

RESEARCH NOTES AND COMMUNICATIONS

OWNERSHIP STRUCTURE AND CORPORATE STRATEGY: ONE QUESTION VIEWED FROM TWO DIFFERENT WORLDS

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In their response to our paper, Amihud and Lev (1999) and Denis, Denis, and Sarin (1999) claim that disciplinary differences don't matter and that methods and evidence should speak for themselves. In contrast, we argue that important differences exist between financial economics and strategic management, leading to differing beliefs, norms, methods, and interpretations of empirical results. Using a strategic management perspective to review the evidence presented by Amihud and Lev in their earlier study (1981) and in their and Denis et al.'s critiques of our work (1999), we find no reason to revise our original conclusion: there is little theoretical or empirical basis for believing that monitoring by a firm's principals influences its diversification strategy and acquisition decisions. Copyright © 1999 John Wiley & Sons, Ltd.

In Lane, Cannella, and Lubatkin (1998), we questioned the relevant domain of agency theory. We found no evidence that Amihud and Lev's (1981) data from the 1960s or our data from the 1980s support their widely-cited conclusion that managers attempt to diversify their own risk through corporate diversification and unrelated mergers unless restrained by large block shareholders. Rather, our findings suggest that agency theory's cornerstone assumptions about the principal/agent problem have little relevance in explaining the strategic behaviors of public corporations when their managers are neither under siege nor con-

fronted with a situation in which their interests clearly conflict with those of shareholders.

Amihud and Lev (1999) rejected these findings on the grounds that our methods and measures were misguided. They dismissed as irrelevant our attempt to contrast the theories and methods of financial economics and strategic management, the heart of our thesis: 'We, on the other hand, do not believe that the researchers' discipline matters that much. What should matter for the advance of knowledge is the strength of the methodology and the weight of cumulative evidence' (1999: 1067). In a similar vein, Denis, Denis, and Sarin, when commenting on which approach, agency theory or strategic management is the more correct, argued that 'ultimately the relation is an empirical issue' (1999: 1073).

We disagree and stand behind our 1998 results. In this paper we will argue that the differences

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between the finance and strategy views about ownership structure and corporate governance cannot be resolved empirically, because the two views are themselves based on disciplinary differences in core assumptions, norms and priorities. The comments offered by Amihud and Lev (1999) and Dennis *et al.* (1999) further articulate the finance perspective. We will do the same here for the strategic management perspective, which we believe holds a more realistic and balanced view of people and organizations.

Our paper begins with a brief review of the epistemological literature on interdisciplinary discourse. We then explore the assumptions underlying the financial economics and the strategic management perspectives. We show how these disciplinary differences influence the meanings of shared concepts and methodological norms, and bias the interpretation of results. We base our response to the critiques of our research on these disciplinary differences and on a few factual errors that they present. Finally, we show how these disciplinary differences can lead even well intended scholars such as Amihud, Lev, Denis, Denis, Sarin, and ourselves to draw conflicting interpretations from the same data.

DISCIPLINARY DIFFERENCES AND INTERDISCIPLINARY DIALOGUES

A scientific discipline is not simply a body of knowledge, it is also a community of practice where members develop and apply knowledge through their work (Brown and Duguid, 1991). Members of a discipline undergo 'similar educations and professional initiations; in the process they have absorbed the same technical literature and drawn many of the same lessons from it' (Kuhn, 1970: 177). As such, members are socialized to accept certain ways of gathering and communicating evidence. In this process of social construction, editorial boards serve as gatekeepers to further shape consensus as to what is legitimate research.

While 'shared consensus' facilitates communication within a discipline, it poses obstacles to discourse between disciplines, because participants cannot set aside the assumptions, beliefs, and norms they have been socialized to accept as 'good science.' Each discipline is 'focused

on different matters, professional communications across group lines is sometimes arduous, often results in misunderstanding, and may, if pursued, evoke significant and previously unsuspected disagreement' (Kuhn, 1970: 177). Not only may there be a lack of familiarity with research methods and terminology, but the meanings of some words can vary across disciplines due to different social conditioning. This occurs because the origin of scientific concepts is inseparable from the web of hypotheses and assumptions that shape a discipline's view of the phenomena it studies (Zolo, 1989: 170; Jensen, 1983).

Clearly, these communication obstacles frustrate attempts to engage in interdisciplinary discourse. However, attempts at such discourse are essential for the advancement of science, for those who truly want to understand their own theories must examine competing theories wherever they may be found (Feyerabend, 1980). Kuhn (1970: 202) suggests that to facilitate discourse, members of one field should try to translate the work of other fields into their own terms while simultaneously describing the world to which their own theory applies.

This is precisely what we attempted in our 1998 paper when we used a strategic management lens to critically evaluate five assumptions that grounded Amihud and Lev's (1981) thesis. Unfortunately, we do not detect a similarly constructive effort either in their 1999 rejoinder nor in the Denis *et al.* (1999) paper. By rigorously adhering to the theoretical and methodological dogma of their discipline, they convinced themselves that they are correct—a time honored tradition in science (Pfeffer, 1993). One only has to glance at the citations in both their papers for evidence: while there have been many papers published in the strategy literature on topics germane to their thesis, the only strategy papers that they reference are the two that they are now critiquing.

Despite this, we remain committed to an interdisciplinary dialogue because, like Kuhn, we believe it can benefit both fields. Moving the discourse forward requires acknowledging the differences in the financial economics and strategic management world-views that lead to contrasting interpretations of the same data. The parochial judgements of each field arise from divergent intellectual traditions, which have led to equally divergent assumptions, norms, and priorities.

TWO VIEWS OF FIRMS, OWNERS, AND MANAGERS

Finance is the area of applied microeconomics that views capital markets and capital management decisions from the perspective of investors. In keeping with its roots in economics, it makes a number of simplifying assumptions that reduce the complexity of what the discipline studies. This narrows the scope of their research, but it also allows finance researchers to precisely measure their key phenomena. For example, finance researchers assume that investors are risk averse and will hold a portfolio of stocks only if they are compensated for the variance (risk) in the portfolio's returns. They also assume that investors can be fully (efficiently) diversified such that their investment in risky assets (including firms) will have only the risk associated with the market itself. Consequently, finance scholars generally assume that a firm should be primarily concerned with capital market issues. Consistent with this world-view, finance scholars tend to interpret any differences between firms primarily in terms of the impact that these differences have on the risk-return properties of an investment portfolio.

Not surprisingly, finance scholars tend to judge a firm's diversification in similar portfolio terms by focusing on degree to which the cash flows from its different business units are correlated. Also consistent with their world-view is the belief that managers act as impediments to investor interests; i.e., managers are rational economic actors who place self-interest above all else. As such, managers require monitoring and incentives in order to minimize their propensity to act opportunistically. Indeed, many finance scholars seem to wonder how the public corporation survives, given the unbridled self-interest of managers (e.g., Jensen and Meckling, 1976). Some, like Jensen (1989), view the problem of opportunism as so great that they predict the 'eclipse' of the public corporation.

Strategic management is also rooted in applied microeconomics, but it places more emphasis on relevance to managerial practice and on capturing the complexities of real-world organizations and competition. Consequently, strategy scholars tend to reject economics-style reductionism and a dogmatic adherence to a single paradigm. Our discipline's constructs, theories, and methods can

involve multiple causes and different levels of analysis, some inspired from economics and others coming from an eclectic set of behavioral sciences like psychology, social psychology, and sociology. This leaves our field with a disorderliness that those in other fields sometimes view disparagingly (Mitchell, 1998). However, we take it as a sign of the field's intellectual vitality and ability to address the changing challenges firms face. Strategic management's distinctive role among the social sciences is to integrate behavioral and economic theories with our own unique understanding of the purposeful management of complex organizations, and in the process, provide insights and guidance to the individuals who manage those complex organizations.

For example, whereas financial economists often simplify firms to little more than portfolios of investments, with performance impacted primarily by market forces, strategy scholars view firms as portfolios of resources and capabilities linked by the people who create and utilize them. Consistent with strategic management's more complex world-view is the cornerstone claim that the management of a firm requires constant attention to human relationships. In making this claim, strategy scholars are not denying the importance of market forces, but rather viewing the choices exercised by individual actors when dealing with those forces as paramount (Child, 1972; Bourgeois, 1984; Hambrick and Finkelstein, 1987; Chatterjee, Lubatkin, and Schulze, 1999). Consistent also with strategic management's more complex world-view is the claim that self-interest is not necessarily the primary motive behind managerial behavior. Instead, the discipline assumes the propensity for self-serving actions is jointly influenced by individual differences, the organizational environment, and the context of the management-shareholder relationship (Davis, Schoorman, and Donaldson, 1997). Put differently, whether a manager acts opportunistically depends in large part on how that manager feels about their work situation; i.e., whether they feel morally justified to act on their self-serving tendencies (Ghoshal and Moran, 1996). As such, whereas finance scholars and agency theorists view managerial discretion as an opportunity for self-serving behavior, strategy scholars believe that it is also an opportunity for value-enhancing entrepreneurship (Finkelstein and Boyd, 1998).

The broader view of managerial motivation, discretion, and opportunism embraced by strategic management has significant implications for Jensen and Meckling's (1976) 'theory of the firm' which continues to be a cornerstone of modern financial thought (Jensen, 1989). If individuals are not hyper-rational 'homo economicus', then owner-management can engender 'self-control problems', which compels them to take opportunistic actions that can 'harm themselves as well as those around them' (Jensen, 1994: 8). These 'agency problems with oneself' (Thaler and Shefrin, 1981), which vary with the owner-manager's level of self-regarding preferences, can increase the agency costs and prevent increased ownership share from aligning the owner-managers' interests (Schulze, Lubatkin and Dino, 1999). Interestingly, Jensen admits that he 'failed for more than a decade to see the generality and importance of this self-control issue' (1994: 8).

In summary, we argue that strategic management and financial economics hold different world views, and that these differences in turn influence the disciplines' views on the ownership structure of corporations. Strategic management holds a more complex, less reductionist view, and therefore leads to the conclusion that neither owner-control nor management-control is a panacea. As such, the difference between the two disciplines is largely theoretical in nature. Contrary to the assertions by Amihud and Lev (1999) and Denis *et al.* (1999), therefore, we believe that the differences in theory can not be rectified solely by appeal to empirical evidence. We discuss the research implications of this argument in the following section.

TWO VIEWS OF KEY CONSTRUCTS AND MEASURES

Amihud and Lev (1999) argue that the ultimate test of a theory is based on the 'strength of methodology and evidence' and not on the discipline of the researcher. We disagree. Statistics may appear to be objective, but the methods used to derive them and the nature of the inferences drawn from them tend to be very discipline-specific. This can be illustrated by a brief review of the studies cited by Amihud and Lev (1999) in defense of their 1981 findings. For the sake of brevity, we will not review every paper they

cite as some are tangential to the topic of ownership structure and corporate strategy. The papers we do examine exemplify the disciplinary differences that lie at the heart of this essay.

Relatedness of mergers

In keeping with the field's reductionist norms, finance studies examining ownership structure and merger relatedness tend to operationalize merger relatedness as a dichotomous variable. For example, Amihud and Lev (1981) classified all mergers that were not vertical or horizontal as being conglomerate, or unrelated. Similarly, Lloyd, Hand, and Mondani (1987) classified mergers as diversifying or non-diversifying based on information published in *Mergers and Acquisitions*. Byrd and Hickman (1992) considered any target not in the bidder's 3-digit SIC industry as unrelated. Morck, Shleifer, and Vishny (1990) used two measures of relatedness: a dichotomous measure of the overlap between the bidder's and the target's 3-digit SIC industries, and the correlation of the bidder's and the target's earnings.

Because strategic management is comfortable with the complexity of strategic decisions, dichotomous approaches to classifying merger relatedness have never held wide appeal. Instead, strategy scholars typically see many shades of gray between the white of horizontal mergers and the black of conglomerate (unrelated) mergers. Consistent with the world-view of this discipline, our study used three different measures of merger relatedness. First, we unpacked Amihud and Lev's 'all conglomerate mergers' category, breaking it into the three Federal Trade Commission (FTC) merger categories it was constructed from: product extension, market extension, and conglomerate (unrelated). Strategic management views both market extension and product extension mergers as more related than unrelated, and is replete with explanations of value creation from either type (e.g., Lubatkin, 1983; Porter, 1987). Second, we examined a subset of Amihud and Lev's (1981) sample and a sample of 1980s mergers using a 5-category measure of merger relatedness based on Rumelt's (1974) diversification typology. Third, we examined the 1980s mergers using a measure based on bidder and target ratios of unique and shared 4-digit SICs.

These different operationalizations of merger relatedness highlight the fact that many mergers

considered 'unrelated' by finance researchers such as Amihud and Lev (1981) are considered 'related' by strategy researchers. More importantly, results of empirical studies about merger relatedness can be sensitive to the different operationalizations. For example, when we used all five FTC merger categories, we found that it was product extension mergers that were driving Amihud and Lev's (1981) results for 'conglomerate' management-controlled firms. As there is widespread agreement among strategy scholars that product extension mergers are related not unrelated, we see no evidence of an agency problem here. Our conclusions about lack of cause and effect were bolstered by our tests of Amihud and Lev's (1981) sample using the Rumelt diversification categories, which are widely, though not dogmatically, accepted in the strategic management literature, to classify the mergers by their relatedness characteristics, and two parallel tests conducted on 1980s mergers.

Amihud and Lev's (1999) choice of supporting evidence also demonstrates the extent to which disciplinary biases can influence the interpretation of empirical results. Two of the studies they cite as supporting their conclusions regarding ownership structure and merger relatedness (i.e., Byrd and Hickman, 1992; Morck *et al.*, 1990) *never* test that relationship.

Diversification of firms

The diversification measures which finance studies tend to use are also in keeping with the traditions of their discipline. Their logic flows directly from the investment portfolio metaphor: the more closely the variations in a firm's cash flow follows those of a broad market, the more diversified the firm must be. For example, the two studies of ownership structure and diversification cited in Amihud and Lev's (1999) response measured firm diversification using the R^2 of a market model (e.g., Amihud, Kamin and Ronen, 1983; Lloyd *et al.*, 1987); i.e., the correlation of a firm's cash flows with those of the market. In contrast, strategy research is more concerned with a range of resources (including, but not limited to financial capital), and strategic management's measures of diversification reflect this broader view. Our study measured diversification using two methods of capturing the breadth of a firm's resources and activities: Rumelt's five categories

and Wood's (1971) narrow spectrum diversification (NSD) measure based on the firm's SIC codes (Lubatkin, Merchant, and Srinivasan, 1993).

The different ways in which finance and strategic management conceptualize and measure firm diversification can be a major source of the misunderstandings, for they divide the two disciplines with a seemingly common language. Indeed, in our 1998 study, we compared the market model measure of diversification with Rumelt's classification from strategic management, and found no statistical convergence, suggesting that the two measures are capturing very different constructs.

Denis, Denis, and Sarin (1997)

The study by Denis, Denis and Sarin (1997) spans both disciplines as it develops its theory from the finance literature but uses measures of diversification compatible with strategy norms. Their tests of the mean levels of diversification across seven levels of management control show all five diversification measures to have 'a near monotonic negative relationship' with management stock ownership (Denis *et al.*, 1997: 141). Multivariate analyses using only a business segments measure reveal a curvilinear relationship between management ownership and diversification. The positive relationship predicted by Amihud and Lev (1981) was found only at very high levels of managerial ownership (greater than 67.5%), only 13 out of 933 firms. This led Denis *et al.* (1997: 137) to conclude that their results contrast somewhat with Amihud and Lev's 1981 findings as their analyses indicate that managerial risk reduction is *not* an important motive behind diversification decisions.

Denis *et al.* (1997) interpret the negative association between management ownership and diversification as indicating a convergence of managers' and shareholders' preferences (managerial self-interest). However, they measure management ownership using the percentage of stock owned by the firm's officers *and* directors (1997: 138), two groups which both strategy and finance research generally view in the context of the principal/agent problem as distinct groups.

Officers are the senior managers who oversee the day-to-day operation of the firm, while directors monitor managers' actions on behalf of the shareholders. Outside directors are believed to be

especially effective monitors (Fama and Jensen, 1983; Coughlan and Schmidt, 1985), and their incentive to vigilantly monitor management increases even further with the size of their equity ownership in the firm (Baysinger and Butler, 1985; Johnson, Hoskisson and Hitt, 1993). That is why we included a measure of board vigilance based on the proposition of outside directors on a company's board and the stock ownership of those directors in our tests of the 1980s data (Lane *et al.*, 1998: 567–568).¹ Aggregating the stock owned by officers and directors obscures these key distinctions and raises questions about what their results actually indicate. Is it managerial self-interest, board vigilance, or simply an artifact created by combining two conceptually independent owner groups? The answer is unclear based on the data and results reported by Denis *et al.* (1997).

The same multivariate analyses also found outside ownership to be associated with reduced diversification in keeping with Amihud and Lev (1981). However, the reduction that they observed is of such a small magnitude and weak significance as to be of questionable importance. The regression coefficient for the percentage of stock owned by outside blockholders is -0.007 , and it decreases to -0.006 with addition of industry controls to the model (Denis *et al.*, 1997: 143). By comparison, the coefficients for firm size (log transformed) are 0.244 and 0.243, and the coefficients for firm age (log transformed) are 0.627 and 0.519. Furthermore, the outside ownership coefficients are only weakly significant with *t*-statistics from -1.66 to -1.92 ($p < 0.10$) despite an extremely large sample (933 firms). The *t*-statistics associated with firm size and firm age are about three times as large in absolute value and highly significant ($p < 0.001$). It is not surprising therefore that other analyses in their study found no relationship between ownership structure and the value added (or lost) through diversification. In Denis *et al.*'s (1997: 156) own words: 'we cannot conclude from this evidence

that agency problems led managers to engage in value-reducing diversification strategies.'

In summary, Denis *et al.* (1997) represents the finance study that has best spanned the research norms of strategy and finance. While Denis *et al.* (1999) argue otherwise, we interpret the findings from their 1997 study as more in keeping with the predictions of strategic management than with those of finance, and more in keeping with their own 1998 interpretations. Their management ownership results are ambiguous due to measurement problems, and their results regarding outside ownership and diversification do not meet Cohen's (1990) criteria for proof of a non-trivial association (despite a sample three times larger than our 1998 study). Thus, the findings by Denis *et al.* (1997) do not contradict our earlier conclusion: monitoring by a firm's shareholders has no meaningful influence on its diversification strategy and acquisition decisions.

TWO VIEWS OF METHODOLOGY

Amihud and Lev (1999) raise several questions about the methodology in our study. Some of these are factually incorrect while others reflect misunderstandings arising from disciplinary differences and unfamiliarity with the strategic management literature. A brief examination of their major points will further highlight the differences between the finance and strategic management world views.

Support for 1981 merger findings. Amihud and Lev suggest that when we disaggregate their 'all conglomerate mergers' category into product extension, market extension, and pure conglomerate mergers, we find that outside owner control reduces all three types of mergers which supports their earlier findings (1999: 1064). We are puzzled by their interpretation. Table 1 in our 1998 paper shows that only the model for product extension mergers is significant. Our analysis does indicate that outside owner control reduces the likelihood of product extension mergers. However, acquisitions which allow a firm to expand the range of products it sells to its existing customers (i.e., product extension mergers) are not viewed as inherently detrimental to firm performance in the strategy literature. Thus, there is little reason to believe that product extension mergers are, on average, harmful to shareholder interests.

¹ We found no relationship between board vigilance and the relatedness of a firm's mergers or its diversification, but did find a negative relationship between vigilance and the firm's market-to-book ratio. We did not examine managers stock ownership as our study examined shareholders' monitoring of 'the discretion that managers can exercise in following their own preferences' with regard to mergers and diversification (Amihud and Lev, 1981: 609).

Reliance on Rumelt's diversification measure. Amihud and Lev contest our use of Rumelt's (1974) diversification categories as a criteria for testing their hypothesis. They imply that his classification scheme is unproven when they argue that a 'lack of statistical significance may reflect on Rumelt's (1974) analysis rather than on A&L's' (1999: 1066). As strategy researchers have known for years, Rumelt's classification procedure is carefully documented in his book, widely used in strategic management research, and the resulting classifications are routinely replicated. The fact that Amihud and Lev could make such a comment only serves to confirm our earlier contention that those authors have given very limited attention to published work in strategic management. Furthermore, our results using Rumelt's measure are fully consistent with the results we find using a product count measure of diversification (Lane *et al.*, 1998: 577-578), a measure which Amihud and Lev do not contest.

Contradictory Q-ratios. Amihud and Lev (1999: 1065) state that Table 3 of our 1998 study shows that the Q-ratios—the ratio of market value to asset value—for the three types of control 'do not contradict the standard agency theory as LCL propose (Lane *et al.*, 1998: 566), but rather support it' (i.e., $Q_{mc} < Q_{voc} < Q_{soc}$). Our own examination of Table 3 indicates that $Q_{mc} < Q_{voc} > Q_{soc}$. Further, the only significance occurs between Q_{mc} and Q_{voc} .

Low power of diversification Test. Amihud and Lev suggest that our failure to reject the null 'may result from a low power of the test. In their table that classifies companies by control and diversification, the number of observations in two thirds of the cells (10 out of 15) is nine or below, two cells have only one (!) observation, one has two, and one has three observations. For 'Strong owner-controlled' in particular, the median number of observations in the five cells in this class is three. It is very hard to obtain reliable statistical results with such data' (1999: 1066).

We share their concerns about the results of tests using such data, and therefore conducted a second study using 1980s data as part of our 1998 paper. In that second study, we ran two tests and used two different measures of diversification, and again found no relationship between outside owner control and firm diversification. Nevertheless, we are puzzled by Amihud and Lev's concern about the data's reliability because

the data that they are referring to is their own! We reconstructed their data set using their methodology guided by 'helpful conversations with Yakov Amihud' (1998: 561). Our analyses of 1960s mergers using that data does have small numbers of observations in some cells. However, we believe that it is **far** better to present the analyses that our theory calls for than to lump together categories of a phenomena which are clearly theoretically distinct. As we note throughout this paper, theory must precede analysis before evidence can be meaningfully interpreted.

Inappropriate market model measure. Amihud and Lev argue that we inappropriately use unsystematic risk (σ^2) as a parallel to their R^2 diversification measure. We do use unsystematic risk (actually, σ rather than σ^2), but only in a test of ownership structure's effect on risk, not on diversification. Further, our use of this risk measure follows Amihud and Lev's (1981: 607) own suggestion that the trade-offs managers face given potential projects with similar systematic risk but different unsystematic risk create 'a classical agency problem.' Having said this, Amihud and Lev (1981) never tested ownership and unsystematic risk. Instead, they use diversification as a proxy for risk based on the finance belief that risk and diversification are inversely related.

Null findings. In Footnote 8 Amihud and Lev note that 'while LCL claim that their tests 'reject' Amihud and Lev's hypotheses, *none* of their tests does' (emphasis theirs). On the contrary, we spent several paragraphs discussing that our study cannot 'reject' any hypotheses, since our position is a null one—that is, we expect to find no effect. Our method of examining the null hypothesis is in keeping with Cohen's (1990) guidelines (Lane *et al.*, 1998: 573), and our manuscript does not use the word 'reject' but instead uses words like 'find no support' or 'null findings.' Tests of the null may be less common in finance, given the traditions of the field. We therefore conclude that their comment on null findings, like their comments on our measures of mergers and diversification, reflect an unfamiliarity with our field's methodological norms.

Understating agency costs. Amihud and Lev (1999: 1063–1064) suggest that we claimed that 'the agency problem that results from the separation between corporate ownership and control imposes *no* cost on the corporation' (emphasis added). We made no such claim and have no

reason to doubt that agency problems do impose important costs on public corporations. However, based on our hypotheses, findings, and power tests, we do claim that the costs related to diversification are not as extensive or as far-reaching as suggested by Amihud and Lev (1981).

Favoring unrelated diversification. Amihud and Lev conclude from our 1998 paper that we are in favor of unrelated diversification (in their words 'conglomerate mergers') (1999: 1063). We do not state this and are perplexed as to how they could have deduced this opinion. It is inconsistent with the central theme of over two decades of strategy research on merger and diversification including numerous papers on the subject by one of the authors of this paper.

...AND NEVER THE TWAIN SHALL MEET?

Financial economics and strategic management see two fundamentally different worlds and this gap will be difficult to bridge. As Max Planck, renowned physicist of the early 1900s, noted 'a new scientific truth does not triumph by convincing its opponents and making them see the light' (1949: 33). In this essay, we argued that the disciplines of financial economics and strategic management see two oblique views of 'the light.' We think that the rejoinders by Amihud and Lev (1999) and Denis *et al.* (1999) are useful in that they remind us just how difficult it is to bridge these two world-views. It is unlikely that anything we present here will change their beliefs. Hopefully, however, it has improved their awareness of other world views.

On the other hand, the arguments and evidence presented by Amihud and Lev and Denis *et al.* have not changed our belief that monitoring by a firm's principals does not influence its corporate strategy concerning mergers and diversification. We still conclude, therefore, that while agency theory may predict managers' behavior during battles for corporate control and situations in which they are otherwise under siege (i.e., when there are sharp conflicts of interest), it does not inform us as to the corporate strategies they may pursue in more ordinary situations.

Our findings add to a growing list of strategic management studies that provide evidence of agency theory's limitations. For example, Walsh

and Kosnik (1993) found little support for the disciplinary role agency theorists assume is played by the market for corporate control. Finkelstein and D'Aveni (1994) found that agency theory alone could not explain why boards adopt CEO duality. Barkema and Gomez-Mejia's (1998) review of research on executive compensation points out the inability of agency theory by itself to explain CEO pay. These studies, along with behavioral critiques of other economic theories (e.g., Ghoshal and Moran, 1996), are helping to free strategy from the paradigmatic straightjacket of economic thought and lead us toward a more open-minded and eclectic theory of the firm where self-interest and opportunism tell only part of the story. Because strategic management embraces a more complex, more balanced, and, we believe, more realistic view of firms, managers, and decision making, it is primed to make important contributions to the understanding of principal/agent relationships in corporations.

Our concern over the narrowness of the financial economics perspective does not mean that we reject all finance or economics research. Indeed, we use the theories, measures, and methods from both of those disciplines in our own work. However, it is important to remember that the objective of our field, to paraphrase Bettis (1991), was never to do first class financial economics, but rather to use financial economics to help us do first class strategy research.

Nor do we expect financial economists to produce first class strategy research. But we do believe that they can benefit from a greater awareness our literature. For example, Denis *et al.* (1999: 1075) cite four recent papers that 'generate new insights' into why diversification is negatively related to firm valuation. Three of the four studies they cite examine problems with internal capital markets. As we noted in our 1998 paper and again in this one, the argument that internal capital markets are inefficient is not new insight to strategy scholars. The responses by both Amihud and Lev, and Denis *et al.* indicate to us a distinct unfamiliarity with strategic management research on corporate-level strategy. As more finance scholars move into arenas that strategic management scholars have studied for years, they might first consider the contributions from strategic management, for otherwise they run the risk of reinventing the wheel.

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