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A BEHAVIORAL THEORY OF ECONOMISTS' BEHAVIOR

1. Introduction

This essay uses elements from a behavioral/post-Keynesian analysis of choice (developed at length in Earl, 1983a, 1983b) to explain why a behavioral—or indeed some other, more realistic alternative to the dominant neoclassical theory—has not been adopted by 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 behavior. The main point is 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 rather because they are salable as tools which enable their users more easily to reach their goals.

A neoclassical reader will find it difficult 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. Owing to its lack of concern with the complications caused by complexity and ignorance, neoclassical theory cannot justifiably be used to model the behavior of scientists, and

The author's indebtedness to Brian Loasby for inspiration should be obvious to anyone familiar with his work. He should also like to acknowledge helpful discussions and/or correspondence with A. W. Coats, G. S. Harcourt, T. W. Hutchison, J. Irving. S. J. Latsis, F. S. Lee, W. Samuels, P. Tompkinson, and two anonymous referees. However, he alone is responsible for any errors and for the views contained in the paper.

then explain its own success and the relative neglect of behavioral economics. To avoid inconsistency, a neoclassical theorist must either reject her theory in favor 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 behavioral 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.

During the course of investigating the neglect of behavioral economics we shall not refer only to the American organizational theorists, such as Cyert and March, Simon, and Williamson. We shall also devote considerable 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 behavioralists 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 applicable, furthermore, to explaining why some ideas are more salable 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 nonmarginalist, 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 had little influence on the way 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's 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 behavioral 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 essay 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 behavioral approach and the well-known work on scientific research programs 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 behavioral 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. It assumes instead that the academic scientist's position is 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 labeled "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, just 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 certain priorities among their goals rather than by trading off the characteristics of activities against each other. In behavioral theory it is recognized that lexicographic forms of choice are 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, 1983b).

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 rewritten along such lines so that it occupies less of a no-man's-land between neoclassical and behavioral economics.) We bear the behavioral 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 toward meeting them.

The following goals seem likely to be among those that an academic economist will rank highly, though they will not necessarily be ranked in the order shown:

(1) to acquire, at a particular rate, the ability to predict and control aspects of the economic environment (scientific aspiration);

(2) to achieve a particular level of fame and a place as an originative thinker in the history of economic thought ("self-image" aspiration);

(3) to obtain, at least, a particular configuration of consumption activities (life-style aspiration);

(4) to obtain particular target levels of attainment in her social, natural, and teaching environments, which conform with her image of how a university ought to be (environmental aspiration(s);

(5) to expend no more than a particular amount of effort while seeking knowledge and income (indolence aspiration);

(6) to keep situations of unfamiliarity within particular tolerable bounds (anxiety-avoidance aspiration).

Since it is not clear, in a world of partial knowledge, which is ultimately going to be the best approach to solving economic problems (see further, section 3) and since there are many economic problems to be solved, an economist should, in principle, have scope for much discretion as she goes about attempting to meet her goals. There is no single scientific rule necessarily requiring all academic economists to adopt the same practices, any more than it is necessary for managers to attempt to maximize profits when it is not clear how to do so. Academic economists may rank their goals differently, have different endowments of human capital, and exhibit different degrees of interest in particular areas of the subject. However, in the present competitive academic environment it is easy to see that, if such an economist wishes to achieve a high rate of published contributions to knowledge, high prestige, and high income, it will be rational for her to attempt to be an orthodox neoclassical theorist or econometrician. Furthermore, sunk costs of investments in such behavior militate against subsequent changes of a dramatic kind.

In order to acquire fame in her discipline, an economist will need to turn out works that the *profession* ranks highly, whether or not she believes her contributions to knowledge take her very far toward reaching her own scientific aspirations. Life-style aspirations will also be more easily attained by producing works that receive the professional seal of approval. A famous economist will be able to obtain positions in prestigious institutions, as well as consultancy and government-sponsored research work. Teaching loads may be lighter in more prestigious universities, which will mean there is more time for research and consultancy, leading to more fame, and so on. High-status institutions may also enable environmental aspirations more easily to be met, since they will attract better students, famous colleagues, and donations of private funds to improve their stocks of already elegant buildings.

If the majority of economists are orthodox neoclassical theorists, then, in order to obtain prestige or, even, for the young, relatively unknown economist, *any* kind of academic position at all, the wisest strategy may be to be a neoclassical economist too and carry out research along similar lines. It will certainly be the most anxiety-free career strategy, quite apart from a neoclassical economist's being able to feel comfort in the fact that she is not alone in her views. Conformity with the orthodox viewpoint, which will enable contributions to knowledge to receive the profession's seal of approval, is particularly advantageous in career terms, since an information impactedness situation typically exists in academic promotion and 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, noneconomists are not in a position to disagree. Candidates whose research implies 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 unfavorable background for the academic attempting to move elsewhere, since even those writing letters of references 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.

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 which will survive 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 its not being exposed as nonsense. She has to decide on the relative merits of investing further time and effort in rewriting 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, these can only make a limited contribution to removing the economist's anxiety in such a situation, unless the contribution in question conforms pretty much with some conventional mold. With idiosyncratic new ideas, all notions of a "probability of acceptance" are meaningless; anyone considering devoting effort to writing unorthodox papers must have a high tolerance of anxiety and be willing to risk a failure to obtain professional approval.

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 concerned with the areas of research 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 labor, industrial organization, and economic history. The second-ranking specialties 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 subdisciplines. 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 from journals for their contributions. 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.

The neoclassical style of research also happens to be much more economical in terms of effort than that which characterizes the group of economists with whose neglect we are concerned. The essence of the behavioral approach is stated by Cyert and March (1963, p. 1) at the beginning of their book, where 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." Andrews (1951, p. 172), in urging the abandonment of the concept of static equilibrium, observes, likewise, that the alternative patterns of analysis "will have to be built up out of empirical studies, just as Marshallian concepts were largely informed by their founder's studies of historical processes. No amount of spinning out logical chains of analysis based upon static concepts will help in this task." As is evident in, for example, Andrews (1949), Andrews and Brunner (1950, 1952, 1975), Cyert and March (1963), Penrose (1971), Richardson and Leyland (1964), and Williamson (1964), the behavioralists have certainly 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.

This kind of behavior 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 caliber will aspire to the easy approach to hypothesis testing. Such economists are able very easily to generate respectable publications by noting where, say, U.K. data are deficient and then virtually plagiarizing articles based on U.S. data the moment U.K. figures become available. Reekie (1980) has argued that this has been how a number of important U.K. 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 outputs of the behavioral 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, 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 that 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 nonavailability 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 possible only 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.

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 learned 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 behavioral as well as neoclassical economics) and, if she is being brought up in the "vulgar" neoclassical mold, "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 will have noted 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 behavioral/post-Keynesian theory simultaneously with general equilibrium analysis. Most concentrate almost entirely on the orthodox paradigm and are then required to come to grips with modern techniques upon beginning graduate work. They are then encouraged to use their technical expertise, particularly their skills as econometricians, in doctoral work. (Econometric work is favored in this context because it is much more assured of *some* kind of results than research in pure theory; it 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 analyze. 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 behavioral economist. In order to be able to continue to push back the frontiers of their economic knowledge, academics will usually shut their eyes to the Popperian problem that a framework may one day 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 resoluble 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 discussion of the nature and use of scientific research strategies on the way toward meeting the economist's overall goals has some features in common with the work on Scientific Research Programmes (SRP) associated with Lakatos (1970) and, more recently, Latsis (1976) and Remenyi (1979). Lakatos calls the set of background presumptions that scientists take without question when building auxiliary hypotheses the "hard core" of the SRP. The sets of procedural rules—the "dos and don'ts" of the SRP—he calls the "positive and negative heuristics." If anomalies are discovered when auxiliary hypotheses are being tested, this is taken to indicate that something is wrong with the hypothesis in question, or the test procedure, not with the hard core.

But this similarity between our behavioral analysis and the Lakatosian view of the evolution of ideas is only partial. The SRP approach is excessively rationalistic and neglects the role played by scientists' personal motivations in determining the popularity of ideas. Lakatos (1970, p. 55) suggests that scientists have an objective criterion by which they can decide whether to switch between competing research programs. 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 *even if* they do yield testable hypotheses. Furthermore, test results may be interpreted in different ways or test methods queried (see 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 would trouble to pursue new ideas until they had generated empirical results: *someone* has to adopt a rival SRP before Lakatos's criterion can be confronted with any new set of results.

The lack of a clear-cut dividing line between progressive and degenerating research programs enables scientists whose SRPs have different logics of appraisal to apply different choice criteria and different justifications for their practices. For this reason, economists' justifications of their continuing adherence to a particular SRP, or for switching to a rival SRP, need to be examined carefully. If the potential of any given SRP is ambiguous, there is, in principle, no reason why an economist may not concoct a justification for her behavior that enables her to conceal her "real" motivations, which may have more to do with her desire to preserve her self-image and not look a fool or with anxiety avoidance, indolence, or absolute careerism.

The arguments in the previous paragraph, and the discussion of economists' effort outputs in section 2, may seem to imply that we believe economists are willing to embrace dishonesty if it generates "contributions to knowledge" that advance their careers. We believe that such extreme careerist behavior 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. An economist may, for example, rank the preservation of her own self-image more highly than her desire to make genuine contributions to economic analysis. If a switch to a new SRP would have no positive career payoff, yet would involve an admission that she believes she has hitherto been foolishly wasting her time, the economist may carry on as before, despite attacks from "minority theorists," and claim that in the long run her SRP will provide the answers (cf. Hahn's 1972 statement of faith in the promise of general equilibrium theory).

The economist who behaves in such a way may indeed *believe* she is telling the truth and see herself as a humble seeker after truth rather than a careerist. However, 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). Hence the cognitive processes of any economist who is not a self-confessed opportunist will ensure that her perceptions of her own scientific endeavors are molded 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.

A 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 unreasoanble 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 program, 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 program's having run out of puzzles to solve, which are 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 greater rigor and formalism to their work. Few economists could act as Hicks did in his early sixties when, while 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 come 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 alternative SRP, or even to another profession.

(b) The discovery of important empirical anomalies

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. While 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 that individuals engage in profit-behavior 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.

Where minor adjustments do not resolve anomalies without additional ad hoc 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 U.K. textile and footwear industries. These discussions made him aware of the importance that was attached to goodwill, and of 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's 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's closely related, nonmarginalist theory of investment came about similarly, as a result of a lengthy business history investigation carried out with Elizabeth Brunner (Andrews and 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. Case study investigations by Hall and Hitch (1939), Saxton (1942), and Barback (1946)—the last of which was greatly influenced by Andrews's work—have been accused of containing conclusions that can be interpreted to be consistent with both views and that are based on small sample sizes with biased questionnaires. Economic investigations conducted more recently have failed to settle the dispute to the satisfaction of participants on both sides. Laidler and Parkin (1975) alleged that the antimarginalist 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's well-publicized 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 productivity 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's macroeconomics.

The controversies provide a nice demonstration of the sequential search processes and defense mechanisms of the neoclassical SRP. Eventually, Samuelson and his neoclassical colleagues conceded that there was no way round the logical flaw.[1] But this did

[1] See the essay by Chase in this volume.

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.

(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. Behavioral 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 behavioral 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.

Changing patterns of ownership and control in companies over the past century have repeatedly threatened economists' aspirations with regard to assumptive realism, forcing them to develop new theories of the firm. In the early editions of his *Principles*, Marshall depicted individual firms as family businesses which always died off in the long run because the quality of their owner-dominated management declined through time. A firm never lived long enough to obtain a monopoly hold on its market by undercutting its rivals and exploiting economies of scale. However, joint stock companies could hire superior management from outside and might never die off, so the question of what stopped monopolies from emerging was once more in need of a solution. Joan Robinson's (1933) *Economics of Imperfect Competition*—which followed Sraffa's (1926) suggestion that the growth of firms was restricted by the difficulties of expanding sales without bidding prices down and encountering negative marginal revenues—was one solution. Less well known is the work of Andrews (1951) and Downie (1958), which amends Marshall's disequilibrium analysis to incorporate the possibility of corporate longevity by allowing firms to jostle for industrial leadership. Central to this work is an abandonment of the neoclassical assumption that firms produce given products, always at minimum cost, with some objectively given production function. This leaves 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 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.

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 make the impact intended by their authors only if they are discovered and comprehended as containing what their authors believe 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 specialty.

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 have the potential to influence the conduct of a discipline only 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 of 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 (see also Cooley and LeRoy, 1981).

Second, work by Crane (1967) seems to suggest that the evaluation of scientific articles is affected to some degree by nonscientific 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 nonscientific 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 repsonse to an article.

She was proposing, in effect, that the bias may arise either because academics with similar backgrounds have 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 protégée 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 mold.

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 mold, 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 behavior 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 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 make only 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 ideas 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 future 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 conjectures about the demand price for its output, *even if* 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 honor 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. 67-68). 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 devote attention to only the first of the following two types of uncertainty that affect economic transactions: (1) that uncertainty due to states of nature not being known in advance (which affects a good's value in use), and (2) that uncertainty caused 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 deals only 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 behavioral theories of the firm now that Radner, 1975, has attempted to model satisficing behavior 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's 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 (possible only in a situation of competition between small 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.

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.

(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) behavioral theory of the allocation of resources to corporate research and development (R & D). If a manager knew what the result of R & 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 perpectives often rule out mathematical expositions, will look only at books with a high enough ratio of words to notation to conform with their image of economics. Furthermore, books may be selected only if they are stocked.

Clearly, then, authors of new contributions to knowledge are in the same position as sales managers in Andrew's theory of the firm, desperately trying by nonprice 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. Just as critics' reactions can often make or break a Broadway show on its opening night, so academic reviewers may affect beliefs about a contribution to economics depending on how they distinguish a work from related contributions, assess how logical are its arguments, and delimit its favorable or unfortunate characteristics.

(d) The comprehension screen

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 rigor, 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 understanding. This is perhaps the reason that reviewers of Andrews's 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 (see Irving, 1978).

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's objective criterion for choosing between competing research programs, 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's 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, on the value of entire research programs, or even on the choice of 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, by 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 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 mold 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's 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 be treated only in a restricted way, allusions to general applicability, illustrated with the aid of diverse case studies and analogies, help reduce anxiety when one uses it.

Andrews's contributions to economics, unlike, say, Keynes's (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. First, he made the mistake of attacking potential allies such as Cyert and March, and Penrose, failing to see that their ideas were actually compatible with his own. He also published his ideas in stages over a long period, in an inconvenient order, and never integrated them as a single volume (see Andrews, 1949, 1950, 1951, 1958, 1964, Andrews and Brunner, 1952, 1975, and Andrews and Friday, 1960).

7. Conclusion

In this essay we have attempted to use a synthesis of behavioral 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 behavioral 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' behavior 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 behavior 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.

We have taken the position that an academic worker is not fundamentally different from any other workers, since the products of academic labor 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 as to 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 program 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 nonacademics 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 unanimously 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 behavior 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 life-style 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 tractability 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 rigor. 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.

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 behavioral 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 essay to suggest that we should never see a radical, nonequilibrium economist, or perceive mainstream economists as members of a mutual admiration society which the public at large lacks 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 favor only later to be replaced by a resurgence of the originally supported research program (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 occur only 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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