

Introduction

Issues in financial contracting and financial system architecture

This issue of the Journal contains three papers presented at a conference in Berlin, “Bank Relationships, Credit Extension and the Macroeconomy,” jointly organized by DIW (the German Institute for Economic Research), The Federal Reserve Bank of Philadelphia, and the *Journal of Financial Intermediation* in June 2005. A subset of the papers presented at the conference made it through the Journal’s reviewing process and are included in this issue. It is an excellent set of papers devoted to the general topic of how different aspects of financial system architecture affect borrowers and the real sector, and the role of financial contracting as well as the impediments to explicit state-contingent contracting (or lack thereof) in the interactions between borrowers and savers. In this brief overview, I summarize the key questions asked in these papers and their most important insights, drawing out their connections, as well as implications for future research.

In their paper “Banks, financial markets and growth,” [Deidda and Fattouh \(2008\)](#) ask an important question: how does the transition from a bank-based financial system to a financial system with a bigger role for the financial market affect economic growth? One could argue that the most fundamental motivation for studying financial system architecture is to address questions of this sort. While the existing empirical evidence on this issue is mixed, [Deidda and Fattouh \(2008\)](#) present a novel way to explore this question theoretically and then provide supporting empirical evidence.

Deidda and Fattouh develop a model with many of the familiar features in models of financial system architecture (e.g., [Boot and Thakor, 1997](#)): banks both monitor and screen borrowers, whereas market finance does neither. The key is that screening by the bank involves a fixed screening cost, so it takes a sufficient amount of bank finance to make it privately optimal for the bank to screen. However, because of monitoring, bank finance is always more expensive than market finance, so borrowers seek to minimize bank finance. But as long as entrepreneurs cannot credibly disclose that they have borrowed from banks and will therefore be monitored, they will be unable to procure market financing, as in [Holmstrom and Tirole \(1997\)](#). This means that bank financing is the only viable financing source for entrepreneurs in such a setting. But now suppose the financial market imposes a disclosure law that results in entrepreneurs credibly signaling their financing source. This would enable entrepreneurs to minimize their use of bank financing, and there are conditions under which the amount of bank financing that would be obtained in equilibrium falls below the critical threshold that induces banks to screen. If screenings help to discriminate between projects of varying NPVs, then the loss of bank screening associated with the emergence of a financial market with (seemingly sensible) disclosure rules will actually

reduce economic growth. In other words, counter to conventional wisdom, the transition from a bank-based financial system to one that both banks and markets co-exist can be inimical to economic growth.

This conclusion is striking because the existing literature suggests that a better-developed financial system is good for growth and it matters little whether the development of the financial system comes via the development of banks or markets (e.g., Levine, 2002). Empirical support for this hypothesis appears in Beck and Levine (2002) and Demircug-Kunt and Levine (2001).

However, when Deidda and Fattouh (2008) test their novel predictions using the same data set used by Demircug-Kunt and Levine (2001), they find that while both bank and market development affect economic growth positively, the *growth impact of bank development is affected by market development*. The interaction term is empirically documented to be *negative*, providing support for the model's main prediction.

This paper not only is significant theoretically but has important policy implications. There has been a great deal of sentiment in support of the policy view that spurring capital market development in emerging markets is a key to encouraging economic growth. In part, one suspects that such policy prescriptions stem from observing the role of capital markets in economic growth in the US. What Deidda and Fattouh's (2008) analysis provides is a note of caution in contemplating such policy initiatives.

Berger et al. (2008) address a somewhat different issue. They ask: does the nature of bank ownership—foreign, state-owned, or private-domestic—matter for banking relationships? This is a question of import because permitting foreign bank entry into the financial systems of emerging economies is considered to be relevant to financial system development and growth. Using Indian banking data, the authors find three results: (1) Foreign banks establish relationships with relatively transparent borrowers. (2) Firms that borrow from foreign banks keep relationships with multiple banks and are more likely to maintain a larger number of banking relationships. (3) Firms that borrow from foreign banks are more likely to diversify across bank ownership types. Firms that borrow from state-owned banks behave very differently in that they are far less likely to have multiple banking relationships and do not diversify as much across bank ownership types.

The theoretical underpinning of the hypothesis being tested here is that foreign banks may have a lesser expertise than domestic banks in processing “soft” information, so they are more likely to transact with relatively transparent firms (Stein, 2002). Moreover, firms may view their foreign banking relationships as potentially less durable, so they may choose to diversify across many different banks. The authors explain that this may be because foreign banks have weaker ties to the nation and a greater number of alternative lending opportunities. Another possibility is that foreign banks may have a larger fraction of their funding accounted for by purchased money, which would then encourage these banks to engage in borrower relationships that do not need durability to be valuable (see Song and Thakor, *in press*).

The Berger et al. (2008) paper raises an interesting possibility with respect to the findings in the Deidda and Fattouh (2008) paper. In particular, if we assume that foreign banks screen and monitor less than domestic banks, then it follows that the effect identified by Deidda and Fattouh (2008) will be stronger in economies with fewer foreign banks. However, it has also been shown that government-owned banks are more politically inclined in their lending practices (e.g., Dinc, 2005), so while the effect of financial market development on banking development may be weaker in economies where state banks crowd out foreign banks, there may be other effects as well. Thus, future financial system architecture research may need to account explicitly for the nature of bank ownership.

Carletti et al. (2007), a conference paper that appeared already in last year's volume of the Journal, also examine multiple-bank relationships—firms borrowing from multiple banks—in a theoretical model. The question they ask is: why do we observe multiple-bank relationships when exclusive bank–firm relationships involving a single monitor avoid free-riding problems and duplicated monitoring? This is a fundamental question that needs to be addressed in light of the extensive empirical evidence that multiple-bank relationships are ubiquitous. The starting point of the Carletti et al. (2007) paper is that the diversification opportunities available to banks are limited. As in Holmstrom and Tirole (1997), there is “double” moral hazard. Banks need to monitor borrowers who may shirk in providing effort, and bank monitoring is costly and unobservable as well, so it is subject to moral hazard too. An imperfectly diversified bank's monitoring incentive depends on its level of equity, borrower profitability, and whether the bank is part of a coalition of multiple banks lending to the borrower. The key is that when the bank has “capacity constraints”—as represented by bounded diversification opportunities—the number of borrowers it can lend to is limited, unlike say in Ramakrishnan and Thakor (1984). Thus, lending to multiple borrowers facilitates diversification. The main benefit of diversification is that, conditional on fixed bank monitoring levels and a fixed repayment promise to depositors, the expected return to depositors—whose claim is *concave* in the *total* project payoff to the bank—is increasing in the extent to which the bank diversifies across (conditionally) i.i.d. projects. This, in turn, lowers the bank's repayment obligation to depositors and improves monitoring incentives. Countering this beneficial effect of diversification are the free-riding and monitoring-effort-duplication effects engendered by multiple banks lending to the same borrower. Hence, multiple-bank lending is efficient when the benefits of diversification more than offset the costs.

This model generates numerous interesting empirical implications. The clearest prediction is that since deposit financing is essential to generating a benefit for diversification, multiple-bank lending will be lower as banks accumulate more equity capital. There are other predictions as well that are more closely tied to specific features of the model that I do not go into here; these are clearly delineated in the paper.

The Carletti et al. (2007) paper offers an alternative explanation for the multiple-bank relationships documented by Berger et al. (2008). It is important to note, however, that the two papers deal with *different* directions of causality. In Berger et al. (2008), the hypothesis is that it is the borrowers who are choosing how many banks to establish relationships with, whereas in Carletti et al. (2007), it is the bank that determines how many borrowers to have relationships with. There is probably a set of conditions related to the competitive structure of the credit market such that one problem is the dual of the other, but this is not obvious since the Berger et al. (2008) analysis is primarily empirical. Nonetheless, the findings in Berger et al. (2008) suggest the need to explicitly incorporate the nature of bank ownership and the implied incentives in future analyses of the sort undertaken by Carletti et al. (2007).

In all of the papers, what precludes first-best credit allocation outcomes are informational frictions and other impediments to contracting. For example, if banks could cooperate in Carletti et al. (2007), diversification would be unambiguously welfare-enhancing because free-riding in monitoring can be costlessly eliminated. This is simply an example of the more general observation that informational frictions can impede attainment of the first best. In fact, this argument has been made specifically and forcefully in the context of the real estate market. Bernanke and Gertler (1989) argue that the friction generated by borrowers being unable to credibly commit to repay their loans means that borrowing must be secured, so that any decline in real estate prices lowers collateral values and diminishes the external financing available to borrowers. This lowers

real estate prices even further (see also Stein, 1995), suggesting that financial frictions can act as a “financial decelerator.”

In his paper “Collateral, credit history, and the financial decelerator,” Elul (2008) turns this intuition on its head. He asks: under what conditions can financial frictions act as a *stabilizing* force in the housing market?

Using an ingenious argument, Elul (2008) shows that when two frictions are present—borrowers cannot credibly precommit to repaying loans and there is adverse selection in the sense that lenders know less than borrowers about borrowers’ propensities to default—they may combine to serve as a *stabilizing* force. The key to Elul’s analysis is that borrowing constraints are treated as *endogenous*, rather than exogenous as in the previous analysis. Thus, borrowers are allowed to strategically choose to default. In the standard analysis, when housing prices fall, (exogenously specified) borrowing constraints become tighter, reducing borrowing and exacerbating the price decline. In Elul’s model, when housing prices fall, some borrowers may choose to strategically default, leaving them with more income than if they had repaid their loans. This permits more current spending on housing, counteracting the price decline and stabilizing prices.

This approach generates a number of empirical predictions. The clearest prediction is that the response of house prices to demand shocks should be larger when house prices are higher. Strategic default at low prices dampens the equilibrium price response to demand shocks.

Elul’s analysis has intriguing implications. For example, if we believe that adverse selection and other frictions are more severe in emerging economies than in well-developed ones, then the analysis implies that housing prices may be driven less by demand shocks in emerging economies. Moreover, his analysis also suggests that we need to include additional features of financial contracts, such as collateral and strategic default, into analyses of financial system architecture. In addition, the Carletti et al. (2007) analysis suggests the importance of including bank equity. Cross-sectional heterogeneity in these aspects of the contracting environment may affect the interaction of banks and markets (see Song and Thakor, 2007, for a start).

In closing, the papers in this issue not only have generated interesting new results, but also have raised exciting new questions for future research. As we address these questions, we will add much more granularity to our understanding of financial system architecture and the myriad ways in which it affects economic growth.

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