

# On economic paradigms

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Is there progress in economics and, if so, how can we be certain? This may seem a strange question. After all, one has only to compare articles in any leading economics journal today (say, the **Economic Journal**) with those in the same journal fifty years ago to see a radical change. Sophisticated mathematical modelling and econometric testing using large data sets are now commonplace in modern economics articles: yet half a century ago most articles were verbal and statistical testing rudimentary.<sup>1,2</sup> However, advances in technique should not be confused with improvements in understanding. Neither should the fact that if one picks up, say, a standard micro-economic or macroeconomic textbook,

the impression may be gained of a smooth cumulative accretion of economic knowledge, with older theories being progressively improved or replaced by better ones. This is not necessarily the case.

Nobel prize winner Solow (1983) succinctly summed up the state of macroeconomics when he wrote: “Why, then, is macroeconomics in disarray? ‘Disarray’ is an understatement. Thoughtful people in other university departments look on with wonder. Professional disagreements exist in their field too – but as outsiders, they are shocked at the way alternative schools of thought in macroeconomics describe each other as wrong from the ground up. They wonder what

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<sup>1</sup> An example of the increasing mathematisation of economics is demonstrated by the fact that Joan Robinson’s (1933) **The economics of imperfect competition** was considered to be highly mathematical when it was first published. Now, a second year undergraduate should be able to understand it with ease.

<sup>2</sup> The General theory contains only one diagram and only a handful of equations. There is virtually no empirical evidence.

kind of subject economics is". Take another example – endogenous growth theory. There has been outpouring of theoretical models in recent years trying to explain the complex phenomena of growth in terms of a few highly aggregated variables (such as homogeneous output, labour, and capital) but with mathematically complex techniques.<sup>3</sup> Yet, most of the insights were known and discussed fifty years ago (the importance of human capital, education, increasing returns, etc.) (Nelson, 1998). How much further do the new models take our understanding of the qualitative process of economic development? Do they give us new insights which are useful for development policy? Why does a distinguished economic historian, when explaining the same phenomena, dismiss these models in a footnote? (Landes, 1998). Moreover, economics seems beset with fashions, a sure indication of an immature discipline. Monetarism was seized upon with alacrity in the United Kingdom in the 1980s by the Thatcher government and controlling the money supply became the *sine qua non* of government policy – although it was never quite clear which definition of the money supply was to be controlled. By the 1990s,

discussion of the money supply figures had virtually disappeared from the economic scene. How can we account for all this?

The obvious place to turn for an answer to this question is the Philosophy of Science. Most of the work in this field has been with respect to the natural sciences, but economic methodologists have drawn heavily on these ideas.<sup>4</sup> (The early development of neoclassical economics was based largely on an analogy with physics). Here, we are immediately faced with a problem in that there is no generally accepted corpus of thought in the philosophy of science on which we can draw. There was what Suppe (1974) terms the "received view", but this has largely gone. This view is that scientific progress can be understood as being determined in Popperian fashion by the falsification of theories, so that there are criteria that can rationally demonstrate that science has progressed. In other words, there are canons that mean any two scientists faced with the same evidence will come to the same conclusions.

However, this was dramatically thrown into question in the 1960s by two important works: Thomas Kuhn's (1962) **Structure of scientific revolutions** and Paul

<sup>3</sup> At one stage, economic growth theory drew heavily on optimal control theory which had been developed for calculating the paths of rockets. Whether this was of any help to those engaged in the practical problems of stimulating growth in, say, the less developed countries is debatable.

<sup>4</sup> Chambers (1982) provides an excellent introduction to the philosophy of science, Blaug (1992), Caldwell (1994), and Pheby (1988) are all good introductions to economic methodology.

<sup>5</sup> Feyerabend's magnum opus, **Against Method**, was not published until 1975. Feyerabend's approach is deliberately more polemical than Kuhn's, with a provocative style and should be read in that spirit. Feyerabend did not suffer fools gladly, who seem, in his opinion, to be a large number of his contemporary philosophers of science, although he obviously had a great respect for Lakatos, whose approach is discussed below. Feyerabend considered that if one pushes Lakatos's approach to its logical limits, Lakatos's position becomes almost identical with his own. Hence, Feyerabend's jocular reference to Lakatos as a fellow anarchist.

Feyerabend's (1962) article "Explanations, Reduction and Empiricism".<sup>5</sup> The publication of Kuhn's and Feyerabend's work caused great controversy because they seemed to imply that theory choice was simply a subjective matter and thereby "irrational". In 1970, Lakatos attempted to rescue the situation by putting a Popperian spin on Kuhn with the development of his **Methodology of scientific research programmes** (SRP) and which caught the attention and admiration of a number of economic methodologists, who used it to assess the development (progress?) of economics. See, for example, the collection of essays in Latsis (1976).

However, it is by no means agreed that Lakatos was successful, and indeed Feyerabend (1975), in his classic **Against method**, explicitly considers why this was not the case. The upshot of all of this is that if there is no objective way in which one can determine (independent of anyone's whim) whether or not theory  $T_2$  is unambiguously superior to theory  $T_1$  in the natural sciences, perhaps this can explain why there is no universal agreement over economic theories. (This is not to deny there is a dominant approach – far from it. In the Anglo-Saxon world, it is neoclassical economics). In this paper, I shall consider why economists will never come to agreement even over the fundamentals of their discipline.

## THE KUHNIAN REVOLUTION IN THE PHILOSOPHY OF SCIENCE

Thomas Kuhn published his influential book, **The structure of scientific revolutions**, in 1962. The book was not only short and lucidly written but, unlike many other tomes in philosophy, was also immediately accessible by scientists who recognised in it a description of science as they practised it. Soon after its publication, the book generated a good deal of controversy within the philosophy of science. Today, the ideas expressed in it have been largely assimilated into the literature and its revolutionary implications diluted for some philosophers, by, for example, the work of Lakatos. It has, in a sense, moved from the status of being a highly controversial polemic to a classic.<sup>6</sup> Nevertheless, Kuhn's work gives some illuminating and enduring insights into scientific endeavour.

Kuhn was primarily concerned with the development of the natural sciences, most notably physics. He approached the subject from the viewpoint of a historian and, unlike most philosophers of science, he also had a formal training in physics and a knowledge of actual scientific practice. He found this bore little resemblance to normative prescriptions that then dominated the literature on the philosophy of science. Either scientists were hopelessly at fault in their scientific approach or something was wrong with the philosophy of science. By and large,

<sup>6</sup> This may be understating Kuhn's (and Feyerabend's) influence. As one philosopher of science recently put it: "It is now common for philosophers of science to deny that there are invariant standards for the assessment of theories" (Sankey, 1997, p. 3). That this is the case is due in no small measure to Kuhn and Feyerabend.

Kuhn thought the latter was true. At that time, the normative methodological rule for scientific progress was, at the inevitable risk of oversimplification, that science should behave along Popperian lines of conjectures and refutations. Hypotheses should be formed (it didn't matter how) and then rigorously tested. A theory could not be proved to be true, but it could only fail to be refuted. If it were refuted, the implication was that it should be immediately rejected; if it were not rejected, it should be subjected to further more demanding tests. Science progresses by learning from its mistakes and by developing hypotheses that rule out progressively more outcomes. The greater number of instances with which a theory is compatible, the more general it may be said to be. It is also more likely to be refuted. Consequently, science progresses by progressively refuting more improbable or restrictive hypotheses.

Kuhn, however, noticed that this procedure was rarely, if ever, adopted in scientific practice. Scientific theories were not immediately abandoned after a single, or even several, refutations. In nearly all cases, there was a good reason for this. A scientific test of a particular hypothesis will invariably involve some other implicit hypotheses or laws that are assumed to hold and which are not the immediate focus of attention. A particular experimental result that would seem to be at variance with that predicted could well be due to a failure of these "auxiliary hypotheses" to hold, or, as economists

would put it, a breakdown in the *ceteris paribus* condition. This, of course, was already well established in the philosophy of science before Kuhn's work.

But the insight Kuhn brought was more profound than just emphasising this. He argued that it is possible to identify a scientific school of thought, or "paradigm" as he termed it, within which certain laws were made untestable by fiat.<sup>7</sup> A physicist working, say, within the Newtonian paradigm would never accept that any experimental result could refute Newton's laws of motion. If this, perchance, seemed to occur, the scientist would immediately look for experimental error, other faults in the design of the experiment or failure of the auxiliary hypotheses to hold. If he failed in this, the results would, in all probability, be quietly shelved as requiring further work and conveniently forgotten.

This approach has certain well-defined advantages. The paradigm sets the agenda and provides the scientist with problems or "puzzles", as Kuhn termed them. It also provides the tools with which to solve these puzzles while ensuring it will only be the lack of the scientist's ingenuity that will prevent them from being solvable. The paradigm protects the scientist for most of the time from the deeply disturbing problems that can question the whole rationale of his subject area and which may lead to a sense of nihilism – a feeling that nothing can be certain and all science is built on a base of shifting sands.

<sup>7</sup> Kuhn later termed this the "disciplinary matrix" to avoid what was seen as terminological confusion in his book. However, the term "paradigm" is so well ingrained in the literature that I shall persevere with it.

Over time, though, “anomalies” accumulate, becoming more and more difficult to ignore. These build up until there is a scientific crisis. The leading scientists become enmeshed in philosophical discussions and the very foundations of the discipline are no longer taken for granted. The position is only resolved with the emergence of a competing paradigm which can more satisfactorily explain the anomalies and eventually comes to dominate the subject. (Indeed, the existence of an alternative, even though fledgling, paradigm is necessary for precipitating a crisis). The earlier paradigm, although not necessarily refuted, is merely abandoned. However, the decision to change from one paradigm to another is not one made for objective reasons, based on the demonstrable superiority of the one compared with the other. It fact, paradigm choice is more like an act of faith. This is, Kuhn argues, because competing paradigms are *incommensurable*; concepts in one paradigm may have no counterpart in the other or the interpretation of theoretical constructs may differ crucially between paradigms. He argues, for example, that the concepts of Newtonian and Einsteinian mass are incommensurable and have different meanings within each of the paradigms.

This means that theory choice results from what may be best described as a *gestalt* switch; scientists suddenly see the new paradigm as providing a more fruitful structure within which to work. Some old ideas are given new meaning and some are abandoned as now being meaningless. The paradigm also shapes the way the evidence is interpreted.

The reason why this Kuhnian view of science proved so controversial was that it was interpreted as denying the possibility of making a rational choice between paradigms. Many philosophers of science at the time found this profoundly disconcerting. The term “rational”, however, is used in the literature in a rather narrow context as being synonymous with “objective”. In other words, a rational decision implies that any two scientists given the same evidence will, by the canons of logic, arrive at the same decision as to which theory is to be preferred. The corollary, that Kuhnian paradigm choice is irrational, is rather unfortunate because of the pejorative nature of the term “irrational” in its normal usage. As Kuhn points out, by these lights the whole of philosophy is irrational, but that certainly does not mean that any philosophical discourse necessarily lacks reason.

Kuhn argues that the rules of the paradigm, the metaphysical or untestable elements, are not explicitly laid down. There is no overt set of procedures to determine what sort of puzzles the scientist should study and how he should proceed. Rather, the methodology is acquired by demonstration and ostentation, through worked examples and solved problems. It is inculcated as the scientist undergoes his apprenticeship as both an undergraduate and a postgraduate student.

As I noted above, neoclassical economics may be considered to be the dominant economic paradigm, at least in the Anglo-Saxon countries. Like the elephant, it is difficult to describe simply,

but is instantly recognisable if encountered. Any argument based on this paradigm will be readily apparent as being neoclassical through the shared concepts and underlying assumptions. For example, the notion of maximising  $U = U(x, y)$  subject to  $M = p_x X + p_y Y$  or a profit function subject to the relationship  $Q = F(L; K; t)$  will be second nature to anybody having taken a course in Principles of Economics. It is not even necessary to define the variables. The marginal concept, the notion of opportunity cost, equilibrium and disequilibrium (from the physics analogy, mentioned earlier) are all paradigmatic assumptions that may literally have no meaning in heterodox economic paradigms. The paradigm, for example, removes the problem that the concept of utility may be inherently tautological and thus unscientific, as Joan Robinson has argued.<sup>8</sup> To take another example, it also obviates the necessity of continually justifying the use of  $K$ , the symbol for a unique index of the capital stock which the “Cambridge Controversies” and the possibility of “reswitching” has proved to be untenable (or not, depending on your paradigm). (See Harcourt, 1972, 1994)

This paradigm provides the neoclassical economist with his puzzles and his method. The approach is individualistic or one of *methodological individualism*. It

considers individuals (“agents”) divorced from their social or institutional setting. Firms are not the complex bureaucratic businesses that dominate modern capitalism but are “actors” with a well-defined objective function: that of profit maximization or occasionally sales, or growth, maximization.

The approach is highly abstract, often based on formal deductive mathematical modelling with deterministic solutions seen as almost a necessary requirement.<sup>9</sup> As such, it ignores the actual institutions of the advanced capitalist economies. Neoclassical classical economists see this as its strength – it is a *general* theory of choice under certain constraints and the allocation of scarce resources. Its critics, at the same time, see this, together with the ahistorical nature of its approach, as its great limitation.

Typical neoclassical analyses often begin with such statements as “Let us assume that society consists of individuals with identical preferences of the form  $U = U(\dots)$ ” and “Let us assume that there is perfect competition with factors of production being paid their marginal products ...”. The paradigm reassures the economist that whatever comments referees make about a paper submitted for publication, they won’t be, for example, “Tell us how you propose to determine empirically what is the representative individ-

<sup>8</sup> “Utility is a metaphysical concept of impregnable circularity; utility is the quality in commodities that makes individuals want to buy them, and the fact that individuals want to buy the commodities shows they have *utility*”. (Joan Robinson, 1962)

<sup>9</sup> Mathematics is used to a much greater extent in economics than in any other of the social sciences. Mathematics has the great advantage of making any argument rigorous; but at the same time it poses limitations on the way a problem is to be examined because very often the mathematics become intractable for all but the most simple representation. It is, of course, of little use for the analysis of qualitative change. We may regard the predominant use of the calculus as a paradigmatic heuristic.

ual's utility function?"; "How do you justify the assumption of perfect competition when the top hundred companies produce half the total output and oligopolistic pricing is the rule, not the exception?". In many cases, a display of mathematical virtuosity is paramount as a criterion for publication.

A criticism often made of neoclassical economics is that its assumptions are so at variance with reality that it renders the model vacuous for discussing actual economies. This, the paradigm allows the economist to dismiss as being misplaced concreteness or merely to ignore it altogether. It will not be a criticism other professional economists brought up in the neoclassical tradition will (mistakenly) make. If the economist is forced to consider it as a legitimate critique, the defence will normally be along the lines of Friedman's (1953) famous essay "The Methodology of Positive Economics", which is probably the most widely read work on economic methodology.

Friedman puts forward what is known as the symmetry thesis. This is that prediction and explanation are the different sides of the same coin. He further extends the argument so that the realism or otherwise of the assumptions does not matter; all that does matter is the predictive ability of the model. This has generated what seems to be a never-ending controversy in economic methodology, but for our purposes here it is only necessary to make a couple of points. The first is the rather obvious one that a model must, of course, abstract from reality. As Joan Robinson once pointed out, a map on the scale of one to one is of no

use whatsoever to anybody. This, though, is not to say that while no assumptions can be completely realistic degree of realism is irrelevant. This is because two completely different models, while generating the same set of predictions, may have widely different implications. For example, the assumptions of perfect competition and oligopoly with price leadership will both predict that there will be an insignificant variation in prices between firms. Moreover, given the lack of controlled experiments the problems incurred by "auxiliary hypothesis" are likely to be even greater in the social sciences.

It was at one time hoped that the rapid postwar development of econometrics would resolve once and for all which were the most productive theories. But the work of Kuhn should have made us sceptical of such expectations. Solow (1983), in discussing the role of econometrics in macroeconomics, has neatly encapsulated the situation when he wrote:

I think we suffer from econometric illusion. We overestimate the accuracy and reliability of our models. Too few econometric routines can respond to a question with "Don't ask". If you are honest you will agree that, although you are fundamentally right and monetarists and new-classical theorists are wrong, they can always find sample periods, data sets, econometric specifications, and time series methods according to which they are at least as good as you (we) are. This is again a piece of unavoidable bad luck – the collinearity of the world, the shortness of stationary time series, the inapplicability of the experimental method. But the result is Babel, or even babble.

Even a cursory examination of any of the leading journals will show that many

models are, in fact, never intended for direct empirical testing. Hahn (1973), for example, has argued that General Equilibrium Theory was not designed as an explanation of how economies actually work. Rather the theory was constructed rigorously to show the assumptions that would be necessary for certain economic statements to be true. For example, it is sometimes argued that there is no need to worry about the exhaustion of finite resources. The price mechanism will ensure that the rate of depletion is optimal. As a resource becomes relatively scarce, its price will rise and this will encourage the search for and the development of alternative substitutes. General Equilibrium Theory shows that a necessary assumption for this to be true is that there are futures markets for every possible contingency and this is clearly not satisfied in practice. The orthodox theory of international trade may be regarded in a similar light. It highlights the necessary assumptions which must be met if free trade is to be optimal and which many critics find totally unconvincing. But equally, it has become the rationale for the call for free trade; after all, as any introductory textbook shows, no country can lose from free trade – indeed all will be made better off, if not to the same degree.<sup>10</sup> The neoclassical economist thus has a hard time in understanding why all developing countries do not immediately embrace free trade.

We have noted above that Kuhn argues that paradigms are incommensurable in certain respects. Certainly, this is true if we consider the neoclassical and Marxist paradigms. The latter is a holistic approach and the concepts of social class and class interest are paramount in any explanation of the functioning of the capitalist economic system. There is no reference to such concepts in the neoclassical analysis since they have no analytical meaning in a system based on methodological individualism. Likewise, the concept of utility is vacuous in the Marxian approach.

Nevertheless, incommensurability is perhaps rather too rigid a demarcation criterion. A characteristic of the paradigm is, as we mentioned earlier, that certain assumptions are made untestable, either directly or indirectly, by fiat. It is thus possible to identify differing paradigms that are *not* incommensurable in the sense that there are concepts in one approach that literally have no meaning in the other. Rather, the incompatibility arises in what is deemed to be untestable. An example in macroeconomics is whether more meaningful insights are obtained into the determination of the level of economic activity by assuming the price auction model or the fixprice model. In other words, which is more useful – the New Classical Economics or *The General theory*?

<sup>10</sup> See Joan Robinson (1962, p. 61-65) for a discussion of how many of the arguments suggesting free trade may not be optimal, were “simply lost to view” in the early development of the free trade argument. While these arguments are acknowledged today, they are normally relegated to footnotes as unimportant.

## LAKATOS'S METHODOLOGY OF SCIENTIFIC RESEARCH PROGRAMME

The implications of the Kuhnian approach were an anathema to many Popperians, because by arguing that there were no objective canons for theory choice, it opened the gates for relativism.

Lakatos (1970), a disciple of Popper, set out to rescue the logical empiricist position, but ironically, in doing so, confirmed many of the insights of Kuhn. Like Kuhn, Lakatos accepted the argument, that may be traced back to Dunham, that it is appropriate to assess only a series of theories rather than a solitary theory. Theories are not treated in isolation but are linked within the framework of a *scientific research programme* [SRP]. As with the paradigm, the SRP directs the attention towards those problems that are likely to be resolved by techniques provided by the SRP (the “puzzles” of Kuhn). The SRP, according to Lakatos, thus “outlines the construction of a belt of auxiliary hypotheses, foresees anomalies and turns them into examples all according to a preconceived plan”. Not only does the SRP act in this way as a *positive heuristic* but it also has a prohibitive role in steering the individual away from a consideration of problems the SRP is not capable of answering, the *negative heuristic*. Each SRP contains a *hard core* which, by methodological agreement, is deemed irrefutable. (Note that here conventionalism comes in – by what “objective” criterion are we to determine what is to be

irrefutable?). It consists of statements that are in principle refutable, and which actually may be, but which are considered by fiat not to be falsified. It is here that Lakatos's agreement with Kuhn is at its closest. A single counter-instance or discovery of an anomaly is not sufficient to lead to the abandonment of the programme. “There is no falsification before the emergence of a better theory. Only with the benefit of subsequent theory and hindsight can an experiment be seen as refuting the former theory” (Lakatos, 1970). Since historiography has shown that in spite of numerous anomalies such systems as Newton's three laws were not abandoned, Lakatos asks “why aim at falsification at any price?”. “The Popperian notion of ‘conjectures and refutations’: that is the pattern of trial by hypothesis followed by error shown by experiment is to be abandoned: no experiment is crucial at the time – let alone before it is (except, possibly, psychologically)”. One might then legitimately ask, how then does the SRP differ from the Kuhnian concept of the paradigm and how can it be regarded in any sense as Popperian?<sup>11</sup> The reason is that Lakatos proposes what he sees as meta-scientific rule for theory choice. SRPs are dichotomised into those that are progressive and those that are degenerating. In the latter, in contradistinction to the former, no new facts are discovered and theories are defended by *ad hoc* modifications to the auxiliary hypotheses.

An example may help here, although

<sup>11</sup> It is ironical that Kuhn (1970a) writes “I have read of no scientific method that expresses opinions so closely paralleling my own and I am necessarily encouraged by the discovery, for it may mean that in the future I shall not be quite alone in the methodological area as I have been in the past”.

this is possibly not quite what Lakatos had in mind. The modifications to the auxiliary hypotheses may be likened to the economists' use of the *ceteris paribus* condition. Suppose we have a regression equation that when estimated gives an unexpected or statistically insignificant regression coefficient. One defensive strategy is to argue that there is a missing variable leading to bias in the estimate which would have been significant (or of the expected sign) if this variable had been included. It could be argued that adequate data on this variable are not readily available. One can rescue any hypothesis along these lines and repeated use of this strategy will lead to a degenerating SRP. A progressive problem shift occurs when a (usually new) programme  $P_1$ , which supersedes a (usually) old programme  $P_0$ , explains some novel fact in addition to accounting for all that  $P_0$  did, so we can legitimately dismiss any attempt to retain  $P_0$  in preference to  $P_1$  as irrational – or can we? Lakatos himself concedes that this principle cannot be used contemporaneously since at any time a degenerating SRP may become a progressive one; we cannot ever know what is around the corner. No time limit is attached to how long it is rational for scientists to maintain working within a degenerating SRP. This is because the purpose of this methodology is to assist in what Lakatos terms the *a posteriori* “rational reconstruction” of theory change. It is not intended as a method to determine which programme to choose. Lakatos's SRP proved an attractive framework for a number of economic methodologists (see, for example, Blaug, 1992,

and the collection of essays edited by Latsis, 1976).

However, this attempt to rescue Popper founders on two rocks. The first is the notion of incommensurability. (See Feyerabend's, 1975, Chapter 16 for a demolition job on these lines). In many cases as one moves from one paradigm (or SRP) to another, questions or concepts in one literally have no meaning in the other. It would be extreme to argue that paradigms or SRP are always completely incommensurable (which is sometimes argued in an attempt to discredit the notion). But suppose that there is some commensurability. The probability of discovering novel facts or advancing the subject (however we wish to define this) may well be a function of the amount of resources devoted to this approach and the number of scientists engaged on research in this area. Thus, on the one hand, a SRP may appear to be progressive as numerous little “puzzles” (deliberately mixing Lakatosian and Kuhnian terms) are solved. On the other, it is possible that a putative “degenerating” programme, with just a few extra scientists researching on it, would have led to a major breakthrough that would have changed the path of science.

But resources and manpower are scarce: so how does one choose which projects to fund? For Lakatos, editors of scientific journals and research foundations should be the final arbiters by their refusal to publish papers and provide research grants respectively. This is in fact what happens in practice and may be, *faute de mieux*, in the sciences the most effective (only?) way for science to pro-

ceed. But since there are no objective principles (independent of anyone's whim) to help editors make their choice, the implications of Lakatos's and Kuhn's approaches have become conflated. However open-minded editors attempt to be, they are products of the prevailing paradigm, as will be the two (or three) referees to which papers are sent. In economics, the problem is even more precarious because, unlike in the natural sciences, the process of testing theories is qualitatively different and far less likely to be conclusive. (Although in the natural sciences, anomalies can be legitimately ignored, there is no doubt that if the cold fusion experiments of Fleischmann and Pons could have been replicated, this would have led to a major reassessment of the theory behind hot and cold fusion. According to standard nuclear physics, the neutrons produced should have been more than enough to kill both experimenters (See Pinch, 1995).<sup>12</sup> Economics editors are trapped, to a large extent, in their own paradigm. There is no doubt that the dominant paradigm is that of neoclassical economics, which is intolerant of what may be loosely termed heterodox economics. This may be seen by the necessity for heterodox economists to set up their own journals (such as the **Journal of Post Keynesian Economics** or the **Cambridge Journal of Economics**).<sup>13</sup>

If economic methodology cannot be prescriptive, then one view is that its appropriate scope is to be descriptive; to analyse what actually makes economists prefer one paradigm to another. McCloskey (1986, 1994) has been instrumental in introducing the techniques of literary criticism, especially rhetoric, as an aid to understanding what persuades economists to adopt one approach and write off another. This development in the methodology has proved controversial and there is not the space to discuss it here, but suffice it to say that it is nearer the "psychology of research" than the "Logic of discovery". (Kuhn, 1970b)

## CONCLUSIONS

This paper has provided an introduction to the problems of assessing economic theories and a reason why conflicting schools of economic thought have persisted for so long. Lakatos, in his response to Kuhn, has not provided a methodological rule by which we can judge theories, except *a posteriori*, and even then there are difficulties with his approach. There may, in practice, be no alternative to leaving this to peer review. (This increases the importance of the sociology of knowledge approach to understanding the forces driving the direction of research in economics). But there are

<sup>12</sup> Fleischmann and Pons were chemists and in 1989 were claiming to have achieved in a test tube what the physicists had failed to achieve by spending literally billions of dollars. It was not just a clash of paradigms, but of two major natural science disciplines.

<sup>13</sup> The major annual economics conference in the United Kingdom is organised by the Royal Economics Society. The state of affairs is now that heterodox economics is not included in the conference. Consequently, heterodox economists have felt it necessary to set up a rival conference that is deliberately being held at the same time as that of the Royal Economics Society, but at a different venue.

two inherent dangers in this. Once a paradigm becomes dominant, other paradigms are starved of research funds; researchers working in the area are not appointed to university posts or promotion prospects are seriously blighted. I have argued above that the extent of progress of a paradigm, especially in the social sciences, will be a function of the number of researchers in the area and research funds available to them. There is the danger of an unfashionable paradigm becoming trapped in a vicious circle of little apparent progress leading to researchers abandoning the paradigm purely because of peer group pressure. While Kuhn

(1962) concedes that there may be no objective rules for theory choice, at least he believes that there is progress in science in the sense that if one were to take two theories, it is possible to tell which was the earlier one. The ultimate safeguard in the natural sciences is their experimental basis. This is absent in the social sciences with the nearest economics getting to it, namely econometric testing, providing no substitute for the controlled experiment. This brings us back to the question with which I started: is there progress in economics, and if so, how can we be certain?

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