Corporate Finance, Incomplete Contracts, and Corporate Control

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This essay in celebration of Grossman and Hart (GH) (Grossman, S., and H. Oliver. 1986. "The Costs and Benefits of Ownership: A Theory of Vertical and Lateral Integration," 94 *Journal of Political Economy* 691–719.) discusses how the introduction of *incomplete contracts* has fundamentally changed economists' perspectives on corporate finance and control. Before GH, the dominant theory in corporate finance was the *tradeoff theory* pitting the tax advantages of debt (relative to equity) against bankruptcy costs. After GH, this theory has been enriched by the introduction of control considerations and investor protection issues. This essay assesses how our understanding of corporate finance has been improved as a result and where the incomplete contracts perspective has not yet been successfully applied. (*JEL* G30, G32, G33).

1. Introduction

It is a great pleasure to write this essay in celebration of Sandy Grossman's and Oliver Hart's classic 1986 article "The Costs and Benefits of Ownership: A Theory of Vertical and Lateral Integration." To appreciate the importance and novelty of their contribution, it is helpful to put it into context of their earlier research and the state of economic theory at the time. Among the hottest research topics in economic theory in the early 1980s were General Equilibrium (GE) theory, rational expectations, and information economics (mechanism design). Both Grossman and Hart had made several important contributions in these general areas. Hart's thesis was in the area of GE theory with incomplete markets, and in the early 1980s he was working on GE with imperfect competition. One central conceptual issue arising in GE with incomplete markets or imperfect competition is what the objective function of the firm is and to what extent there can be *shareholder unanimity*. Grossman and Hart had written several major articles on this topic. They had also made important contributions on *implicit labor contracts* and involuntary unemployment, and on a

general characterization of the principal–agent problem with *moral hazard*.

Much of this work appeared in the most prestigious journal at the time, *Econometrica* and achieved the highest standards of mathematical rigor and generality. Indeed, both Grossman and Hart were seen as belonging to an elite group of young theorists taking over the field of economic theory from the founding giants of mathematical economics, Kenneth Arrow, Frank Hahn, Gerard Debreu, and Robert Aumann. It is important to emphasize that there is virtually no hint in this earlier work of the ideas Grossman and Hart were about to develop in their 1986 article, which by all appearances is a complete break from their earlier research both methodologically and conceptually.

What explains this sharp break? What prompted Grossman and Hart to embark on this *path-breaking* endeavor? There are mainly two motivations. First, although Oliver Hart has long had a deep interest in Coase's (1937) theory of the firm and the early managerial theories of the firm of Penrose (1958), Baumol (1959), Cyert and March (1963), Williamson (1964), he was not persuaded by these *managerial perspectives* of the firm. He thought that as theories they failed because they were too detailed, descriptive, with too many different variables, to lend themselves as useful analytical tools. He was thinking of how to formulate an analytically simple theory of the firm.

Second, the ultimately unsatisfactory outcome of Grossman and Hart's work on the principal–agent problem (Grossman and Hart 1983) must have played an important role in prompting them to change direction. Why did this theory lead to an unsatisfactory outcome? After all, as a piece of mathematical analysis, this is a brilliant achievement solving in one swoop both a fundamental nonexistence problem in principal–agent problems, and a methodological flaw with the *first-order approach* pointed out by James Mirrlees (1974; 1999). The unsatisfactory outcome is that the theory in full generality yields no robust predictions on the shape of optimal incentive contracts, and it produces no robust comparative statics results.

In other words, the theory says that the shape of the optimal incentive contract a principal offers an agent is highly sensitive to the specific environment the parties find themselves in and is likely to be incredibly complex. Far more complex than anything one can see in reality, and far less robust than the fairly standard incentive contracts one typically observes. This concern is clearly voiced in the survey of Contract Theory by Oliver Hart and Bengt Holmstrom (1987) delivered at the World Congress of the Econometric Society of 1985:

> Indeed, the economic credibility of the contractual approach may be called into question when, as often happens, optimal contracts become monstrous state-contingent prescriptions.

How are such contracts written and enforced? (Hart and Holmstrom 1987: 74)

and

The main [concern] is its sensitivity to distributional assumptions. It manifests itself in an optimal sharing rule that is complex, responding to the slightest changes in the information content of the outcome x. Such "fine-tuning" appears unrealistic. In the real world incentive schemes do show variety, but not to the degree predicted by the basic theory. (Hart and Holmstrom 1987: 90)

Interestingly, in response to this critique the contract theory literature has taken two separate directions, which even today remain in competition, and confusingly, sometimes overlap. The first direction taken by Grossman and Hart (1986) is *pragmatic* and simply limits contractual complexity by imposing plausible exogenous contract enforcement constraints. This is the *incomplete contracts* approach they pioneered. The second direction is *fundamentalist* and seeks to endogenously derive simple and realistic optimal contracts—such as linear incentive contracts—from a complex dynamic programming problem in which the agent has a *rich* action set. This is the approach pioneered by the optimal security design literature following Townsend (1979) and by Holmstrom and Milgrom (1987) in the context of dynamic principal–agent problems.

Both approaches have been important in shaping corporate finance theory post Grossman and Hart (1986). The incomplete contracts approach, which Philippe Aghion and I embraced has allowed us to address issues of (contingent) control allocation and renegotiation that had been absent from previous discussions of corporate finance and financial contracting. My other work with David Scharfstein turns out to have contained both elements of the incomplete contracts approach and elements of the *limited commitment approach*, as subsequent work by DeMarzo and Fishman (2007) and DeMarzo and Sannikov (2006) has highlighted.

Before discussing the incomplete contracts literature in corporate finance, I will begin with a brief summary of the state of corporate finance theory pre-GH-1986. I will then turn to a discussion of some of the main themes of the corporate finance theory literature post-GH (1986) and evaluate the relative merits of the incomplete contracts and dynamic programing approaches. I shall argue that while both approaches have considerably improved our understanding of the dynamics of corporate finance, they both still lack operational and practical relevance. Unlike the tradeoff theory inherited from Modigliani and Miller—which has huge practical relevance, despite the fact that tax considerations may, paradoxically, not be first order in determining firms' financing choices—the more modern incentive and incomplete contracting approaches—which, arguably, focus on more first-order issues—have so far had a limited operational impact. That is, while incomplete contracting approaches capture

important dimensions of corporate financial decisions (as recent empirical research has revealed) they do not offer an operational methodology that can be used by practitioners. I will conclude by suggesting a way forward in closing this important gap, based on recent work of mine with Hui Chen and Neng Wang.

2. Modigliani and Miller and the Foundations of Corporate Finance

Modern corporate finance practice is founded on Modigliani and Miller's classic (1958) article on the cost of capital. In this article they lay out a methodology for valuing investments and argue that the value of a firm is independent of how the firm is financed. Economists have mostly focused on the famous Modigliani and Miller (MM) *irrelevance theorem,* stating that when capital markets are competitive and *efficient*, and when there are no taxes and bankruptcy costs, then the way a firm is financed is irrelevant. That is to say, under these conditions the value of a firm is independent of its liability structure, and depends only on the value of its assets. The MM irrelevance theorem, already implicit in Arrow and Debreu's (1954) proof of existence of a general competitive equilibrium, is a central proposition in general equilibrium theory, which pins down the objective function of a firm (and ensures shareholder unanimity).

In the real world, of course, some key conditions of the theorem are not met, in particular, the absence of taxes and bankruptcy costs. Therefore, the way firms are financed in practice is not irrelevant to their value. Nevertheless, the MM logic and their approach to determining a firm's cost of capital has still proved to be of enormous practical value. I am referring here to their famous *weighted average cost of capital* (WACC), and their approach to valuation based on discounting a firm's *free cash flow* using WACC. Combined with a suitable asset pricing model (most often in practice, the capital asset pricing model, CAPM), and correcting for taxes, the discounted cash flow approach using WACC is today the most widely used approach to valuation and corporate investments (see Graham and Harvey 2001).

This approach to valuation is closely linked with a simple theory of the optimal capital structure of the firm: The *tradeoff theory* (see Miller 1977; Brennan and Schwartz 1978; DeAngelo and Masulis 1980; Leland 1994), which is still widely accepted. According to this theory, debt is a cheaper source of capital for the firm after tax if one ignores bankruptcy costs. If it were not for deadweight bankruptcy costs, it would be efficient for the firm to finance itself only with debt. In the presence of bankruptcy costs, the optimum debt–equity ratio is determined by equating the marginal *tax shield benefit* of debt with the expected marginal *bankruptcy cost*.

The tradeoff theory assumes that the firm's cash flow is observable and verifiable, and that any promised debt repayment satisfying the firm's *limited liability* constraint is enforceable. Models of the tradeoff theory typically assume that Debt is a fixed claim (fixed coupon payments plus

principal), which is independent of the firm's realized earnings, and equity is a claim entitling the owners to the firm's free cash flow net of debt obligations. These models also typically assume that the (stochastic) evolution of the firm's cash flow is predetermined, and reduce the firm's financing problem to an optimal *allocation problem* of the firm's cash flow to the holders of debt and equity, the tax authorities and *nature* (in the form of deadweight costs of bankruptcy).

This is a highly reductive theory of corporate finance, which misses many key determinants of leverage. For example, US corporations extensively relied on debt financing in the 19th century, even though bankruptcy could be extremely costly and there were no tax shield benefits to offset expected bankruptcy costs. By the tradeoff theory, they should have stayed away from debt financing altogether. More generally, just as the CAPM, the tradeoff theory has found limited empirical support (see, e.g., MacKie-Mason 1990; Rajan and Zingales 1995). Yet, the tradeoff theory, especially in its "dynamic" version following Leland (1994), retains a central place in corporate finance, mainly because it offers the most operational approach available to the determination of the firm's optimal capital structure and to the valuation of risky debt. True, the Leland (1994) model *predicts* excessively high leverage ratios, but the more dynamic formulation in Goldstein et al. (2001) is able to predict reasonably accurate debt levels and credit spreads.

3. Grossman and Hart: Incomplete Contracts and Corporate Control

How has the corporate finance theory landscape changed post–GH (1986)? One central dimension missing from the MM approach to corporate finance discussed above is *ownership* and *control*. The MM approach remains silent on how a firm's cash flow is determined. It takes the cash flow as given and asks only how it should be allocated among different claimants and how it should be valued. In contrast, the incomplete contracts approach to corporate finance seeks to understand corporate control and how the exercise of control is affected by the firm's choice of financing.

This is a richer and significantly more complex theory than the tradeoff theory. I will argue that it has yielded important new conceptual insights, but that it has so far had only a marginal operational impact. As much as chief financial officers (CFOs) feel that they need to understand the MM approach to corporate finance, they have so far not shown much interest in the more modern theories of corporate finance that emphasize, adverse selection, moral hazard, incomplete contracts, and control. It is not that they are unaware of the importance of these issues, but that the theory so far has offered little operational guidance. There is little doubt that corporate decision makers understand the importance of incentives and control, but it is fair to say that for the most part, they have not relied on the finance theory literature on these issues to inform their decisions. Before

dealing with corporate finance following Grossman and Hart (1986), I must briefly discuss the *agency theories* of corporate finance, which provide a first analysis of the endogenous determination of a firm's cash flow.

3.1 Adverse Selection and Moral Hazard

Jensen and Meckling (1976) provide the first formal analysis of the incentive implications of a firm's choice of capital structure. They start from the Berle and Means (1932) observation that if the managers in control of a firm's operations own only a small fraction of the company's stock, they are likely to run the firm inefficiently, or at least not in shareholders' best interests. They then suggest an argument in favor of debt financing, which has later become the main justification for *leveraged buyouts* (LBOs): By financing a firm with debt, managers are able to retain concentrated equity ownership in the firm, and thus are incentivized to invest in future cash flows that enhance shareholder value. However, they continue, too much debt financing creates its own incentive problems, *excess risk taking*, so that the optimal leverage ratio for the firm trades off the moral hazard benefits in terms of better investment incentives for managers against the excess risk-taking incentives for shareholders of highly levered firms. Excess risk taking by highly levered firms, has, alas, become an all too familiar notion after the financial crisis of 2007–09.

Another familiar cost faced by highly indebted borrowers is *debt overhang*, a concept first formally analyzed by Myers (1977). Highly indebted borrowers facing the risk of financial distress will pass up valuable investment opportunities or even sell assets at fire sale prices. To reduce the risk of debt overhang, it may thus be desirable for firms not to borrow too much. Also, should a firm end up with too much debt on its books, it may be desirable for borrower and lender to renegotiate or *restructure* some of this debt. This latter observation provides the main economic justification for the existence of *bankruptcy reorganization* and *debt resolution* procedures.

Next to these theories of capital structure based on *moral hazard* problems, Myers and Majluf (1984) have proposed a powerful theory based on asymmetric information or *adverse selection*. They argue that the firm as an issuer of claims on the firm's cash flow has to overcome investors' suspicion that the firm may be trying to sell *overpriced claims*. This is easiest to do if the firm is able to issue *safe debt* (say, senior, short-term, collateralized, and debt-like *repos*), for issuer and investors alike ought to be able to easily value such a safe fixed-income instrument. If safe debt is unavailable the next easiest claim to value is risky debt, or possibly convertible debt, and the hardest claim to value may be equity. That is, equity may be the hardest claim to value as it may be the most *information sensitive claim*. Which is why, Myers and Majluf—under their *pecking order theory*—propose that equity claims should be issued only as a last resort and that firms should first use internally generated funds, then safe debt and then if needed risky debt.

These theories are an important advance over the simple tradeoff theory and considerably improve our understanding of corporate finance. However, it should come as no surprise that the pure agency theories of corporate finance encounter similar conceptual difficulties as the principal–agent theory *à* la Grossman–Hart (1983). Indeed, a central problem with agency theories of corporate finance, as Dybvig and Zender (1991) have emphasized, is that it is perfectly possible to separate the choice of capital structure of the firm from the problem of optimally incentivizing a firm's manager by directly designing an optimal compensation contract for the CEO. Under an optimal incentive contract, which admittedly may be very complex and sensitive to the special circumstances a firm finds itself in, it is possible to address directly all relevant moral hazard and adverse selection issues, so that capital structure choice again becomes irrelevant, or is determined by the tradeoff theory. In sum, the firm's capital structure is relevant for managerial incentives, and therefore also for corporate control, only if incentive and financial contracting is limited by enforcement constraints; that is, only if contracts are incomplete.

3.2 Incomplete Contracts and Corporate Control

3.2.1 Contingent Control Allocation.
This last observation is the starting point of the analysis in Aghion and Bolton (1992), which develops a theory of capital structure choice based on the problem of optimal allocation of corporate control. The analysis starts by assuming that not all actions available to a manager and not all states of the world are *describable* (ex ante) in a contract or *verifiable*. If some future action choices cannot be specified in a contract, this creates a problem of control, as some actions (and states of the world) left out of the initial contract will have to be determined ex post. This then further raises the question: Who is charged with taking these actions? In other words, who is in control? But, this is only the starting point, as the next question is why is it relevant at all who is in control? In our bilateral contracting problem with an *entrepreneur* and a *financier*, allocations of control to one or the other party would have to lead to different outcomes for control to matter at all. Different outcomes presuppose different objectives, which could neither be fully aligned through ex ante contracting nor through ex post renegotiation.

Our first approach to this second question was to assume that the entrepreneur and financier had *different beliefs* about which investments were preferable. Based on casual observation, we assumed that the entrepreneur was generally more *optimistic* about the success of risky investments than the financier. This difference of opinion combined with the assumption of limited liability (and limited wealth of the entrepreneur) naturally gives rise to conflicting objectives, which cannot be fully aligned through contracting. It also gives rise to a plausible *contingent control allocation*.

The entrepreneur seeks to keep control in the states of the world where the difference of opinion is largest; that is, where she is likely to be much more optimistic about success than the financier. And, she is willing to give up control in states of the world where differences of opinion are smaller. To the extent that differences of beliefs are likely to increase as the venture's prospects improve, this contingent control allocation could be implemented through debt financing, whereby the financier gains control in the event of default and otherwise the entrepreneur retains control. It could also be implemented through staged transfers of control under a venture capital (VC) contract.

As simple and plausible as this solution seemed to us, the contract theory community at the time was not ready to accept two departures from orthodoxy in the same paper: Incomplete contracts and differences of opinion. We received almost unanimous advice to change the model and do away with differences of opinion. So, instead of modeling differences in objectives arising from different beliefs, we modeled them as arising from the presence of *private benefits*: We assumed that the entrepreneur derives both financial returns and private benefits from the venture, while the investor derives only financial returns. In a way, this new model is more general, as differences of opinion can be mapped into financial returns and a particular form of "private benefits," but vice versa, it is not always possible to transform an objective function combining financial rewards plus private benefits into an objective function with no private benefits but differences of beliefs.

Our model delivers predictions on the separation of cash-flow and control rights that are consistent with common contractual clauses in VC contracts, as Kaplan and Stromberg's (2003) study of VC contracts revealed. It also delivers predictions on cash inventory management under investor control, showing that it may be optimal for the entrepreneur to accumulate cash reserves that may be used to induce the investor to choose an investment with high private benefits for the entrepreneur. Although these are valuable qualitative insights, the model remains in many ways too abstract and general to be an operationally useful analytical tool. Part of the difficulty lies in the somewhat vague notion of private benefits. The other difficulty is that the enforcement limits on financial contracts are exogenously imposed in a somewhat arbitrary fashion. There is also the conceptual difficulty revealed by Maskin and Tirole (1999) that when an action or state of the world is observable to the contracting parties, but not to a court (or judge) it can still be made verifiable through a suitable *revelation mechanism*.

3.2.2 Limited Commitment.   For all these reasons, it is not completely surprising that most of the subsequent literature on incomplete contracts in corporate finance has focused on narrower models in which private benefits are associated with some form of *stealing* or cash flow diversion. The pertinent image here is that of the *cashier* who is able to discreetly lift a few

bills from the *cash till*. This literature also downplays the *observable but not verifiable* distinction, and focuses on what is now generally referred to as a *limited commitment* problem that the borrower faces due to her limited ability to commit to repay a loan.[1]

In Bolton and Scharfstein (1990), we explore such a model of limited commitment, where the firm's realized earnings are private information. To elicit truthful reporting of high realized earnings, the financier must then offer a *carrot* to the firm. In our model this carrot takes the form of allowing the firm access to new loans that are necessary to continue operating the business when the firm repays its old loans. Hart and Moore (1994; 1998) also explore a model of limited commitment, in which, however, repayment of old loans is elicited with a *stick*: the threat of liquidating the firm if it does not repay its loan quickly enough.

Perhaps the main conceptual innovation of limited commitment models is a better understanding of the mechanics of debt default. Under the MM approach, default is assumed to occur when the firm is *insolvent*; that is, when debt liabilities exceed the value of the firm's assets (see, e.g., Merton 1974). This is a natural assumption if financial contracts are perfectly enforceable, for if the firm were to default when it is still solvent, then debt holders could simply enforce payment by seizing the firm's assets. In contrast, under limited commitment it is possible to separate default from insolvency and to distinguish between *liquidity and strategic* defaults.

The former is a situation where the firm is forced to default due to a cash shortage and the latter one where the firm chooses to default and force a debt restructuring because it is in its interest even though it is able to service the existing debt. Allowing for strategic default is a major conceptual breakthrough, because it draws attention to an important practical aspect of debt design that is completely absent from the MM approach, namely the protections that creditors require in the form of seniority, collateral, security interests, and covenants. These are all protections that increase the likelihood that creditors will be repaid by reducing the probability of a strategic default, and by increasing the creditors' bargaining position in a future debt renegotiation. Although the importance of debt covenants and subordination priorities has long been recognized by legal scholars (see, e.g., Schwartz 1998), economists have only started incorporating these protections in limited commitment models of debt. Under the MM approach and the tradeoff theory, all the firm's debts should be junior, unsecured debt. Indeed, these debts would give the firm all the tax shields it wants and would minimize bankruptcy costs, as these debts are relatively easy to restructure or *dilute* with new debt issues.

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1. Another related approach to private benefits proposed by Holmstrom and Tirole (1997; 1998) is not directly related to stealing, but indirectly so through *shirking*. By shirking, the manager obtains private benefits in the form of reduced effort costs and *diverts* (i.e., does not produce) the financial returns promised to investors.

3.2.3 Debt Structure.   In Bolton and Scharfstein (1996), we show how the threat of strategic default can be mitigated by having a well-protected, dispersed debt structure, which is difficult to renegotiate simply because it is more difficult to bring many people around a bargaining table. We also show that depending on the risk of a liquidity default, the firm may or may not want to structure its debt to make it difficult to restructure: If the risk of a liquidity default is high, the firm may be better off with a debt structure that is easy to restructure, whereas if the risk of a liquidity default is low, the firm may prefer to have debt that is hard to renegotiate.

In Bolton and Freixas (2000; 2006) we apply these ideas further and distinguish between expensive (due to intermediation costs) bank relationship-lending, which is flexible and easy to restructure, and cheaper bond issues, which are, however, more difficult to restructure. We then derive a partial equilibrium of the financial system with coexistence of a banking sector and securities markets. In this equilibrium, riskier firms rely on bank lending as an important source of funding, because they value the flexibility it offers, whereas safer firms rely more on bond financing. This model lends itself, in particular, to an analysis of monetary policy through the *lending channel*.

In a series of related articles, Diamond and Rajan (2000; 2001; 2005) also build on the idea of debt dispersion to counteract a threat of strategic default to develop a *limited commitment theory of banking*, bank fragility, and monetary policy. In the process, they substantially upgrade the classic theory of banking as liquidity transformation of Diamond and Dybvig (1983): While in Diamond and Dybvig, banks offer demand deposits to savers as a liquidity service, in Diamond and Rajan, banks offer demand deposits as a disciplining device, to facilitate "exit" by disgruntled investors should the bank make bad loan decisions, or be a "weak" debt collector. In their theory, bank fragility becomes a commitment device in a world of limited commitment, helping banks make more efficient loans than nonintermediated lenders who are vulnerable to ex post strategic default and debt renegotiation.[2]

Limited commitment models of debt point to the importance of debt renegotiation, a topic that has mostly been ignored by the MM approach. One could argue, of course, that as a first approximation this is a valid omission. However, recent empirical studies suggest that debt renegotiation is an important issue in practice. For example, Roberts and Sufi (2009) find that the vast majority of corporate long-term debt (>90%) is renegotiated before maturity in response to changes in the firm's environment. They also find that debt design reflects the parties' anticipation of

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2. The commitment benefits of bank fragility are, of course, available only to banks that are not *too big to fail*. The systemically important banks cannot be disciplined exactly in the way their theory describes, as they can count on being *bailed out* in the event of a run, or they can substitute deposits and short-term lending with liquidity provided at favorable terms by the central bank.

future renegotiation, and attempts to allocate bargaining power on a state-contingent basis. Similarly, Rauh and Sufi (2010) highlights that firms' debt structures vary systematically with underlying firm risk, and that riskier firms have more complex and more collateralized debt structures.

Interestingly, a more recent MM-based literature has incorporated elements of strategic default into its models. Thus, Anderson and Sundaresan (1996) consider a problem of debt valuation in a dynamic setting in which the borrower can strategically default. Not surprisingly, they find that both the default frontier and debt design are modified relative to the Merton (1974) model. Similarly, Mella-Barral and Perraudin (1997) analyze a continuous-time model of debt *à* la Leland (1994) with strategic default and find that the pricing of debt is significantly affected by the possibility of strategic debt renegotiation. Their model, in particular, provides more accurate estimates of credit spreads than other structural models without strategic default.

3.2.4 Nonexclusivity.   This first generation of debt structure models takes a *comprehensive contracting approach* to the design of debt structure: The number of creditors and the seniority structure are optimally determined ex ante in a multilateral (incomplete) contract with the borrower. The implicit premise in this literature is that the debt structures we observe are efficient from an ex ante perspective. A more recent, second generation of debt structure models, relaxes the assumption of ex ante comprehensive contracting and adds another dimension of contractual incompleteness, namely that debt structure is the equilibrium outcome of a *debt contracting game with nonexclusivity*.

The notion of nonexclusivity refers to the fact that a borrower may be able to borrow from a second set of creditors without the agreement of the first set of lenders. The analysis of equilibrium debt structures under nonexclusivity can be formulated as a *common agency game with externalities* (see Bernheim and Whinston 1985, 1986a, 1986b; Segal 1999). Not surprisingly, in the presence of externalities the equilibrium outcome of the contracting game will generally be inefficient. In the context of a corporate debt structure problem, when new debts are piled onto old debts, the expected payoff of old creditors is affected, as the new debts may increase the probability of default and reduce the recovery value of old debts in default. As new creditors do not take account of this externality on old creditors, there tends to be too much debt in an equilibrium with nonexclusivity (see, e.g., Bizer and DeMarzo [1992], for an early analysis of borrowing with nonexclusivity from multiple lenders). This is why nonexclusivity is a major concern for creditors, and to the extent possible creditors will attempt to protect themselves against future lending by the firm through various forms of *debt covenants* in the debt contract.

The efficiency of corporate debt structures and corporate borrowing thus depends to a large extent on the protection offered by debt covenants.

The effectiveness of debt covenants, in turn, depends on how easy they are to enforce and how comprehensive an exclusion they provide. In short, the area where the issue of (endogenous) contractual incompleteness perhaps matters most, when it comes to debt contracts, is the design and enforcement of debt covenants.

As we argue in Ayotte and Bolton (2011), a critical distinction between *property rights* and *contractual rights* lies at the heart of the *nonexclusivity* problem. Following legal scholarship, we define a *property right* as a right *in rem*, that is, a right enforceable against third parties (future potential lenders), while a contractual right is a right *in personam*, that is, a right enforceable only against the parties to the contract. Property law limits which rights can be enforced against third parties. The property rights of creditors come mainly in the form of security rights on collateral that has been perfected (i.e., *liens* on assets that have been *registered*, and for which, therefore, third parties have been notified). All other debt covenants, whether they are *negative pledge* clauses, limitations on new investments, or *acceleration* clauses are contractual rights only against the borrower. In other words, they can be enforced only through legal actions against the borrower (e.g., through injunctions) and they have no force in bankruptcy against new lenders.

Debt covenants are costly to enforce, because they require continuous monitoring of the borrower by the lender. As we argue, the reason why property rights law is structured in this way is to provide basic protections to new lenders against expropriation by old lenders. Indeed, debt covenants can be hard to find in a lengthy debt contract, and if all covenants were enforceable against new lenders, these lenders would face potentially huge expropriation risk, which could lead to severe credit rationing in equilibrium.

Given that most covenants are costly to enforce, lenders concerned about non-exclusivity prefer to rely on the property rights offered by *liens* (collateral and security interests). For many borrowers, however, such as financial firms, there are too few tangible assets that can be used as collateral. For these borrowers, covenants may also be too costly to enforce. As a result, these borrowers may be forced to maintain highly inefficient debt structures. These can take the form of excessively short-term debt, as Brunnermeier and Oehmke (2013) have argued in the context of corporate borrowing (as Bolton and Jeanne [2009], in the context of sovereign borrowing), or debt that is excessively difficult to restructure (Bolton and Jeanne 2007). Note that according to this theory of debt structure, bank fragility arising from short-term liabilities may be an inefficient equilibrium outcome caused by nonexclusivity and not necessarily an optimal outcome to discipline bank lending, as in Diamond and Rajan (2000).

3.2.5 Equity Structure.   Just as they open the way to a theory of debt structure, incomplete contracting and limited commitment models of corporate

finance also provide a foundation for a theory of equity ownership structure. Thus, Fluck (1998) shows how equity can emerge as an open-ended claim receiving a regular dividend payment in a self-enforcing equilibrium in a limited commitment environment in which firm managers can divert cash. Similarly, Myers (2000), and more recently, Lambrecht and Myers (2012), develop a theory of dividend payments as a way of preempting a hostile takeover. Incomplete contracting theories of equity structure can also be divided into efficient equity structure design theories and inefficient equilibrium equity structure theories.

Among the former theories, Admati et al. (1994) and Bolton and Von Thadden (1998) argue that concentrated ownership in the hands of a large block-holder may be an optimal ownership structure when monitoring of management is important. An alternative theory of limited controlling block size and optimal *managerial entrenchment* (implemented, e.g., through *poison pills*) by Burkart et al. (1997), and Pagano and Roell (1998) is that managers' discretion needs to be protected to some extent to give them optimal incentives to originate new investment opportunities.

Among the latter theories, Bebchuk (1999) and Shleifer and Wolfenzon (2002) argue that ownership concentration arises as an inefficient equilibrium outcome driven by the block-holder's inability to commit not to divert cash from the firm and desire to protect valuable private benefits of control.

4. An Assessment

As the above brief discussion of the corporate finance theory literature post–GH (1986) suggests, the introduction of limited commitment into the MM framework has substantially enriched our understanding of corporate control, corporate debt structure, leverage, and equity structure. As a *positive theory* of corporate finance, the limited commitment theory offers new predictions, many of which have been borne out in empirical studies. In particular, the studies on debt covenants by Roberts and Sufi (2009), Chava and Roberts (2008), Nini et al. (2009), and Bienz et al. (2012) show that the contingent allocation of control rights through debt covenants is a common practice that has significant effects on corporate investment and financing decisions.

As a *normative theory*, the limited commitment approach also offers a useful framework to assess legal interventions shaping equity and debt structures. However, normative analyses based on limited commitment models are often constrained by the lack of realism of these models with respect to the core assumption of nonverifiability of cash flows, investments, and states of nature. One concern, for example, is that under these strict nonverifiability assumptions, we should not observe all-equity firms, which turn out to be fairly ubiquitous (see Strebulaev and Yang 2012). In reality, both investments and earnings are partially verifiable. Moreover, the contracting parties can spend resources to make them more verifiable.

Also, the contractual incompleteness of debt contracts in practice is endogenous and is not simply the outcome of a technological or institutional constraint.

As a result, the contracts, financial structures, and legal rules observed in reality sometimes bear only a distant resemblance with the contracts derived in the theory. Although a good theory inevitably leads to such simplifications and abstractions, these, of course, also make a normative analysis more difficult. Still, relative to the MM framework of complete markets (with or without asymmetric information), the introduction of incomplete contracts and limited commitment has considerably enriched our understanding of corporate finance practice and corporate law.

The one major weakness of limited commitment and agency theories of corporate finance, however, is that they are not operational. Unlike the MM approach (and the tradeoff theory), agency and limited commitment models do not offer a methodology that practitioners can use. This is why 35 years after the publication of Jensen and Meckling (1976) agency, information, and control issues still remain marginal and esoteric topics confined to advanced corporate finance classes. These issues are often treated more like an *afterthought*, something that is mentioned as a caveat following a systematic and thorough valuation exercise based on the MM approach.

As a result, agency issues are often overlooked in practice simply because there is no simple quantitative methodology available to handle them. My view, therefore, is that before we pursue further refinements of the theory to put it on stronger foundations, we need to make more progress on making the theory more operational, even if this means taking shortcuts. The structural models following Leland (1994) offer one direction, but they need to be augmented to introduce simple forms of agency costs and limited commitment.

I have recently become involved in a research project in that direction with my work with Hui Chen and Neng Wang (Bolton et al. 2011; 2013). Basically, what we do is build a corporate finance problem around a continuous-time, stochastic version of the neoclassical q theory of investment à la Hayashi (1982) (which assumes MM neutrality), by simply adding a reduced-form cost of external financing. Granted, this cost could be derived from first principles in a model along the lines of DeMarzo et al. (2012), but our point is that this would involve a more complex model, which would too quickly distract from the goal of developing an operational theory.

As is easy to see, an external cost of financing creates a role for corporate cash balances and risk management along the lines suggested by Froot et al. (1993). Indeed, in our dynamic model the critical state variable is the firm's cash-to-capital ratio (a variable that is easy to construct and track from a firm's balance sheets). When this ratio is very high the firm behaves like a financially unconstrained firm, and when it is low the firm engages in

various forms of dynamic hedging, underinvests, possibly sells assets at fire-sale prices, and as a last resort raises costly external financing.

There is no fundamental conceptual innovation in this model. However, the model straightforwardly lends itself to a quantitative analysis, and by carefully calibrating the key parameters of the model (which can all be easily observed or estimated), one can provide concrete prescriptions to firms on how much they should invest, how they should manage their cash balances, how much they should engage in dynamic hedging, and how they should finance their investments. This is only a start with a highly simplified model. Still, it is richer and more realistic than the *dynamic tradeoff theory* model, and it can provide a quantitative methodology that allows practitioners to take account of agency and limited commitment problems.

5. Conclusion

By introducing a way of modeling incomplete contracts, and by proposing a simple theory of the firm based on the allocation of residual rights of control, the Grossman and Hart (1986) article has opened the way for formal economic theory to address important issues that had been left almost exclusively to scholars of corporate law, management, accounting, and sociology of organizations. At the same time, their article has drawn attention to a largely neglected issue in economics, namely limits to contracting that arise from contract enforcement constraints (as opposed to asymmetric information and incentive constraints). By emphasizing contract enforcement constraints, their article has helped ground the more abstract and general economics of contracts literature in a more institutionally realistic context. Even though it is now 25 years since the publication of their article, this process is still under way and far from complete. There is still too little communication, for my taste, between legal scholars and economists. Still, by taking a bold pragmatic step and introducing somewhat ad hoc (but plausible) constraints on contracting, Grossman and Hart have profoundly changed the field of contract and institutional economics. They have made it more relevant and rescued the field from *monstrous state-contingent prescriptions*.

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