A Behavioural Theory of Economists' Behaviour and the Lack of Success of Behavioural Economics

by

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1. Introduction

This paper is an exercise in reflexive theorizing. It uses elements from a behavioural/Post Keynesian analysis of choice (developed in more detail in Earl, 1982, 1983) to analyse why behavioural economics has hitherto failed to become part of the everyday toolkit for teaching and research of more than a small minority of economists. It arrives at conclusions which support, particularly within the context of economics, Feyerabend's (1975) anarchistic view of 'scientific' behaviour. We argue that ideas find academic acceptance not necessarily because of their intrinsic scientific worth, for there is no unambiguous way of specifying what this means in a world of partial knowledge, but because they are 'saleable' as tools which enable their users more easily to reach their goals. We make this point clear at the outset so that our readers can keep asking themselves whether or not a deliberate attempt is being made in this paper to play upon their own feelings of guilt and anxiety to sell them behavioural theory, as applied to both economics and the history of economic 'science'.

A neoclassical reader will certainly not be able to accept what follows without placing herself in something of a quandary and giving herself cause for anxiety with regard to the adequacy of her normal theory of choice. This is because neoclassical theory cannot be used in a reflexive manner to explain its own success and the relative neglect of behavioural economics. To avoid an inconsistency a neoclassical theorist must either reject her theory in favour of an alternative with such reflexive properties, or exclude the workplace choices of people such as herself from her area of inquiry. To use a behavioural theory of choice to understand the academic's choice of techniques and areas of specialization, and neoclassical theory to explain all other choices, would be to embrace two habitually incompatible frames of reference.

Mainstream equilibrium theorists have of necessity to make simplifying assumptions about the availability of knowledge to generate determinate results. It is for this reason that they cannot use their theories in a reflexive way to aid the explanation of the history of economic thought. Scientific behaviour is of its essence an attempt to overcome a lack of knowledge by a creative reshuffling of elements of that which is already known (see Koestler (1974) p.120) until theories about the nature of things emerge which can systematically be compared with the scientists' perceptions of reality.

A caricature may be offered of how the neoclassical theory of choice might look if used to explain scientific behaviour. The notion of the profit maximizing entrepreneur would be modified to depict the scientist as a single-minded knowledge maximizer. It would assume that she behaves as if she has read all the relevant literature (i.e. she knows the production function) and that there is some objective criterion by which additions to knowledge may be judged and weighed against each other. This information she would use rationally to maximize her contribution to knowledge and, depending on her success in doing this, she would maximize the value of her worth to an academic institution.

Kay (1979) has pointed out how ludicrous are attempts to use neoclassical theory to explain the allocation of resources to corporate R and D. His arguments can also be applied against the notion of the scientist who acts 'as if' she is fully informed. This paper shares Kay's world view of how the search for knowledge should be modelled: he suggests that behavioural theory provides an appropriate alternative way of explaining how companies search for investment schemes because it starts at the outset by addressing the question of how people cope with incomplete or overloading information.

During the course of investigating the neglect of behavioural economics we shall not refer only to the American organizational theorists, such as Cyert and March, Simon, and Williamson. We shall also devote a lot of attention to a group of English economists who have been concerned more with the theory of the firm in its market context, but who share a similar subjectivist, disequilibrium view. The inclusion of this group seems particularly necessary since the American behaviouralists have rather played down the market contexts in which organizations function. The English group's perspective on the nature of markets and the process of building up sales seems, furthermore, applicable to explaining why some ideas are more saleable than others. This group of English disequilibrium economists comprises P.W.S. Andrews, J. Downie, E.T. Penrose, and G.B. Richardson. As a shorthand we shall often refer to them as the Post Marshallian school, since they draw their inspiration in large measure from the non marginalist, disequilibrium elements in Alfred Marshall's work.

With the exception of Downie, all of these economists, both American and English, figure prominently in Loasby's (1976) inquiry into the problem of choice in a world of ignorance and complexity, and how that problem has been treated by equilibrium theorists. All of them have been unable greatly to influence the way in which most academic economists view the world. Loasby's book also makes much of the neglect in mainstream economics of Shackle's (1973) perspective on Keynes' macroeconomic ideas. For reasons of space we shall leave out of the arguments in this paper an analysis of the fate of this view of macroeconomics, even though, as Loasby's work shows, it is very much within the behavioural spirit. A discussion of the history of monetary economics, using the same analysis as the present paper, is to be found in Dow and Earl (1981, 1982, especially Chapter 13 of the latter).

The rest of this paper is structured as follows. In section 2 the goals of the academic scientist are examined in the context of a lexicographic theory of choice. Section 3 considers scientific research strategies in a world of relativistic knowledge. Parallels are drawn between the behavioural approach and the well-known work on scientific research programmes by Lakatos. Section 4 illustrates with case studies the kinds of failures to meet aspirations which provoke the search for new ideas. Section 5 explains how potential aids to the solution of problems may be screened by the academic economist and shows why some of the authors cited in this Introduction are likely to be filtered out long before they are fully understood and perceived to meet the needs of their readers. Section 6 is concerned with the final screen, the choice between ideas felt to be equally well understood. Finally section 7 is a brief summary and conclusion.

2. The Goals of the Economic Scientist

A behavioural theory of the activities of academic economists does not presume their sole interest is in understanding real world economic affairs and being able to offer policy solutions to economic problems. An economist with additional interests may be able to go some way towards meeting them by making particular choices at work, even if these choices do not, in the longer run, contribute to furthering the understanding of economic phenomena, and even despite the competitive pressures of the modern academic environment.

The position of the academic scientist is entirely analogous with that of managers in business enterprises as outlined by Scitovsky (1943, pp.57-60) and Williamson (1964). Scitovsky pointed out that even owner managers must choose between their leisure activities and the pursuit of profit, despite the threat posed by market competition to those insufficiently diligent in the search for profit. Williamson suggested that, even while working, managers might be interested in things other than profits, such as sales volume (since larger sales would justify a larger department and salary) or pet projects, the quality of the work environment, an expense account, and on the job leisure.

In deciding what to do to suit herself the manager has to bear in mind the feedback effects of her choices on the longer term position of her company, if, of course, she plans to stay there in the longer term. Similarly, an economic scientist may be concerned with the long term credibility of her discipline insofar as this affects her future earnings and ability to justify her position to others who are not economists. Andrews (1958, pp.28-31) has emphasized that managers need to be seen to be performing at least as well as those by whom they could be replaced, for the same basic cost, by higher level managers or shareholders. Academics will have similar concerns, particularly those who are attempting to secure tenure or, having achieved this promotion. But it is in the very nature of specialist jobs that they should be associated with what Williamson (1975, p.31) has labelled 'information impactedness'. That is to say, individual departments, or workers within departments, may be able to carve pleasant niches for themselves because the higher authorities who allocate resources and promotion lack the idiosyncratic knowledge that comes with experience as a particular kind of specialist. Information impactedness permits opportunism and the earning of payments, pecuniary and otherwise, in excess of transfer fees.

In the light of the above discussion we suggest that academic economists may be trying to achieve a variety of goals in the course of their work, rather as Williamson's managers have utility functions which contain a variety of arguments. However, while Williamson models the utility functions of his managers in strangely neoclassical terms, we suggest that academics should be seen as choosing their activities according to their goal priorities rather than by trading off the characteristics of activities against each other. In behavioural theory it is recognized that lexicographic forms of choice are much more plausible than compensatory models because they make lower demands on the information processing capabilities of boundedly rational decision takers (see Fishburn, 1974), Bettman (1975) and, for a more detailed treatment with extensions to cover the budgeting of resources, Earl (1983)). Thus we assume that academics set targets for the characteristics in which they are interested, ranking the characteristics in order of priority. They then avoid considering trade offs and attempt independently to pursue as many of the targets as possible. The priority ranking acts as a conflict resolving and filtering tool until only one of the competing plans of action remains. (There is no reason why Williamson's managerial theory cannot be re-written along such lines so that it occupies less of a no-man's-land between neoclassical and behavioural economics). We bear the behavioural analysis of choice in mind as we move on to consider the likely working goals to which an academic will aspire, and look at some of the problems she will encounter on the way towards meeting them. Four goals in particular seem likely to be ranked highly.

GOAL A): <u>To acquire, at a particular rate, the ability to predict and control aspects of the economic environment</u>.

The obvious benefit of success in reaching this goal is a satisfactory reduction in the mysterious nature of economic affairs. But there are three additional, indirect benefits. First, success will open up the possibility of higher academic earnings and non-pecuniary benefits via promotion, while making it easier to obtain funds with which to conduct further research and make further contributions to knowledge. Second, it may increase the possibility of outside earnings and power as a consultant or 'quango' member. Third, it may lead to fame and a place in the history of economic thought: a non-trivial benefit for the academic concerned with her self-image as validated by her peers.

Economists seeking fame will often suffer anxiety that other people will be working along the same lines as themselves, but at a faster rate. The nature of this anxiety is that if they are not first to be credited with an idea their self images as originative thinkers will be less sustainable. This anxiety compounds the inherent concern the academic economist faces while making crucial, non-repeatable strategic choices about which lines of thought to follow, and when or how to market her ideas to the editorial gatekeepers of journals and publishing houses. Early entry with an idea perceived as being insufficiently tight or clear in its logic may lead to rejection, but someone else may get there first if the scientist waits until she has a polished product which is foolproof, whose every ramification has been explored.

As far as the attainment of fame is concerned, being <u>thought</u> to be first seems to matter far more than actually being the first correctly to hatch the idea. This much is well illustrated by the contrasting fates of Keynes' (1936) <u>General Theory</u> and Andrews'(1949) work

<u>Manufacturing Business</u>, which we shall consider in turn.

Keynes' theory of effective demand and employment shares many features with the works of Kalecki and Myrdal which were written around the same time but which first did not appear in English. When the latter works were eventually translated they helped to add to the case for accepting what had by then become known as <u>Keynesian</u> theory while doing little to remove prestige from Keynes. However, according to Joan Robinson, Keynes rushed into print before he had completed his 'long struggle to escape' and having only partially realized how far he was departing from accepted equilibrium theory. On this view Keynes' real message emerged more clearly in his (1937) reply to his critics, but by then the damage was already done. His major work had combined his macroeconomic ideas with the orthodox equilibrium theory of value. This enabled them to be picked up quickly but, as Leijonhufvud (1968) has shown, it also permitted their emasculation into textbook Keynesianism and the neoclassical synthesis. Since Keynes' aim seems to have been to provide a theoretical justification for his public works solutions to unemployment, rather than fame for his own ends, this would not have troubled him so long as his policies were being applies. His legacy, however, seems to have been the pathway his cloudy analysis left open for the monetarist counter-revolution.

Andrews' (1949) normal cost pricing theory of the firm in a competitive oligopoly is certainly not the first work to suggest that prices are based on costs plus a mark-up. But it is the first theory seriously to attempt to explain what determined the size of the mark-up by proposing that prices were limited by the conjectured opportunity costs of production of potential producers. But the culture, the commonsense knowledge, of economists is rather different, and runs roughly as follows.

Hall and Hitch (1939) argued on the basis of a questionnaire to thirty-eight firms that it seems prices were set according to 'full costs' plus a mark-up, rather than with reference to any marginalist condition. But Hall and Hitch failed to provide a theory of what determined the mark-up. Then Kalecki (1943), who like Hall and Hitch was working at Oxford, proposed that firms set prices according to their costs plus a mark-up determined by the degree of monopoly. This was a rather thin explanation but Kalecki's work became very famous amongst Post Keynesian economists because it was he who had the good sense to use '<u>the</u> Oxford mark-up theory' as the basis for his macroeconomics, thereby failing to make the same mistake as Keynes. Andrews' slightly later work is referred to as if it is no more than a restatement of the same basic idea, or is not mentioned at all. No one talks about the evolutionary nature of Andrews' theory, why potential competition is such a powerful force, or the fact that Andrews' final chapter is all about the macroeconomic implications of his disequilibrium theory.

But, for an 'Oxford mark-up theorist', Andrews seems positively famous compared with Saxton, whom Lee (1981) has shown to have provided some very powerful contributions. Saxton's (1941) Oxford D.Phil. thesis was the first detailed work to offer a discussion of the costs to which the mark-up was added, and the role played by the industrial environment in price determination. But for the disruption of the war, he would have been supervised by Hall and Hitch. As it was, he wrote it on his own and included the findings of fifty questionnaires and arguments drawn from his own work as a chartered accountant in Oxford over the previous fifteen years (he was a part time, mature student). However, when the thesis was published (1942) reviewers played down his theoretical contribution and argued that his empirical work was based on too small a sample size and was consistent with marginalism. Subsequently very little reference was made to it and neither Andrews nor Kalecki cited it in their own work.

GOAL B): <u>To obtain, subject to market constraints and other priorities, a particular level and/or rate of growth of income</u>

If goal A) is being met, the desire for economic knowledge may give way to the economic scientist's desire to have an adequate command over the resources that will enable other things in life to be explored, assuming, and it may not be the case, that goal B) is not actually accorded a higher priority. Success in turning out contributions to knowledge rated highly by the profession, whether or not the academic herself believes them to be important, aids this end. The need to make contributions that receive the seal of approval of the profession arises because of the information impactedness situation which exists in academic appointment committees where some members are not economists. If neoclassical economists, who usually comprise the majority of economics representatives on such committees, assert that work of a similar kind to their own is of the highest merit, non-economists are not in a position to disagree. Candidates who researches imply that their potential colleagues and superiors are misguided fools are inevitably going to face hostility from them. The economist who does not conform with mainstream economists' images of an economic scientist is in great danger of being swept aside as one whose values are rubbish, and may find herself unemployed as a result. A previous appointment as an unconventional economist at a university full of similar eccentrics is a doubly unfavourable background for the academic attempting to move elsewhere, since even her referees may not be taken seriously.

Once an employment contract has been obtained, uncertainties and potential trade-offs abound with regard to the most fruitful way of achieving tenure or promotion. Information has to be sought from colleagues about the past decisions and supposed preferences of superiors before decisions can be taken on how best to budget time between teaching, administration and research activities, in the light of a personal assessment of their characteristics.

It is then necessary, in this hierarchy of choices, to decide upon the nature and target rate of publications to be aimed for, given the planned allocation of time to research. The problem is more complex than a mere calculation of how to maximize the length of the list of publications. The academic has to aim for, and achieve, a _curriculum vitae_ the sum of whose component contributions comprises a bundle of characteristics will survive the most stages in the filtering processes of senior staff members on academic development and staffing committees.

Such concerns will be on the academic's mind when a piece she has submitted for publication has been rejected despite not being exposed as nonsense. She has to decide on the relative merits of investing further in time and effort to rewrite it; submitting it to a less prestigious outlet; or simply abandoning attempts to get it published, turning instead to other schemes. From time to time learned journals publish analyses of successful and rejected submissions, and of the time lags between submission and publication, to aid such choices. However, the idiosyncratic nature of new contributions to knowledge clearly prevents the academic from choosing how to spend her time according to the _probabilistic_ present value calculation method suggested by neoclassical choice theory, even despite the availability of publications statistics.

As is evident from the work of Hawkins _et al_ (1973) and Eagly (1975), the journals that command the highest prestige and are most frequently cited are concerns with those areas to which the profession accords the highest status. They do not necessarily need to offer much direct relevance to understanding everyday economic affairs. According to Ward (1972, p. 10) the lowest ranking of the dozen compartments into which economics is usually divided are the history of economic thought, economic development and comparative economic systems. Next come labour, industrial organization and economic history. The second ranking specialisms Ward considers to include international trade, public finance, and money and banking. Pride of place goes to micro and macro theory, along with econometrics. Thus it is that the study of the problems of the Third World comes to contribute less to academic advancement than abstract theorizing about Walrasian contingent commodity systems that do not exist.

Bounded rationality clearly prevents most economists from attempting to be among the leaders in more than a few narrow fields, but academics have a strong propensity to compete with each other at the top end of the ranking of sub-disciplines. Self-supporting 'snob effects' will have a part to play in this, since the economist who can shine in the most prestigious and highly competitive areas will be thought to be particularly outstanding. The academic who desires to maintain a self-image as a leader in her profession, rather than as a worker who is less in control and gets her hands dirty, will find her self-assertive tendencies most fully catered for in the highest status fields, even if their immediate practical contribution is small. This is because they 'define the nature of acceptable research problems in economics and the appropriate procedures to use in solving them' (Ward, 1972, p. 10). The fact that many economists choose to concentrate their talents in top ranking areas does not mean that there is a lack of demand for their contributions from journals. To judge from the proliferation of new journals and the increasing formalism of new ones, Say's Law (i.e. supply creates its own demand) appears to be operating fairly well in the market for contributions to knowledge that display technical virtuosity.

GOAL C): <u>To expend no more than a particular amount of effort while seeking knowledge and income.</u>

Other things equal in terms of the prestige of contributions to knowledge, the larger the ratio of publications to effort that the economist can achieve, the more time will be released for leisure, teaching, and consultancy activities. It may well be the case that the typical economist adheres to neoclassical theory and econometric work not so much out of a single-minded pursuit for truth, but because it is a way of bringing the effort involved in generating an acceptable publications record within tolerable bounds. Certainly, the neoclassical style of research is much more economical in terms of effort than that which characterizes the group of economists with whose neglect we are concerned.

Most of the latter group have been willing to engage in the highly time-consuming activity of going out into the field and talking to managers, in order to be able to construct more realistic theories by an approach verging on induction. (See Andrews (1949), Andrews and Brunner (1950, 1952, 1975), Cyert and March (1963), Penrose (1971), Richardson and Leyland (1964) and Williamson (1964)). Cyert and March (1963, p.1), for example, state at the beginning of their book that they 'propose to make detailed observations of the procedures by which firms make decisions and use these observations for a theory of decision making within organizations'.

This kind of behaviour is most unpopular with positivist neoclassical econometricians, or even purveyors of untestable hypotheses who promise to produce, in the long run, work susceptible to econometric analysis. They allege that case study work is biased due to the nature of the questions asked, and suggest that sample sizes are too small. Criticisms of the latter kind seem particularly hypocritical given that neoclassical theorists are usually quite prepared to use a statistical (probabilistic) approach to the analysis of crucial decisions.

Econometricians can produce articles much more rapidly than those who engage in case studies. It is thus to be expected that economists who believe themselves to be of a high enough technical calibre will aspire to the easy approach to hypothesis testing. Such economists are able very easily to generate respectable publications by noting where, say, UK data are deficient and then virtually plagiarizing articles based on US data the moment UK figures become available. Reekie (1980) has argued that this has been how a number of important UK articles on the economics of advertising have originated. By constructing new regression equations the authors of such papers have clearly 'contributed to knowledge'. But even econometric work may seem arduous when compared with pure theory. The mathematical economist who, as Hahn often puts it, 'likes and can do theory' can generate contributions very rapidly with very little need to read lengthy monographs if she has a measure of creative luck or a new theorem to apply. Furthermore, if theory papers are quick to write, the cost of rejection is also low in terms of time wasted.

In seeking to keep her exertions below some tolerable level the economist will also attempt to avoid, as far as possible, revolutionary shifts in her frame of reference or usual working practices. In doing so she escapes the need for an investment in reading about and understanding new concepts. Because of the investments already sunk in a previous area of research it will often seem worth searching nearby for solutions to patch up perceived holes in the existing approach. Ideas representing or requiring incremental adjustments, which have been proposed by other economists, will be welcomed; those that call for a discontinuous change will be met with outright hostility or simply ignored (cf. Kuhn, 1970).

The contrasting fates of the theory of imperfect competition and Andrews' (1949) theory of competitive oligopoly are consistent with the sunk costs argument and the notion of the indolent economist. Theories of imperfect competition could be incorporated in situations requiring partial analysis in text books and research activities with a simple tilt of the demand curve of the perfectly competitive firm. Andrews' theory, however, is not amenable to mathematical manipulation or even conventional graphical techniques. As Brunner remarks in her introduction to Andrews and Brunner (1975, p.ix), its proper exposition requires a book rather than a piece the length of an article. For this reason, it takes even the sympathetic reader a great deal of time to understand and commit to memory. This is a great disadvantage compared with the orthodox theory of value, which, if Gerald Shove's famous remark is to be believed, only requires twenty minutes of attention (Shove accused Keynes of failing to give even this much time to it before writing his works on macroeconomics).

GOAL D) A Target for the quality of the academic's social, natural and teaching environment.

So long as higher level targets are being met it may seem worthwhile to the economist to sacrifice some income, prestige or research facilities for a pleasant environment which conforms with her image of how a university ought to appear. However, just as the articles with the greatest prestige may require the minimum of background reading, so the best students and most active academic colleagues may be found in the universities which rank highly in terms of status.

In this section we have attempted to outline some of the goals to which an economic scientist may aspire, and the complexity of the environment in which she has to operate. Academics may rank their goals differently, have different endowments of human capital and different degrees of interest in particular areas of the subject. There is thus no reason to expect all academic economists to adopt the same practices. However, it is easy to see that if such an economist wishes to achieve a high rate of published contributions to knowledge, high prestige and income, it will be rational for her to attempt to be an orthodox neoclassical theorist or econometrician. Furthermore, sunk costs of investments in such behaviour militate against changes of a dramatic kind.

The outputs of the behavioural and Post Marshallian economists involve little use of high grade mathematics, often wander outside the accepted boundaries of the discipline, and make frequent use of case studies. As a result, they have acquired little prestige yet require a great deal of effort to digest. On these grounds alone we should not be surprised that they have failed to generate much research or come to be taught as core components of the discipline, even despite their attempts to achieve realism and the absence of clear cut refutations of their theories. However, as the next three sections show, there are other, more complex, obstacles which hinder their acceptance. That these works have been published at all, or their authors have achieved significant academic appointments (even, in H.A. Simon's case, the Nobel Prize), is indicative of the presence of imperfections or a segmented market for academic contributions. Just as, in the work of Richardson (1960) and Hirschman (1970), slack allows financial and product markets to function in a relatively orderly manner conducive to risk taking, so slack in the academic 'market' permits the survival, at least for a time, of maverick thinkers fascinated by particular ideas from which ultimately progress may come.

3. Scientific Research Strategies

In the course of their research, economists are continually faced with the twin problems of bounded rationality and the non-availability of relevant information. To cope with these facts of life they need to choose a set of procedural rules comprising a search strategy for their chosen areas of specialization. Specialization of any kind is only possible if it can be assumed reasonably safe to disregard, or take for granted, certain features of the world and thus escape information overloads. It is necessary to be able to presume that the theories thus constructed are unlikely to go wrong due to a failure to perceive a close coupling of their components with those of interest to other scientists. But the researcher can never know in advance whether or not her chosen strategy will lead her astray.

Consider the predicament of a neoclassical labour economist who, by adhering to neoclassical conventions, avoids taking on board contributions from psychology and sociology. In doing this she escapes both the need to read in these unfamiliar areas and the anxiety that turmoil within these fragmented disciplines might undermine her work. She also does not have to worry if her work generates apparently unrefuted predictions which cause consternation to psychologists or sociologists. However, the unknown opportunity cost of doing this may be that she could have explained away previously accepted neoclassical results while obtaining additional new theoretical results consistent with observations, yet anomalous to neoclassical theory, by adopting a more polymathic behavioural approach.

If such a neoclassical economist finds one of her predictions at variance with the evidence she has to contend with the ambiguous origins of the discrepancy. The fault may have arisen due to the method of testing or data used; with a higher order theory such as the neoclassical theory of the firm; or with core presumptions used to frame the higher order theory, such as the notion that it is adequate to assume that agents act as if they are maximizers and have available all the necessary information. She will be aware that to return to the first principles of neoclassical economics or econometrics to examine them in depth will be a huge, and possibly unnecessary, task. At the end of such an effort she might find that a slightly different lower level hypothesis would have worked after all. The practical economist being asked for policy advice in the immediate future cannot, in any case, spend the time required to return to first principles: she must either take a chance on the adequacy of higher order presumptions and methods of testing, and look for a solution requiring minimal search, or adopt an eclectic posture, taking as a guide policies derived from an alternative research programme which she understands only imperfectly. (An eclectic may be seen, in terms of our priority idea, as one who ranks the ability to say something that fits the facts above being able only to make more cautious statements that are circumscribed by the need to be logically compatible).

The economist's entire academic upbringing will have provided her with evidence that most of the time it is safe to take a large amount on trust and apply simple procedural rules to search for new hypotheses and information, in order to overcome anomalies in her area of interest. She will have learnt the subject layer by layer, gradually adding definition to detailed aspects of subdisciplines after starting with such fundamental tenets as 'there's no such thing as a free lunch' (which is common to behavioural as well as neoclassical economics) and, if she is being brought up in the 'vulgar' neoclassical mould, 'stable Pareto efficient equilibrium conditions can be defined for any and all markets relevant to economic research and analysis' (Remenyi (1979), p.59). She will have seen effective ways of dealing with criticisms and anomalies, and that attempts to propose theories at odds with fundamental postulates are usually met with extreme hostility, sometimes culminating in an institutional response whereby dissidents are ostracized with a refusal to appoint them or publish their work. She will also have been able to infer the successful procedural rules that such dissidents use, such as: 'if publications are refused, set up a specialist journal with like-minded dissidents' (cf. The Cambridge Journal of Economics, The Journal of Post Keynesian Economics).

If the process of learning determines in large measure how an academic will behave once she has served her apprenticeship we should not be surprised to find that most academic economists turn out to be neoclassical equilibrium theorists. It is rare for students to be schooled in Marxian and behavioural/Post Keynesian theory simultaneously with general equilibrium analysis. Most concentrate almost entirely on the orthodox paradigm and are then required to get to grips with modern techniques in the course of M.Sc. programmes. They are then encouraged to use their technical expertise, particularly their skills as econometricians, in doctoral work. (Econometric work is favoured in this context because it is much more assured of some kind of results than research in pure theory; is much less resource intensive or dependent on the cooperation of external bodies than questionnaire-based case study work; and it is felt easier to pronounce upon as a novel contribution to knowledge).

Economists with such upbringings will look for equilibrating forces and equilibrium configurations in everything they analyse. They will be well equipped to find these equilibrium features if they have grasped by some kind of inferential learning process (cf. Chomsky, 1959) the procedural rules of the game for frequently successful decision-taking. Not only this, but, as we argued in the previous section, they will tend to be attracted by the leisure or promotion advantages that come from practicing as a technically competent equilibrium theorist, rather than attempting to swim against the tide as, say, a behavioural economist.

The main interest of what Kuhn calls 'normal' scientific activity consists in the discovery of relationships and the solution of apparently small puzzles. Insofar as an academic's search strategy appears successful as an aid to this end, it will seem as though more and more can be taken for granted and incorporated into the set of core tenets that can be used without anxiety. If she lacks any demonstrably better mode of analysis the scientist will usually shut her eyes to the Popperian problem that her framework may suddenly begin to seem defective because conditions have changed, even though it has performed well in the past. It may then appear inferior to a rival approach or, if it lacks a rival, its heuristic powers might simply degenerate, leaving anomalies resoluable only by the addition of increasingly ad hoc assumptions.

To summarize, the academic makes headway by ignoring as far as possible the interdependencies between theories and the partial nature of her theories; by making the least change necessary to 'resolve' inconsistencies; and by avoiding getting bogged down in methodological arguments about basic principles. In Simon's (1962) terms, she assumes that the world is 'decomposable' and that she has decomposed it in the appropriate way. She can then look at a portion of it at a time and build models involving only a limited number of relationships on the assumption that all others are of trivial importance to the problem at hand. If an anomaly is discovered, information overloads are avoided by not asking difficult questions. A limited rule-guided search will usually provide a way of coping with a difficulty without challenging fundamental assumptions even though, in the long run, the procedural rules may cease to deliver the goods. The procedural rules employed by the scientist will be very much the result of her upbringing. As long as they seem to be working and the scientist is able to meet her aspirations she will have no obvious reason to question them: only with the benefit of hindsight can they be shown to be incorrect and even this is not always possible.

This behavioural analysis of research activity is in some ways similar to the work on Scientific Research Programmes (SRP) by which Lakatos (1970) and, more recently, Latsis (1976) and Remenyi (1979) have attempted to explain the dominance at particular times of certain bodies of thought. Lakatos' starting point was the observation that a scientist cannot test an individual hypothesis without taking other hypothesis for granted. It is therefore necessary, if progress is to be made, for the scientist to accept the soundness of higher level theories and axioms and make a methodological decision not to test them, even when they are not mere tautologies. These 'givens' form, in Lakatos' terminology, the scientist's 'hard core', and from such building blocks she constructs auxiliary hypotheses which are tested. Any anomalies then discovered are taken to indicate that something is wrong with the hypothesis or the way it has been tested, not with the hard core.

Lakatos suggests that hypothesis construction and appraisal is guided by a positive heuristic of procedural rules. An example of a procedural rule from the neoclassical SRP is 'Put in additional assumptions if the model fails to generate determinate assumptions' (cf. Latsis (1976, p. 22) and Remenyi (1979, p. 60)). When hypotheses perform badly in respect of the facts, or are subject to criticism on other grounds, further procedures are brought into play sequentially to form what Remenyi calls errant hypothesis (EH) and institutional response (IR) mechanisms. Attempts to demonstrate that Keynes' General Theory was only a special case of neoclassical economics dependent on money wages being fixed, by adding the real balance effect into a version of his theory cast in the equilibrium mould of IS-LM analysis, are examples of EH mechanisms of the neoclassical SRP in action. Similarly, the attempts made by Dow and Earl (1982) to uphold Keynes' original claims are manifestations of the EH mechanisms of the behavioural/Post Keynesian SRP. The most obvious example of the IR response of the neoclassical SRP is the rejection by mainstream journals of anti-neoclassical papers. This kind of hostile response to criticism should not necessarily be taken as an indication of conscious dishonesty. Until a rival body of thought is clearly and unambiguously established as superior it is quite reasonable to regard it, and its proponents, with suspicion.

According to the SRP approach, faith in the chosen hard core will be enhanced if positive activity with auxiliary hypothesis in the core's 'protective belt' meets challenges without having to resort to ad hoc fudges. If such fudging occurs, or the academic restricts her attention to the study of a shrinking range of phenomena, then her SRP is said to be degenerating. She is behaving just like the non academic person who finds the world in general increasingly difficult to cope with and comprehend, who becomes increasingly withdrawn and antisocial unless she can be provided with an acceptable alternative way of viewing the world.

In Remenyi's development of the SRP methodology it is suggested that subdisciplines will emerge with their own 'demi-cores' of accepted propositions (e.g. macroeconomics with, until the 1970s, the Phillips curve or its common assumption of a marginal propensity to consume between zero and unity). If there is a clash between more peripheral work around demi-cores, higher level specialists will bring into operation their defense mechanisms, as has happened with the 'search theory' attempts to show that unemployment is always the result of choice and not a sign of the failure of the equilibrating mechanisms of supply and demand. Those economists who find the demi-core more interesting, and whose faith in its propositions remains unshaken despite criticism from higher ranking theorists may go on to develop a new, rival SRP centred on the demi-core.

Unfortunately, the SRP treatment of scientific behaviour has two important defects that our proposed behavioural analyses avoids. The first concerns the suggestion made by Lakatos (1970, p. 116) that there is an objective criterion by which scientists decide whether to switch between competing research programmes. This is the ability of a rival SRP to offer excess empirical content while explaining how the past successes of the dominant SRP came about. The problem with this criterion is that theoretical structures may not be commensurable <u>even if</u> they do yield testable hypotheses. Furthermore, test results may be interpreted in different ways or test methods queried (see further, section 4b below). In a world of partial models all knowledge is relativistic and current test results can never be claimed to be unambiguous or definitive. In part, knowledge must always be accepted because of faith. If such a criterion could be applied it would be hard to explain why anyone troubled to pursue new ideas until they had generated empirical results: <u>someone</u> has to adopt a rival SRP before Lakatos' criterion can be confronted with any new set of results.

The lack of a clear cut dividing line between progressive and degenerating research programmes enables scientists whose SRPs have different logics of appraisal to apply different choice criteria and justifications for their practices. Economists' justifications for their own behaviour need to be examined carefully because of this. The economist may believe she is telling the truth but there is a wealth of evidence from the work of cognitive psychologists which suggests that in situations of ambiguity a person's cognitive processes will shape her perceptions so that what she sees fits in with her view of the world and herself (cf. Steinbruner (1974) Chapter 4). The economist may see herself as an honest scientist and present what she believes to be an honest case for adhering to her present SRP, or changing to another, on the basis of the likelihood of this leading to a valuable contribution to knowledge. In 'reality' the choice may be based on the desire not to look a fool, fear of the unknown, or absolute careerism.

Consider the case of a famous general equilibrium theorist, with a massive investment in her SRP, who, rather like Hahn (1972), considers a list of objections to her work and then justifies continuing with it by commenting to the effect that "it is reasonable to take short cuts, but not when we are concerned with finding out". The clear implication of such a remark is that society may have to wait a long time yet for policy conclusions or testable hypotheses, but the economist is convinced they will be worth waiting for. No one can prove she has been wasting her time and is going to continue to do so, though reasons why it is quite possible that this is the case may be found in the work of Kornai (1971), Hutchison (1977), and Loasby (1976). What _is_ clear if she changes sides, however, is that _she_ is admitting that she has been wasting her time, that, _ex post_, she does not think the efforts she has sunk in respect of the general equilibrium SRP were justified. So long as she continues to sink further time and energy into equilibrium economics she escapes the need to make such a confession and, because of the inherent ambiguity of the situation, denies _others_ the right to claim that the experiment has been a failure. The road of general equilibrium analysis may, so to speak, be a very long cul-de-sac, but, then again, it may lead to wondrous solutions to economic problems. To find out one must travel down it, but it will not be clear when we have reached the end.

Now, if the equilibrium theorist's top priority is to preserve her image of herself as a leading researcher who does not make stupid mistakes, her cognitive processes will ensure that she sees her decision not to change direction as entirely consistent with a humble search for truth. The future potential of any rival SRP is ambiguous and if the equilibrium theorist defects she will have to accept, for a time at least, a self-image of relative inexperience in the new field, in contrast to an image as a leading light, and frequent anxiety because of the unfamiliar concepts and authors inevitably encountered. There will also be hostility from her traditional reference group of economists.

A change of sides does not involve merely the deviant's confession that she has been foolishly wasting her time. It also carries the implication that her former colleagues are fools too, and worse still, fools who have yet to see the light. To put it bluntly, it often takes guts to defect to a different SRP, especially if an economist has a powerful reputation with her existing one and her blinkering has hitherto enabled her world view to become sharply defined.

This analysis of the underpinnings of economists' inertia is rather different from that which may have been seen to be implied by the arguments of section 2. Our discussions of economists' effort outputs may have been taken to suggest that the sunk costs of investment in a particular SRP may cause its adherents to embrace dishonesty if it optimizes their expenditure of effort. We believe that such extreme careerist behaviour is really rather rare: few economists will consciously ignore the truth if the effort needed to debunk heretical suggestions is judged too high. But to say this does not preclude the possibility that the choices of many economists are unconsciously shaped by a fear of the sacrifices that their opportunity costs might entail.

It is also important to stress that these arguments about 'personality related' reasons for clinging to a particular SRP are not aimed solely at neoclassical economists. All economists, even behavioural economists, may be susceptible. For example, someone may attempt to justify the study of behavioural theory on the ground that realism of assumptions is all-important, and eschew empirical work 'because' controlled tests of predictions are impossible. She may believe this to be an honest justification but it is perfectly possible that her fears of mathematics, of unfamiliar computers, and of unfamiliar ways of conducting a search for data, have caused her cognitive processes grossly to exaggerate the prospective contribution that this kind of economics might make to knowledge. It should be added that the example is not intended to suggest that <u>all</u> those who choose behavioural approaches suffer such anxieties but, rather, to indicate that if the justifications firms offer in response to questionnaire investigations of their behaviour can be thought to be questionable and prone to subjective bias, the same problems might affect economists' justifications of their own behaviour as economists.

The 'personality related' sacrifices of a switch of SRP will be smaller the younger and less eminent is the economist considering making the switch. Also, the younger the economist, the higher may be the career payoffs from making a move if the market for economic ideas is segmented. Consider the following scenario. An economist obtains a chair early in her career via some respectable contributions to mainstream theory or econometrics. While preparing her inaugural lecture she is introduced to an alternative, minority SRP (e.g. Austrian Economics) whose adoption would not involve her in a long period as a novice, since it is relatively simple. Having considered the possible consequences, she elects to give an inaugural lecture in which she announces her defection to the rival school and publicly rebukes her former peers. She thus delights leaders of her newly chosen SRP, who welcome her with open arms and ask her to speak at conferences they organize to criticize orthodox economics. She thus becomes a 'big fish' in a relatively small pool, well placed should it cease to be a minority interest, and has escaped the possibly long period she would have to endure before achieving further promotion to an orthodox department in a prestigious university, during which time she would constantly be wondering whether she only had 'a bright future behind her'.

Observers of outward behaviour in the contrived example above are given the justification that the deviant particular SRP chosen is going to be superior to the orthodox one. But short of an in-depth psychoanalysis, a technique which is not part of the positive heuristic of the conventional approach to the history of economic thought, it will not be possible to find out if she _really_ is telling the truth. It is possible that career opportunities occupy a higher place in her priority ranking and that career advancement involves less sacrifice by making this change rather than staying where she is or switching to, say, behavioural or Marxian economics. But her cognitive processes will, unless she is a self-confessed opportunist, ensure that her perceptions of her own actions are moulded so that she _sees_ her subsequent career development as a fortuitous complementary development and her choice of SRP as offering the greatest prospect for obtaining economic knowledge.

The second failing of the SRP approach to the history of science is that it appears tacitly to assume that scientists are aware of all the presently discovered anomalies in their fields and all the attempts of scientists who have used other techniques for investigating the problems of interest and have suggested solutions. In a world of bounded rationality this is an unreasonable assumption to make. In the next three sections we shall attempt to show how ideas forming a coherent SRP may fail to take hold because they are not perceived as forming a coherent programme; are not perceived as necessary because the scientist is unaware of difficulties with her work; or are simply not perceived at all, even by the scientist who is not a careerist but a humble seeker after truth.

4 The Failure to Meet Aspirations

Inquisitive activity is a process alien to a state of equilibrium. The successful construction of a new theory, or satisfactory completion of empirical work, enables the scientist to begin to search for solutions to knowledge puzzles which previously she had not found sufficiently worthy of attention. If she thinks she is failing to meet her target rate for contributing to knowledge the scientist must step up her search activity, following her usual procedural rules, or, if something more fundamental appears to be wrong, look for an altogether new strategy. There are four kinds of inadequate attainment, in addition to the obvious one of a research programme having run out of puzzles to solve, particularly likely to make an academic economist amenable to new ideas, should she come to discover them in the process of search. We shall illustrate them with case study examples.

a) An inability to cope with growing technical demands

Economists may fail to achieve the publication rates to which they aspire if they cannot keep abreast of the mathematical developments that will lend rigour and formalism to their work. Few economists could act as Hicks did in his early sixties when writing his (1965) book Capital and Growth: he realized that it was necessary to use mathematical techniques that were new to him and, with some assistance from Professor Morishima, successfully managed to get to grips with them. Lesser economists, in analogous situations, will be forced to retreat from work at the frontiers of their SRP, or consider the possibility of switching to an alternative SRP, or even to another profession.

b) <u>The discovery of important empirical anomalies</u>

Initially such anomalies will be approached as if they represent merely the result of using inadequate auxiliary hypotheses. They will thus be tackled as a part of the business of normal science using the procedures of the positive heuristic. A neat example of this is Baumol's (1958) attempt to construct a theory of the firm in which managers were assumed to wish to maximize sales revenue rather than profits. Whilst acting as a consultant he noticed that managers of large corporations did not seem to treat changes in fixed costs or profits taxes as the existing theory predicted (i.e. they attempted to pass them forward into higher prices). The managers also claimed to be more interested in the value of sales than the level of profits. Baumol produced a model consistent with these observations which kept the core neoclassical assumptions of individual maximizing behaviour and that firms know their cost and demand constraints. Managers maximized their utility by maximizing sales revenue subject to a minimum profit constraint, which was more demanding the less imperfect the workings of the stock market control mechanism. Profit maximization was allowed in this model as a special case. In Remenyi's, as opposed to behavioural, jargon, this was a case of the operation of the EH response and absorptive reaction principle.

Where minor adjustments do not resolve anomalies without additional <u>ad hoc</u> fudges being necessary, a more wide ranging search may be carried out. Where anomalies are discovered not by econometric investigation but by fieldwork, the findings may sometimes seem instantly to provide a new hard core, permitting an approach to theory formation that is not far removed from induction. As Andrews

(1951, p.140) explains, his (1949) disequilibrium theory of the competitive oligopoly firm came about as a result of his discussions with managers in the UK textile and footwear industries. These discussions made him aware of the importance that was attached to goodwill, and fears that the charging of excessively high prices or the failure to provide adequate deliveries of an adequate product to regular customers would result in the permanent loss of hard-won markets. Such factors, absent from the marginalist equilibrium model, became central to his new theory. Andrews' theory, it must be added, provided an alternative solution to Baumol's anomaly even before Baumol perceived the problem, since it showed that long run profit maximization and sales revenue maximization amounted to the same thing in a disequilibrium framework. Andrews' related non marginalist theory of investment came about similarly, as a result of a lengthy business history investigation carried out with Elizabeth Brunner (1952).

But it should be stressed that one person's empirical anomaly is often another's supportive evidence in a world of partial and interdependent models. An obvious example of this is the debate about whether firms set their prices according to marginalist rules or in the light of normal costs. We have already mentioned how Saxton's (1942) work was accused of containing conclusions that can be interpreted to be consistent with both views. A similar reception, coupled with equally similar comments about small sample sizes and biased questionnaires, was accorded to the case study work of Hall and Hitch (1939) and the Andrews-influenced investigation by Barback (1964). Econometric investigations conducted more recently have failed to settle the dispute to the satisfaction of participants on both sides. Laider and Parkin (1975) alleged that the anti-marginalist conclusions drawn by Godley and Nordhaus (1972) from a battery of regression equations were the reverse of what the data really implied. The reply of Coutts, Godley and Nordhaus (1978) has received very mixed reactions. The debate thus continues, with the possibility that the normal cost view might be correct posing a severe threat to the monetarist theory of inflation (see Dow and Earl, 1982, Chapter 15).

c) The discovery of a fundamental logical flaw

The demi-core of macroeconomics emerged as a result of Keynes' well publicised discovery that previous theories attempting to relate changes in unemployment and money wages were beset by a fallacy of composition. But the removal of this logical flaw led to the discovery of another. When followers of Keynes attempted to extend his ideas into the realm of growth theory they discovered that the definition of an essential feature of the neoclassical theory of aggregate income distribution, namely, the marginal producitivity of capital, rested on a circular argument. The ensuing 'Cambridge Controversies in the Theory of Capital', which Harcourt (1972) has documented, greatly stimulated the development of an alternative Post Keynesian SRP based on the demi-core of Keynes' macroeconomics.

The controversies provided a beautiful demonstration of the sequential search processes and defence mechanisms of the neoclassical SRP. Eventually, Samuelson and his neoclassical colleagues conceded that there was no way round the logical flaw. But this did not lead them to abandon their SRP. Instead they seem to have adopted an ultra-positivist stance, for they now argue that they will treat their logically defective theory as an 'as if' model until someone demonstrates to their satisfaction the real world existence of aggregate production functions that exhibit reswitching or capital reversing. They seem utterly oblivious to the objection that Post Keynesian economists have set against their approach, namely, that such a demonstration will never be possible. Such an impossibility does not arise because the production function perversities, that attracted the bulk of the attention during the controversies, are only problems of pure theory. The real problem, as Joan Robinson (1975) emphasizes, is that the 'given' production functions of neoclassical theory cannot exist in the irreversible real world of technical change and historical time. If capital is not some malleable, putty-like substance, and if the book of blueprints keeps adding new pages, it is meaningless to speak of given production functions along which it is possible to move in any manner, well-behaved or otherwise, as conditions change in factor markets.

Sometimes, due to the blinkering that is produced by research strategies, economists fail to realise that the closure of one logical gap has opened up another. A good example of this concerns the upshot of Coase's (1937) attempt to use standard optimizing analysis to explain the existence of firms. Coase argued that firms were devices for avoiding the transactions costs which would have to be incurred if production and exchange were organized through perfectly specified factor supply contracts. Incompletely specified employment relationships and the use of discretion within a managerial hierarchy avoid the need frequently to redraw contracts in a disequilibrium environment.

Equilibrium theorists before Coase had discussed firms as if they were black boxes, implicitly considering their input and output choices as if they took place in a world of zero transactions costs. They had not addressed the question of how agents cooperated and took decisions once across the transactional boundary of the firm, or whether the ways in which they did so could affect the way the firm responded to changes in market conditions. The implication of Coase's analysis was that there was no role for firms in equilibrium theories which excluded transactions costs from their assumptive structures (cf. Clarke, 1980). But neoclassical theorists seemed oblivious of this implication, rested content that their existing tools of analysis could explain the existence of firms, and continued to treat firms as black boxes.

It was almost twenty years before Cyert and March (1955, 1956, 1963), and, later, their then research student Oliver Williamson (1964, 1975), began to investigate the interactions between organizational structure and firm behaviour. That they saw matters in this novel way may be explained in large part by the fact that March was an organization theorist and not an economist by training, and he was, in the midst of this work, busy collaborating with Simon to produce the (1958) book <u>Organizations</u>. However, in order to look at firms in this way they had to abandon key features of the neoclassical hard core. Behavioural theorists study features that are uninteresting to (do not cater for the aspirations of) conventional economists, such as the formation, maintenance and breakdown of coalitions. Worse still, they find it necessary to reject determinacy and objectively given cost functions.

Apart from Arrow (1974) and Radner (1975), mainstream economists have either not troubled to study what behavioural theorists do and how they do it, or, if they have understood behavioural analysis, have chosen to ignore it: their methodology requires determinacy, the ability to build formal 'single-exit' models (Latsis, 1972). If the hard core of an academic's SRP is threatened because an alternative methodology displays in it a logical inconsistency and lack of realism, the academic will lower her aspirations with regard to these dimensions if they have lower priorities than other goals, such as the desire to construct models that are formal or determinate, which cannot be met by perceived alternatives.

It is difficult to recognize the existence of significant transactions costs and informational problems without then being forced into the behavioural mode of analysis. Neoclassical theorists such as Machlup (1967) prefer to sidestep the existence of these complications and the logically questionable nature of firms in an 'as if' world devoid of complexity and ignorance. They claim this is an approximation that has to be made to get concrete results, citing favourably Friedman's (1953) argument that realistic predictions are what really matter, rather than realistic assumptions. Positivism of this naive kind may be dangerous and misleading, a point which neoclassical theorists fail to mention. As a result of their neglect of matters of internal organization, and decision processes, such theorists are confined to the structure/conduct/performance paradigm and can only consider efficiency in terms of market size or the location of firms. They seem oblivious of other, perhaps more important, factors, such as those disucssed by Loasby (1967a, 1967b), Cyert and George (1969), and, most of all, Kay (1982).

d) The discovery that assumptions may no longer be realistic

When criticized for extreme 'as if' theorizing the neoclassical economist displays herself as an ardent positivist. Beneath this outward appearance there actually lies a more reluctant follower of Friedman's methodology. As Latsis (1976, p.22) notes, part of the positive heuristic of the neoclassical SRP consists in the procedural rule that, once it has been set up to yield a determinate solution, attempts should be made to refine a model to incorporate more realistic assumptions. This reluctant positivism means that even neoclassical theorists will be seeking to amend their models as conditions change, so long as they can preserve the notion of equilibrium. Behavioural theorists aim for realistic assumptions at the outset, even if this means that their models lack determinacy and are often ill suited to econometric testing. Therefore, when there is a change in what constitutes a realistic assumption, the attainments of neoclassical and behavioural SRPs will be affected. If this causes assumptions to become insufficiently realistic we should expect there to be a search for ways of incorporating the new environmental features in theories explaining how components of the world fit together.

We shall use the effects of the growth of joint stock companies on the evolution of the theory of the firm as an illustration of how economists behave when faced with a failure to meet their aspirations with regard to assumptive realism.

The increasing dominance of joint stock companies at the turn of the century conflicted with the core assumptions of Marshall's disequilibrium theory of the determination of firm sizes and industrial prices. In the early editions of Marshall's (1920) Principles individual firms were seen as family businesses which always died off in the long run because the quality of their owner-dominated management structures declined through time. As a result he did not believe that there was the serious problem of monopoly that might otherwise exist if one firm in a market acquire scale advantages, which eventually would enable it to grow by undercutting rivals until it dominated the market. For Marshall, increasing returns to scale led not to monopoly but to falling prices as the number of units produced increased through time; he believed that prices were determined on an industrial basis by the average costs of a 'representative' firm in the industry in question. Joint stock companies seemed to threaten this theoretical structure. Such firms had no need to rely on their owners for supplies of managerial inputs. If they discovered inefficiencies soon enough they could hire new managers from outside. It was thus no longer obvious that all firms would arrive at the long run 'equilibrium' of corporate death due to senility.

Marshall's own reaction to this development was very much the incomplete response of a very old economist. But to a behavioural economist it seems, nonetheless, to be on the right track. Marshall suggested that, while large joint stock companies may not have relatively short lifespans, they were beset by organizational forces ('vis inertiae') which would cause them to suffer periods of stagnation when their competitive performances would be relatively poor. Marshall's suggestion seems to have gone largely unnoticed for thirty years until it resurfaced in the disequilibrium theories of the competitive process offered by Andrews (1951) and Downie (1958) - theories which were, in turn, neglected by most economists.

Andrews and Downie combined the observation that firms often go through phases of stagnation with their separate recognitions of the differences between the short and long run elasticities of demand for the products of individual firms in situations where entry was possible. From this starting point they developed analyses of price formation where industrial concerns were dominant, and, by allowing firms to be jostling for industrial leadership, amended Marshall's theory of the competitive process to incorporate the possibility of corporate longevity. Central to their work was an abandonment of the neoclassical assumption that firms produce given products, always at minimum cost, with some objectively given production function. This left firms with scope to fight back against the transfer of their markets to firms currently enjoying superior competitive positions, by innovation and the discovery of hidden potential. In many ways this rejection of static analysis for a Post Marshallian approach anticipates, but does not appear to have inspired, Cyert and March's (1963) views on the emergence and uptake of slack.

The lack of attention paid to Marshall's comments about the tendency for joint stock companies to stagnate may be explained largely by the fact that most economists were more interested in the implications of the growth of corporations for the adequacy of the theory of the perfectly competitive firm. This theory had not long been developed, having evolved largely out of Pigou's attempts to force Marshall's ideas into a rigorous marginalist equilibrium framework. Joan Robinson _was_ well aware of Marshall's views on the efficiency of joint stock companies, but she regarded his remarks in this respect merely as _ad hoc_ adjustments to prop up his idea that everything in the garden was rosy, that the consumer was not exploited by large firms. She believed that if companies could take advantages of economies of scale the competitive process would best be described with the analogy of a 'pike in a pond', gobbling up small fry, rather than with Marshall's 'trees of the forest' life cycle analogy. Perfect competition theory, along with the welfare implications attached to it, seemed to her to be utterly at odds with the rise of the corporate economy.

When Joan Robinson set out to develop her alternative (1933) _Theory of Imperfect Competition_ she was not trying merely to repair an assumptive defect in existing theories of the firm. She also aimed to deal with an empirical anomaly and a defect of logic. As she recalls in the introduction to the (1969) edition of her book, perfect competition implied that 'the optimum size of firm, with minimum average cost, is always tending to be established', yet '(h)ere we were in a deep slump, and this is what we were being asked to believe. ...Imperfect competition came in to explain the fact, in the world around us, that more or less all of the plants were working part time' (Robinson, 1969, p. vi). The logical defect to be overcome was that exposed by Sraffa (1926), namely that the theory of perfect competition only made sense on its own terms if there were constant returns to scale, in which case the size of the firm was indeterminate. (To allow increasing returns to scale in a static equilibrium framework would lead to a failure of the average cost curve to slope upwards, and to monopoly; to obtain an upward sloping cost curve by assuming decreasing returns to scale would mean breaking the laws of physics, for, if a firm could buy unlimited quantities of factors without affecting their prices, costs could only be rising with output if a physical expansion of inputs led to a less than proportionate expansion of output).

In fact, in Joan Robinson's case, <u>these</u> aspirational failings were not the main driving force leading her to construct the theory of imperfect competition. She did not construct the theory <u>necessarily to be taken seriously</u> as a means of explaining what happened in the real world. The theory is static and excludes oligopoly, her actual view is that it is essential to see competition as a dynamic process involving oligopolistic firms (cf. Robinson, 1969, pp. vi-xi). Her <u>real</u> aim in constructing the theory of imperfect competition was, in effect, to give neoclassical theorists enough rope to hang themselves by showing that their own logic generated <u>a priori</u> conclusions that were at odds with their world views. For Joan Robinson, the function of her book was to demolish the concept of consumer sovereignty and, even more importantly, to show 'within the framework of the orthodox theory, that it is not true that wages are normally equal to the value of the marginal product of labour' (1969, p. xii).

Unfortunately for its creator, the theory of imperfect competition was taken seriously as a device for explaining economic affairs. It found a highly receptive market amongst partial equilibrium theorists, who found it easy to accommodate in their existing frameworks (cf. goal C in section 2 above). However, Joan Robinson's own serious remarks about the absence of consumer sovereignty and wasteful, superficial, product differentiation felt flat, since theorists predisposed to see the virtues of capitalism found a welcome retort in the work of Edward Chamberlin. Starting from a different point, and with entirely different motives (see Loasby, 1971), Chamberlin (1933) had constructed a theory which looked formally identical to the theory of imperfect competition. In Chamberlin's theory capitalism produced excess capacity, too, yet it was not seen as an inherent defect in the system but as the result of the attempts of firms to satisfy consumer preferences so diverse that they prevented maximum utilization of economies of scale.

The appeal of the theory of imperfect competition might have been greater still, had it not appeared around the time that economists such as Hicks were trying to build a rigorous approach to general equilibrium theory. For Hicks (1939, pp. 83-5), economic theory was synonymous with general equilibrium analysis. Imperfect competition theory seemed to threaten the destruction of all of his attempts to demonstrate the stability of a general equilibrium. In the theory of imperfect competition a strengthening of demand for a product may lead to a reduction in its price, and vice versa. The income effects thus produced may lead to similar shifts in the demand curves for other products, with similar disequilibrium consequences. In essence, the mechanism which conflicted with the stability requirements of general equilibrium theory was the mechanism that drove Adam Smith's primitive theory of economic growth. Hicks seems to have been more unwilling to abandon his general equilibrium framework than to face up to the disequilibrium implications of falling cost curves. He offered the usual 'as if' justification and confined himself to a theoretical world of perfect competition. In recent years general equilibrium theorists such as Arrow and Hahn (1971) have been trying, without much success, to incorporate non-convex production sets and monopoly elements into their models. However, the attempts by Young (1928), Wu (1946), Kaldor (1972), and Richardson (1975), to persuade economists to tackle the disequilibrium implications of falling cost curves seem to have made little impact on the ways in which most economists think.

The rise of joint stock companies also led to attempts to decide whether firms continue to be controlled by shareholders as the total number and value of shares grows, or whether they become dominated by managers keen to pursue interests of their own which conflict with shareholder welfare. Attempts to establish precisely what constitutes a realistic assumption about patterns of ownership and control are still in progress after almost fifty years of controversy, during which time the rise of institutional shareholders such as insurance companies and pension funds has given rise to concern as to whether or not the pendulum is, so to speak, swinging back whence it came. The associated debate over the relative performance characteristics of 'owner' and 'manager' controlled companies also has yet to be resolved.

One of the most recent and thorough contributions to both of these debates is to be found in the work of Cosh (1978). He shows how many of the early attempts to use measures of the dispersion of share holdings and the proportion of shares owned by executives as guides to the likely goals of large companies may have been based on an unsound premise. It is unwise to presume that a company in which shareholdings are highly dispersed, and in which executives hold a very low proportion of the shares, will be more likely to be managed so as to maximize the rate of growth of assets (as in Marris' (1963) model) than the rate of profit. This is because executive shareholdings, despite being low in proportion, may be large in value relative to executive salaries: in such cases companies should be classified as owner controlled. Cosh's suggested alternative method of classification led him to find that owner controlled companies grew faster and were more profitable than those in his sample that were controlled by managers. This finding is at odds with Marris' theory yet not inconsistent with behavioural ideas of slack and X-efficiency in situations of information impactedness.

Given that there is no clear cut rule on how many industries must perform in a way supportive of a theory before it becomes acceptable (Cosh's study, for example, looks at firms in only three industries), and given the ambiguities in results or questionable methodologies of many of the attempts to assess what might be the appropriate assumptions to make about managerial motivations, it is hardly surprising that adherents to traditional theories of the firm can remain unimpressed by the managerial theories which were offered once it seemed at least possible that product and capital markets might both be sufficiently imperfect to permit managers some discretion. What is perhaps surprising, however, is that the inventors of these theories do not seem to have realised that they did not have to wait for the managerial revolution to justify building models where controllers of companies were assumed to wish to maximize things other than profits. Company histories are replete with examples that show that owner managers may seek to attain, even at the expense of profits, technical excellence or turnover volume as top priority goals.

5 The Screening of Contributions to Knowledge

When the economist's aspirations exceed her attainments she will be most receptive to novel ideas. However, such contributions will only make the impact intended by their authors if they are discovered and comprehended as containing what their authors believed them to contain, and if, once understood, they seem to fit the economist's image of an acceptable theory. In a world of bounded rationality there is no guarantee that a work will reach the attention of its latent market of potentially receptive economists, quite apart from the profession in general. It is not possible for an individual to know everything about which economists have written in the past, or are working on at present, even within a fairly narrow specialism.

Before a contribution can become part of normal science it has to pass through a series of screens, just as does any consumer good before it is selected for purchase. The screening process may filter it out of a scientist's attention long before it is even appreciated as a work that perhaps ought to affect the way in which she views the world, even if ultimately its characteristics fail to conform with her image of what is acceptable.

a) The Publication Screen

Unless they spread by personal contact, ideas will only have the potential to influence the conduct of a discipline if they are actually published or receive widespread circulation as discussion documents. If referees are insufficiently diligent or perceptive, incorrect contributions may get into print and lead others astray until their deficiencies are discovered. Similarly, novel ideas may be wrongly condemned, sometimes with traumatic results: Phelps Brown (1980, p.9) recalls, for example, how Harrod suffered a nervous breakdown after his paper on what is now known as the marginal revenue curve was rejected on its first submission to the *Economic Journal*. There are four particularly unsatisfactory features which must be mentioned as affecting the way in which this screen works.

First, as Feige (1975) has pointed out, there is a tendency for econometric contributions to be accepted only if they contain strong results. This being known, careerist economists have a strong incentive to tailor their submissions so as to leave out any discussion of related, but 'inconclusive', work. Such tailoring may take the form of adjusting the sample source, size, or time period, until impressive relationships are shown, or the failure to include work with slightly different specifications whose weak results would cast doubt on allegedly impressive discoveries. The result may be that other academics waste a lot of time duplicating the 'weak' results and, because these fail to achieve publication, the process continues. Feige suggested to the editors of the *Journal of Political Economy* that they should accept such articles prior to calculations being made from specified data samples. It was a suggestion to which a distinctly cool reception was accorded.

Second, work by Crane (1967) seems to suggest that the evaluation of scientific articles is affected to some degree by non scientific factors. Journals were found to contain a disproportionate number of papers by people with the same backgrounds as their editors. She proposed two possible explanations of the role played by non scientific elements (1967, p.200):

1) As a result of academic training, editorial readers respond to certain aspects of methodology, theoretical orientation and mode of expression in the writings of those who have received similar training;

2) Doctoral training and academic affiliations influence personal ties between scientists which in turn influence their evaluation of scientific work. Since most scientific writing is terse, knowledge of details not usually contained in journal presentations may influence the reader's response to an article.

She was proposing, in effect, that the bias may be due either to academics with similar backgrounds having in mind a similar image of what constitutes a contribution to knowledge as they prepare or referee a paper, or because when referees know the background of an author they will be more tolerant of particular omissions or shortcuts that have been taken. A statistical investigation of these interpretations, in which she attempted to find out whether a journal that did not get articles refereed anonymously was any more prone to bias (she used the American Economic Review as an example, but it must be added that since 1974 it has stopped the practice), led Crane to conclude that the first, rather than the second, was the most likely explanation. Matters are not, therefore, quite as bad as they might be: an academic does not have to be a protegé of members of an editorial board to find a place for her work, but she will increase its possibility of acceptance if she construes correctly what referees are looking for by studying the characteristics of their work and then forces what she submits into the appropriate mould.

The third factor which makes this screen particularly hard to penetrate is the tendency for journals to include a disproportionate volume of contributions by members of their own editorial boards. This is hardly surprising given that, as we noted earlier, many journals are set up by academics who have been unable to get their ideas accepted in mainstream publications (either because they did not appreciate the importance of making them appear to fit in the usual mould, or because they were inherently incompatible). But this is little consolation for the young academic who lacks the prestige required to achieve an editorial position. This factor becomes particularly important if an academic wishes to write a critique of a piece by a member of an editorial board which has appeared in her own journal. Eminent academics do not easily accept images as incompetent researchers who should know better and, since the conventional practice is to send a copy of the critique to the victim in the first instance, they are particularly well placed to suppress threatening work if they enjoy editorial powers.

Finally, we note that the practice of sending pieces to referees judged to have expertise in the same field, while it ought to result in greater critical insight being applied, is not without its disadvantages. In such situations it is not really the editor who acts as the final gatekeeper, for she is not sufficiently competent to judge the accuracy of what referees' reports say. Information impactedness allows opportunistic behaviour by careerist referees who can see that a piece of work is complementary or competitive with their own. This problem is particularly acute with drafts or synopses of academic books, since publishers (unlike most journal boards) pay referees and the cost of doing so means that the convention is not to appoint more than one referee unless the first report is ambiguous. Either this, or the second referee is asked merely to comment on the general impression given by the work.

b) The Agenda Screen

If a work has achieved publication there is no guarantee that a scientist will read it, however relevant it may be for the problem she is trying to solve. She must first discover it and perceive that it might be useful. But she must search selectively and cannot know in advance whether she is casting her net unnecessarily wide or even whether she is casting her net unnecessarily wide or even whether she has, in the event, cast it wide enough. Literature search strategies thus involve an element of faith, just as do the more fundamental strategic decisions the scientist has to make about which concepts to allow into the hard core of her SRP, which we discussed in section 3.

Political, parochial, and technical considerations will be the main agenda restriction factors employed in routine scanning (e.g. neoclassical choice theorists will not normally read The Journal of Consumer Research even if their economics library happens to stock the journal, but will read Econometrica; Chicago monetarists will not normally read the Cambridge Journal of Economics or Capital and Class; and so on). Insofar as works are cross-referenced their titles and author reputations (about which we shall have more to say in the next part of this section) will be crucial, along with the sequence in which they are read, insofar as related works only make partial reference to each other. Publications such as Contents of Recent Economics Journals, the fact that libraries usually display new acquisitions in a separate, conspicuous section, and the tendency for authors to cite their previous works (which makes their discovery much easier) all help to ensure a concentration of routine scanning attention on recent publications.

Agenda restriction means that potentially important ideas placed in obscure journals, or even hitherto ignored ideas in old issues of mainstream journals, or in books no longer in print and thus not listed in publishers' catalogues, may go unnoticed for long periods. The rate of growth of knowledge is thus slowed down and effort is wasted on reinventing ideas. In economics, a good example of the consequences of inefficient screening techniques is the (re)discovery of the problem of investment coordination and the attainment of equilibrium in economies which operate without futures markets. This problem deserves to be known as the Richardson Problem after the Post Marshallian theorist G.B. Richardson, who spent most of his academic life investigating and unsuccessfully trying to persuade his fellow economists to take it seriously.

The essence of the Richardson Problem is that, in any market which is not naturally destined always to be occupied by a vertically integrated monopoly producer, the profitability of the investments of any single firm will depend not only on aggregate investment and consumer choices but also on the amount of competitive and complementary investment undertaken by other firms. Unless there are fairly narrow bounds on who else might see a market opening and be able to act upon it, a firm will have no way, short of collusion or espionage, of forming a potential surprise curve concerning the demand price for its output, <u>even if</u> it has accurate knowledge of consumer preferences. Furthermore, it cannot know the future supply price of its inputs or whether it will be worth investing in vertical integration unless it knows who else is planning to invest in future supply capacity.

Richardson was not, in fact, the first person to discover the difficulty. That honour seems to rest with Morgenstern, who aired his concerns about the prospects of attaining economic equilibrium without perfect foresight in papers published in German in 1929 and 1935, some time before he began to work seriously on the Theory of Games, with its related prisoners' dilemma problem. Useful discussions of these papers are to be found in the contribution of Borch to Hicks and Weber (eds) (1973, pp. 67-8). Dobb (1937) and Joan Robinson (1954) raised the same kind of question and, writing from a left wing viewpoint, presented the coordination problem as an inherent defect in capitalism. However, they provided no evidence to show how serious were its consequences. More open minded treatments were offered by Williams (1949) and Richardson himself (1956, 1959). Richardson then went on in his (1960) book and subsequent (1967, 1971, 1972) articles to consider how serious a problem it had to be, and whether or not planning might necessarily be better.

Eventually a formal mathematical discussion was provided by Radner (1968), who was at pains to emphasize that most economists only devote attention to one of the first of the following two types of uncertainty that affect economic transactions: that due to states of nature not being known in advance (which affects a good's value in use), and that cause by people not knowing what other traders are going to do (which affects value in exchange). It was, as Loasby (1977) has pointed out, rather unfortunate for Richardson that Debreu's (1959) axiomatic attempt to 'handle uncertainty' within a general equilibrium framework, which only deals with 'state of the world' uncertainty in a highly implausible institutional context, appeared at the same time as his own less formal work. It must have been a major distraction. Now that Radner has set out the nature of the Problem in the language of the general equilibrium theorists they should have less reason to neglect it (and a similar point can be made with respect to behavioural theories of the firm now that Radner (1975) has attempted to model satisficing behaviour by managers in formal terms).

However, the problem is that, while one can set up the Problem in the language of general equilibrium theory, it is not possible to 'solve' it except by making assumptions that are utterly unrealistic, in the manner of Debreu. Thus the common practice of those who are aware of Radner's paper is to cite and then assume there is an auctioneer or a recontracting process whereby equilibrium prices may be generated in all markets (including a complete set of futures markets) prior to production taking place. General equilibrium theorists remain unaware of Richardson's disequilibrium analysis, in which the scale of the Problem was limited by the existence of knowledge imperfections about profit opportunities, by forms of implicit or explicit collusion (only possible in a situation of competition between saml1 numbers of firms); by the existence of goodwill and other 'institutional' ties between buyers and sellers of inputs; and by barriers to entry (which might include limit pricing of the kind suggested by Andrews). All of these factors are, in any case, at odds with the formal perfectly competitive system of general equilibrium analysis.

In perfectly competitive analysis, the tendency to assume traders face equilibrium prices, <u>which already exist</u> and which they cannot affect, tends to blind theorists to the existence of the Problem. The general equilibrium theorists' tendency to be concerned about the efficiency of equilibrium states, and not the bankruptcies and chaos that might occur on the way to attaining them, also tends to blind them to the importance of the Problem: this author remembers being told by Oliver Hart that, if one waited long enough, a stochastic process might ensure equilibrium without futures markets being needed even if there were perfect awareness of profit opportunities, free entry, and if agents were in complete ignorance of each other's plans.

The example of the Richardson Problem illustrates particularly well the effects of the agenda screen (and perhaps the tendency for people to filter out ideas at odds with their core beliefs), since there is a complete absence of cross referencing between those who discovered it. Furthermore, apart from Morgenstern's original contributions, none of the articles appeared in obscure journals, while Joan Robinson's paper figures in a well known collection. Radner's (1968) article and Richardson's (1971) article have both been reprinted, the latter in a widely used text currently in its second edition (Wagner (ed.) 1981). Particularly ironic is the fact that, while most economists are still unaware of the Problem or do not appreciate its significance for policy formulation, representatives of the power station turbine supply industry _did_ discover the right person - Richardson himself - to argue their case when taken to court by the UK Monopolies Commission for engaging in collusive tendering. They had been driven to this practice by the situation of excess capacity in their industry which resulted from the fiasco of poor coordination that was the so-called National Plan (see Richardson, 1969).

c) _The Novelty Screen and the Role of Reviews_

The fact that an economist has discovered a work which seems a possible aid to the solution of puzzles in her area of interest does not guarantee she will actually read it for herself in its original form. When trying to decide whether or not to examine a work, and in what detail, the economist is in a position entirely analogous to that of managers in Kay's (1979) behavioural theory of the allocation of resources to corporate R and D. If a manager knew what the result of R and D expenditure was going to be she would have no need to undertake it, but how can she know whether it is worth making if she has little idea of what the result could be? Once more, the inquisitive person is driven to choose a set of rules for choice which experience suggests will provide an adequate way, so to speak, of separating the wheat from the chaff, a way of distinguishing helpful schemes from those, now on her agenda of possibles, which it might be a waste of time to read.

Academic rules for the selection of works worth serious study are just specialized forms of the cybernetic decision rules we use to simplify the process of shopping in a supermarket. The works of authors with established reputations for innovative flair or expertise in a particular area are more likely to be picked from library shelves than works by those who are unknown or who are known to repeat the same ideas over and again. At this stage in the screening process vital roles will usually be played by the precise wording of titles and the clarity of abstracts, along with the reports of colleagues and reviewers. Mathematical equilibrium theorists may very rapidly shut a book if a brief examination indicates low mathematical content, while disequilibrium theorists, whose methodological perspectives often rule out mathematical expositions, will only look at books with a high enough ratio of words to notation to conform with their image of economics. Furthermore, books may only be selected if they are stock.

Clearly, then, authors of new contributions to knowledge are in the same position as sales managers in Andrews' theory of the firm, desperately trying by non price means to attract goodwill from people who will usually look to offerings from suppliers of consumer and industrial products with established reputations. Like such managers, unknown academics may find it easier to acquire reputations via product differentiation, so long as they do not confuse potential readers or lead them to believe that their work will destroy existing understandings and replace them with anxiety. Since reviews may play a very significant part in shaping early beliefs about a book, just as they do with other products, it seems instructive to consider how, for example, the works of Richardson and Andrews fared in the hands of reviewers. Beliefs may be affected in unjustifiable ways depending on how the reviewer distinguishes a work from related contributions, assesses how logical are its arguments, and delimits its favourable or unfortunate characteristics.

Would be readers of Richardson's (1960) book would hardly have been encouraged by the observation of Power (1961, p.761), in an otherwise perceptive and favourable review, that

> The method of the book is essentially armchair reasoning with only occasional reference to empirical studies.... Readers may find the concluding section of the volume disappointing in the light of earlier bold statements about the omissions of conventional theory.

The implication of the second comment is that the book does not offer much of an alternative to fill the gap it exposes.

While again broadly favourable, Lesley Cook (1964, p. 168) can only have damaged the book by suggesting that 'He is largely concerned with problems related to the cobweb theorem.' Conventional theorists tend to be predisposed to view the cobweb theorem as a special case of poor coordination, which affects only agriculture and the construction industries. Thus they would have been inclined to agree with Cook when she argued that Richardson was probably exaggerating the importance of the coordination problem when he applied it to investment decisions in general.

Cook argued that an orderly process of market adjustment through sequential entry would have seemed much more plausible, especially if he had chosen to analyse the problem in disequilibrium terms rather than with comparative statics. In fact Richardson <u>did</u> present a detailed examination of sequential entry and showed that it did not eliminate the difficulty, particularly in industries such as chemicals with long gestation times (cf. Beck, 1972). While he used comparative statics to make his arguments manageable he was, like Marshall, overwhelmingly concerned with the analysis of evolutionary processes rather than equilibrium states. Cook further failed to emphasize that Richardson's aim was not to demonstrate, as Joan Robinson wished to do, that the coordination problem always causes chaos and 'the impossibility of profits' but, rather, to show how such problems could be avoided in practice. He would then have a means of appraising the implications of competition policies based on conventional neoclassical theories which neglect the fact that markets are, as Loasby (1978, p. 11) puts it, 'information structures.'

The possible reasons for the lack of success of Andrews' work have been investigated in great detail by Juli Irving (1978). Her summary of the reactions to his (1949) book reveals very clearly the variety of interpretations that reviewers provided.

> (Y)ou can take your pick. Andrews' normal cost theory as set out in Manufacturing Business was a theory; was not a theory but a description; was the same as/different from a full cost principle; destroyed imperfect competition, was the same as it; was the same as perfect competition, was not the same as it; was a theory of competitive/collusive oligopoly (Irving, 1978, p. 162).

Fifteen years later Andrews' book On Competition in Economic Theory appeared and provoked another set of strange reviews. It was a critique of the theories of imperfect competition to which the earlier book had proposed, for manufacturing firms, an alternative analysis. The (1964) book did not systematically or explicitly reproduce this alternative. Andrews' critique was generally well received, but most reviewers appeared absolutely oblivious of his previous contributions, even though they were listed at the start of the book. A typical reaction was that of Heath who, despite having published widely on competition theory and policy, revealed his own unawareness in a review in <u>Kyklos</u>. He suggested (1965, pp. 709-10) that

> (Andrews) provides such a challenge to traditional theory that the reader can only conclude that to reach an adequate theory of competitive behaviour it would be best not to start from static marginal equilibrium analysis at all.

But he then went on to describe the book as

> endlessly frustrating. Time and again he takes us to the brink of new ideas and analyses which offer the prospect of further advance in this difficult subject, and then with masterly self control restrains himself from taking the plunge.

He did not know that Andrews had long since taken the plunge, that his ideas had caused more controversy than many books do immediately after their publication (particularly in the pages of the <u>Economic Journal</u> between 1950 and 1952), and that they had then sunk almost without trace, cropping up occasionally in footnote references on mark-up pricing.

d) <u>The Comprehension Screen</u>

After a scientist has decided to explore the contents of a particular piece of work she has continually to ask herself whether she has devoted sufficient attention to it. If it is not easy to understand and requires several readings before it can be grasped fully, the impression may begin to develop that the arguments contained in it are misconceived. The comprehension screen is a communications barrier that any product, be it a scientific theory or a traded commodity, _must_ overcome in order to demonstrate that it _is_ superior. Academics will not lightly allow their time to be wasted and will bring further procedural rules of appraisal into operation to prevent this from happening. If their personal strategies have hitherto seemed to be working adequately there is no clear reason why they should choose to use any new rules of appraisal to decide whether they have got enough out of a particular contribution to knowledge. A work which is in some doubt and does not conform to economists typical images as laid down by their rules of appraisal will lose their attention.

The works of Andrews, Penrose (1959), and Williamson (1975), are particularly good examples of contributions presented in ways which may cause a mainstream reader's attention to be removed too soon, even supposing they actually survive earlier screens and get read at all. The heuristics of the neoclassical SRP demand rigour, and the technically competent practitioner of this SRP is used to dealing with compactly presented models in which she can check, say, the general structure and first and second order conditions, rapidly to complete her complete her understanding. This is perhaps why reviewers of Andrews' work, despite supposedly being captive audiences, often came to the conclusion that it was only a description of what firms might do, set out for managers, rather than a set of interwoven arguments that exposed the problems that confront firms in disequilibrium, oligopolistic markets, and then deduced the kinds of business policies that would lead to long run viability.

Manufacturing Business appeared at a time when it was not conventional for theories of the firm to examine matters of internal organization on the way to achieving what was then their main goal, the statement of how prices and outputs were determined. Economists who read reviews of the book may come to think that it only contained a discussion of normal cost pricing, padded out with descriptive chapters. The extra chapters, however, contain the additional heretical components that are necessary for the price theory to seem credible, but a theory of such complexity is utterly alien to the vision of the neoclassical economist. A neoclassical reader is predisposed to see all the interwoven arguments as a set of connected insights and not as a theory, just as Heath could understand Andrews' later critique of orthodox retailing theory but could not see that <u>the basis of the critique was Andrews' rival theory</u>. That is to say, Andrews was saying that the marginalist approach was wrong because, in his view of the world, it seemed rational to set prices according to the normal costs of the relevant unit (the 'basket' in the case of general stores). His critique was not based merely on the findings of his empirical work, and a full appreciation of it required the reader fully to understand his theory.

The absence of any attempt at marginal analysis seems to have been the biggest bar to the comprehension of <u>Manufacturing Business</u>: it certainly troubled Austin and Joan Robinson. In his highly critical review Austin Robinson seemed completely unable to conceive of an approach whereby firms could seek to maximize profits by any means which did not involve setting marginal costs and marginal revenues equal to one another. He was therefore forced to conclude that <u>if</u> Andrews was saying anything different, once the nature of what he was trying to do had been disentangled, then he had to be threatening 'the whole body of economic reasoning' (1950, p. 771) by abandoning the notion of profit maximization. In papers written somewhat later and reprinted in her (1960) <u>Collected Papers</u>, Joan Robinson veered uneasily between seeming to construe the Oxford approach to oligopoly as an improvement on her own theory of imperfect competition (1960, pp. 230-233), to saying that normal cost price theory, though 'couched in the form of an attack on imperfect-competition analysis' .... 'seems to come to pretty much the same thing' (1960, p.234, fn. 2, in which she is referring to Brunner's (1952) guide to what Andrews had in mind, not the original book, which she considered to be 'full of dark sayings'). Finally, she settled (1960, pp. 242-3) on the same sort of conclusion as Austin Robinson, namely, that the theory denies profit maximization and 'leaves us in a state of perfect nescientness - anything may happen.'

Many of those who reviewed Andrews' book showed little understanding of the roles of goodwill, potential competition, or the principle of entry prevention, while not a single one mentioned that he was trying to write about disequilibrium processes. This last fact rather implies the need for a change in the wording of the general conclusion arrived at by Irving (1978) on the ultimate reason for Andrews' failure. She suggests that his theory was rejected <u>because</u> it clashed with the hard core of the neoclassical SRP. More precisely, we can say that insofar as reviewers are typical representatives of Andrews' potential readership, as well as economists with a power to influence readers' opinions, the documentory evidence of their attempts to come to terms with his theory indicates that the effects of past adherence to the neoclassical SRP were such that <u>economists were prevented from seeing</u> that it was a logical <u>dis</u>equilibrium theory. Instead, their cognitive processes ensured that what they saw was a theory founded on irrational behaviour or something that, if indeed it was a theory, could be forced into conformity with a preconceived equilibrium construct (cf. Steinbruner, 1974, especially Chapter 4).

Part of the difficulty of understanding what Andrews was saying is linguistic and expositional. His book is not by any means badly written but a person with a background firmly rooted in equilibrium analysis is inherently unlikely to perceive that it is concerned with a disequilibrium process since it does not keep stating this explicitly. Since Andrews did not see competition in equilibrium terms, and was deliberately writing the book so that it would be comprehensible to managers, who were equally well aware that competition is a dynamic process, it would not have seemed unnecessary to him to mention either term very frequently. Had he understood the blinkered nature of the academic imagination he would have had good cause to write in a different style, and with a different terminology.

Now, the case of Leibenstein and X-inefficiency is a pointer to the rewards in terms of fame that may follow what is little more than a linguistic innovation. Andrews also chose to introduce unusual terminology but in his case the sequel was rather different. He defined his cost curves in keeping with accounting conventions, only to cause greater confusion amongst reviewers as to the nature of normal cost theory (and was then rebuked by Austin Robinson for writing for managers rather than economists). However, what has not been realized hitherto is that when he discussed efficiency and made a seemingly strange distinction between 'managerial' and 'technical' efficiency, he was already attempting to grapple with the phenomenon economists nowadays call X-efficiency and with the issues that are coming to be associated with the fact that labour is very often best seen as a 'fixed factor'.

Penrose's work, likewise, was let down because it was in the conventional rigorous mould. In his foreword to the second edition of The Theory of the Growth of the Firm Slater (1980, p. XXV) went as far as to say that

> (S)he has provided a framework which is capable of incorporating much of the older analysis into a more general and more realistic theory. But it is just a framework and the actual construction of that theory has yet to be carried out.

Williamson does not even give his readers the chance to come to such a conclusion about his (1975) book Markets and Hierarchies. He continually describes it personally as making use of the 'organizational failures framework', as though its complexity prevents it from being described as anything other than a meta-model.

The acceptance of behavioural and Post Marshallian works has been hindered by their complexity. This makes them hard to understand and commit to memory, as well as difficult to convert into determinate mathematical forms. Models which cannot be expressed mathematically and involve determinacy inevitably leave scope for a variety of interpretations. The kind of response they have received leads one to ask, therefore, 'when do generalizations that are not a complete description of reality become sufficiently easy to manipulate as to deserve the title 'model'?' This question has no straightforward answer. As Loasby (1976, Chapter 3) has been at pains to emphasize, the abstractions a theory makes can only be judged according to how adequate the theory seems for meeting the ends for which it has been designed. Sometimes theories can be exceedingly simple and abstract, yet yield results of great power. Einstein's $E=MC^2$ is ultra basic and in no sense describes the reality that Newton saw in the force he called gravity. Likewise, the simple but powerful paradox of thrift defies what an individual consumer sees in reality. However, in the case of the theory of the firm we seem to be nearer the opposite pole. Our argument for constructing in this area an analysis centred on assumptive realism does not result from a desire for realistic assumptions 'for their own sake.' Rather, we would contend that economists may only be able to offer practically applicable results in this area if they sacrifice the elegant simplicity of neoclassical choice and production theory for an approach which takes into account the complications caused by historical time, complexity and uncertainty. Complexity in a theory, which may cause its inadequate investigation and development, may thus be a necessary reflection of the complexity of the area of the world it seeks to render less mysterious.

6 Choice Between Alternative Bodies of Thought

There is no guarantee that an attempted contribution to knowledge will survive all of the screens discussed in the previous section. But, if someone chooses to publish it; if academics choose strategies comprising procedural rules that lead them to discover it and deem it worthy of detailed attention; and if their cognitive processes cause them to understand it as something not obviously logically defective or at odds with reality, it still has to survive competition with the academic's existing body of ideas before she will adopt it. This is so even if her existing ideas presently seem inadequate as tools for enabling her to meet her aspirations. We have come back, after our discussion of aspirational failures, search and the screening of ideas, to the difficult area of choice exposed in section 3 in our critique of Lakatos' objective criterion for choosing between competing research programmes, as well as in section 4. In this section we reiterate the main thrust of the arguments behind that critique, boldly state our view of the nature of the choice between ideas that get this far in the screening process, and consider a factor, which we have hitherto neglected, that seems to play an important part in determining the fate of a body of work.

The essential difficulty with Lakatos' view of the reasons why scientists switch between sets of theories is that in a world of partial models all knowledge is relativistic and current test results can never be claimed to be unambiguous or definitive. Unless research is controlled by some form of dictatorship, then, there is no necessary reason why scientists should agree on the merits of competing theoretical explanations of particular phenomena, of entire research programmes, or even which problems are worthy of investigation. Ambiguity is antithetical to the idea that there should be a generally accepted logic of appraisal. If test results are ambiguous, adherence to positivism must rest on faith and the fact that, as a way of proceeding in research, it fits in with its user's world view and priority system. The fact that there can be many theories to explain the same phenomenon, none of which can be a complete model, likewise means that those who criticize positivism have no obviously more secure basis for maintaining that a particular model that they accept on a priori grounds for its assumptive realism will not lead them astray.

Each theoretical framework will have a particular set of perceived characteristics, just as will competing investment or consumption schemes outside the realm of academic work. Since there is no obviously acceptable logic of appraisal to use when choosing between rival theories, just as there is no obviously best investment or consumption scheme, choice between theories ultimately rests on personal preferences and perceptions, shaped as they are by predispositions, upbringing in a social/academic/economic context, and by the selectivity of cognitive processes. Economists can do no more than assert what they believe to be the appropriate priority rankings for economic scientists to have over the characteristics that rival theories might be construed to contain, and then make their choices accordingly.

It is rare for a novel way of thinking to be able to provide all of its potential users with a means of meeting some of their goals without preventing them from meeting others that it seems possible to attain if they adhere to their existing frameworks. In order to catch on widely new ideas may therefore need to be marketed in a way which makes it seem that they offer a bundle of characteristics which will survive better than their rivals the filtering processes of the bulk of their potential users. If they cannot be made to appear in such an image, and if it is not possible to persuade potential users to change their aspirations and priority rankings to form a mould which they will fit, they will be restricted in appeal to only a small segment of the academic world. As an example of how not to sell a body of ideas even to a potentially receptive minor segment we shall consider once more the case of Andrews' work. But, before we do so, we should note that we have so far failed to discuss one particular characteristic which scientists seem to rank highly when deciding whether or not a body of thought is acceptable.

Skinner (1979) has shown that a characteristic scientists frequently demand from a contribution to knowledge is that it should be able to account for as many features from as few principles as possible. Skinner demonstrates that Adam Smith, Popper and Shackle have strikingly similar views on the need for economy and universality in theoretical approaches to science. He quotes Adam Smith as saying '.... it gives us pleasure to see the phenomena which we reckoned the unaccountable all deduced from the same principle (commonly a well known one) and all united in one chain.' When a scientist has a simple theory which seems to explain many things she has less need to be a specialist and restrict the scope of her inquiry. Furthermore, since any theory is but a partial model which can only be treated in a restricted way, allusions to general applicability, illustrated with the aid of diverse case studies and analogies, help reduce anxiety when using it.

Andrews' contributions to economics, unlike, say, Keynes' (1936) General Theory, were launched completely without regard to their ability to satisfy economists' aspirations for generality. The features which disadvantage them in this respect also hinder them with regard to other characteristics in which theorists seem to be interested. His contributions dispense with marginalism, emphasize the characteristics of aggregates, are not reductionist (unlike neoclassical theories) and concentrate on disequilibrium processes. Each of these features violates the conditions required to define states of Pareto Optimality, without offering any obvious alternative criteria for making judgments about changes in welfare. As a result only a specialized industrial economist is likely to be very receptive to his work. But, even within the narrow area of industrial economics, Andrews made two blunders, the effect of which was to make his work seem less general than it was.

The first mistake that Andrews made in this respect was, paradoxically, a result of an attempt he made to claim generality within industrial economics. Theorists who present a partial approach should not attack potential allies. They should discover who these are and publicize them in their work, even at the expense of their own claims to originality, in order to encourage reciprocal citations. But Andrews attacked other disequilibrium theorists, particularly Penrose, and Cyert and March. He did not seem to understand how his work could be synthesized with their ideas to the benefit of them all.

Andrews (1961) attacked Penrose for neglecting problems related to goodwill. He ignored his own comments on the role of differentation as an aid to entry and diversification as an important component in growth strategies of firms. In fact, elements of the work of Penrose and Andrews ~~have~~ recently been used by Moss (1981) as the basis of his Economic Theory of Business Strategy. Andrews (1964, p. 39) attacked Cyert and March for neglecting the competitive market environments in which oligopolistic firms often operate, and the competitive pressures amongst employees within a firm (cf. also Andrews and Brunner (1962) and (1975) Chapter 1). Despite these attacks, the discussions of managerial efficiency and industrial learning in his (1949) and (1951) works show quite clearly that, like Cyert and March, he certainly did not believe that firms always operate on some objectively given cost curve.

In a world of information impactedness strong competition and slack can both exist, so Andrews' work and that of Cyert and March are not inherently incompatible despite what may seem a surface contradiction. Normal cost theory does not require firms to have identical profit margins, merely to charge identical prices for physically identical products, while Andrews' theory of 'internal competition' does not require people in identical job slots to put in an identical effort to that which would be offered by those by whom they could be replaced; they merely have to offer what their superiors will conjecture to be the opportunity cost output. Agents enjoying informational advantages can thus reap supernormal returns for their efforts but this does not mean they should be thought of as facing downward sloping 'demand functions' for their outputs.

Andrews also failed to agree with authors of other works on cost-determined prices. As Eichner (1978, p.1437) points out, his different political perspective caused him to be at odds with his Oxford colleague Kalecki. He thought that Kalecki's 'degree of monopoly' term had pejorative connotations. In marketing terms, he would have been wiser to argue that his own work made sense of Kalecki's rather vague idea by showing that the mark-up depended on the difference between a firms normal costs and the conjectured opportunity costs of potential competitiors, i.e. the 'degree of monopoly' depended on the 'degree of competition'.

Given the nature of academic screening processes it is most important that authors should not make unfavourable references to the works of other contributors who are actually, but not conspciuously, using the same hard core concepts. Where such criticisms are made, their impact will vary according to the order in which works are read. Economists who have not read Cyert and March (1963) may well decide not to bother after seeing Andrews' remarks against behavioural theory. By contrast, organization theorists who have taken on board Cyert and March's theory, and wish now to consider the firm in its market setting, will be predisposed to defend this theory should they come across Andrews' work. In the process they may perceive scope for a synthesis around the common theme of information. (Then, if these theorists discover Williamson's (1975) work, which emphasizes how organizational structure affects the pressure of competition within firms, they will not construe it as neoclassical theorists have done, namely, as a work which shows how internal labour markets and a rational choice of organizational structure can remove organizational slack and restore determinacy. Rather, they will see it as supportive of their ideas, despite the fact that it is partly Williamson's complete failure to relate his arguments to Cyert and March's ideas on slack (he lists their (1963) book, to which he contributed a chapter, in his bibliography, but nowhere refers to it) that lays his work open to the neoclassical interpretation. Williamson's book, hardly surprisingly, also makes no mention of Andrews or his views on internal competition.

Andrews' other marketing error was to present his ideas in stages and never combine them in a single book. Initially Andrews (1949) presented a theory of manufacturing firms without a discussion of investment criteria or the economics of retailing. The theory of retailing appeared over a fifteen year period (Andrews (1950, 1964), Andrews and Friday, 1960) and incorporated fragments of a disequilibrium theory of consumer behaviour. The investment theory appeared as the last chapter of a business history work he wrote with Elizabeth Brunner (1952). But it would not be obvious to readers that Andrews held a disequilibrium view of the world unless they had read his critique of existing approaches to competition theory, for which they had to wait till 1964, or his (1951) paper on the concept of industry in the light of Marshall. Furthermore one of the key concepts in Andrews' work - potential competition - was explained most convincingly only after his death (in the third chapter of Andrews and Brunner, 1975).

We conclude this behavioural discussion of the reasons for the failure of Andrews' economics by considering a significant passage from Pickering's review of Studies in Pricing. He argued (1976, pp. 621-2, emphasis added) that

> (C)ompetition is not only about price, and in paying so little attention to other aspects of competitive independence of firms in a market it is doubtful whether, between them, Andrews and Brunner have succeeded in producing a sufficiently general model of the behaviour of oligopolies.

Pickering's dismissal of their theory, in a review that is otherwise sympathetic and displays an awareness of Andrews' earlier works, demonstrates both the importance he attaches to the universality characteristic and the limitations of his understanding of Andrews' work. The complaint of 'insufficient generality' obscures the possibility that, in some markets at least, the theory proposed by Andrews may offer powerful insights where theories of imperfect competition offer none. This review allows theorists who would not normally attach much importance to 'non price factors' suddenly to convince themselves that these are very significant. Thereby, they 'justify' to themselves their decisions not to study Andrews' work and, with it, the problem of oligopoly, which, if Pickering's review is accepted, still looms threatening and insoluble. Pickering makes no attempt to suggest that, with some thought, readers of the book might be able to extend its thesis to incorporate omitted non-price factors; he wants a neat, prepackaged-theory. In fact, non price aspects of competition figure much more prominently in the work of Andrews than most other theorists, for they are central to his entire vision of the competitive process. This is quite obvious in his discussions of the economics of stockholding and discount warehousing, or when he considers the relationships between excess capacity, goodwill, and the abilities of firms to deliver regular supplies to their customers. It is true that advertising is rather thinly discussed, but there is no reason why marketing costs, including the conjectured marketing costs of potential competitors, cannot figure in an analysis of price formation based on Andrews' work.

Andrews realized that marketing and the exploitation of complementarities are important to the success of produced commodities but failed to realize that they can affect the fate of economic theories too, and this contributed to the lack of impact his ideas had. But it must be emphasized that their failure to gain the approbation of fellow economists does not necessarily mean they are bad ideas.

Insofar as academics accord a high priority to universality, it would seem that the more general a research programme can be made at the outset the more chance it stands of being adopted, so long as it is presented in such a way as to survive the comprehension and selection screens described in section 5. Once scientists have been tempted to explore it so far as to see it as a coherent rival to their usual theoretical system it has overcome much of the sunk costs advantage enjoyed by the latter. The more general an approach is, the easier it is for academics to whom it appeals to justify to others their selection of it and any contributions to knowledge they make by extending it still further. In part, the lack of popularity of disequilibrium contributions to economic theory may therefore be explained by the lack of a single work that synthesizes and expands upon compatible elements from these works. Such a work, which would be rather akin to an up-to-date version of Marshall's principles, could then rival neoclassical 'state of the art' books on general equilibrium theory. It would also simplify the task of overcoming the key remaining sunk cost advantage of neoclassical theory, namely its stock of textbooks and teaching programmes. Our theory of the economist in the academic environment predicts that there will be few disequilibrium theorists who would attempt to write either the state of the art treatise or its textbook sumptification. It would be a herculean task and the very nature of the theories involved would require a work so long it would not survive the publication screen.

7. Conclusion

In this paper we have attempted to use a synthesis of behavioural ideas, with a strong marketing perspective, as a novel way of approaching the history of economic thought. This seems to represent an important expansion of the generality of behavioural analyses of choice. Philosophers of science characteristically do not consider the motivations and decision taking process of scientists. We have used our proposed theory of economists' behaviour to criticize Lakatosian SRP analysis, but our arguments are entirely consistent with Feyerabend's (1975) anarchic extension of the (1970) work of Kuhn, and help to explain the behaviour patterns these authors have observed in the physical sciences. To illustrate how the new theory works we have made a case study of the failure of earlier attempts to write disequilibrium economics (to which this paper is complementary) to affect more than a small minority of academic economists. In this respect we have paid particular attention to the fate of the work of P.W.S. Andrews, which has previously been studied by Irving (1978) in a thesis which used a more conventional approach to the history of economic thought.

We have taken the position that an academic worker is not fundamentally different from any other workers, since the products of academic labour are both monetary and psychic income, while the incumbent of an academic job inevitably faces anxiety as to whether she is going about it the right way and with how her peers will view what she does. Whether or not academics are aware of the possibility, there is much scope for personal anxieties to shape their perceptions about the nature of the contributions they are making. This scope exists because, in a world of partial models, it can never be shown unambiguously that one model is, or will be, superior to another in terms of its empirical performance or even the realism of its assumptions. Thus although each academic research programme will have some logic of appraisal there is no reason for a universal set of choice criteria to be applied when theories are being evaluated.

Contributions to knowledge have been treated as if they are not fundamentally different from consumer products or investment goods, about whose merits non academics have to argue in the ordinary business of everyday life. As in everyday life, there is no guarantee that the economic scientist will be aware of a pressing need to consider trying out something new as an aid to understanding economic events. She may be blissfully ignorant even of high level difficulties in her framework of analysis. If these are pointed out by someone else she may remain 'justifiably' convinced that it will be possible to resolve them without abandoning the framework. This will especially be the case if the framework has hitherto been very helpful and is not under attack from all directions. There is no guarantee that she will be aware of alternative theories or, if she discovers them, will take the trouble to ensure that she has understood them in the ways intended by their authors before she rejects them. The greater the time pressure and volume of potentially useful ideas, the more selective her search processes have to be to ensure she escapes information overloads and makes enough tangible contributions to knowledge.

It will be rare for a body of thought to be perceived as dominating over its rivals in all of its characteristics. Because of this, the academic's own priority ranking, rather than any unaminously agreed logic of appraisal, has the final say in determining which theories she will accept. Feelings of anxiety may cause cognitive processes to shape perceptions of the merits of contributions to knowledge in regard to lower priority characteristics in situations where inconsistencies would otherwise be implied by a particular choice of analytical tool. Thus, whatever choice the academic makes, she will feel that she is making an honest selection, except in rare cases of careerist behaviour by charlatans. Such cases are entirely possible in a world of incomplete and impacted information. It is not easy to draw the line between the charlatan and the seeker after truth whose perceptions have been shaped by the selectivity of her cognitive processes and reading.

The pressures of the modern academic lifestyle make it particularly hard for a scientist to take a detached view of why she is doing what she is doing. Other academics pass judgment on the soundness of this and, during her upbringing as an economist, shape her expectational environment. The signals most economists receive are that mathematical tractibility is to be rated above realism of assumptions, or 'presently available' testable predictions. The economist who writes disequilibrium theories of words restricts both the prestige and number of potential outlets for her work, and may be subjected to hostile taunts to the effect that, say, what she is doing is mere 'economic poetry' because it lacks mathematical rigour. Such an economist also has to read lengthy monographs or undertake time-consuming case studies instead of being able merely to inspect formal constructions and engage in 'number crunching' with published data.

Dissident economists not unnaturally become embittered and withdrawn, or remove the 'rubbish' of the academic world from their sight by leaving it for the real world of their theories, a possible case of Gresham's Law applying to economists as well as to money.

Some, before making such 'exits', first attempt to employ 'voice' (to use the (1970) terms of Hirschman) to attract the attention of members of what they see as a decaying profession and suggest the time has come for a reorientation of their behaviour such that it conforms with the dissident world view.

Our analysis leads to two connected ways of explaining the dominance of neoclassical economics. One is that it is safer and more rewarding to be an equilibrium theorist of the conventional kind. The other is that upbringings affect the constructions young economists form of what it is that economists do and they then act in conformity with this image unless given an exceedingly strong cause to behave otherwise. Kuhn's (1970) suggestion that a scientific revolution will not succeed until older scientists have died off seems entirely reasonable from a behavioural standpoint. If a mature scientist is to undergo a personal scientific revolution she will have largely to dispense with a well-formed world view. Since the choice will not usually be clear-cut such a transition, if made, would entail a period during which she suffered nothing short of a scientific nervous breakdown.

But it has not been the intention of this chapter to suggest that we should never see a radical, non equilibrium economist, or see mainstream economists as members of a mutual admiration society which the public at large lack the qualifications to criticize. The market for ideas is sufficiently segmented, and contains enough slack, to permit some economists with different upbringings, perception tendencies, and past reading, to be able to follow unorthodox schools of thought if they wish to do so. For such economists, slack in the economic system provides hope. A given body of thought (such as, at the subdiscipline level.Keynesian macroeconomics) can initially win favour only later to be replaced by a resurgence of the originally supported research programme (cf. Dow and Earl (1982) Chapters 13 and 18). It can itself come back to the fore, so long as some academic mavericks take an interest in it, in a way rather akin to Downie's (1958) depiction of the competitive struggle between firms. A permanent transfer can only occur if there is no entry by a new body of thought or if there is an inadequate innovative response from what most would presently judge to be an outclassed approach.

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