

## Research Statement

Per the University guidelines for this statement, I begin with a brief narrative of my career. After graduating with a Ph.D. in Economics from the University of St. Gallen, Switzerland, in 1997, I pursued a Habilitation—a post-doctoral degree commonly required to become a full professor in Germany—at the University of Mannheim, Germany, under the supervision of Prof. Martin Hellwig. In the fall of 2001, short of finishing my Habilitation, I moved to New York University, where I began my career as an Assistant Professor of Finance at the Stern School of Business. I finished my Habilitation in 2003, being awarded the *venia legendi* in Economics—the entitlement to teach Economics at a German university.

### Optimal Contracts in Corporate Finance and Corporate Governance

Optimal contracting in the setting of a particular corporate finance or governance problem is the theme of seven of my recent papers. In some cases, the focus is on the optimal contract itself. In other cases, the focus is on the broader implications of contracting inefficiencies, e.g., for organizational or market structure, or to explain empirical regularities. I shall describe these seven papers first.

In “Internal vs. External Financing: An Optimal Contracting Approach” (Journal of Finance, 2003), Roman Inderst and I examine the conditions under which different projects should be optimally combined under one roof, so that corporate headquarters raises external finance on behalf of multiple projects (“centralized borrowing”). The alternative is to have each project raise funds separately on the external capital market as a stand-alone entity (“decentralized borrowing”). We use the incomplete contracts model by Bolton and Scharfstein (American Economic Review, 1990) as our base model. In this model, the mechanism allowing firms to raise external finance is the threat by investors to terminate future funding. Centralized borrowing has both costs and benefits. On the benefits side, corporate headquarters can use excess liquidity from high-cash flow projects to “buy” continuation rights for low-cash flow projects that would otherwise be inefficiently terminated. On the costs side, headquarters may pool cash flows from several projects and self-finance follow-up investments without having to return to the external capital market. This weakens the investors’ termination threat, thus tightening financing constraints *ex ante*. In equilibrium, projects with a high

likelihood of high cash flows optimally remain stand-alone firms, as these projects are likely to generate large internal funds allowing headquarters to self-finance follow-up investments. Accordingly, the model provides a theoretical foundation for the “conglomerate discount,” arguing that the discount may be the result of optimal self-selection by poorly performing projects into conglomerate firms.

In “The Effect of Capital Market Characteristics on the Value of Start-Up Firms” (Journal of Financial Economics, 2004), Roman Inderst and I consider a double-sided moral hazard problem between an entrepreneur and a venture capitalist. The Pareto frontier is strictly concave, implying that there exists a uniquely optimal second-best sharing rule that trades off the costs and benefits of the two parties’ efforts. In practice, equity shares are determined by bargaining, however, implying that the output produced in the venture will generally be lower than under the second-best optimal sharing rule. (Lump-sum transfer payments ameliorate the problem only partly, as it is assumed that the entrepreneur has no wealth.) To close the model, we embed the contracting-cum-bargaining problem in a stationary search market in which the relative scarcity—i.e., the relative demand and supply—of venture capital endogenously determines the venture capitalist’s and entrepreneur’s outside options in the bargaining, and thus their respective equity shares and the output produced in the venture. The relative demand and supply of venture capital, in turn, is endogenous and depends on the profitability of investments, entry costs, and the transparency of the venture capital market. We analyze both the short- and long-run (i.e., after new entry) effects of shifts in the relative demand and supply of venture capital for the pricing, contracting, and overall output created in new ventures, which provides us, inter alia, with a stylized picture of the Internet boom and bust periods. The model also makes empirical predictions regarding the effects of changes in investment profitability, entry costs, and capital market transparency on the supply of venture capital, the pre- and post-money valuations in new ventures, the success likelihood of projects, and the overall value created in new ventures.

In “Informed Lending and Security Design” (Journal of Finance, forthcoming), Roman Inderst and I examine the decisions of lenders to finance projects after screening them. Screening generates an informative, albeit private, signal on which the lender bases her accept or reject decision. As the lender does not internalize the full costs and benefits from the project, the decision is generally inefficient. Specifically, the lender may be “too conservative,” in which case she rejects all negative- but also some positive-NPV projects. Alternatively, the lender may be “too aggressive,” in which case she accepts all

positive- but also some negative-NPV projects. Using an optimal contracting framework with a continuum of cash flows, we derive the optimal security that minimizes the inefficiency associated with the lender's credit decision. Unlike most existing security design models, the objective is thus to mitigate an incentive problem on the part of the *lender*. The uniquely optimal security when the lender is too conservative is standard debt. Standard debt maximizes the lender's payoff from financing low-NPV projects, thus maximizing her payoff from precisely those projects that she inefficiently rejects. By the same token, the uniquely optimal security when the lender is too aggressive is levered equity. Levered equity minimizes the lender's payoff from financing low-NPV projects, thus minimizing her payoff from precisely those projects that she inefficiently accepts. Whether the lender is too conservative or aggressive is endogenous and depends on observable project characteristics. In equilibrium, projects that are quite likely to break even are financed with debt, while less profitable projects are financed with equity.

In a follow-up paper with Roman Inderst, "[A Lender-Based Theory of Collateral](#)" (Journal of Financial Economics, forthcoming), we consider a similar setting as above. Unlike above, however, the focus is not on optimal security design—the follow-up model has only two cash flows—but on the role of collateral. The paper develops a novel theory of collateral that, unlike existing theories, is not based on borrower moral hazard or adverse selection. Applying the same logic as above, collateral is optimal when the lender is too conservative, because it improves the lender's payoff from precisely those low-NPV projects that she inefficiently rejects. If the borrower can pledge sufficient collateral, the lender's credit decision may become first-best optimal. (It is not optimal to fully insure the lender against all cash-flow risk, however, for she would then either accept or reject *all* projects.) A main focus of the paper is on deriving testable implications that help to distinguish our lender-based theory of collateral from theories based on borrower moral hazard or adverse selection. (In a recent empirical study, Jiménez, Fumás, and Saurina (unpublished, 2006) test our theory, finding support both for our theory and borrower-based theories.) For example, our model predicts that, controlling for observable borrower risk, collateralized loans are more likely to default *ex post*, which is consistent with the empirical evidence. Theories of collateral based on borrower moral hazard or adverse selection tend to make the opposite prediction, namely, that collateralized loans are safer, not riskier. The model also predicts that technological innovations that reduce the information advantage of local relationship lenders vis-à-vis transaction lenders, such as small business credit-scoring, lead to an increase in collateral requirements in local lending relationships.

In “[Financing A Portfolio of Projects](#)” (Review of Financial Studies, forthcoming), Roman Inderst, Felix Münnich, and I consider the implications of contractual provisions commonly found in venture partnership agreements that limit a venture capital fund’s initial capital and make it difficult to add more capital once the initial fund is raised. We consider an investor who initially finances two projects. Under “unconstrained finance,” the investor has sufficient (financial or human) capital to refinance both portfolio projects. In contrast, under “constrained finance,” the investor can only refinance one portfolio project. Refinancing by outside investors is more costly, or impossible, due to a lemons problem at the refinancing stage. As contracts are assumed to be incomplete, there is bargaining at the refinancing stage. Constrained finance has both costs and benefits. First, the investor’s stronger bargaining position at the refinancing stage may weaken entrepreneurs’ ex-ante incentives to exert effort. Moreover, as the investor can refinance at most one project, positive-NPV projects may not get refinanced. On the other hand, as projects must have not only a positive NPV at the refinancing stage, but one that is higher than that of competing portfolio projects, entrepreneurs’ incentives to exert effort may be improved. Effectively, the entrepreneurs compete in a tournament for scarce informed capital at the refinancing stage. For projects with low ex-ante success probabilities—the sort of projects typically financed by venture capitalists—the benefits of constrained finance are likely to outweigh the costs.

In two recent working papers, Roman Inderst and I examine optimal CEO and employee compensation. In “[CEO Compensation and Strategy Inertia](#),” we consider the joint optimal design of CEOs’ on-the-job compensation and severance pay in a general optimal contracting framework with a continuum of cash flows. The CEO privately observes a signal of the likely success of his strategy, while the board observes the signal only at a later time. If the signal is bad, the CEO may entrench himself by undertaking an irreversible investment that makes it suboptimal for the board to fire him. If the board cannot observe the signal, the CEO may entrench himself by simply misreporting the signal. While severance pay reduces the CEO’s incentives to become entrenched, it is costly: there is a one-to-one mapping between the CEO’s severance pay and his informational rents. The optimal CEO on-the-job compensation scheme minimizes the use of costly severance pay, thus minimizing the CEO’s rents. The functional form of the optimal on-the-job compensation scheme depends on the constraints imposed on the model—it may either be a high-powered bonus scheme or an option-like contract. Generally, however, the optimal CEO on-the-job compensation scheme shifts all of the CEO’s compensation into the highest cash-flow states. This minimizes the CEO’s *expected* on-the-job compensation—and thus his incentives to

become entrenched—after observing a bad signal, thus minimizing the use of costly severance pay. The model makes several empirical predictions, notably, that CEO on-the-job compensation and severance pay should move in the same direction. The model is also consistent with the observed increase in CEO compensation, severance pay, and CEO turnover during the past decades. In a recent empirical study of CEO severance agreements, Rusticus (unpublished, 2006) tests our theory, finding support both for our theory and alternative theories of severance pay.

In “Specific Human Capital and Broad-Based Incentive Pay,” Roman Inderst and I revisit the classic holdup problem associated with specific human-capital investments. The novel take of the paper—inspired by Lazear (unpublished, 2003) and Gibbons and Waldman (Journal of Labor Economics, 2006)—is that human capital may not only be firm-specific, but specific to the firm’s “business mix”: its unique combination of technology, products, markets, etc. If the firm changes its business mix, the value of the employees’ human capital erodes. Whether changing the current business mix is optimal depends on the state of nature, which is stochastic. To induce employees to invest in specific human capital, they must be promised an above-market wage. As long as the firm’s current business mix is sufficiently better than the next best alternative, there is no holdup problem: the threat of firing employees is not credible. But when the firm’s current business mix is only marginally better than the next best alternative, the firm may threaten to switch to the alternative. While such switching is inefficient, it allows the firm to renegotiate the employees’ above-market wage, as the threat of firing is now credible. The optimal employee compensation is to give employees broad-based incentive pay: it ties the firm’s expected “wage overhang” to the profitability of its current business mix, thus solving the twin task of inducing optimal business decisions and mitigating the holdup problem associated with specific human capital. The model predicts that broad-based incentive pay should be more prevalent in volatile industries, and that the more specific human capital is, the more employee compensation should be tied to firm risk. Both predictions are consistent with the empirical evidence.

### **Other Recent Work**

I have written two papers in the last few years that are not related to optimal contracting. In “Tender Offers and Leverage” (Quarterly Journal of Economics, 2004), Fausto Panunzi and I revisit the free-rider problem in tender offers discussed by Grossman and Hart (Bell Journal of Economics, 1980). In that paper, as well as in the

subsequent literature, it is assumed that the raider finances his bid with cash out of his own pocket. In practice, however, and in particular during the 1980s takeover wave, bids are often highly leveraged. Raiders would typically set up a shell company that issues debt, and, upon succeeding with the tender offer, combine the shell company with the target firm in a second-step merger. The debt issued by the shell company is backed by the combined firm's assets and cash flows. Importantly, that the shell company is indebted lowers the combined firm's post-takeover share value, reducing the incentives for target shareholders to hold out in the tender offer and mitigating the free-rider problem. In a sense, a leveraged two-step acquisition ("bootstrap acquisition") can implement an outcome similar to the dilution mechanism envisioned by Grossman and Hart, except that (i) it is consistent with the law and legal practice in the United States, and (ii) it has been widely used in practice. In addition to showing how the use of leverage can lubricate a takeover deal in a Grossman-Hart type setting with free-riding shareholders, the paper provides a discussion of the legality of bootstrap acquisitions, with a focus on the second-step merger. Besides, the paper analyzes the role of leverage in going-private transactions in which minority shareholders are cashed out ("freeze-out merger"), as well as the role of bankruptcy costs and defensive leveraged recapitalizations for takeover premiums and the efficiency of the market for corporate control.

In "Concentrated Ownership and Labor Relations," a recent empirical study with Thomas Philippon, we show that differences in the quality of labor relations across countries can explain cross-country differences in ownership concentration documented by La Porta, Lopez-de-Silanes, and Shleifer (Journal of Finance, 1999). We find that, controlling for differences in minority shareholder protection, countries with hostile labor relations tend to have more family ownership than countries with cooperative labor relations. Importantly, it is not just some aspect of labor relations that matters, but it is precisely the quality of labor relations: neither labor union power nor labor regulation are significant in our regressions. Neither are (other) measures of social capital, law enforcement, income inequality, private control benefits, and measures of a country's left-right political orientation. To establish causality, we argue that differences in the quality of labor relations across countries are, at least partly, the outcome of historical and cultural developments. In an intriguing book, Crouch (1993) shows that differences in the quality of labor relations among European countries can be attributed to political struggles between the emerging European liberal states and the Catholic church in the 19th and late 18th centuries. Indeed, when we instrument our survey measure of the quality of labor relations using either the fraction of Catholics or

Protestants in 1900, we find support for the hypothesis that the quality of labor relations has a causal effect on the extent of family ownership. In a second part of the paper, we use actual strike data instead of survey measures of the quality of labor relations. We find that differences in strike activity among Western countries in the 1960s can predict cross-country differences in ownership concentration thirty years later. We also examine the relation between strike activity and ownership concentration in Canada. Consistent with our previous findings, we find that strike activity is higher and ownership is more concentrated in French-Catholic Quebec than it is in the rest of Canada. Moreover, we find a markedly strong time-series correlation between strike activity and changes in ownership concentration in Canada during the second half of the 20th century.

### **Early Work in Contract Theory, Information Economics, and Organization Economics**

As described above, my recent work (and my work in the foreseeable future) is in the areas of applied corporate finance and corporate governance theory and, as exemplified in my most recent paper with Thomas Philippon, in empirical work in these areas. However, let me close by briefly discussing five early papers—written during my Ph.D. at the University of St. Gallen and my first three years at the University of Mannheim—which represent more theoretical contributions in the areas of contract theory, information economics, and organization economics.

In “The First-Best Sharing Rule in the Continuous-Time Principal-Agent Problem with Exponential Utility” (Journal of Economic Theory, 1998), I derive the optimal risk-sharing contract in the Holmström-Milgrom (Econometrica, 1987) continuous-time principal-agent model. Like the second-best optimal sharing rule under moral hazard derived by Holmström and Milgrom, the optimal risk-sharing contract is linear in aggregate output.

In “Asymptotic Efficiency in Dynamic Principal-Agent Problems” (Journal of Economic Theory, 2000), I show that for linear sharing rules to be optimal in the Holmström-Milgrom model, it must be the case that the agent chooses his effort continuously. If the agent can change his effort only in (arbitrarily small) discrete time intervals, then a solution no longer exists. In fact, the first-best outcome can then be approached

asymptotically with a sequence of Mirrlees-type step function contracts that punish the agent increasingly harder for ever lower outcomes.

In "Competitive Search Markets for Durable Goods" (Economic Theory, 2002), Roman Inderst and I consider a dynamic version of Akerlof's lemons problem in which buyers and sellers must engage in search, and where goods of different qualities are traded in different submarkets and at different prices. In equilibrium, high-quality goods are traded at a higher price but must circulate longer than low-quality goods, which implies that the time goods must circulate before they are sold serves as a sorting device.

In "Inside vs. Outside Ownership: A Political Theory of the Firm" (RAND Journal of Economics, 2001), Karl Wärneryd and I consider the distribution of surplus in a closed firm, such as a partnership. In the absence of complete contracts, the partners may engage in rent-seeking activities to tilt the division of surplus in their favor. For example, a partner may divert firm resources to finance private activities and claim that the money was used for legitimate business purposes. As rent-seeking activities are wasteful, the firm's objective is to minimize them. We derive conditions under which it is optimal for the partnership to go public, or more generally, to sell the firm to outside owners. While there are now two levels of wasteful distributional conflict—first, the outside owners must "fight" against the insiders (i.e., the former partners), and then the insiders fight over whatever they managed to retain in the first conflict—the total deadweight loss may be smaller than under the partnership structure. Intuitively, that the insiders must fight collectively against the outside owners creates a free-rider problem in rent-seeking, leading to low equilibrium levels of wasteful conflict.

In a companion paper with Roman Inderst and Karl Wärneryd, "Distributional Conflict in Organizations" (European Economic Review, forthcoming), we consider a similar setting as above. (While the paper is forthcoming now, it dates back to 1999.) Instead of focusing on outside ownership as a way to ameliorate distributional conflict, however, we focus on the optimal design of hierarchy. Contrary to what has been frequently argued in the literature, we show that multi-divisional organizations may involve lower deadweight costs from distributional conflict, despite having more rounds of conflict. The reason is similar to above, namely, organizing productive units into multiple divisions creates free-rider problems in rent-seeking. We apply this insight to capital budgeting procedures, corporate divestitures, and the transition from the "U-form" to the "M-form" organization by U.S. firms in the 1920s.



## **External Recognition of My Research**

As I have mentioned earlier in this statement, two of my theory papers—the one on collateral and the one on CEO compensation—have inspired empirical work by other researchers testing my theories. Moreover, my papers are included in Ph.D. syllabi at other schools, and I have had the opportunity to present my work at conferences and over 40 universities (see curriculum vitae). Some of my work is mentioned in textbooks. For example, my paper on internal vs. external financing is mentioned in Bolton and Dewatripont's Ph.D. level textbook on contract theory (MIT Press, 2005), and three of my papers are mentioned in Tirole's Ph.D. level textbook on corporate finance theory (Princeton University Press, 2006): the paper on informed lending and security design, the one on collateral, and the one on tender offers and leverage. (The last paper is discussed in some detail on pp. 433-434.) Finally, my work has been cited by other academics. As of July 2006, I have 104 citations in published or forthcoming articles and books, not including citations in working papers (unless they are forthcoming).

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