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**Scientific Research Programmes,
Corporate Strategies, and the
Theory of the Firm**

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ABSTRACT

Two recent developments in the analysis of firms and market structures--the theory of contestable markets, and the transactions cost analysis of corporate strategy--are argued to imply the need for a change in methodology that their proponents mostly do not seem to have perceived: namely, that economists must develop workable ways of anticipating the behaviour of actual firms, instead of continuing to think about the world in terms of purely theoretical 'representative' firms. The paper then explores the possibility of modelling corporate decision processes as if firms operate in ways akin those hypothesized by Kuhn and by Lakatos for the case of academic sciences. Firms are depicted as behaving differently essentially because they have different corporate philosophies, different methodologies for survival in a world of uncertainty and complexity. This 'methodology of methodologies' points to new ways of anticipating corporate behaviour, which may be of practical use to economists whose role it is to advise on industrial policy.

'All firms do not behave in the same way in similar circumstances and a theory which helps to explain why they do not is perhaps to be preferred to one which asserts that they should' (Loasby, 1967, p. 167).

INTRODUCTION

If corporate strategies or government policy packages are not to produce disappointing results, it may be necessary for decision-makers to anticipate business behaviour. Economists advising such decision-makers may be deluding themselves if they think they can claim to do more than provide, for each of the firms in whose behaviour they are interested, a list of rival courses of action, any one of which the particular firm might reasonably be expected to try to pursue. A definite statement, that firm X will do Y, is likely to be the result of a failure to think carefully about the range of things the firm in question might do, and hence be dangerously misleading if policies are finely tuned to it. On the other hand, however, an unbounded listing of what the firm might do, which is not accompanied by any suggestions as to how seriously any of the possibilities should be taken, is unlikely to be very helpful. The kind of advice economists might give about possible patterns of a firm's behaviour, that a policy-maker might find helpful and not unduly blinkering, would be advice concerning a limited range of scenarios that warrant serious attention (see further, Jefferson, 1983). Economists with creative minds may be very good indeed at listing things that could happen in markets and inside firms, but that does not mean they will find it easy to narrow down the ranges of possibilities such that they do not encourage their paymasters to become over-confident or needlessly pessimistic. Guessing what a firm could reasonably be expected to do may be difficult unless one has a good idea--a reliable 'as if' approximation--of the reasoning processes employed inside the firm. The need for economists to

understand judgmental processes of firms, and how such processes might be modelled, are the subjects of this paper.

The paper begins by examining problems of indeterminacy and the consequent role of managerial judgment in two contexts that have recently been the subjects of voluminous literatures, namely market entry and exit (in the theory of contestable markets, associated with the work of Baumol, Panzar and Willig, 1982) and diversification decisions (in the transactions cost analysis of markets and hierarchies, associated with the work of Williamson, 1975, 1985, 1986). In both of these contexts, informational issues loom large. To ignore them may be dangerous for policy purposes, yet to recognize them seems to run into the spectre of a forced abandonment of claims to be undertaking a predictive kind of research in these contexts: theoretical analysis lead us to a clear perspective on the problems that decision-makers could recognize, but it seems to offer us little hope of guessing which possibilities corporate decision-makers in particular contexts could take most seriously.

Without a theory of how judgments are made in the face of ambiguity, the analysis of entry, exit and diversification is shown to be totally open-ended and the economist seems unable to do more here than outline unhelpfully large ranges of scenarios--for example, 'all firms with the capacity to enter a market might do so if incumbents allowed supernormal profits to emerge, but then again all might fear this and avoid commitment, or any outcome between these extremes could arise'--or list possible ways of trying to arrange transactions that have been identified so far (and even here one might be surprised by an innovative transactional mode), and note that the chosen one would depend upon how traders perceived the payoffs.

The rest of the paper therefore is an attempt to provide an empirically tractable framework for closing this analytical gap. It involves an exploration of the possibilities for using theories of the growth of knowledge as elements in economic analysis (rather than as devices for analyzing the growth of disciplines such as economics). It is argued that one might model firms 'as if' their decision-makers face basically the same kinds of problems as scientists. The theories developed by Kuhn and by Lakatos of how scientists try to cope with their working lives provide insights concerning what one needs to know about scientists to anticipate, within usefully narrow bounds, how they may behave in particular situations. Generalized to the context of management behaviour, these theories of the evolution of ideas seem to point towards ways of anticipating corporate evolution.

CONTESTABILITY AND CO-ORDINATION

Over the past decade there has been a revolution in the way in which most economists view the determination of industrial structure and the welfare implications of alternative structures. I refer, of course, to the theory of contestable markets associated particularly with the work of Baumol, Panzar and Willig (1982). Anyone with an ideological predisposition in favour of leaving things to market forces must have been delighted by the burgeoning of literature on contestability. Prior to this literature, mainstream economists (in contrast to deviants such as J.M. Clark, P.W.S. Andrews and many Austrians) had tended to see corporate threats to consumer welfare as varying along a market structure spectrum which had perfect competition at one end and monopoly at the other. The fewer the firms actually servicing a market, the greater was seen to be the likelihood that consumers would

suffer from higher prices and reduced innovation. By contrast, the contestability literature (like that produced earlier by Clark and by Andrews, to whom due credit is not usually given) emphasizes the power of potential competition as a device to 'keep the bastards honest'. To the extent that their markets are vulnerable to hit and run raids by existing producers in other markets, incumbent producers are restrained from seeking to exploit their customers. The vulnerability of incumbents depends upon the ease with which would-be producers can transfer their resources--both physical and cognitive--to the incumbents' line of production. If there is much scope for employing resources in new contexts, and if such moves can be made without incurring new investments in equipment and training, then any tendency by incumbents to set prices above opportunity costs of would-be producers will provoke a rapid response that will drive profits down to normal levels.

Proponents of the contestable markets methodology have conducted most of their high-powered theoretical analysis with reference to the abstract case of perfect contestability--that is, the situation in which both entry and exit are absolutely free with no sunk costs being incurred by entrants. This case happens to be convenient for formal modelling, as well as being seen to provide the basis for a welfare ideal. However, the focus upon it is something which proponents seek to justify in instrumentalist terms, as the following passage from Baumol (1982, p. 8) serves to indicate:

While the industry structures which emerge in reality are not always those which minimize costs, they will constitute reasonable approximations to the efficient structures. If this is not so it is difficult to account for the similarities in the patterns of industry structure that one observes in different countries. Why else do we not see agriculture organized as an oligopoly in any free market economy, or automobiles produced by 10,000 firms? Market pressures must surely make any very inefficient market structures vulnerable to entry, to displacement by foreign competition or to undermining in other

ways. If that is so, the market structure that is called for by contestability theory may not prove too bad an approximation to what we encounter in reality.

With the aid of such arguments, industrial economists are getting themselves accustomed to thinking 'as if' easy entry is the normal state of affairs.

What seems to have been forgotten in this revolution in monopoly theory (except by Wilson, 1984, pp. 226-30) is that the new literature is entirely orthodox in its focus upon outcomes of competitive battles rather than upon processes by which industries evolve and achieve some semblance of order (which it presumes they do). In equilibrium, incumbent firms recognize the disciplinary capacities of potential producers and do not misbehave themselves. No signals of potential profit opportunities are generated, so no entry takes place. Would-be hit and run raiders concentrate their attention on markets that are still out of equilibrium, and they make their forays into the most attractive of these. However, at no point does the new literature discuss the question of how these raiders come by the information necessary to make such decisions. Contestability theory ignores the question of co-ordination and what deserves to be called the Richardson Problem, after the work of G.B. Richardson (1959, 1960), the neglect of which has recently prompted a useful review paper by Loasby (1986).

Richardson's work centres on the fact that when a firm is contemplating production in a particular market it needs to have an idea not merely of the demand for the product in question, but also of the output plans of other producers; for the supply policies of the latter will constrain the sales revenues that the former can achieve. The inability of individual farmers to outguess each other's behaviour is, of course, the basis of one of the essential set pieces of any introductory course on supply and demand, namely, the 'cobweb' or 'hog-cycle'. But most economists,

unlike Richardson, never seem to stop and consider potential for similar co-ordination failures outside of agricultural markets in a world where there is no Walrasian auctioneer to ensure that producers' plans are pre-reconciled.

Richardson argues that co-ordination is facilitated by two main factors. Unfortunately, the first of these that I will consider is called into question by the new literature on contestability. It happens to be the case that consideration of the second factor seems to point us in the direction of studying actual firms if we are concerned with practical policy formation.

The first possibility that may preclude co-ordination failures is the inability of some producers who are aware of a profit opportunity to act upon it. In agricultural markets, it is the ability of farmers to change the crops that they are growing at seeding time that opens up the possibility of destabilizing price shifts: there can be very large changes in the amount of land committed to particular crops if many farmers can grow these crops and believe it could be profitable to do so. However, if there are only a few firms who have the capacity to produce, the scope for wild lurches in production is limited: the individual firm will find it difficult to expand production in giant leaps without running into managerial problems, even if it can obtain finance to do so (Penrose, 1959; Richardson, 1964). Moreover, if only a few firms can produce, then collusion, whether implicit or explicit, will be easier to conduct, while it may be expected to be easier for these firms to get an idea of each other's plans, whether by industrial espionage or as a result of deliberate pre-emptive announcements by first-movers in the race to acquire market share (see Porter, 1980, Ch. 15).

The contestability literature is clearly at odds with this route to market coherence. Incumbents may recognize the need to behave competitively, but that does not guarantee they will, if they act independently, produce a total volume of output that is large enough to prevent them from earning supernormal profits. If they misjudge things, they will make the market in the next period interesting to other producers. Even if incumbents otherwise would judge correctly, potential producers might believe that, unless some new entry takes place, supernormal profits will be enjoyed in the present period by incumbents. The incorrect appraisals by the potential producers would then tempt them inadvertently into contributing towards the production of a glut.

Of course, if entry involved no new sunk costs (for example, firms might use existing sales teams and spare machinery capacity), any failure of co-ordination only imposes a cost on consumers in terms of wrongly allocated current costs of raw materials and labour time--a situation obviously less black than that painted by Joan Robinson (1954) in her paper on the entry problem. However, if the system struggled to achieve coherence over a long period, even these non-capital losses could turn out to be considerable. Such worries become all the more acute when one notes that many products have market lifecycles. Whenever demand is expanding or contracting, ease of entry combined with costless exit would forever be opening up scope for production plans that aggregated to produce supernormal profits or losses on current commitments. The awareness by firms of the scope for a disastrous entry could, of course, result in them holding back and leaving the profit opportunity there for the taking, with consumer wants going unsatisfied and workers unhired (cf. the use of Richardson's work in macroeconomics by Leijonhufvud, 1968, pp. 69-70). There is no reason to suppose that, amongst

those who are aware of the profit opportunity and could enter, optimism and pessimism weigh against each other in such a way as to produce precisely the right amount of output at each point. No longer does it seem logical to presume that something approximating perfect contestability is to be encouraged as a means of enhancing consumer well-being. Static contestability theory is potentially misleading for public policymakers.

To be on the safe side, then, we should take a dynamic view of contestability and feel drawn to Richardson's second co-ordination-facilitating phenomenon: the possibility that firms with the ability, in principle, to offer competitive performances in particular markets where equilibrium does not prevail simply do not see these markets as places where profit opportunities exist. They may be unaware of the scope for making money by transferring their resources to these market niches or may see better returns to using them elsewhere because, in these other contexts, they see fewer risks from competitive entry.

Richardson's analysis here depends on firms perceiving things differently, and clearly conflicts with usual tendencies of economists to think in terms of 'typical' firms. It seems to imply that we can come to no conclusions about the likelihood of chaos or coherence in a particular market unless we can:

- (a) model the ways in which firms who would consider themselves able to produce in the market of interest make judgments about the merits of that market and size up whom their rivals might be and how seriously they should be taken as threats; and this requires that we can
- (b) anticipate which firms might consider themselves potential entrants.

In other words, to make use of Richardson's insight when considering the merits of alternative policy proposals, we clearly need a means for anticipating the subjective opportunity costs of actual firms.

TRANSACTIONS COSTS AND DIVERSIFICATION

Surprisingly rarely mentioned simultaneously with the contestability literature is the recent 'markets and hierarchies' literature. This explores the possibility of explaining the extent to which economic activities are conducted within the boundaries of a firm, rather than between separate firms, with the aid of the idea that some transactions are more difficult to organize than others in a reliable manner. The new literature, much inspired by Coase's (1937) seminal but long-neglected paper on the nature of the firm, is particularly associated with the work of Williamson (1975, 1985, 1986), but Williamson's work has been perceptively developed by Kay (1982), who integrates it with themes from the business policy literature. The basic idea is that contracts between traders can be costly to draw up, the more so the more complex and surprise-prone the environment (because there will need to be more 'small print' to cover significantly different states of the world) and the smaller the number of potential buyers and sellers (since the greater is the scope for haggling, with one or both parties feeling that the other is behaving in a guileful manner, giving less than they would give if pushed). The literature highlights two possible advantages of bringing several related activities within a single corporate whole. First, guileful behaviour may be attenuated (if would-be opportunists recognize it is in their own interests to help the corporate whole). Second, the costs of specifying innumerable contingent obligations and exclusion clauses, or arranging very short term contracts, can be reduced by using a team of

managers to direct the people who are employed through loosely-specified contracts. Attempts to expand infinitely the size of a firm in a bid to economize on the costs of using markets are, however, thwarted by the limited ability of managers to handle information.

Much of Williamson's work gives the impression that he believes corporate decision-makers can discover optimal ways of drawing up boundaries between firms and of organizing managerial systems to co-ordinate activities inside firms. The business history literature, by contrast, suggests that similar end products may be produced successfully over significant time periods by companies that embrace very different collections of activities and organizational structures. I will illustrate this claim with reference to examples of vertical integration and horizontal diversification.

In the transactions cost literature, strategies involving vertical integration are assumed to arise because firms have experienced or fear difficulties in obtaining reliable input supplies or downstream distribution via market contracting and now judge it better to incur the opportunity costs of 'doing it oneself'. However, as Kay stresses, vertical integration places the firm more at the mercy of sweeping environmental and technical changes: it locks the firm into a particular way of making money, so should not be undertaken lightly. Not only this, but, once internalized, upstream and downstream production and distribution stages may feel a lessening in pressures not to behave in an opportunistic manner, the more so the bigger the exit costs the firm would face if it went back to using subcontractors, distribution agents and so on. Subtle variations on vertical integration seem possible as means of exerting pressures to reduce the incidence of opportunistic practices, making both the firm's market environment and its internal activities behave as if they were more contestable. For example,

partial shareholdings may be taken in one's component suppliers, whilst one might engage in taper integration--make some inputs of a given kind oneself, subcontract the rest--to hedge one's bets against internal disruptions or dramatic market changes.

The risks that firms associate with alternative vertical integration approaches need not be identical even for firms involved in producing similar products at a similar volume: processes by which risks are assessed may be different, while whether a risk is considered at all may depend upon the actual experiences of the decision-makers or their capacities for indulging in paranoid and pessimistic thinking. The early years of the British car industry illustrate neatly the indeterminacy of the vertical integration problem. William Morris relied very heavily on subcontracting, in contrast to his rival Herbert Austin (see the respective business biographies by Overy, 1976, and Church, 1979). Austin's revealed preference for do-it-oneself seems to owe much to his early experience of being let down by suppliers whilst working for the Wolseley sheepshearing company (see Church, 1979, Ch. 1). Admittedly, both entrepreneurs gradually came to employ mixed strategies, but the process of convergence took many years. In the same industry today, one can observe all manner of vertical integration policies being used with firms moving in opposite directions as they experiment with what they each see as possible ways of ensuring their survival.

A similar lack of 'obvious' solutions is evident when one looks at strategies of horizontal diversification. In Kay's analysis of the problems that firms may perceive, diversification is seen as a means of avoiding the risks of having 'all one's eggs in one basket' in times of rapid technical change and product obsolescence. It is facilitated by the existence of spare

corporate capacity: financial resources, to purchase new assets or take over existing firms; managerial resources, necessary to co-ordinate the larger product portfolio; and spare machinery, skills, and market reputation which may be transferred in a synergistic manner to other uses insofar as they enable the firm to avoid to incur the sort of start-up costs that might deter other firms or place them in positions of weakness. A firm with spare resources could return them to the market instead of using them as a basis for taking on new activities. Bigger dividends could then be used by shareholders as a basis for their own diversification. Funds could be used to purchase non-controlling stakes in other companies, as an alternative means of hedging. Spare equipment and factory space could be rented out. Spare skills could be made available on a consultancy basis, while royalties could be charged on the use of one's brand name. The list of possibilities goes on and on. The transactions cost approach suggests that the options preferred by firms will be those that economize on transactions costs, whether as a result of the existence of economies of scale in transacting (small shareholders might prefer not to have to reinvest dividends, for example) or which could arise through attempts to trade synergy (a user of one's brand name might fail spectacularly, damaging one's own product images without there being scope for obtaining redress through the courts; or technological expertise might leak out of the firm's control). Once again, the possibility arises that different firms could be observed experimenting, without obviously getting into trouble, with very different strategies.

Kay (1982, pp. 50-1) demonstrates this point by noting the case of the UK soft drinks industry in 1969-70, which was dominated by three firms. Allied Breweries could be construed as aiming for synergy, with little attempt being made to hedge, for they manufactured and marketed only soft

and alcoholic drinks. At the other extreme, Reckitt and Coleman were involved with a seemingly largely unrelated mix of products: in addition to soft drinks, they made and marketed pharmaceuticals, mustard, shoe polish and disinfectants. Somewhere in between, Cadbury/Schweppes seemed to be exploiting the food and drink theme across a range of cakes, preserves, canned foods and convenience foods, in addition to soft drinks.

It is not hard to see why, despite their competitive nature, such heterogenous experiments may be failing to reveal the strategy to adopt. A product portfolio with few linkages between activities would offer little scope for synergy, but it would be easy to organize the firm as a series of profit centres, and thereby put pressure on the various divisions to deliver strong performances. A potentially synergy-rich firm might be operating with surprisingly high outlays per unit of output, owing to the incompatibility of a 'profit centres' approach to organization with the need for a function-based organization as a means of benefiting from shared resources (see Kay, 1982, pp. 150-2). Such scenarios point to the usefulness of the transactions cost framework as a device for alerting the economist to possible modes of industrial organization and possible diverse risks and advantages associated with them. But they also highlight the predictive limitations of a methodology which provides no means for anticipating whether and how such possibilities might be judged by actual decision-makers.

TOWARDS THE STRUCTURE OF CORPORATE REVOLUTIONS

In suggesting that diversification strategies might be characterized as on-going experiments concerned with corporate survival and prosperity, I was trying to foreshadow the paper's move in the direction of a full-blown

'firms are like sciences' analogy. However, thoughts about the non-replicability of experiments in business policy may have been enough to raise doubts in some readers' minds about the merits of pursuing such an analogy. The case for doing so is perhaps most easily appreciated if one has an idea of how I stumbled across it: by noting how an economist learns, one may achieve a better grasp of how corporate decision-makers learn to cope with the world of business--there is a reflexive dimension to this paper which should not be overlooked. The analysis emerged originally between 1977 and 1979, from the conjunction of four elements: (1) my growing interest in behavioural economics because of the limitations I perceived in more mainstream approaches; (2) a related interest in the competitive struggle between different economic methodologies, which led me to read Kuhn (1962) and, later, Lakatos (1970); (3) my broader interest in comparative economic development, in reasons for the relative rise and decline of whole economies; and (4) my discovering the potential for reflexive thinking, as a result of reading Loasby (1976).

One of the distinguishing features of a behavioural economist--indeed the feature responsible for the adjective 'behavioural'--is a tendency to try to use observations of actual behaviour for theoretical inspiration (cf. Cyert and March, 1963, p. 1). To theorize about firms, the behavioural economist begins by studying the operations of firms, either in the field or with the aid of documentary materials such as business history works and case-study reports in management journals such as Fortune or Management Today. This is essentially an inductive kind of procedure, but the behavioural theorist of the firm would, of course, accept the Popperian view that observations can only be made with the aid of a prior theory, to define both the area of search and what the theorist expects to find: whether or

not something is noticed as present or absent will depend upon whether one was looking for it; whether or not things seem surprising will depend upon what one expected. The analysis developed in my (1984) book and in the following sections would have been most unlikely to have emerged were it not for the context in which I was pursuing my interest in the behavioural methodology.

My journey in the direction of a Kuhnian view of the firm began with a consideration of the implications of one of the innovative elements in the behavioural theory of the firm proposed by Cyert and March (1963), namely, the concept of 'organizational slack'--the difference between the returns that members of a firm are enjoying and the minimum returns they require in order to ensure their continued membership. Ideally, individuals would prefer higher returns than they are presently receiving, even if they are enjoying some 'slack payments' of their own. But to try to claim a bigger share of the corporate pie for themselves is a hazardous undertaking: if they push their luck too far, some parties may quit, leaving the former worse off than before. In good times, therefore, people may hold back from possibly disastrous experiments in finding out how much others are prepared to give up. In bad times, when inaction may also be disastrous, experiments aimed at preserving one's own position, at the expense of someone else, may seem worthwhile. Then, managers may risk the wrath of their shareholders and bankers by cutting dividends, or by running up huge losses and running down their liquidity positions. When it looks like further moves in these directions may be too risky, they may turn their attention to their workforces, and cut manning levels in ways they would not dare to do in prosperous times. By taking up such forms of slack, the managers may keep their firms going along their previous pathways despite buffeting from the

environment, and they may avoid making any major changes of direction in terms of the products they make, the markets they serve or their techniques of production. A major implication of organizational slack seemed to be that it drove a wedge between market incentives and corporate responses, and called into question the conventional economist's deterministic philosophy.

While the possible significance of organizational slack was uppermost in my mind, I had occasion to encounter some works on the gloomy macroeconomic histories of some Latin American banana republics. These led me to question the wisdom of presuming that devaluations and import controls will necessarily evoke an appropriate and swift response from corporate decision-makers: such measures might simply augment the buffering permitted by organizational slack and enable firms further to postpone structural changes. My worries about the practical implications of this possibility actually concerned structural problems in the British economy, at the time when the Left were arguing the case for import controls and Margaret Thatcher was seeking election on the promise that she would tighten up the economic environment and produce an industrial structure that was 'leaner and fitter', more able to survive the long-run pressures of international competition. Ten years later, the prospective lack of responsiveness of the Australian economy gives me a similar cause for concern: following the fall of the Australian dollar, the question arises as to how long we need wait for the J-curve to turn up, and for changes taking us away from an undue specialization in primary products. It did, of course, occur to me that it could be dangerous to generalize the Argentinian or Uruguayan experience when considering the prospects for Britain (and now, Australia) under alternative policy regimes. Nonetheless, the issue of possible tendencies to

resist change seemed worth investigating and I was further encouraged to pursue the slackness/tautness dichotomy by the work of Hirschman (1970).

As it turned out, more often than not business histories did indeed reveal strong resistance to change on the part of large firms, and I found a similar worry being voiced by Checkland (1970, pp. 559-60) at the end of his review of Coleman's (1969) history of Courtaulds. He asked:

Is it inherent in the growth of a firm, and indeed of all great organizations, that they cannot adjust to change continuously, but must reach some critical level of vulnerability before a response is forthcoming?

It suddenly dawned upon me that there were striking parallels between the patterns of evolution that firms often seemed to trace, and Kuhn's (1962) view of the development of scientific knowledge, in which periods of steady evolution down particular channels were separated by revolutionary phases. In fact, one could replace references to science in Kuhn's analysis with references to business and end up with a description of what was evident in the business history literature. The following amended summary of Kuhn's views on the structure of scientific revolutions (based on one in Ward, 1972, pp. 33-4) seems to fit pretty well in the context of long-run corporate change:

- (1) A new strategy will normally emerge in a firm only after a pronounced failure of the problem-solving activities of the old one.
- (2) A significant sign of the breakdown of the strategy is the proliferation of alternative possibilities and of methodological debates.
- (3) The solution to a corporate crisis will have been at least partially anticipated before, but such anticipations will have been ignored or swept aside in the absence of a crisis.

- (4) The conceptual framework of the old strategy exerts a powerful inertial effect on the manager who uses it, and older managers usually do not absorb the concepts involved in the new strategy.
- (5) The new strategy redefines a number of puzzles the firm has been facing and may generate new ones. The process of redefinition often means that adherents to old and new strategies will talk past one another.
- (6) The new strategy emerges over a limited timespan. The full emergence of a corporate crisis and of a solution which attracts significant adherents may take as long as a decade or more. Coming to terms with the new framework is also a lengthy process.
- (7) Managers who succeed in making the transition from believing in the acceptability of one strategy to favouring a new one will often experience a discontinuous shift in their views of their firms and the business environment.

This analysis clearly presumed some definition or other of the term 'strategy', a term not then common in economists' writings on the theory of the firm. The sort of definition I had in mind is the same as that recently employed by Lorsch (1986, p. 95), in a paper which reveals a similar line of thinking to some of that which I eventually published under the title The Corporate Imagination (Earl, 1984). Lorsch writes that:

By strategy I mean the decisions taken over time by top managers, which, when understood as a whole, reveal the goals they are seeking and the means used to reach these goals. Such a definition of strategy is different from common business use of the term in that it does not refer to an explicit plan. In fact, by my definition strategy may be implicit as well as explicit.

The 'implicit' part of a strategy involves things which managers will bring to mind only if they realise that they are considering possible courses of

action which conflict with their unwritten views of the nature of themselves and/or their firm's business; indeed, managers may not even be conscious of some of the means they use to reach particular (written or implicit) goals, and a lot of the time could be usefully thought as being on 'autopilot'.

Having drawn this parallel between business and science with respect to revolutionary, crisis phases, it was but a short step for me to consider whether it might be worth trying to characterize firms in normal times in terms of notions from theories of the growth of knowledge. This is not the approach that Lorsch uses, for his paper is part of a new wave of literature produced by organizational sociologists and members of the management profession on the concept of 'corporate culture' (see, for example, Burker, 1983; Jelinek, Smircich and Hirsch, 1983; Uttal, 1983; Adler, 1986; Kilmann, Saxton and Serpa, 1986; cf. also the broader perspective in Douglas and Wildavsky, 1982). Whilst it is nowadays fashionable to talk of corporate cultures in such circles, the concept itself is hardly new: it is a major theme in the work of Selznick (1957), which had been one of my first sources of inspirational case histories. Although the corporate culture literature complements the present paper and my (1984) work, I believe the 'growth of knowledge' perspective goes somewhat further, for it provides a theoretical basis for the existence of corporate cultures and strategies, not merely another language for characterizing them.

DECISION-MAKING METHODOLOGIES

In pursuing the 'firms are like sciences' analogy, an initial question to ask was: in what ways are the day-to-day problems faced by corporate decision-makers similar to those faced by scientists? Two similarities are particularly apparent.

First, there is the basic problem of the complexity of the puzzles with which both groups are dealing, and the very many ways in which they might be tackled. Both groups have to form hypotheses about the situations they need to understand in order to meet their goals (these hypotheses include conjectures about which situations are worth their attention). In forming their conjectures, both groups have to decide upon the level and mode of abstraction that is appropriate for the problem at hand. Both groups often recognize that failure to simplify may result in one being unable to 'see the wood for the trees', while over-simplification may mean that one is blind to many significant possibilities. This point can be illustrated particularly well if we recognize that the problems faced by managers are precisely the problems that economists and other social scientists seek to model as onlookers. Just as economists may argue amongst themselves over, for example, the need to incorporate an organizational perspective in their models of corporate behaviour, so we should not be surprised to find debates amongst managers about how far they can simplify their own tasks and the pictures they build up of their rivals, customers and general market environments. The sheer complexity of their firm's operations will mean that, at best, top-level managers are going to be working with an approximate picture of the capabilities of the human and physical resources at their disposal. They will only have an approximate idea of the outcome of any directive that they give, for they cannot be sure what will happen at lower levels in their own organizations, let alone what will happen in the market as a result of the decisions taken by other firms and potential customers.

On top of the need to simplify to cope with potential information overload, social scientists (much more so than physical/natural scientists)

and corporate decision-makers have to build more simple models than they would ideally like owing to the impossibility or high costs of obtaining relevant information. Of course, managers in a firm do have one advantage over economists and other social scientists: in principle, at least, they have ready access to their own information systems and can interview their own employees about the operations of the firm, even if in practice they simply do not have the time or mental capacities to gather and process such knowledge. Otherwise, though, the situations are similar, and economists and managers will be frequently cast in the role of external observers of firms and individuals whose behaviour they need to anticipate. In such situations, they may have to form conjectures very much in the light of past market observations, and with the aid of published statements, interviews and 'leaks'.

The second problem common to firms and sciences concerns the interpretation of available information that relates to the conjectures they are using. The two groups can be said both to have a problem of knowledge, of deciding what they know about their areas of interest and where they are uncertain. Here they must grapple with the Duhem-Quine problem (after Duhem, 1906, Quine, 1951): in trying to find things out, they can never test hypotheses one at a time. For example, when a chemist encounters surprising results, these could be due to any one of, or any conjunction of, a variety of causes: faulty temperature measurements, impure compounds, dirty equipment, or even--and this is what really interests the chemist--a flaw in the theory being tested. However, to check, for example, one's thermometer requires the use of other instruments, which may give spurious indications of accuracy or inaccuracy, and which can only be checked themselves with the aid of yet other instruments. Likewise, disappointing corporate profits

might indicate that one's hunches about the market are wrong (not as much demand for the product as expected), that the production department has not delivered the expected standard of product, that the sales staff have been unexpectedly incompetent at selling it, and so on. Alternatively, there might simply be something strange happening in one's internal reporting system. It is impossible to check any of these possible explanations, or any others, without taking on trust other, potentially misleading assumptions or predictions from theories (cf. Loasby, 1976, pp. 138-9). Scientists and managers alike seem to face a potential problem of infinite regress every time they make a judgment.

As far as scientists are concerned, Lakatos (1970) argued it may be useful to think of them as trying to make headway in the face of these two problems by working according to the dictates of particular scientific research programmes--what Kuhn would call paradigms. So long as they do not find themselves having to make ad hoc adjustments to their theories in order to explain what they observe, they will have no inclination to change from one research programme to another. A new research programme will not necessarily be attractive even if a hitherto reliable one experiences difficulties in matching up expectations with evidence: the new one may not yet have been developed to the stage where it can deal with the same kind of range of questions as the old, even if it offers a way of coping with the particular area in which the old one seems to be failing.

Lakatos portrayed a scientific research programme as consisting of a 'hard core' buffered from the outside world by a 'protective belt'. The hard core has descriptive and normative components. The former specifies the fundamental characteristics of the scientist's view of the world, and is the means by which the Duhem-Quine problem is confronted. The scientist treats a

set of propositions, which may or may not be in principle open to empirical challenge, as if they are unshakeable truths which cannot be modified in the light of evidence. The protective belt, by contrast, is the scientist's collection of propositions--'auxiliary hypotheses'--that have been classified as potentially malleable or, as a last resort, disposable. (Turning the 'firms are like sciences' theme around, one might want to call the protective belt the research programme's zone of organizational slack!) So long as anomalous observations can be dealt with by modifying part of the protective belt without recourse to ad hoc measures or jettisoning part of it outright, then the research programme is functioning satisfactorily even though it has not achieved such perfection as to require no further development.

The normative part of the hard core guides the development of the research programme's range of compass and how it copes with potentially threatening anomalies. Lakatos called it the 'positive heuristic' (prohibiting elements in it are often grouped separately as the 'negative heuristic') and his pupil Latsis (1976, p. 16) characterized it succinctly as 'a set of imperatives which contain guidance as to how the programme should unfold, how it should be defended, what falls within and what falls outside its scope... [and] it cannot be given up without giving up the research programme itself'. The Lakatosian view of how scientists choose to develop their ideas and cope with difficulties is essentially the same as Simon's (1976) view of decision-making as a process of procedural rationality. Rather than consider myriad possibilities seriously, scientists tackle problems with the aid of sets of rules that define appropriate conducts; they use 'recipes for success' rather than getting bogged down in thinking about alternative means to their ends.

A Lakatosian view of the firm treats the nature of a 'corporate strategy' as identical to a scientific research programme. Managers in firms may be expected likewise to build their strategies around particular assumptions--about the business world and their own capabilities--that they would not for a moment think of challenging in the normal course of business. These hard core notions may or may not be explicitly written down or verbalized. Examples could include: 'The government will not allow a company as big as our one to go under', or 'Our geographic position provides us with a cost advantage', or 'Major customers like big suppliers committed to the industry'. The latter two examples come from the Kuhn-like work of Lorsch (1986, p. 99)--work which, like that of Lakatos, portrays beliefs as being hierarchically structured with the result that, in times of trouble, managers may initially 'bend the less-central principle than those at the core' (p. 100). A good example of some of an individual worker's normative heuristics occurs in the paper by Kilmann et al. (1986, p. 90): 'Don't disagree with your boss, don't rock the boat, do the minimum to get by, don't socialize with your boss, only wear dark business suits to work...'. An example at a higher level would be 'retreat up-market in the face of Japanese competition, for there they will not be able to match our quality and undercut the costs of craftsmanship through mass production techniques' (this was the rule disastrously employed by the British motorcycle industry in the late 1960s and early 1970s: see Boston Consulting Group, 1975).

Once one starts trying to think of the firm from the standpoint just outlined, several questions can arise:

- (1) What are the origins of managers' decision-making methodologies: in other words, how do managers learn how to learn?

- (2) Does the use of a Lakatosian framework offer the economist the prospect of an improved way of making sense of past decisions of firms and of anticipating future behaviour by them?
- (3) Those familiar with the work of Remenyi (1979)--who extended Lakatos' analysis to examine the evolution of subdisciplines and emergence of new research programmes from within subdisciplines--might also wish to ask: Can Remenyi's theory of core/demi-core interactions help us understand the long term evolution of corporate strategies in cases where the original core line of business becomes peripheral or non-existent (for example, Bendix no longer makes washing machines, and the Adelaide Steamship Company is nowadays little interested in shipping)?

The next two sections deal with the first two questions; to do justice to the third question would require a separate paper.

THE ORIGINS OF MANAGEMENT PHILOSOPHIES

In some cases, a decision-maker learns how to cope purely through a series of personally designed and possibly traumatic experiments: the decision-maker herself creatively constructs a representation of the problem at hand and infers a possible solution. This construction is then tried for its fit against reality and the results examined: the fit might appear, from the particular higher level judgmental standpoint that is being employed, to be excellent, ambiguous or very bad (cf. Kelly, 1955, Ch. 1). Doubtful or poor fits inspire the creation of alternative constructions and further experimentation. In due course, so long as the experimenting decision-maker does not run out of resources beforehand (in the context of a business,

so long as the manager does not drive her firm out of business), a methodology which seems to promise satisfactory results may emerge.

The go-it-alone trial and error route to a personal paradigm/research programme/philosophy of life/strategy is probably of far less importance than social learning of particular, pre-existing doctrines (though this is not to say that all who listen to the same source of inspiration will come out of the experience with the same picture of how they should subsequently proceed, for each will have personally to make sense of what is being suggested). Just as the ways in which economists think may be in large part a consequence of where they received their formative training and the departments within which they pursued their initial research activities, so management 'styles' may emerge from particular university or business school backgrounds (for example, managers may have learnt 'The Gospel According to Harvard Business School' and see the best-selling texts by Porter [1980, 1985] as their 'bibles' for appraising markets and their competitors); from the experience of seeing management consultants' alternative perspectives on their firms; or, perhaps most importantly, from a gradual assimilation of their firm's established 'corporate culture', perhaps after an initial highly intensive induction programme.

Obviously, things could start getting unmanageable if it were necessary for economists always to think of management teams as collections of many individuals, each of whom had been through somewhat different experiences and possessed unique personal paradigms. While it is undoubtedly true that, even despite the best attempts of management induction programmes, corporations are not staffed by clones, we may often be able usefully to think about an individual firm 'as if' its top-level decision-makers all work with the same underlying philosophy, which may go far beyond

what has been explicitly set down on paper in a current corporate plan. We could call such a philosophy the firm's 'corporate imagination'.

In making such a simplification one should not, of course, forget the co-evolutionary nature of the corporate whole and the individuals that are members of it: individual creativity is unlikely in the long-run to be totally swamped by prior views that 'this is how we think and what we do at XYZ Pty. Ltd.', so the corporate philosophy will normally evolve as a result of the particular contributions of individuals, just as individual personalities will in some degree be affected by their experiences in the organization. Nonetheless, in the short-run, it may be useful to think of the firm as a whole, with a particular personality of its own, and as an organization whose behaviour is in large part channellized by its philosophy despite turnover of personnel. Significant new appointments fortunately tend to be accompanied by statements in the business press by the appointees, concerning any major changes of philosophy that they hope to inculcate.

In a firm where goals are proving difficult to meet and the seeds of a corporate revolution are starting to germinate, the firm's corporate imagination may seem horribly confused, with a variety of different warring camps bickering with each other. On these occasions the competing philosophies may suggest very different scenarios concerning the future development of the firm, events depending crucially on which group succeeds in winning control and staging its revolution. In the interim, one could well expect there to be difficulties when it came to reaching decisions about patterns of diversification: wildly inconsistent, schizophrenic behaviour or simple drift could both be serious possibilities in this situation, which could be problematic for policy-makers in other organizations.

PHILOSOPHIES AND PREDICTION: SALVATION FOR THE SUBJECTIVIST?

Although individual and social processes of learning leave managers equipped with methodologies for trying to cope with their jobs, it should not be forgotten that the learning processes do not leave them with fully fleshed out understandings of cause and effect, of how the system they seek to understand works in all its complexity, and of what it would be best to do in particular situations that one day may arise. They have may have judged that, in previous situations, particular routines seem to have worked; yet, as with experience in using recipes from a cookbook which does not attempt to explain the underlying physics and chemistry of cookery, they may have little idea why the results were produced. Their knowledge is fragmentary and of its essence conjectural, but this will not stop them from trying to apply it to situations that often bear only questionable similarities with past experience. The limitations of managerial methodologies may sometimes produce outcomes that are not intended by those that use them, but they turn out to be a boon for the economist.

To suggest that we start thinking of firms as composed of individuals who use particular collections of rules for sizing up problem environments may strike some readers as likely to take us away from the possibility of anticipating behaviour. Despite the 'corporate imagination' argument for glossing over differences amongst the philosophies of individual managers, the approach I have outlined may seem merely to add another dimension of complexity to the firm. However, Heiner (1983) suggests that economists could find themselves hopelessly lost were it not for the use of stereotyped rules and routines by decision-makers. Such rules reduce the range of possibilities with which we need deal, for, in using rules as substitutes for complete insights into underlying 'objective' constraints and

technologies, decision-makers treat unique situations as partial replications of others instead of trying to derive singular, optimal solutions for the decision problems that they pose.

Now, to the extent that managers in firms do take major strategic decisions on the basis of broad corporate philosophies rather than in the light of an extensive knowledge of lower level constraints and of the internal operations of their competitors, it seems the economist may need only to know the key features of these philosophies in order to be able to anticipate with tolerable accuracy the behaviour of the firms that employ them. Indeed, if these rules are of a simple 'if ..., then ...' form, courses of action are being selected without any consideration of alternatives: anticipating likely choices becomes simply a matter of finding out which such rules might be used, rather than the more complex task of understanding which rules are used to construct agendas of possible courses of action and which other rules are then used to pick particular options from these lists.

Consider the position of an economist who happens to know the background of the managers of particular companies, and which 'cookbooks' they use as their 'bibles' for decision-making. Suppose, further, that the economist has access to the same information as the actual decision-makers could be expected to possess about their rivals (that is, the published reports, interviews, leaks). Then it ought to be simple enough to narrow down considerably the ranges within which particular teams of strategists might seriously expect each other's behaviour possibly to fall, and/or narrow down the directions in which they are themselves likely to wish to diversify, horizontally or vertically. It would be rather like trying to guess the kind of essay a student could write on a particular topic if one

knew which source books she had consulted and which textbooks she was using. The economist might thus expect firms operating according to, say, 'Porter' to behave differently from firms that use 'Argenti' or 'Hofer and Schendel', or who have picked up the message of McKinsey rather than the Boston Consulting Group.

Naturally, I would not go so far as to suggest such methods would be foolproof. For one thing, even the biggest-selling guides to practical corporate planning do require planners to make judgments for themselves: they do not come complete in every detail. Thus even if an economist knew which bibles were in use and which information inputs were being used, there would still be scope for actual decisions to fall outside the range the economist took seriously as possibilities. Such surprises could be due to managers making particularly creative personal inputs or peculiarly misplaced (in the view of the economist) applications of particular notions. Knowledge of corporate philosophies and bibles may still be worth acquiring and using despite its fallibility: the key question to be asked is not whether this framework eliminates surprise, but whether the costs it involves are worth incurring given the extent to which its use reduces the incidence of significant surprises.

The picture painted in the previous two paragraphs is of course somewhat fanciful in that, in the absence of detailed case-study work, it is doubtful that economists will know details of corporate operations at anywhere approaching the level of which technical bible is employed, or even which educational backgrounds managers possess. However, workable assessments of likely corporate moves probably can be made with far less detailed information, which either involves a public report of key elements of the firm's corporate philosophy or can be used as a basis for inferring

the philosophical line now being taken. Furthermore, we should not forget that, just as with economics texts and degree programmes, guides to practical corporate planning and to the appraisal of competitive situations may exhibit considerable overlap in core areas. By combining idiosyncratic but publicly voiced philosophies with 'what every manager knows/ought to know'--what we could call the 'commonsense knowledge' of managers--economists may be able to go surprisingly far in the direction of putting themselves in the minds of corporate decision-makers and hence be able to pin down quite precisely the different evolutionary pathways different firms might try to follow in the not-too-distant future.

CONCLUDING COMMENTS

In the course of their training, mainstream economists are not usually given encouragement to focus upon the perceptual peculiarities of individual firms. To the extent that firms do behave differently, the orthodox methodology would lead its users to seek to explain the phenomenon by saying the firms must have different endowments arising from past decisions, and therefore have different comparative advantages when it comes to operating in different parts of the objectively 'given' market environment. Identically endowed firms should behave identically. (Note that firms are normally classed as facing similar 'costs' on the basis of reported outlays, even though opportunity costs are foregone schemes of action that exist only as personal constructs in the mind of the decision-maker: cf. the collection of subjectivist essays edited by Buchanan and Thirlby, 1983.) It is not conventional to address the processes whereby decision-makers in firms make appraisals of market opportunities and their internal environments. Rather, the whole approach bears a striking resemblance to that adopted in

neoclassical consumer theory, where the starting point is to assume, as Frank Hahn likes to put it in his lectures to undergraduates, 'the guy knows what he wants and he knows how to get it'.

This paper, by contrast, has been built on my 'hard core' belief as a subjectivist behavioural economist that, although the conventional philosophy may be especially convenient as a basis for some purely theoretical discussions, there is a serious risk that it may blinker the outlooks of economists if they later come to act as advisors on matters of policy. The impact of any set of policy measures may vary according to how these measures are construed by decision-makers (if indeed they are noticed at all), and on how each decision-maker construes the implications of the policies for other decision-makers whose own choices have some bearing upon the kind of choice that he or she should make. Therefore, in the interests of offering improved policy advice, economists could usefully study major differences in the behaviour of particular firms from a standpoint which centres on the different methods of forming judgments that the firms may use, and which does not focus essentially on supposed 'objective' competitive advantages or disadvantages enjoyed by the firms in particular areas.

My knowledge of the hard core of the neoclassical research programme leads me to anticipate that many neoclassical theorists would choose to reject the Kuhnian/Lakatosian vision of the firm as unacceptable, owing to its emphasis on indeterminacy, on the use of heuristics and on the experimental nature of corporate behaviour. However, I would expect there to be less resistance from those who are familiar with the recent book by Boland (1986) in which a new research agenda for neoclassical economists is outlined, following a Richardson-inspired critique of the rational

expectations literature. Boland's agenda involves research aimed at dealing with four related questions: (1) How do individuals choose their learning techniques? (2) To what extent does the choice of one technique over another imply a different pattern of behaviour? (3) To what extent does the frequency distribution of these techniques over any given population affect the stability of the neoclassical equilibrium? (4) If the distribution does matter, how do we explain it without violating the commitment to methodological individualism? Although Boland is concerned with the scope for reaching equilibrium states, rather than with continually evolving processes and structures, and although he is strongly opposed to attempts by behavioural theorists to replace maximizing notions with satisficing ones, his research agenda clearly exhibits a good deal of complementarity with the present analysis. In economic science, as in business (cf. the discussion of the UK soft drinks industry), philosophies do not have to be identical in order to promote commitment to similar activities.

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