regulatory restrictions. He concludes that in the future, large banks are likely to offer customized "hard information" products, whereas small banks are likely to specialize in dealing with "soft information" transactions (see Stein (2002)).

Flannery and Rangan take up the third question. They document that bank capital in the U.S. has been rising steadily all through the 1990s and in the new century. The average bank holding company had 400 basis points more equity capital in 2001 than required under Basel I. They explain this excess capital on the grounds that it is evidence of market discipline (one of the pillars of the Basel II capital accord) working, as banks are voluntarily choosing to keep capital beyond that required by regulation in response to market signals about the optimal level of capital to absorb asset risk.

In the last chapter in this section, Kupiec addresses the fourth question. Flannery notes in his section review that the U.S. implementation of Basel II will involve an additional "leverage standard", essentially mandating a minimum level of equity capital as a fraction of on-balance-

sheet total assets. For several banks, this may supersede the Basel II standard, i.e., be more binding. Under Basel II, many large banks will be able to determine their own capital levels based on guidelines related to a fairly comprehensive assessment of various risks. Kupiec develops a simulation model to assess the effects of Basel II, including procyclicality in capital standards. Based on his simulation analysis, Kupiec is skeptical about the economic justification for Basel II, and even wonders if Basel II could be considered a minimum global capital standard, given the wide latitude it will provide national regulators. Kupiec's conclusions complement the finding of the many interesting papers on this topic in a special issue of the *The Journal of Financial Intermediation 13-2, 2004 " Special Issue on bank Capital Adequacy Regulation Under the New Basel Accord"* (see, for example, von Thadden (2004)).

Although not covered by the chapters in this section, bank regulation is intricately connected with the topics covered earlier in the book. This is for at least three reasons. First, regulation inspires innovations in the designs of banks as well as the securities with which they raise capital, e.g. if it is not privately optimal to raise equity capital to meet regulatory capital requirements, banks may choose other types of securities that may qualify - - such as perpetual preferred stock - - or come up with other innovations such as Euro deposits to circumvent reserve requirements. Second, regulations such as capital requirements can also induce banks to introduce new products for their *customers*. Witness the emergence of "debt consolidation" products offered by banks that provide additional mortgage financing to borrowers to help them pay off credit card debt, given the higher bank capital requirements against credit cards than against mortgages under Basel I. Third, several innovations, like developments in securitization, help banks to optimally manage their balance sheets in light of the capital requirements that they face.

The final section of the book, edited by Xavier Vives, extends the regulation perspective of the previous section to include interbank competition. This is an important issue because regulators worldwide have been relaxing barriers to entry into banking and encouraging competition. These initiatives have not been without controversy, however, since there are many who believe that (excessive) competition can diminish banking stability. The key issue for research is *how* to design an appropriate competition policy for banking that is consistent with the regulatory framework. The big-picture questions addressed in this section are: (1) *How does interbank competition affect banking stability and the efficacy of regulation?* (2) *What are the sources of bank rents and how does interbank competition affect these rents?*

The first question is addressed by Carletti within the context of the theoretical literature. She notes that the standard view is that competition reduces banks' rents, thereby reducing their charter values and inviting greater recklessness in risk choices, which contributes to diminished bank stability. However, she concludes that the standard competitive paradigm is inappropriate for banking because asymmetric information, switching costs and network externalities create entry barriers of their own and facilitate the differentiation that counteracts to some effect the rent-sapping effect of competition (see Boot and Thakor (2000)). Indeed, it is possible to reach exactly the opposite conclusion from what is commonly believed, namely that competition can actually enhance bank stability (e.g. see Boyd and De Nicolo (2005)). It is therefore not even clear whether there is a tradeoff between competition and stability. Carletti notes that this remains an unsettled issue, particularly on the question of how regulation should be designed in the face of a potentially complex interplay between competition and banking stability (see Carletti, Cerasi, and Daltung(2007), and Degryse and Ongena(2007)).

The second question is examined by Degryse and Ongena within the context of the empirical literature. They conclude that an increase in interbank competition causes banks to rely more on fee income from stable relationships with customers (consistent with the theoretical prediction in Boot and Thakor (2000)), and switching costs (e.g. see Kim, Kliger and Vale (2003)) as well as regulatory protection are important sources of rents for banks.

The messages that emerge from this section suggest that much work remains to be done on the issue of how bank competition affects bank stability. A largely unexplored issue is the effect of interbank competition on the types of securities banks will be induced to design. How will bank competition, financial innovation incentives (security design) and banking stability interact?

To summarize, the six topics covered in this book have touched upon a wide range of issues pertaining to the designs of securities, institutions, trading mechanisms and markets, industry structure, and regulation. The dazzling array of insights emerging from these different yet connected strands of the literature have been nicely summarized by the section editors, and a host of unanswered questions for future research have been catalogued. We hope that this encourages bold new initiatives to tackle these important and exciting research questions.

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