

RESEARCH STATEMENT

January 25, 2008

1. Overview of Research

Most of my research stems from the idea that the cost of external funds for firms and individuals is frequently higher than the benchmark cost obtained in a neoclassical (Modigliani and Miller) framework. This external financing premium can arise, for example, from agency problems and asymmetric information between borrowers and lenders. In an international context, the cost of external funds reflects how efficient the legal and institutional environment is in protecting outside investors (La Porta et al., 1997, 1998). Costly external finance creates a link between financing frictions and real investment decisions, and thus establishes a channel through which financial variables can affect economic growth and welfare.

The main focus of my research is to characterize the ways in which costly external finance affects firms' financial and investment policies and shapes the ownership structures and contractual arrangements that we observe in different parts of the world. My research has contributed both to the identification of the effects of financing frictions in real and financial outcomes, and also to the understanding of the broad consequences of such frictions for the economy as a whole.

There is substantial evidence that in many countries the financial system does an imperfect job of allocating capital across the economy's investment opportunities. These capital allocation frictions arise partly from the financing frictions discussed above. For example, in environments with poor investor protection firms may not be able to fully undertake their investment opportunities due to the lack of cheap external finance. However, firms are not passive observers of such problems. Rather, they can try to overcome the limitations of their home countries' institutional environments by tailoring their ownership structures and financial policies (among others) to their particular environment.

In a series of papers on **international corporate finance**, I apply this particular idea to investigate the prevalence of structures such as business groups and pyramids, and their effect on economic welfare. One of the contributions of my research is to show that a pyramidal business group can be understood as a mechanism to maximize the value of internal funds in environments with poor investor protection (and thus costly external finance). My research also contributes to the understanding of the welfare effects of business groups and the accumulation of internal funds. It is not surprising that internal funds and business groups' internal capital markets can be valuable for firms in poor investor protection countries. However, my research has also gone a step further to examine the implications that these variables have on the efficiency of *equilibrium* capital allocation. I show that, while individual firms might be better off by using internal funds and internal capital markets to avoid raising external capital, these same actions might actually decrease the efficiency of economy-wide capital allocation in poor investor protection countries. The novel and surprising results that arise from this research are entirely due to its focus on *general*, as opposed to partial equilibrium effects, a focus that is relatively unique in theoretical corporate finance.

While the importance of costly external finance in explaining cross-country variation in investment efficiency is probably uncontroversial, the same cannot be said of the role of costly external finance in explaining corporate behavior in the US (see Hubbard, 1998,

and Stein, 2003, for broad surveys of this literature). Because US capital markets are more developed, US firms should be better able to overcome the types of frictions that plague allocative efficiency in other countries. On the other hand, firms are heterogeneous and thus it is possible that financial frictions still matter for those that might have more difficulty in accessing cheap external funds, such as small and young firms. Building on this argument, the literature has focused on cross-sectional comparisons between groups of firms to identify the effect of financing frictions on corporate policies (i.e., Fazzari et al., 1988). Nevertheless, recent literature has posed challenges to the interpretation of standard tests of financial constraints.

In a series of papers on **financial constraints**, I investigate theoretically and empirically the implications of costly external finance for firms' investment and financial policies. My main contributions to this literature have been two-fold. First, I have used new theoretical insights to design empirical tests of financial constraints that sidestep some of the problems pointed out by existing literature, such as the Kaplan and Zingales (1997) critique, and problems stemming from unobservable variation in investment opportunities. In doing that, my research has provided fresh evidence that financing frictions do affect investment and financing decisions of an important fraction of US firms. Second, my research has helped shift the focus of research in financing constraints from corporate investment (capital expenditures, R&D, etc.) to corporate *financial* policies (i.e., cash and debt management). A key insight that guides my research on financial constraints is that a firm facing costly external financing will alter its *current* financial policy to facilitate *future* investments. I believe this is one way to operationalize the idea of financial flexibility, which is one of the most important managerial considerations when setting corporate financial policy (see the survey evidence in Graham and Harvey, 2001).

More recently, I have explored a different dimension in which financing frictions manifest themselves, namely **financial distress** costs. In standard neoclassical models, corporate bankruptcy cannot lead to substantial losses of true economic value. Indeed, a standard application of Coase's theorem suggests that if such losses are large, then creditors, managers and other claimants should try to negotiate them down. However, the real world of financial contracting does not conform well to this neo-classical paradigm. For example, the same frictions that increase the cost of external funds in normal times can be exacerbated in times of financial distress, making it harder for distressed firms to raise fresh external funds, and possibly affecting investments and other corporate decisions.

Despite the plausibility of such distress costs, whether they are high enough to matter for corporate valuation practices and capital structure decisions is hotly debated (i.e., Asquith et al., 1994, Opler and Titman, 1994, and Andrade and Kaplan, 1998). My research in this area has contributed to a better understanding of the magnitude of financial distress costs. I show that the present value of future distress costs depends on risk premia, since financial distress is more likely to happen in bad times. The magnitude of this risk premium is quite large, and can help understand the observation that US firms appear to use debt conservatively (Graham, 2000).

Besides this main thread of research on the broad implications of financing frictions, I have also pursued some research on **corporate governance and managerial decision-making**. My interest in this area stems from my days as a PhD student in the economics department at the University of Chicago. During my PhD, I was also interested in political economy and wrote an empirical paper relating concentration of decision-making at the country level (i.e., democracy) to variability in economic growth.

This research has led me to pose a similar question in a corporate context; that is, how does the concentration of power in the hands of the CEO affect corporate performance? In turn, I became interested in a particular dimension that is associated with CEO power, namely whether the CEO is also the founder of the firm. This research goes beyond standard agency models to derive empirical implications, drawing also on management and organizational economics. It also provides results that question standard views about CEO power. Namely, CEO autocracy is not necessarily bad for firm performance (in fact, founder-managed firms are more valuable than other, similar firms), but it does lead to greater variability in performance, much like the cross-country evidence of my earlier paper.

2. External Recognition

My research has achieved a fair amount of recognition. I was made a research fellow of the NBER in 2005. I won the Jensen Prize for second Best Paper in Corporate Finance and Organizations at the Journal of Financial Economics in 2005, and nominations for the Journal of Finance's Brattle Prize in 2004 and 2008. In addition, three of my papers have won prizes at NYU. As of January 2008, I have 94 total citations for my papers at the Web of Science. This citation count does not include citations in forthcoming papers and books.

I have presented my work at all the major finance meetings, including the NBER, AFA and WFA meetings. I have given more than 50 seminars at research universities, including MIT, Wharton, Princeton, Columbia and Chicago. I am frequently called upon to discuss papers and chair sessions at major conferences, such as the AFA and the WFA, and I have been asked to give a keynote lecture at the 2006 meeting of the European Finance Association. I am an active ad hoc reviewer for the major finance and economics journals, receiving more than twenty papers a year.

While it is difficult to assess accurately, it seems that my papers are now widely used in PhD programs throughout top schools in the US and Europe, including Stanford (Ulrike Malmendier), Chicago (Steve Kaplan), Wharton (Joao Gomes), NYU (Eli Ofek), Purdue (David Denis), Texas-Austin (Jay Hartzell) and others. My work has also influenced policy makers. For instance, my work on financial constraints and cash policy has been prominently cited in a recent speech by FED Board Governor Kevin M. Warsh at the American Enterprise Institute, Washington, D.C., on July 18, 2006 (*Corporate Cash Balances and Economic Activity*), and also by the President of the European Central Bank, Jean-Claude Trichet, in a welcome address given at a recent ECB Workshop in Frankfurt am Main on May 15, 2006 (*Corporate Finance and Monetary Policy*).

3. Research Contributions

3.1. International Corporate Finance

In a series of three published papers, Daniel Wolfenzon and I investigate some of the ownership structures and contractual arrangements that firms use to mitigate the effect of underdeveloped financial markets on their decisions and performance. We are interested both in understanding the emergence of structures such as business groups and pyramids, and also in their effect on the efficiency of economy-wide capital allocation.

The first premise behind our work is that firms should try to adapt their ownership structures and financial policies to overcome the distortions imposed by external financing costs. These distortions may take the form of ex ante distortions in investment

levels and in the composition of investment, and also ex post costs associated with the inability to commit not to expropriate outside investors (see, e.g., Shleifer and Wolfenzon, 2002). Firms' optimal responses to these distortions may include saving more internal funds (Pinkowitz et al., 2003), and forming business groups that can mitigate external financing frictions by allocating capital internally (Hoshi et al., 1991). However, the second premise that guides our work is that we also need to think about the effects of these firm-level adaptations on the efficiency of *economy-wide* capital allocation. For example, can the concentration of capital in business groups inhibit the growth of small independent firms that lie outside the business group?

Our most recent published paper, entitled [A Theory of Pyramidal Ownership and Family Business Groups](#), argues that a pyramidal business group can be understood as an optimal adaptation to an environment of poor investor protection. Pyramidal business groups, whereby a controlling family owns a number of firms through pyramidal ownership chains, are very prevalent throughout the world (see, e.g., La Porta et al, 1999). Despite their prevalence, pyramidal business groups are poorly understood. The literature argues that pyramids arise to separate ownership and control, and to ensure expropriation of minority shareholders. However, this argument is inconsistent with recent empirical findings showing that families sometimes hold very large cash flow stakes in firms they own through pyramids (see, e.g., Franks and Mayer, 2001). Furthermore, the traditional story ignores the fact that, in many countries, families can use dual-class shares to separate ownership and control.

Our model solves both problems. We argue that a pyramid allows the controlling family to access all retained earnings of a firm it already controls to invest in new firms. This dimension helps differentiate pyramids from dual-class shares. In addition, the family has an incentive to maximize its ownership in new firms to minimize ex post diversion. It does so by using retained earnings from existing firms to minimize the amount of new external finance that needs to be raised. The resulting cash flow stake in new firms could be small or large, depending on investment requirements, profitability and other parameters. We also provide additional empirical implications, some of which are consistent with existing research, while others are waiting to be tested. The paper was published in the *Journal of Finance* in 2006, and won the 2003/2004 Glucksman First Prize Award for Best Working Paper in Finance at Stern/NYU. It also won a nomination for the *Journal of Finance's* Brattle Prize in 2008.

Our second paper on business groups, entitled [Should Business Groups Be Dismantled? The Equilibrium Costs of Efficient Internal Capital Markets](#), looks at the effect of business groups on the efficiency of economy-wide capital allocation. The paper is motivated by a recent policy debate about the role of business groups in developing countries. While some argue that these organizations substitute for missing markets by allocating capital internally to member firms, others in favor of dismantling business groups argue, among other things, that they inhibit the growth of small independent firms by depriving these firms of finance.

Crucially, the existing literature on internal capital markets has little to say regarding these issues, because it considers conglomerates in isolation, abstracting from the effects that conglomeration might have on other firms in the economy. We consider such effects in our paper. We assume that conglomerates' internal capital markets allocate capital to divisions with the highest growth opportunities inside the conglomerate. However, we show that conglomerates can indeed dry up the external capital market when the allocation of scarce capital is constrained by poor investor protection. Our results suggest that one has to be careful when drawing welfare and policy implications

from studies of conglomerates in isolation. Even if conglomerates' internal capital markets are efficient, one cannot infer that the presence of conglomerates should be encouraged. This paper was published in the *Journal of Financial Economics* (2006).

Our third paper, entitled The Effect of External Finance on the Equilibrium Allocation of Capital, is very similar in spirit to the second one in that it also analyzes the efficiency of capital allocation in an equilibrium context. Our starting point is the observation that poor investor protection affects not only the flow of finance from households to firms, but also the flow of funds *across firms* (the capital reallocation process). Because of poor investor protection, firms' decisions are biased towards internal investment: firms will want to continue or invest more funds in their existing mediocre investment projects, even when other projects in the economy have higher productivity. It is efficient for firms to invest internally instead of reallocating capital, because capital reallocation entails external financing costs. A similar intuition explains the equilibrium costs of internal capital markets of the previous paper.

We show that an increase in firms' external financing requirements can improve the flow of capital across investment opportunities in the economy. The big picture of the results is similar to that of the conglomerates paper. Internal capital markets and internal financing are optimal firm responses to an environment of poor investor protection, because they economize on external financing costs associated with liquidation and capital reallocation. However, they might have unintended negative consequences for the efficiency of capital allocation, *precisely because of* poor investor protection. We also provide some empirical evidence that is consistent with the main implication of our external finance paper using Wurgler's (2000) measure of allocative efficiency and commodity endowments as instruments for external financing requirements (Easterly and Levine, 2003). This paper was published in the *Journal of Financial Economics* (2005). It won the 2005 JFE Jensen Prize for Best Paper in Corporate Finance and Organizations (Second Prize), and the 2002/2003 Fitch Runner-Up Award for Best Working Paper in Finance, at Stern/NYU.

3.2. Financial Constraints

In a series of four papers on financial constraints, Murillo Campello and I investigate theoretically and empirically the implications of costly external finance for firms' investment and financial policies.

The first goal of our research is to use theoretical models to design new empirical tests that can identify the effect of financing constraints on firm behavior. Whether financing constraints matter or not for investment and other policies is hotly debated by recent literature. Fazzari et al.'s (1988) original test of financial constraints involves a comparison of investment–cash flow sensitivities across samples of firms sorted on proxies for financing constraints (such as size, and age). The underlying hypothesis is that the sensitivity of investment to internal funds should increase with the wedge between the costs of internal and external finance—the *monotonicity hypothesis*. Thus, for example, finding that the sensitivity of investment to cash flow is higher for small firms than for large ones is evidence for the presence of financial constraints. There is a very large literature both in finance and economics that uses this methodology to identify and measure financial constraints.

Nevertheless, there have been two main sources of criticism to the Fazzari et al. test. First, Kaplan and Zingales (1997) argue that the monotonicity hypothesis is not a necessary property of optimal constrained investment, and report evidence that

contradicts Fazzari et al.'s findings. Second, a series of recent papers show that the results reported by Fazzari et al. and other papers can also be consistent with models in which financing is frictionless, because cash flow captures information about investment opportunities (the so-called *measurement error problem*). Given these criticisms, there has been a demand for new tests of financial constraints that can be implemented in a number of different contexts, and which are not subject to these two criticisms. Trying to fulfill this demand has been one of the goals of our research on financial constraints.

The second goal of our research has been to consider not only the traditional relationship between financing constraints and investment, but also aspects of the firm's financial policy that touch upon cash, external financing and derivatives. Our research on financial constraints and financial policy is guided by one unifying insight: that a firm facing costly external financing will alter its *current* financial policy to facilitate *future* investments. It is somewhat surprising that the literature on financial policies has not emphasized this idea a lot more, especially given the survey evidence that managers consider financial flexibility one of the key considerations in setting corporate financial policy (Graham and Harvey, 2001). Our research aims at bringing this idea to the forefront of the debate on corporate financing practices.¹

My earliest paper, entitled Financial Constraints and Investment–Cash Flow Sensitivities: New Research Directions, is the first attempt by Murillo Campello and me to use theory to design new empirical tests of financing constraints. Still focusing on investment, we develop a theory explaining the relationship between investment and cash flow when firms face credit quantity constraints. The specific goal of this paper is to shed some light on the Fazzari et al. vs. Kaplan and Zingales debate. This paper set the stage for our future work and inspired our most recently published work, entitled Financial Constraints, Asset Tangibility and Corporate Investment (published in the *Review of Financial Studies* in 2007). Basically, we realized that the argument that we started developing in our first paper could help address *both* criticisms of standard tests of financial constraints.

In the paper, we use the link between tangibility and constrained investment to formulate a test of financial constraints based on a differences-in-differences approach. We compare the effect of tangibility on investment–cash flow sensitivities across different regimes of financial constraints. The underlying idea is that tangibility affects investment through its effect on debt capacity, but *only* if firms are financially constrained. In addition, we argue that increases in tangibility *increase* investment–cash flow sensitivities for constrained firms. In other words, tangible assets relax financial constraints through their effect on debt capacity, and this relaxation in constraints *increases* investment–cash flow sensitivities. Our argument explicitly incorporates the Kaplan and Zingales suggestion that investment–cash flow sensitivities are not monotonically related to the degree of financing constraints. In contrast to Kaplan and Zingales, however, and in accordance with Fazzari et al., we argue that investment–cash flow sensitivities *can* be used to gauge the effect of financing frictions on investment. Our approach also sidesteps the measurement error problem. In the paper, we provide empirical evidence that, unlike standard Fazzari et al. results, our findings cannot be reconciled by models in which financing is frictionless.

Crocker Liu, Murillo and I have applied a similar idea to the housing market. In our empirical paper, entitled The Financial Accelerator: Evidence from International Housing

¹ Other papers exploring the link between financial constraints and financial policies are Froot et al. (1993), Faulkender and Petersen (2006) and Sufi (2006).

Markets (*Review of Finance*, 2006), we use international variation in loan-to-value (LTV) ratios to show that housing prices and the demand for new mortgages are more sensitive to income shocks in countries with higher LTV ratios. The role of the LTV ratio is similar to that of tangibility in the previously discussed paper. Among agents whose housing demand is constrained by the availability of collateral, those who can borrow against a larger fraction of their housing value (achieve higher LTV ratios) have more procyclical debt capacity. This paper gives evidence that financial constraints amplify fluctuations in asset prices and credit demand in an important market. The paper won the Goldman Sachs Prize for the best paper published in the *Review of Finance* during 2006.

Our first paper that focuses on financial policy is a joint effort with Michael Weisbach, entitled The Cash Flow Sensitivity of Cash. The literature on financial constraints mostly ignores the link between constraints and financial policies. However, this link is arguably more direct and unambiguous than the link to investment that was previously explored. The reason why the measurement error problem is so important for investment-based tests of financial constraints is that, under the alternative hypothesis of frictionless markets, we still expect investment and cash flow to be correlated. To find evidence of frictions in this correlation, it is critical that we effectively control for the information that cash flow might contain about future investment opportunities.

Nevertheless, tests based on cash policy are not subject to the same problem. In a frictionless world, the optimal level of cash balances is indeterminate (a Modigliani and Miller-type result). Based on this idea, we replace investment with changes in the stock of cash in regressions that are otherwise identical to those in Fazzari et al. (1988). The regressions are motivated by a model of liquidity demand that shows that constrained firms should display positive cash flow sensitivity of cash; that is, they save more when their cash flows are higher. This behavior is driven by an intertemporal optimization of investment. Firms save additional cash balances to equate the current and future marginal productivities of investment. Because we expect no such pattern if firms are financially unconstrained (even if investment opportunities are measured with error), the cash flow sensitivity of cash provides for a clean test of financial constraints. Our empirical results confirm the hypothesis that the cash flow sensitivity of cash is a theoretically justified and empirically useful variable that is correlated with a firm's ability to access capital markets. This paper was published in the *Journal of Finance* in 2004. It was also nominated for the *Journal of Finance's* Brattle Prize in 2004.

Following the cash paper, Murillo and I thought that the next natural step in our research was to more broadly consider the impact of financial constraints on financial structure. As in the first cash paper, our underlying motivation is to think of current financial policy as a way of securing the ability to undertake profitable investments in the future. This is the motivation for a paper jointly written with Viral Acharya: Is Cash Negative Debt? A Hedging Perspective on Corporate Financial Policies. We believe the choice between cash and debt is one of the most important choices that corporations face in practice, but the traditional capital structure literature has largely neglected this issue to focus on the determination of leverage ratios.

In the paper, which was published in the *Journal of Financial Intermediation* in 2007, we argue that the choice between cash and negative debt (which can be thought of as additional debt capacity) does affect future investment patterns of constrained firms. The essence of the argument is that different combinations of cash and debt (keeping net leverage constant) imply different distributions of investment across future states of the world. In other words, firms can use (negative) debt and cash to transfer financing

capacity across future states. Our results suggest that there is an important hedging component to standard financial policies such as cash and debt management. While the existing theoretical literature characterizes hedging in general theoretical terms (see, e.g., Smith and Stulz, 1985, and Froot et al., 1993), most of the empirical implementations of hedging arguments have focused on derivatives. Our paper suggests that firms can manage their cash and debt policies to mimic the outcomes that they could achieve with the use of derivatives, *if derivatives were available*. However, derivatives can only hedge risks that are easily marketable (such as those associated with commodity prices). This creates a role for alternative operational and financial hedges (such as cash and debt). The idea that corporate financial policy has an important hedging dimension has additional implications that we plan to pursue in further research.

3.3. Financial Distress

I have also explored in my research a different dimension in which financing frictions manifest themselves, namely, financial distress costs. While most financial economists would agree that financial distress is potentially costly for firms, there is little agreement as to how important these costs are for valuation and capital structure decisions. In particular, existing literature argues that the NPV of financial distress costs is probably not large enough to balance tax benefits of debt, and consequently that US firms could be too conservative in their use of debt (Graham, 2000).

In the paper entitled The Risk-Adjusted Cost of Financial Distress, Thomas Philippon and I revisit the valuation of financial distress costs. The most important innovation of our paper is that we estimate the impact of risk premia in the NPV of distress costs. Risk premia are potentially relevant for distress costs because financial distress tends to happen in bad times. In contrast, existing comparisons between tax benefits of debt and distress costs usually assume risk neutrality (see, e.g., Graham, 2000, and Molina, 2005). The key question to answer is, how large is this risk premium?

Thomas and I were lucky enough to be able to draw on a recent and growing credit risk literature to answer this question. We use bond prices to estimate the *risk-adjusted* probability of financial distress, under the assumption that distress states and default states coincide. Given an estimate for the losses in value given distress (such as those provided by Andrade and Kaplan, 1998), we can provide a risk-adjusted valuation of distress costs. One of the basic findings of existing asset pricing research is that asset risk premia appear to be large, both for stocks and bonds. Due to this large risk premium, we find that risk-adjusted distress costs can be as large as the marginal tax benefits of debt estimated by Graham (2000). The conclusion is that debt conservatism might, in part, be due to a large market price of credit risk. Our paper was published at the *Journal of Finance* in 2007 as the lead article. It also won the 2005/2006 Glucksman Prize Honorable Mention for Best Working Paper in Finance at Stern/NYU.

3.4. Corporate Governance and Decision-Making

I have also done some work on the effects of the distribution of power inside firms and their implications for governance and performance. This work is co-authored with Renee Adams and Daniel Ferreira. Daniel Ferreira and I had a common interest on the differences between autocratic and democratic decision-making systems from our times as PhD students in the economics department in Chicago, which generated our paper Democracy and the Variability of Economic Performance (the lead article in the November 2002 issue of *Economics and Politics*).

The first paper that Renee, Daniel and I wrote is directly related to our previous research in political economy. In the paper, entitled Powerful CEOs and Their Impact on Corporate Performance and published in the *Review of Financial Studies* (2005), we argue that powerful CEOs should have an impact on firm performance that is similar to that of an autocrat on a country's economic performance: firms whose CEOs have more decision-making power ("autocratic CEOs") should experience more variability in firm performance. Using firm-level characteristics of the executive office (such as whether the CEO is the founder of the firm), we develop a proxy for the CEO's power to influence decisions. Our results suggest that more powerful CEOs do have a greater impact on corporate decision-making, producing greater variability in the realized outcomes of these decisions. In other words, firms with autocratic CEOs have both the best and the worst performances in our sample. Our result contributes to a recent literature in finance and economics on the role of managerial characteristics and managerial biases in shaping corporate policies and performance. We suggest that in order to identify the effect of managers on performance, one should also consider the distribution of power within firms. Decisions and performance depend on the interaction between executive characteristics and organizational variables, and not on characteristics alone.

One of our measures of CEO power is whether the current CEO is the founder of the firm. We were able to find very good instruments for this variable, which we use in the *Review of Financial Studies* paper and also in the paper entitled Understanding the Relationship Between Founder-CEOs and Corporate Performance. There is some evidence in the literature showing a positive correlation between CEO-founders and firm performance, but there are also papers suggesting that founders might have a negative effect on firm performance, most notoriously, the study on founder deaths by Johnson et al. (1985). Using our instruments, we find a *positive, causal* effect of founder-CEOs on firm performance, and also find that good firm performance reduces the likelihood that the founder retains the CEO title. These results paint a very different picture of corporate founders than the one from traditional agency literature. Rather than being entrenched and detrimental to firm performance, founders appear to add substantial value and then wait for good times to pass the baton to their successors.

4. Future Research

Rather than describing the details of specific research projects, let me outline some major themes that will characterize my research going forward.

Despite recent advances towards an understanding of the role of business groups for corporate finance and economic development, there is still much to be done. In particular, there has been little empirical work examining the determinants of group structure. Existing empirical literature on business groups and pyramids takes group membership and ownership structure as exogenous, and examines differences in performance between group and non-group firms. This limitation is associated with a lack of adequate data to analyze the formation of business groups. Indeed, most datasets that have been used in the literature lack a time series dimension that allows the researcher to observe firms being added and dropped from business groups over time. For that purpose, I am in the process of assembling time series data on the structure of the major business groups in Korea. The final goal is to use these data to advance our understanding of the determinants of group structure and their effect on corporate performance.

More broadly, I continue to be interested in studying the structure of financial markets in less developed, emerging economies. In particular, we still know little about financial

structure and corporate financing patterns in China. I am in the process of assembling data on the Chinese banking and corporate sector to help us understand the role that banks play in Chinese corporate financing, and the determinants of credit allocation and credit risk assessment by Chinese banks.

A major goal of my research is to explore the link between a firm's current financial policy and the need to finance future investments. As some of my previous research has shown, the associated financial policy choices cannot be easily summarized in a choice of leverage ratio, as emphasized by existing capital structure literature. They also involve the optimization of cash balances, and potentially several other policy choices, such as lines of credit, derivatives, payout policy and the choice of firm financial portfolios (i.e., in which types of financial assets should a firm invest its savings). In my future research I would like to develop an integrated theory of financial policy that explains financing choices from this perspective. Ultimately, we might be able to further our understanding of all firm financing choices under this hedging-like perspective.