

## Theory of the Firm, or Framework ?

*JC Spender, ESADE & LUSEM*

This Note is about the 'theory of the firm' (ToF)- of real flesh and blood firms, as Penrose put it, rather than mere mathematical models. As is widely recognized, in microeconomic analysis the firm, as management theorists imagine it, tends to get disappeared behind the formalized interaction of supply and demand curves. In recent decades a group of micro economists has labored to pull 'the firm' out of this shadow to consider why it exists and how it might really work. Many credit Coase as starting this though one should also mention Marshall, Knight, Schumpeter, Penrose and a number of others unhappy with the aridity of the mathematical models. From this project we have a rag-bag of alternative ToFs based on transaction costs, principal-agent reasoning, property rights allocation, team production, etc. (Foss & Klein, 2005; Gibbons, 2005; Mahoney, 1992). There is also Penrose's theory. Plus there are two other ToFs familiar to management and strategy theorists that the micro economic literature pays little attention to - Porter's 5-force model and the RBV. Strategists have their tool-bag of models, each of which carry implications for a theory of the firm, though these are seldom worked out - the BCG matrix or the Balanced Scorecard, for example. In the related field of organizational theory (OT) there is a handful of ToFs that underpin their theorizing, wonderfully summarized by Morgan - a bureaucratic machine, a culture or psychic prison, a political entity, and so on (Morgan, 1997).

I believe the strategy and OT fields have suffered greatly in recent years as a result of what some call its 'colonization' by micro economists pushing their 'more rigorous' ToFs. By suffer I mean general depression, loss in intellectual vigor, academic prestige, access to the core curriculum, attractiveness to PhD students, space in the journals, faculty promotion opportunities, funding for its research and in all those other ways in which our profession rank orders its members. At the same time the last decade has seen a rising chorus of criticism of micro economic thinking as a basis for management education - the MBA especially - and, in particular, its adoption of *homo economicus* as axiomatic (Ghoshal, 2005; Khurana, 2007; Mintzberg, 2004; Pfeffer & Fong, 2002). Another aspect of this pushback is evident in raised interest in the ethical, moral and spiritual dimensions of managerial

activity (Bok, 1978). Of course, the difficulty with critiquing 'rational man' is deciding what to put in his place. Behavioral economics, proposing an alternative computable but 'biased' model of the individual, has proved attractive. Likewise many look to 'humanize' management theory by bringing other models of the individual into play - social man, family man, just man, sympathetic man, and so on, all of which are typically less computable and formalizable.

My talk is certainly a critique of rational man-based theorizing, but instead of replacing rational man *tout court*, I adopt a different strategy. I believe we might make better progress by modifying him. If done carefully we would still be able to hold onto and leverage the huge intellectual effort that has gone into economics over the last few hundred years. In the flyer for this seminar we suggested rational man-based theorizing has cut itself off from the world of practical management and that it might focus on building bridges back by making use of some different intellectual materials.

In this spirit I bring in the idea of the managers' imagination. Given the contradiction between prediction and imagination this may seem to be trashing any possibility of rigor and making sure we end up with no more than emphatic arm waving. To avoid this I propose managers as individuals possessing imaginative capabilities relevant to the strategic and organizational problems at hand, just as we normally presuppose they have logical reasoning capabilities without considering how these have been acquired and honed to their purpose. The key here is to differentiate the exercise of our native imagination or judgment from this evident ability to reason rationally. The managers in my discussion have two capabilities, both to reason and imagine, and are able to combine them.

Being objective implies, on the one hand, not using the imagination, sticking to the facts, but on the other having enough facts to drive the choices to be made to their logical conclusion. This is where we get into difficulties outside the classroom, in the world of flesh and blood firms, for there we find pervasive uncertainty. This is Simon's bounded rationality point. Not only do managers not have all the facts, they often do not know how to connect the dots they have. Similarly, they issue instructions without being able to fully forecast the consequences, and so on. My argument is that under the conditions of uncertainty that mark the real 'lived' world, even when reduced as much as possible by methodical research,

the managerial imagination must always be brought into play - which leads me to question the popular distinction between managers and leaders.

My talk is about how managers might handle the uncertainty of real situations rather than ignore it as we normally do in the classroom. As a result I believe we might get to do some fun and interesting work and be of greater service to the managerial community. I set about this in a slightly oblique way. The key is to differentiate between (a) theorizing and (b) establishing an empirically relevant framework within which the managerial imagination is deployed. Theorizing, in the natural science tradition, gives us insight into fully determined conditions, the relationship between cause and effect. We build models with independent variables and are able to predict the result of actions. In contrast, a framework is a way of characterizing the constraints to the manager's imagination. A painter is constrained by the two-dimensionality of her canvas and the many technological issues around the paint's pigments, drying rate, miscibility, reflectivity and so on. She has to master the medium's practical complexities before anything can be achieved. She is also constrained by history and the prevailing notions of what counts as art.

This suggests a framework's boundedness can always be explored in two ways. First, to probe the limits to its possibilities, second, to comprehend what it means to break through the constraints into a new range of possibilities. In business, the first accentuates efficiency and competition, the second the kind of innovation Schumpeter labeled 'destructive' or 'architectural'. Note the distinction disappears if the set of possibilities is unbounded. To illustrate this the first part of my talk is about the concept of bounded framework that underpins Michael Porter's influential *oeuvre*. It leads towards a methodology for researching frameworks that is not at all the same as our methods of theorizing. They link together in a curious way. While frameworks are empirically grounded, understanding how they lay out and delimit the bounded space for the manager's imagination calls for a multi-dimensional form of theorizing. The space, which some might associate with the Japanese notion of '*ba*' (Nonaka & Konno, 1998), is bounded by contrasting theories. My approach here is based on von Clausewitz and Heidegger with a topping of Knight, Barnard and Simon. Looking carefully at Porter's methodological analysis we see the 5-force diagram (FFD) indicated a space for the firm's imaginative strategists.

The second part of my talk applies the same methodology to another of the economist's ToFs, principal-agent theory. I suggest some fundamental problems with Jensen & Meckling's formulation of principal-agent theory (Jensen & Meckling, 1976) and that it can also be usefully contrasted against Fama's early paper on the same subject (Fama, 1980). Again, the point of this analysis is to indicate the bounded space for the strategist's imagination. I go on to argue the collection of micro economic ToFs is not a rag-bag at all but actually comprises a logical set that indicates another dimension of strategic managerial choice.

I finish up by asking you to think about transaction cost theory from the same framework method point of view. The bottom line is that all the theories of the firm can be seen as frameworks rather than theories in the conventional sense, which helps explain why micro economists such as Demsetz, Foss and Klein remind us that, as yet, we have no satisfactory theory of the firm (Demsetz, 1988; Foss & Klein, 2005). The deeper implication is that the questions that interest us about strategic and entrepreneurial management should probably be re-characterized as non-conventional problems and be researched with non-conventional methods - as authors such as Shackle and Lachmann insisted (Lachmann, 1994; Shackle, 1979). The challenge is not so much to replace rational man with some other entity but to understand the way the researcher's chosen model of the individual and the methodologies required to research her/him are in a mutual embrace. We cannot research the managerial imagination with causal models. We need something else, which actually turns out to be familiar rather than strange.

### **Porter's Analysis**

Strategic analysis is not generally considered part of the ToF literature even though strategic analysis clearly presupposes a theory of the firm. In Chandler's analysis, for instance, the firm is (a) the production function that meets a particular market segment, and (b) should be internally structured to do this optimally. Strategy drives structure. The SWOT model is of a firm comprised of resources situated within a market environment. The strategist's toolkit is actually a set of alternative models of the firm - the BCG matrix, for example, is about firm sustaining funds movement. The strategic analyst or strategy course

student is invited to intuit which of these is most appropriate to the situation at hand. Micro-economists, who do not think like strategy theorists, have a different toolkit and four models are especially notable: transactions cost, principal-agent, team production, and industrial organization models, the most relevant of which is Porter's 5-force version. While the RBV has attracted a great deal of attention, I believe it is actually an emasculated rendition of Porter's model.

One of Porter's under-rated contributions to strategic analysis is his discussion of the differences between theories and frameworks. Two of his papers are especially relevant, his 1981 AMR paper and his 1991 SMJ paper (Porter, 1981, 1991). Together they comprise the methodology chapter many critics argued was missing from his 1980 blockbuster *Competitive Strategy*. Rather than recapitulate these papers I assume some familiarity with them and go directly to the points I want to draw from them. In the first, Porter takes off by comparing his thinking with the LCAG 4-box framework (Learned, Christensen, Andrews, & Guth, 1965). He argued the LCAG 'paradigm' offered no help with assessing the contents of each of its boxes, a matter that was left to the practitioner (1981:610). He claimed the 5-force analysis did more in this respect. He then went into the industrial organization (IO) analyses of Mason and Bain whom he regarded as his intellectual predecessors.

Porter first used the LCAG framework as an MBA student when he arrived at HBS in 1969, for it was then the basis of HBS strategic analysis. It is not generally known that it reflects the earlier work of Commons, another IO predecessor. The LCAG framework advises the analyst to consider four questions; "What the firm can do, what it might do, what its senior management wish it to do and what they think it ought to do?" These 'can, might, want, ought' verbs are taken directly from Commons and comprise a typically Victorian heuristic. But they imply a broad notion of strategy that goes well beyond the mere maximization of profit or shareholder wealth. We might say it is 'stakeholder theory' in Victorian language.

Even though Porter may not have fully appreciated the richness and subtlety of the LCAG paradigm, he grasped the essential difference between it and a theory. In 1981 he wrote: "the paradigm offers a systematic model for assessing the nature of competition in an industry" (1981:611). He went on to argue business policy (BP) - the HBS term for strategic analysis - engages a broader set of issues than those considered within the IO paradigm. He

got into the methodological issues as he observed: “Determinism was an element of IO theory” (1981:613). The IO analysis took the industry structure as an independent variable while BP writers “have long observed that firms can fundamentally change to the structure of their industries through their actions. The policy field has a long tradition of emphasizing the insight, creativity, and even vision that some firms have exhibited in finding unique ways to change the rules of the game in their industries” (1981:613).

Porter’s problem lay in negotiating the narrow ridge between the openness he saw implicit in BP analysis and the closure explicit in theorizing. A theory provides a rigorous deterministic relationship between independent and dependent variables. It is closed and claims to be predictive and objective, independent of the person doing the analysis. Its closure excludes any creative input by the analyst. Since we cannot predict creativity until we have a theory of it - and Porter did not - his mentioning it seemed to draw the analysis beyond the reach of rigorous theorizing and into a non-academic la-la land where either nothing or almost anything can be said. In spite of this, Porter has obviously handled the relationship between his thinking, his writing and his public with great success in both the private and public sectors, and internationally too. Indeed one of the unremarked aspects of Porter’s work is that it has been widely accepted by management practitioners even while most of what appears in our journals has been ignored or rejected - giving rise to the rigor and relevance debate. Hambrick publicly hit this nail on the head with his 1993 Academy of Management Presidential address when he wondered ‘what if the Academy actually mattered’ (Hambrick, 1994). But he seemed to ignore the empirical evidence that while the work of most of the Academy’s leading lights did not matter much to practitioners; Porter’s work certainly did and continues to do so.

Struggling to step away from the closure and determinacy of economic theory Porter injected some indeterminacy by arguing for feedback loops (1981:616) - familiar to organizational sociologists from Giddens’s ‘structuration theory’. Plus, in the earlier paper’s short methodology section he argued for IO’s grounding in empirical work - implicitly arguing against the theoretical emphasis of neoclassical microeconomics. In the later 1991 paper he was more explicit but began by suggesting that theory-building, especially by economists, made it necessary to simplify and exclude variables and situational features real world practitioners would take into account (1991:97). These exclusions made the

result increasingly irrelevant to management practice. But at the same time Porter argued early strategic analysis had so little structure that it was hopelessly arbitrary. He sought a middle path by proposing 'frameworks' (1991:98). These would encompass many variables, especially those relevant to the "questions the user must answer in order to develop conclusions tailored to a particular industry and company". While the interactions between the variables considered could not be established rigorously, the framework would help the analyst 'think through the problem by understanding the firm and its environment and defining and selecting among the strategic alternatives available' (1991:98). In this sense a framework is a heuristic, not a theory. Thus the LCAG framework is an aid to strategic analysis, a way of drawing the strategist's attention to the most important questions to be answered. It is not a theory that enables the strategist to determine optimum resource allocations or forecast precisely the consequences of her/his actions.

Porter argued theory-building and framework-building were complementary, both being related to the situation at hand, challenging each other in terms of their differing rigor and relevance. The justification for the framework lay in its ability to capture the empirical evidence, while the justification for a theory lay not only its rigor, but also in its surviving empirical tests and thereby establishing its relevance to the particular phenomena being considered. Porter revealed he turned from rigorous economic theorizing towards case studies as the competitive behavior that interested him 'ran ahead' of the econometric data available (1991:99). He extended his earlier appreciation that firms were 'sometimes able to change the industry situation', which made it impossible to regard it as an independent variable in the SCP (structure - conduct - performance) tradition, by noting also the strategic interactions between competing firms (1991:100). This, in turn, led him to think about the basic unit of his analysis, an issue central to Common's analysis and revisited by Williamson when he argued the basic unit of analysis in his TCE theory of the firm is the 'transaction'. Given Porter was likewise working on a 'theory of the firm' as a rents-managing entity he argued the basic unit is a 'discrete activity' (1991:102). He explored some of the implications of this insight while missing what I believe is the most crucial - which is that it shifts the analysis from the realm of decision-making (the realm of theory) into the realm of situated practice (what Heidegger calls being-in-the-world). The linkages, how one action affects other parts of the real-world situation, led Porter to set out the 'value chain' as a second internal framework.

In presupposing theorizing and framework-building are mutually complementary, Porter drew on the conventional methodological view we see expressed as the productive interplay of quantitative and qualitative methods. This reflects the modernist view of the interplay of deductive and inductive or objective and interpretive approaches. A pilot interpretive phase helps the researcher uncover the variables the actors consider relevant. Then the researcher establishes a rigorous model and tests it objectively with a statistically valid sample. Such framework building might be followed by a more rigorous theory-building phase. This style of research, which dominates our journals, was not what Porter suggested. In a significant rephrasing of his earlier statements about how firms might be able to affect the variables economists took as independent, Porter wrote: "Firms inherit positions that constrain and shape their choices, but they do not determine them. They have considerable latitude in reconfiguring the value chain with which they compete ... and influencing important dimensions of their industry environment. Strategy is not a race to occupy one desirable position, but a more textured problem in which many positions can be chosen or created. Strategy requires the choice of a relatively attractive position given industry structure, the firm's circumstances and the positions of competitors. It also requires bringing all of the firm's activities into consistency with the chosen position" (1991:104).

This led Porter talk of 'managerial choices', defined as those 'made under uncertainty about the future' (1991:105). This was crucial shift away from the interplay of inductive and deductive method approach to theory for it introduced a third type of independent variable or cause - the strategist's creativity. Instead of explaining or justifying this methodological switch - on which his whole analysis stood - he blithely continued: "Numerous case-studies illustrate vividly that highly successful firms often arise out of creative acts where there were few initial strengths." (1991:105). He went on: "Many strategies clearly reflect some combination of initial conditions and creative choice. The balance ... varies ... yet there may be a tendency to overstate the role of initial conditions" (1991:106). Having made managerial choice central - and without citing one of sociology's most famous papers on 'strategic choice' (Child, 1972) - he left the reader hanging, wondering what might be said about 'creativity' and 'managerial choice'. Obviously Porter had no theory of strategic creativity, how one might tell the more creative from the less creative except ex-post and

thereby move towards a theory of the strategic firm. Instead he went on to discuss, somewhat critically, several alternative approaches to a theory of the firm, including game theory and the RBV. Little in these discussions, nor any of his other writings, specifically engaged the creativity dimension in 'managerial choices' beyond noting that the choices of previous time periods shape the contextual constraints of subsequent periods, what is often called 'path dependency'. Likewise his discussion of the relationship between activities and resources was interesting but inconclusive (1991:109).

In a long section titled '*The Origin of Origins*' Porter struggled with the contradiction between the openness implicit in creativity and the closure necessary for positive theory. He sidestepped and suggested the problem to be solved might be understood better as about creating a flexible organization that learns and is able to continually redefine itself, thereby shifting the creativity of managerial choice from the manager to the firm level, though without addressing its relationship to the firm's strategist/s (1991:110). He then went a step further and argued the originating element may lie in the firm's environment. This shift of level let him to bridge into a discussion of national competitive advantage - the 'diamond', a third Porterian framework. The bottom line here is that Porter was able to leverage successfully off the openness of the LCAG paradigm by switching levels from the solitary firm first to its industry and then second to the national economy, all the while depending on his readers' intuition that the analysis was not about theorizing or excluding the originating impact of the strategist. On the contrary, it was about generating empirically grounded heuristics that helped responsibility-bearing managers make sense of the uncertain situation they confronted. He concluded: "This style of research nudges strategy research, and indeed industrial economics, into the world of the historian" (1991:116). While this may well be true, he provided no way to connect historical and strategic research and substantiate his point.

### **Methodological Options**

At the beginning of this talk I said it is mainly about methodology but that it bears strongly on management studies, especially those dealing with innovation, entrepreneurship and strategy. It is always about the place of uncertainty in the analysis for that sets up the

questions our methodology is supposed to help us answer. Without uncertainty research is simple, we would be able to look at the world and see it as it is. Research only begins to be interesting when we realize we cannot take things for what they seem to be, and this begins with Descartes's observation that our knowledge of the world is mediated by our senses, and we know for certain these can be deceived. We are forever in a state of uncertainty about the real. Hence our questions and the methods chosen to address them get intertwined into a research strategy. There are many situations where average and aggregated behavior can be analyzed usefully within a causal model, and this may be fine for policy-makers. But it is less so for managers who deal with specific situations. The roots of our rigor and relevance problem lie in our eagerness to adopt a methodology that is intimately bound up with subordinating the strategic (creative) aspects of management to the determining firm-wide, market-wide or economy-wide generalities captured in our causal models. In the extreme, as in population ecology, we deny the significance of management altogether (Hannan & Freeman, 1977).

If we are serious about helping practitioners - and we may not be, preferring to publish and so promote our careers - we have to grant them a role more demanding than one that can be handled by an appropriately programmed computer. In my first section, I argued Porter's work's appeal and usefulness to managers lies in successfully providing them with an empirically grounded 'lens' or heuristic for paying attention to what history suggests, but cannot prove, matters. They can then focus their creativity where it makes most difference, provided they judge the uncertain situation in the particular way implied by the heuristic. For instance, the driver's heuristic, watch the front wheels of the other car to know what is going to happen next, is irrelevant on ice. Thus Porter was absolutely not telling managers that their situation is subject to deterministic theories they had better know about if they are to avoid being surprised. He was telling them that (a) their creativity mattered but (b) that it was not unbounded, rather it was constrained in ways that history could illuminate.

There are methodological issues here around the relationship between theory and heuristic, and Porter touched on these in his papers - but not very insightfully. It would have been good for him to read von Clausewitz whose treatment of this particular question is sound. Von Clausewitz argued theory helps the strategist define the constraints to the strategic alternatives, pulling together the data available and distilling its conclusions in as rigorous a

manner as possible (von Ghyczy, von Oetinger, & Bassford, 2001). Von Clausewitz was data-driven, demanding the best possible information about the battle situation. But he knew data's limits, given the 'fog or war', and he did not expect data to answer the strategist's questions. If you know the relationship between the rate of fire and the life of a cannon's barrel you are in a better condition to understand the limits to deploying a particular battery against a particular emplacement. But the soundness of the theory does not have much to do with the strategist's judgment of its relevance to the situation being confronted. It may make better sense to lay siege and let the enemy starve.

There are further methodological questions here about the relationship between generalities and particulars. Theory presupposes generalities while action is always in the realm of the particular, so there can never be a rigorous determining relationship between the two in uncertain situations. For von Clausewitz the concept of strategy presupposes uncertainty, just as it does in the work of Knight who argued that it was the source of profit (Knight, 1965). The historical method as Commons and the other industrial organization theorists would similarly be meaningless if the human condition was marked by not marked by uncertainty; and Porter's parting shot about economics pushing into history is an appeal to this tradition.

The researcher's key choices are about the notion of the situation being researched, what managers do and, in turn, how the manager is placed within that notion. These choices have major methodological implications. Much management research presupposes management means rational decision-making. This puts a premium on objectivity and keeping the manager's creativity out of the analysis. Porter's work and his distinction between theory and framework illustrated the possibilities for an entirely different research strategy, one that happens to be appealing to practitioners, perhaps because they intuit a place for themselves. In the FFD Porter used IO theorizing to point towards 5 characteristic constraints on the strategist's choices, so defining a 'strategic space' for the strategist to work within. Porter implied a theory of the firm, of course, but one only made coherent by the strategist's choices. One might say that in defining a strategic space he identified only the reasonable possibility of a firm rather than its actualization. Its closure and enactment was left to management. Methodologically this firm is very different from the firm envisaged in neoclassical economics - for that offers a prescriptive theory, denying the

manager any strategic choice. The two belong to different research programs in the same way that Gestalt psychology and sedatives are different ways of treating mental patients. That said we could question their relative value for practitioners. On the one hand the search for positivist theory which restricts managing to rational decision-making, on the other the search for heuristics which admit and shape managers' creative capabilities and choices.

I have explored Porter's work in some detail to flesh out a way of approaching managerial creativity and the theory of the firm it implies. We can now turn briefly to some other theories of the firm and wonder if they would not be understood better as frameworks. I mentioned earlier that I view the RBV as an emasculated echo of Porter's work, a position argued in my draft paper on Porter's work with Kraaijenbrink (Spender & Kraaijenbrink, 2010). The argument is simple. A single resource does not a firm make - so there is an immediate question about the RBV and the implication that there are other resources around, complementary perhaps in the manner theorized by Teece. As Grant has suggested for many years, resources need to be integrated before there is a firm (Grant, 1991). Thus one dimension of the strategic task is the integration one. Insofar as it helps with this the RBV - as a strategic heuristic - indicates only that special attention should be paid to those resources that have rent-streams attached, an attention that must address their acquisition as well as their deployment. The lack of any theory of resource integration shows the RBV is not a theory of the firm, even though it may well aspire to be a theory about firms and competitive advantage (Kraaijenbrink & Spender 2010).

There is another question to which managerial creativity might be an answer, which we can call the Penrose question. Ironically the RBV literature is inclined to cite her as its intellectual Godmother. This is unfortunate because the RBV authors are generally blind to the key Penrosian distinction between a resource and the services it provides the firm (E. T. Penrose, 1995: 25). The possibility that managerial creativity can make the services provided exceed the resource's cost is the hinge of her analysis. Those puzzled by this 'Austrian' stance may not be aware her doctoral supervisor at Johns Hopkins was Machlup, one of the movement's founders (P. Penrose & Pitelis, 2002: 19). Her thinking was a version of Knight's argument that profit comes from creatively addressing strategically selected

uncertainties. In contrast, the RBV focuses on single resources ignoring how the firm might change their value.

In Porter's framework the strategic problem was defined as sustaining the value of the firm's rent-stream against the situation's competitive pressures to reduce it. This rent-stream only arises from the firm's ownership of a 'bundle of resources'. The key point is that the value of these resources' value is their service to the firm, not their cost in the markets in which the firm is embedded. Their value is merely shaped by the market situation in which the firm's resource bundle is deployed, not determined. The value that matters is the services they provide and these are post-strategic choice rather than pre-. Again, the FFD only makes sense in terms of the firm's unique set of resources and strategically chosen ways of making them of value to the firm. The popular notion that the FFD provides the strategist an objective picture of the industry, unrelated to the services the firm's resources provide, is simply a misunderstanding of the way Porter's framework works. Thus the FFD and the RBV both presume the firm's constituent parts are resources though only Porter appreciates they are of no value until (a) they are integrated through the activities he regards as the basic unit of analysis and (b) become services and sources of value as they cohere into the firm's value chain. In contrast the RBV ignores both their integration and the activity that leads to value and profit. In short, the RBV fails to provide even a framework for identifying the situation calling for management's strategic inputs (Kraaijenbrink, Spender, & Groen, 2010). Not surprisingly the RBV, which sometimes aspires to be a theory rather than a framework, has little appeal for managers because it offers them no place for their creative inputs (Conner, 1991).

### **Principal-Agent Theory**

Other microeconomic theories of the firm may be more useful in offering managers a strategic space. My argument here is that if a ToF is conclusive, such as equilibrating supply and demand, then it denies the managers a strategic space. But if it is inconclusive, as is Porter's FFD, then it may then be a framework rather than a theory and have considerable heuristic value for managers.

Principal-agent theory is an important theory of the firm, but is it a theory or a framework? It deals with the integration of the fundamental elements of an enterprise of the form imagined by Adam Smith with its division of labor. How are heterogeneous workers to be brought into purposive collaboration when doing different things they are likely to have different knowledge and interests? Through the exercise of administrative or managerial power, obviously; but how is this to work? Are employees passive about managerial power? Principal-agent theory does not only apply to the power relationship between owners and managers. As Jensen & Meckling argued, it deals with the universal managerial problem, how people with divergent interests and, perhaps, knowledge, are to be integrated and managed (1976:309). Being economists, of course, they collapse the notion of power into financial incentive - as economists are wont to do. But the principal-agent relationship must be properly regarded as one of the basic building blocks for any theory of the firm. While bureaucratic theory deals with it loosely, the importance of the economic approach is its markedly superior rigor and ability to penetrate to the relationship's heart.

J&M defined the agency relationship as 'a contract under which one or more persons engage another person (the agent) to perform some service on their behalf which involves delegating some decision making authority to the agent' (1976:308). They noted that 'If both parties are utility maximizers there is good reason to believe the agent will not always act in the best interests of the principal. The principal can limit divergences from his interest by establishing appropriate incentives for the agent and by incurring monitoring costs designed to limit the aberrant activities of the agent' (1976:308). They defined 'agency costs' - those of controlling the agent - as the sum of (a) monitoring expenses, (b) bonding expenses, and (c) residual loss. The last refer to the fact that given non-zero (a) and (b) costs, there is some residual loss of welfare to the principal - a necessary consequence of non-zero transactions costs.

While J&M's thinking can be applied very widely their paper was focused specifically on the effect of managers sharing ownership with outside investors. The analysis went through stages of increasing complexity, first ignoring (a) and (b) costs, then taking them into account. Midway through the paper J&M wondered why, given the non-zero costs of the principal-agent relationship, the corporate form of diffused ownership was so common, and they explored various answers. In Section 5 they closed in on a formal (mathematical)

theory of corporate ownership structure. This enabled them to determine the optimal scale of operations and amounts of external financing. In Section 6 they discussed various caveats to this analysis. But the real question is whether their analysis was conclusive as theory or not. Was it actually determining? J&M believed it was. But if the analysis admitted uncertainty it would not be. So it is interesting to see how J&M sealed their analysis off from the kinds of uncertainty that practical managers take for granted. First, the J&M analysis was explicitly a single period analysis, implying spot contracts rather than incomplete contracts, thereby eliminating the uncertainties of forecasting (1976: 351). It is difficult to see how this can be related to production functions (or value chains) that are not instantaneous.

There is a more serious issue. The formality and coherence of J&M's analysis was contingent on the capital markets being 'efficient', absent which their mathematics collapse (1976:315 & 345). This assumption excludes Knightian uncertainty, of course, meaning the analysis cannot be more than an abstract exercise of no immediate relevance to managers operating under uncertainty. But it suggests an even more fundamental question. The principal-agent relationship differs from a market-based relationship between two principals precisely and only because of the decision-making authority the principal delegates to the agent within their principal-agent agreement. The principal is only going to do this when there is no efficient market for the service needed. So the circumstance that drives the principal and agent into a relationship in the first place is one in which the relevant market is not efficient. Yet their solution only works when markets are efficient. This is the contradiction at the heart of the J&M analysis. Once we abandon their efficient market assumption their formal analysis falls apart and their paper does little more than restate what has long been understood about the principal-agent relationship - that there is a practical and strategic multi-dimensional trade-off between the contractual arrangements made, the risks involved to both parties, the agency costs incurred and the inevitable sub-optimality of the outcome.

In the background are four key points. First, that the principal-agent relationship is a framework that describes a strategic space of great practical significance for managers, and that they need to pay attention to the constraints raised in J&M's paper, understanding that there is no theory that determines their 'managerial choices'. Second, the arrangement is

only entered into under conditions of uncertainty - such as when the markets are imperfect and the principal's rationality bounded. Third, the key to real principal-agent relationships is the way the parties learn to work together over time (White, 1991). Fourth, and related to the previous point, the only resource considered in J&M's analysis is financial capital. The analysis is mono-dimensional for there is no attention to human or social capital, knowledge, learning or creativity, nor such matters as reputation, trust and so on to which practical managers pay considerable attention. Many have noted how the J&M analysis of the principal-agent relationship denies the economic significance of trust and mutuality. Given their assumptions J&M's analysis is less than useful as a framework because (a) the market prices on which their analysis stands are not, in fact, available, and (b) the knowledge deficiencies and differences which make the principal-agent relationship necessary in the first place are ignored and (c) the learning and trust that characterizes real principal-agent relationships is denied.

The point of this look at J&M's analysis is less to critique it and its giant reputation than to contrast it as a framework with the somewhat related one offered by Fama. Fama's notion of the firm was a 'nexus of contracts' with resource providers. Given managerial ability as one of these resources, Fama set out to explain how the separation of ownership and control might be an efficient form of economic organization (1980:289). First he distinguished managing - the task of coordinating inputs - from the risk bearing that results from the uncertainties of the value-adding process. Risk bearing is tied up with ownership of the firm's capital, which Fama distinguished sharply from the notion of owning the firm. He regarded the second as irrelevant; obscuring the fact that control over a firm's decisions was spread widely and not concentrated in the hands of 'security owners'.

Second, he pointed out risk-bearers have access to real capital markets that enable them to spread their risks. Third, managers who rent their abilities or 'human capital' to the firm have access to another very different market for managerial labor. But they are related for the managers' value in the labor market is associated with their firm's performance. That gives them a long-term stake in its success. Having set up this multi-period multi-resource framework of a managed firm engaged with different real and imperfect capital and labor markets, Fama asked: "To what extent can the signals provided by the managerial labor market and the capital market, perhaps with other market-induced mechanisms, discipline

managers?” He went on: “ We first discuss, still in general terms, the types of discipline imposed by managerial labor markets, both within and outside the firm. We then analyze specific conditions under which this discipline is sufficient to resolve potential incentive problems that might be associated with the separation of security ownership and control” (1980:292).

In his first step, Fama considered the control exerted by the managerial labor market, noting that outside board members with ownership stakes were both invested in the firm’s reputation and seemed to be in a good position to monitor and discipline its management. The implication is that: “the role of the board in this framework is to provide a relatively low-cost mechanism for replacing or reordering top managers” (1980:294). Against this he noted that: “When management and risk-bearing are viewed as naturally separate factors of production, looking at the market for risk-bearing from the viewpoint of portfolio theory tells us that risk bearers are likely to spread their wealth across many firms and so not be interested in directly controlling the management of any individual firm.” (1980:295). Thus he concluded: “The viability of the large corporation with diffuse security ownership is better explained in terms of a model where the primary disciplining of managers comes through managerial labor markets, both within and outside the firm, with the assistance from the panoply of internal and external monitoring devices that evolve to stimulate the ongoing efficiency of the corporate form, and with the market for outside takeovers providing discipline of last resort” (1980:295).

In his second step Fama turned to analyze the impact on managers of sharing their risk-burdens with non-managers, the principal-agent problem as J&M defined it. Fama agreed that any separation gives the manager an inducement to ‘consume more on the job than is agreed in his contract’. Even though the managerial labor market process is surrounded by much uncertainty Fama argued its action leads to *ex-post* wage revaluation. Thus the key condition for the labor market to fully discipline managers is that the weight of the wage revision process is sufficient to resolve any potential incentive problems (1980:297). This is an empirical criterion, not a theoretical one, and it has two implications. First, given the uncertainty surrounding the managerial performance, its measures, and the changeability of the manager’s tastes and interests, the *ex-post* process may not work well; whether it does or not is an empirical matter and must be assessed by the strategist. Second, the wage

revision process can only work in a multi-period manner, exposing the control apparatus to the additional uncertainties of missed targets and unexpected events.

The bottom line here is that Fama sketched a dynamic time-full market-defined strategic space in which the group of those among whom strategic responsibility is diffused have to make 'managerial choices' that synthesize managing the residual risks and contracting with managers to give them decision-making authority. In contrast to J&M's single-period time-less formal model, which implied the possibility of a determining theory and shutting out managerial choice, Fama set up a framework bounded by a number of empirically grounded strategic constraints. Chief among these were the available imperfect markets for risk and managerial labor. These are different markets, of course, and the relationship between them is neither specified nor known. It follows that articulating their signals into the firm's decisions calls for strategic judgment and creativity rather than rigorous analysis. In short Fama's principal-agent framework made a strategic space for managers, just as Porter's FFD did.

### **Frameworks and Researching Strategic Choice**

The purpose of the previous sections is to suggest 'theories of the firm' are more useful to practitioners when they are frameworks and empirically-demarcate a real uncertainty-riven space for their strategic inputs. The more usual way to discuss individuals making inputs into an under-determined situation and thereby making a difference is to turn to the literature on 'agency'. Those interested in principal-agent theory have mostly ignored this substantial body of sociological and philosophical work. Plus, because J&M's article has been so widely referenced and studied, many now associate the term 'agency' with J&M's use of the term agent, which is unfortunate. Sometimes 'agent' simply means actor, as in agent-based modeling, and a firm or even a nation can be an agent in this sense.

Historically, though, agents are properly defined as those who act on someone else's behalf, such as 'FBI agent'. Ironically, J&M's paper was actually about shutting down or controlling the agency of the agent, preventing her/him from making any differences that are not in the principal's interest. To be clarify my terms, I shall use agent in the J&M sense, as one who acts on another's behalf, but when I write about an actor making a strategic input and

thereby changing a situation I shall describe her/him as being 'agentic', which is consistent with the agency literature. The effect of this is to switch from talking about managers making strategic inputs, to speaking of them as being agentic. This provides a link from the literature on management, which is relatively narrow in this area, into the literature on human agency that is much broader. It is important to note that neither ways of speaking make sense unless there is a 'strategic space'; for which the situation has to be under-determined. The agentic person then acts towards a specific place in this situation that can be changed through the agent's agentic action, a place where s/he has the capacity to make a difference.

In the first section I looked at Porter's work to help scope out the argument - which is also to sketch a method for analyzing frameworks. It contrasts with our conventional theory-oriented methodologies. I also argued that one must choose methodologies in the light of the fundamental character of the research project. Frameworks appeal to practitioners because they see a place for doing what they believe they do, making a difference. Theories do not appeal in the same way. They appeal when they help specify the strategic space more precisely. For instance, when senior managers discuss the impact of changing the price of their firm's product, it helps to have someone who can 'run the numbers' and estimate the impact on their market share. The results obviously depend on the theory of the product-market adopted - perfectly competitive perhaps, or quasi-monopolistic, or even one in which price is seen as an indicator of quality - very different theories that limit their strategic options in different ways.

The first point then is that if we are to research managers' agentic activity rather than their rationality-based rule-following, we need a framework rather than a theory, plus we need to understand how the strategic space in question is formalized or framed by multiple theories. Thus there is a variety of methods for researching management - on the one hand as decision-making, on the other as strategic choice. First, when we are theorizing about what they cannot change we proceed in one of two ways; deductively or inductively (Van de Ven, 2007). Deductive means choosing axioms, perhaps warranted by previous research, and then deriving some testable statements or hypotheses. There are two issues here. One being the validity of the deduction - a matter of logic - the second being its empirical validity - a matter of analyzing empirical findings that typically calls for statistical analysis of a mass

of empirical data. The data is itself is contingent on the validity of the observation methodology.

The issues here are the stuff of the philosophy of science and stand on the certainty, knowability and unambiguous nature of the thing to be known. If the world itself is illogical and not 'well-formed', these methods collapse. We can dub these methods Newtonian. In the social sciences we also deal with thinking actors not just unthinking objects, so there is a companion to the Newtonian method. Instead of selecting axioms from the literature, the researcher may question the thinking agents immersed in the practice and, using interpretive methods, discover the axioms they bring to their reasoning about their actions. This is generally the reason for doing a 'pilot' study. The axioms found can then be tested more formally. Thus deductive methods can be complemented by inductive methods that seek patterns in a mass of empirical data, what we might call the Baconian method. The interplay of Newtonian and Baconian methods leads towards theories.

Looking for frameworks is different. The first choice is not of the axioms that articulate what the researcher takes as certain but of the agentic entity that acts under uncertainty. It seems obvious that we should focus here on individuals - entrepreneurs, leaders, strategists and so on. But this is more contentious than it seems. There is a strong tradition of regarding firms, institutions, political parties, nations and other social collectives as agentic actors, a tradition hugely reinforced by the systems thinking we have inherited from Parsons and Barnard. Against this there is the 'Austrian' economics notion of 'methodological individualism', that argues collectives can have no characteristics or capabilities other than those of the individuals who comprise them. It is quite one thing to refer loosely to Apple Inc. as an innovative firm, quite another to believe there is something fundamental and emergent about the interaction of its employees, suppliers, customers etc. that is responsible for the firm's agentic capabilities, something that goes beyond those individuals' capabilities. This is an ontological debate and in the absence of a compelling argument for collective agency I adopt a methodological individualist position. But the choice is (a) controversial and (b) part of a larger debate about the nature of collectives of human beings and other things (Felin & Spender, 2009). Since the collective of immediate interest is the managed organization there is a risk of circularity in one's reasoning, that one is only able to find evidence that confirms one's methodological assumptions.

But does this mean individuals are the Universe's only agentic entities? This is what the Greeks called *hubris*. Given the uncertainty surrounding the human condition it may make better sense to argue that while we may have a degree of agency and are able to change some aspects of our condition, we can only think about our agency as compared against that of Nature or some-such transcendent entity, a catch-all category for every non-human agentic entity. Thus while human agency is limited by Nature, we might do well to recognize that Nature has her own evolutionary processes that lead to changes in our situation that are beyond our causing - a collision with a large meteorite for example. The point here is to suggest that the distinction between deduction and induction in theory-building methodologies should be paralleled by a distinction between human agency and non-human agency in frame-seeking methodologies (see figure 1 below).

deductive	inductive
evolutionary	agentic

Figure 1: Alternative Research Methodologies

While we do not know the full range of constraints on Nature's agency, scientific theories help us flesh some of them out - such as the Second Law of Thermodynamics. When it comes to researching human agency, we know the objective is not to theorize and thereby predict the outcome of our creative acts, just as we appreciate that 'evolutionary theory' has no predictive content. It is to discover the constraints to our agentic options, just as the second Law of Thermodynamics constrains Nature's creativity. Thus Porter highlights the firm's customer's negotiating power and the other 'forces' that bound the strategic space as a set of strategic options.

Turning back to the ToF literature we can see the FFD and the RBV deal with the strategic management of the market power that derives from ownership of valuable resources. In contrast principal-agent theory deals with the fundamental interpersonal relationship that brings the firm into existence, a relationship of administrative power. These are not the

only models we find in the ToF literature. There are at least two others (a) Alchian & Demsetz’s notion of ‘team production’ (TP) and (b) transactions cost economics (TCE). We can arrange these as a logically complete set of analytic possibilities by seeing they deal either with human beings and their capabilities or with non-human entities, such as rent-earning resources (see Figure 2 below). Note the distinction between people and resources is the one about which Penrose cautioned us. In contrast with the RBV which presumes resources have some kind of value in and of themselves - as in the VRIN mantra - Penrose told us it is the services these resources provide that matters, and these are a function of the management team’s imagination and learning.

	<i>people</i>	<i>things</i>
<i>elements</i>	principal-agent	IO/FFD/RBV
<i>entities</i>	TP	TCE

Figure 2: Alternative Theories of the Firm

### Concluding Comments

All the previous discussion is actually about setting up a research question for you - “Are TP and TCE theories or frameworks?” Are there logical inconsistencies or empirical assumptions that make theoretical closure impossible? Do they actually sketch strategic spaces managers can identify with? These are the questions we might examine in our discussions later.

My overall argument is that for us management school types the ToF literature suggests two very different research programs. The first is our version of the neoclassical economists’ search for positive theory that which shows how firms ‘must be’ if they are to be optimal. This is about us trying to be better micro economists than those in the Economics Schools. This program denies all types of uncertainty bar the strategist’s ignorance of the facts and it is attracting a fair amount of bad press. Still, the argument for this program being helpful to practitioners is that it tells them how things must be, given the full facts, and that should

helps direct them towards the relevant facts and economize on their attention and discovery processes. But, ironically, were all the relevant facts to become known, these same managers would be denied the possibility of making things different, generally something they believe they do. They would be left to do the calculating. Researching entrepreneurship, innovation, strategy and even leadership within this program seems to make little sense.

A second research program focuses on managerial creativity, but the methodological implications are immense. If we grant managers a measure of human agency, the only way we can research it is to uncover the constraints that characterize the situation in which they are being agentic. To do this with some intellectual rigor we ultimately depend on analyzing how different theories might be brought to bear as constraints, so becoming the facts of the situation. There must be a plurality here, for if a single theory was able to capture all the constraints, or if all the theories could be resolved into one, then it would determine the outcome and there would be no space left for managers to be agentic. Positing a plurality of incommensurate theories is another way of describing the uncertainty of the situation - a 'cross-sectional' way that complements the multi-period way of labeling the uncertainty of the relationships between past, present and future. Integrating these into a coherent analysis parallels the task of integrating heterogeneous resources into a coherent firm. A side issue is that theorizing tends to shut time out of the analysis, markets clear instantly or in ways that are known and can be discounted into the present, resources are transformed instantly or in ways that can be captured as a plan-able production process. Yet ignoring the uncertainties around time denies the managerial maxim that 'timing is everything'.

I outlined the second research program and gained a sense of its methodology by analyzing Porter's work. His success with the practitioner community, both private and public, reinforces the practical value of the second program and underscores the irrelevance of much of what we do within the first program. With a second program methodology in hand, we can apply it to principal-agent theory and see the sharp difference between the abstraction and internal incoherence of J&M's analysis versus the practical strategically resolved framework sketched by Fama.

Having a good look at TCE to see if it sketches a strategic space for management seems like a logical and potentially rewarding next step. Does it really do more than reframe the make or buy decision? To determine the facts here requires the manager to do more than discover the market price. S/he must also calculate the full cost, as opposed to the marginal cost, of 'the transaction' being considered. This, in turn, takes one into the uncertainties of the difference between direct costs and overheads and requires a cost analysis of the firm's infrastructure and history. Managing direct costs take us into governance under conditions of uncertainty. How, in the end, are 'information impactedness' and 'atmosphere' to figure in the analysis?

Earlier I suggested the Figure 2 indicates a deeper dimension of managerial strategy. With publication in mind, the researcher's initial characterization of the firm, selecting from among Figure 2's boxes, is the core of her/his research strategy. But this brings into view the possibility of a theory of the firm that synthesizes all four boxes. Clearly effective strategic managers are those able to conceive of all four characterizations at the same time - reminding us of von Clausewitz's notion of the intelligent person as one who can hold two or more contradictory ideas in mind at the same time, ultimately synthesizing them into a contextualized action that reflects all.

As a final note and to justify my switch into the language of agency theory, aside from suggesting a method of researching strategy and entrepreneurship etc., the second program has two broader implications. First it obviously 'respects' managers. Allowing them a creative role puts them at the same level as the researchers who, one imagines, also consider themselves creative and not bound by fully determining causes, such as their thesis supervisor or the accident of access that led Chandler to study business strategy (Chandler, 2009). The manager is then no longer objectified and 'distanced' from the researcher.

Second, noting the congruity between strategic choice and agentic action, there is a direct link to the notion of managerial responsibility. In the lived world agency, suggesting that individuals can make a difference there entails responsibility towards those affected. Thus strategic choices entail moral and ethical responsibility and connect our analysis to the Enlightenment philosophers'. There is growing interest in managerial and business ethics

and corporate social responsibility and it is difficult to know what these mean in a closed theoretical system, especially one founded on *homo economicus*. Our second program, being grounded in everyday practice, allows us to import them immediately into the analysis as the personal, psychological, cultural, religious, social and philosophical constraints to managerial agency.

## Bibliography

- Bok, D. C. (1978). *The President's Report 1977-1978*. Cambridge MA: Harvard University.
- Chandler, A. D. (2009). History and Management Practice and Thought: An Autobiography. *Journal of Management History*, 15(3), 236-260.
- Child, J. (1972). Organisation Structure, Environment and Performance. *Sociology*, 6, 1-21.
- Conner, K. R. (1991). A Historical Comparison of Resource-Based Theory and Five Schools of Thought within Industrial Organization Economics: Do We Have a New Theory of the Firm? *Journal of Management*, 17(1), 121-154.
- Demsetz, H. (1988). The Theory of the Firm Revisited. *Journal of Law, Economics, and Organization*, 4, 141-161.
- Fama, E. F. (1980). Agency Problems and the Theory of the Firm. *Journal of Political Economy*, 88, 288-307.
- Felin, T., & Spender, J.-C. (2009). An Exchange of Ideas about Knowledge Governance: Seeking First Principles and Microfoundations. In N. J. Foss & S. Michailova (Eds.), *Knowledge Governance: Processes and Perspectives* (pp. 247-271). Oxford: Oxford University Press.
- Foss, N. J., & Klein, P. G. (2005). The Theory of the Firm and Its Critics: A Stocktaking and Assessment. *Center for Knowledge Governance, Copenhagen School of Business, Working Paper 2/2005*.
- Ghoshal, S. (2005). Bad Management Theories are Destroying Good Management Practices. *Academy of Management Learning & Education*, 4(1), 75-91.
- Gibbons, R. (2005). Four Formal(izable) Theories of the Firm? *Journal of Economic Behavior & Organization*, 58 200-245.
- Grant, R. M. (1991). The Resource-Based Theory of Competitive Advantage: Implications for Strategy Formulation. *California Management Review*, 33(3), 114-135.
- Hambrick, D. C. (1994). 1993 Presidential Address - What if the Academy Actually Mattered. *Academy of Management Review*, 19(1), 11-16.
- Hannan, M. T., & Freeman, J. (1977). The Population Ecology of Organizations. *American Journal of Sociology*, 82, 929-964.
- Jensen, M. C., & Meckling, W. H. (1976). Theory of the Firm: Managerial Behavior, Agency Costs, and Ownership Structure. *Journal of Financial Economics*, 3(4), 305-360.
- Khurana, R. (2007). *From Higher Aims to Hired Hands: The Social Transformation of American Business Schools and the Unfulfilled Promise of Management as a Profession*. Princeton NJ: Princeton University Press.
- Knight, F. H. (1965). *Risk, Uncertainty and Profit*. New York: Harper & Row.
- Kraaijenbrink, J., Spender, J.-C., & Groen, A. (2010). The Resource-Base View: A Review and Assessment of Its Critiques. *Journal of Management*, 36(1), 349-372.

- Lachmann, L. M. (1994). *Expectations and the Meanings of Institutions*. London: Routledge.
- Learned, E. P., Christensen, R., Andrews, K., & Guth, W. (1965). *Business Policy: Text and Cases*. Homewood IL: Richard D. Irwin.
- Mahoney, J. (1992). Organizational Economics Within the Conversation of Strategic Management. *Advances in Strategic Management*, 8, 103-155.
- Mintzberg, H. (2004). *Managers not MBAs: A Hard Look at the Soft Practice of Managing and Management Development*. San Francisco CA: Berrett-Koehler Publishers.
- Morgan, G. (1997). *Images of Organization* (New ed.). Thousand Oaks CA: Sage.
- Nonaka, I., & Konno, N. (1998). The Concept of 'Ba': Building a Foundation for Knowledge Creation. [Article]. *California Management Review*, 40(3), 40.
- Penrose, E. T. (1995). *The Theory of the Growth of the Firm* (3rd ed.). New York: Oxford University Press.
- Penrose, P., & Pitelis, C. N. (2002). Edith Elura Tilton Penrose: Life, Contribution and Influence. In C. N. Pitelis (Ed.), *"The Growth of the Firm" The Legacy of Edith Penrose* (pp. 17-36). Oxford: Oxford university Press.
- Pfeffer, J., & Fong, C. T. (2002). The End of Business Schools? Less Success Than Meets the Eye. *Academy of Management Learning and Education*, 1(1), 78-95.
- Porter, M. E. (1981). The Contributions of Industrial Organization to Strategic Management. *Academy of Management Review*, 6(4), 609-620.
- Porter, M. E. (1991). Towards a Dynamic Theory of Strategy. *Strategic Management Journal*, 12(Special Issue, Winter), 95-117.
- Shackle, G. L. S. (1979). *Imagination and the Nature of Choice*. Edinburgh: Edinburgh University Press.
- Spender, J.-C., & Kraaijenbrink, J. (2010). Why *Competitive Strategy* Succeeds - and With Whom. In R. Huggins & H. Izushi (Eds.), *Competition, Competitive Advantage, and Clusters: The Ideas of Michael Porter* (pp. xx-yy). Oxford: Oxford University Press.
- Van de Ven, A. H. (2007). *Engaged Scholarship: A Guide for Organizational and Social Research*. Oxford: Oxford University Press.
- von Ghyczy, T., von Oetinger, B., & Bassford, C. (Eds.). (2001). *Clausewitz on Strategy: Inspiration and Insight from a Master Strategist*. New York: John Wiley & Sons.
- White, H. (1991). Agency as Control. In J. W. Pratt & R. J. Zeckhauser (Eds.), *Principals and Agents: The Structure of Business* (pp. 187-212). Boston MA: Harvard Business School Press.