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Empiricist Methodology and the Development of Economic Thought

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ON EMPIRICIST METHODOLOGY AND THE DEVELOPMENT OF ECONOMIC THOUGHT

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1. Methodology and Scientific Progress

(a) Paradigms and Revolutions

Kuhn's thesis goes something like this: The modern conventional picture of the scientific process – hypothesis, analysis leading to prediction, testing (falsification) leading back to adjusted hypothesis – does not square with observation of what scientists actually do. The supposed permanent revolutionary character of science is thus a myth. Most of the time scientists are in reality merely solving 'puzzles' that are thrown up by the prevailing 'paradigm'. The motive force behind scientific activity is not the testing of the paradigm but the desire to solve puzzles successfully. This process is called 'normal science'. From time to time, however, the number of outstanding unsolved puzzles – 'anomalies' – may begin to proliferate, at which stage that discipline is in a state of crisis. Then sets in a period of extra-normal activity which leads either to the solution of the troubling anomalies or to a scientific revolution, in which the old paradigm is overthrown and a new paradigm is accepted by all members of the profession.

That is the thesis in a nutshell, and it is well enough known to economists not to need a full elaboration. Here we consider just two of the issues that have subsequently been raised. These are, first, the definition and description of science, and, second, the question of the 'rationality' of scientific progress.

On the first issue, the debate concerns the line of demarcation between science and other pursuits. Essentially Kuhn's thesis is a critique of the Popperian school which maintains that a scientific theory has to be testable and, in particular, falsifiable. Thus any statement which cannot be falsified is necessarily non-scientific. Science consists of making provisional hypotheses and attempting to falsify them. Against this, Kuhn maintains that only in times of revolutionary science are genuine attempts at falsification made. During periods of normal science the characteristic feature is puzzle solving attempts. Kuhn's line of demarcation is therefore between those pursuits that are, and those that are not, puzzle solving attempts. "Finally", he says, "and this is for now my main point, a careful look at the scientific enterprise suggests that it is normal science, in which Sir Karl's sort of testing does not occur, rather than extraordinary science which most nearly distinguishes science from other enterprises. If a demarcation criterion exists (and we must not, I think, seek a decisive one), it may lie in that part of science which Sir Karl ignores."[2] Popper's reply to this is to accept that there may be something in the distinction between normal and revolutionary science but the difference is not nearly so sharp. Moreover the normal scientist is something to be deplored; it is even dangerous.

However, it is not nearly so widespread as Kuhn imagines.

The main brunt of Popper's critique of Kuhn concerns the second issue: the 'rationality' or otherwise of scientific progress. According to Popper, Kuhn has made the philosophical mistake of slipping into a pure relativism – 'the myth of the framework'; according to this, scientific activity requires a common framework and language, that is, a paradigm. The myth is that rational discussion and criticism is only possible once fundamentals have been agreed. The framework itself is beyond criticism. Popper maintains therefore that Kuhn's thesis is a logical one and hence is wrong because it makes the philosophical mistake of relativism. We may note that Popper clearly misreads Kuhn here, as it is not possible to ignore the sociological and historical aspects of Kuhn's thesis which indeed Popper calls on in the course of his own argument.[4]

(b) **Research Programme**

We return to this issue after considering Lakatos' major contributions to the debate to be found in his (1970) and his (1971). The first one might be described as an empiricist's tour-de-force in that the reader is taken through centuries of philosophical thought right up to the position of Popper, and beyond that to his own. We are thus able to see Lakatos' 'Methodology of Scientific Research Programmes' (MSRP) in the context of the major schools of empiricist epistemology, and doubtless Lakatos wishes us to see MSRP as the intellectual culmination of these previous epistemologies; indeed, half of his (1971) is an attempt to show that, *according to his methodology,* MSRP is indeed superior to, i.e. progressive with respect to, the previous methodologies.

Once it was thought that all knowledge, if there was such a thing, had to be proven knowledge. This school can be labelled 'Justificationism'; it includes classical intellectualists, who accepted a priori knowledge, classical empiricists who used the principle of induction and even sceptics, who, not accepting that latter principle, held that there is no proven knowledge and hence no knowledge at all. A watered-down version of Justificationism is Probabilism, whereby it was held that scientific theories need only be probably true.

The more recent position which Lakatos labels 'naive falsificationism' held that science progressed by repeated overthrow of theories, using 'hard facts', and most successfully in the context of crucial experiments. Leaving out the crucial experiments proviso, we venture to suggest that something like this manner of procedure is in the minds of those adherents of positive economics methodology who have not yet thought the problems through. Yet Popper is adamant that this is not his position and Lakatos reminds us of three

reasons why this position will not do.

First, there can be no such thing as, for instance, a mind, empty of theoretical content, ready to perceive an empirical proposition. Second, factual propositions are fallible but are not provable or disprovable except in relation to other propositions. Third, no observation can ever be forbidden by a theory. This is because it is *always* possible to provide *ad hoc* remedies to unexpected results.

Lakatos attempts to remove these three obstacles in his own epistemology. We can move one step closer to his position by considering the 'Methodological Falsificationist' viewpoint. Essential to this is that it recognises the conventional element in knowledge; any falsifying that is going on has to be carried out against the background of 'unproblematic background knowledge', which is roughly to be regarded as an extension of the senses. Thus theories are to be *rejected* but not *disproved*, and a theory is scientific if it has an 'empirical basis'. In fact the propositions of naive falsificationism are saved by this acceptance of conventionalism, and this is expressed by writing all the key words inside inverted commas – to remind one always of the presence of conventional decisions regarding what is and what is not unproblematic knowledge.

It is naive, however, to think that science progresses even according to this pattern. The history of science does not bear it out. Remarkable is the observation that no theory is ever rejected without another theory being available; i.e., fights are three-cornered, not two-cornered between only a single theory and nature. Instead therefore of examining individual theories and debating the question of rational methodology with respect to that alone, it becomes necessary to consider series of theories. Suppose a new fact or 'anomaly' appears, not explained by the existing theory, T. We can now *always* adapt T to explain this new fact, thus creating a new theory, T'. Does T' predict any more new facts? If so, there is a *'theoretically* progressive problem-shift'. Is the new prediction empirically corroborated? If so, there is an *'empirically* progressive problem-shift'. If T' is both theoretically and empirically progressive it is termed simply 'progressive'; if not it is called 'degenerate'. This device amounts to a down-grading of ad hoc theories; and the reason for that is, according to Lakatos, rooted in the history of science – i.e., ad hoc explanations have a tendency in the long run at least, to be discarded by scientists.

This last comment brings us at last to Lakatos' overall position which he calls the Methodology of Scientific Research Programmes (MSRP). For at this point he abandons Popper's apparent position whereby what scientists *ought* to do is considered independently of *what they actually do.* Lakatos gives us a picture of science progressing within the framework of scientific research programmes. Each programme has, first, a 'hard core', or 'negative heuristic', consisting of certain unquestioned hypotheses (e.g., Newton's three laws of motion); and, second a 'positive heuristic', consisting of a research policy whereby a protective belt of auxiliary hypotheses is drawn up around the original hard core. Scientists' action is influenced positively by the programme, but is not affected by the presence of many anomalies at first. Turning now to our definitions of progressive and degenerate problem-shifts, these can be applied to the whole research programme; thus any programme may at any time pass through progressive or degenerate phases. If successive anomalies are explained away by ad hoc additions to the protective belt (as, e.g., in pre-Copernican astronomy, by the addition of one more epicycle) then the programme is degenerate. If on the other hand more possibilities are opened up with each new version of the programme, then it is progressive. Now comes the crunch hypothesis: namely, that scientists tend to switch from degenerate to progressive research programmes. With this crucial hypothesis we are into the realm of history and largely out of epistemology. Lakatos thus claims to provide a framework for the 'rational reconstruction of history'. Let us complete the argument, which is expounded in his (1971): namely that it should be possible to write 'internal history', using MSRP, which would be prior to 'external history'. Writing internal history consists of isolating research programmes, interpreting them as either progressive or degenerate and showing how the former are, generally, adopted. External history — which could include anything else, e.g., sociology, psychology, etc. — is there to explain the exceptions and is confinable to footnotes.

Before appraising Lakatos' MSRP alongside Kuhn's paradigms we may make two comments. First, the claim that MSRP's reconstruction of history is 'rational' must be treated with care. For MSRP has conventionalism imbued in it right at the heart, in the concept of the hard core (which is, by *convention,* unquestioned). Thus, the rejection of a degenerate in favour of a progressive research programme is not rational in the sense that it is in accordance with universally accepted axioms of logic. Rather, 'rational' is to be understood as deriving from the concept of MSRP. Let us therefore label as 'rational' a reconstruction of the history of a science which conforms to MSRP; while at the same time we reserve judgment as to whether. the normative criteria of MSRP amount to a correct solution of the problems of empiricist epistemology. Second, Lakatos distinguishes between a pre-diction and a post-diction of history. MSRP claims to provide the latter, not the former.[5] Yet this view must be qualified. If the hypothesis is that progressive research programmes prosper, then if we can identify which research programmes are currently progressive, we can predict that these will develop (though we cannot specify how). Post-diction is easier than

pre-diction only to the extent that it is easier to identify progressiveness from the vantage point of history.

(c) Unconscious Bedfellows?

For all the apparent difference between Kuhn's Structure of Scientific Revolutions and Lakatos' MSRP, for practical purposes the two are much more alike than at least Lakatos cared to admit. Indeed, as Kuhn points out: ". . . . hard core, work in the protective belt, and degenerate phase are close parallels for my paradigms, normal science, and crisis". The hard core, i.e., the negative heuristic, consists of precisely those propositions which scientists do not and should not criticise whilst engaged in a progressive programme. Precisely the same function is served by Kuhn's paradigm, though some confusion has arisen as to precisely what a paradigm is; (e.g., is it a metaphysical world-view; or, more concretely, something like an experiment, or a piece of apparatus, or even some well-known scientific achievement?)[6] Work in the protective belt is generated by the positive heuristic; while puzzles are generated by the paradigm. (Hence paradigm serves both the negative and the positive function.) Finally a crisis is characterised by a proliferation of anomalies. Anomalies can always be accounted for on an ad hoc basis, and hence this is equivalent to the degenerate phase of a Lakatosian research programme.

The one important difference is that Lakatos allows the coexistence of alternative research programmes whereas Kuhn stresses the uniqueness of paradigms (in the mature stage of a science). According to Feyerabend,[7] Kuhn is correct to separate the two principles of 'tenacity' and of 'theory proliferation', but incorrect to separate them in time. Thus the principles do, and should, co-exist. This means that two progressive research programmes could presumably develop side by side until one became degenerate and hence in the long-run died out.

But now we come to the most significant issue which ostensibly divides the two systems but in fact does not: the question of rational theory choice. We have mentioned above how Lakatos' use of the word 'rational' is misleading and how it must therefore be treated with care; and we venture to suggest that Kuhn's work was stimulated intellectually by a rejection of the supposed rationality of science which Popper had been proposing. Rationality, as defined by naive falsificationism's demarcation criterion between science and non-science, was rejected by Kuhn, and hence he devised his own demarcation criterion. This involved the concept of 'normal' science — a puzzle-solving activity — which conveniently defined as science most of those things generally regarded as such. (Feyerabend, however, has pointed to the danger of this simple criterion, namely that it would include as 'science' such activities as, for example, organised crime; which should be ruled out by an expansion of the demarcation criterion to include the aims of science.)

Kuhn's rejection of the 'rationality' of falsificationism is of course anathema to Popperians, who are concerned because out of the window with 'rationality' goes the supposed claim to objectivity. But what Lakatos has done is *to reach the same substantive conclusion as Kuhn without admitting it.* First of all, as noted above, part (b), Lakatos' rationality concept is defined with respect to his MSRP, and this implies the *unquestioned* acceptance of the basic propositions of the hard core. Second, more importantly, there is no way of telling from Lakatos' criteria how long it is rational to keep going with a degenerate programme. Feyerabend makes the point as follows: the most important stage at which the conventional element enters is in deciding the time limit for adherence to a degenerate programme; i.e., when should the principle of tenacity give way to the principle of proliferation. "In these circumstances one can do one of the following two things. One can stop appealing to permanent standards which remain in force throughout history and govern every single period of scientific development and every transition from one period to another. Or one can retain such standards as a *verbal ornament,* as a memorial to happier times when it was still thought possible to run a complex and often catastrophic business like science by following a few simple and rational rules. It seems that Lakatos wants to choose the second alternative."[8]

It is to be noted finally, though, that while Lakatos (falsely, in our view) claims superiority for his MSRP on grounds of rationality, Kuhn has also strenuously rejected charges of 'irrationality', regarding the label as a 'shibboleth', as an obstacle to understanding. While he sees that theory choice is not a matter simply of logical proof, he nevertheless believes that theory development has direction, invoking the analogy of an evolutionary tree of knowledge. This is in contrast to the attitude of Feyerabend who is prepared even to make a virtue out of irrationality in science, and who has more recently developed his position to its conclusion which has been rightly termed an 'epistemological anarchism'.[9,10] But this particular development in empiricist philosophy is not the object of concern in this paper.

And this is therefore an appropriate point to stop and to take stock of what the philosophical developments so far discussed provide for an analysis of a science. In summary, both MSRP and the paradigm concept of Kuhn emphasise the unity of groups of theories and that developments in a science must be analysed with this unity in mind. We have argued that the two methodologies are *substantively* the same. They do differ in matters of emphasis and detail. Thus

MSRP might be described as 'richer' in that it specifies separately the positive and negative parts of a research programme, while with Kuhn's method both are contained within the 'paradigm' concept. Moreover, 'paradigm' has suffered from loose usage. On the other hand, Kuhn's methodology gives more recognition to the sociological aspects of science. Whether we use the language of paradigms or of research programmes depends therefore only on the emphasis we are giving in a particular context. What light can be shed on the history of economics — which is generally claimed to be a scientific discipline — by this work of the philosophers?

2. The Development of Economic Thought

Conveniently, those who have applied the Kuhnian demarcation criterion to Economics have concluded that, since it is normally a puzzle-solving activity but is also subject from time to time to revolutions it *is* a science.[11]

Their conclusion as to the scientificity of this subject is therefore no different from the viewpoint of 'positive' economics. At the same time the various contributions are notable for the diversity of interpretations of the history of economics which are squeezed into the paradigm framework, testimony in part to the imprecise usage of the 'paradigm' concept in Kuhn's own writings.[12] Gordon (1965) was the first to comment, seeing no revolutions since the eighteenth century, with the subject being dominated by the paradigm of the maximising individual. Subsequently Coats (1969) has pointed to the Keynesian revolution, and also the marginalist revolution[13] of the late nineteenth century, as possible candidates. Other candidates have included the 'formalist' revolution (referring to the growth, post-war, of mathematical economics); and a laissez-faire revolution (see, e.g., Bronfenbrenner (1971)); and even the revival of 'radical political economy' has been (certainly mistakenly) seen as a Kuhnian paradigm switch (Worland, 1972).

A second point common to these propositions is that they are all speculative, though this is not meant in a derogatory sense; given the imprecision and inadequacy of the concepts the analysis can not be sufficiently elaborated for any of the various interpretations to carry conviction. This fact has clearly been recognised in a recent paper by Blaug (1975), who therefore turns instead to Lakatos' MSRP for a 'richer' analysis of the one event which most of the contributors agreed to accept as a Kuhnian 'paradigm' change; the Keynesian revolution.

We have already attempted in the previous section to minimise the differences between the Kuhn and the Lakatos approach. If we are right we should expect to find the same similarities in their applications. This section is therefore a critique also of Blaug (who takes the opposite view: that the extra richness of Lakatos' picture does really constitute a different analytical framework); if this appears too strong for a paper which was clearly meant to be suggestive and not definitive, it is nonetheless pertinent to indulge in what might be called a suggestive critique.

(a) In the Kuhnian version, the Keynesian revolution uprooted the paradigm best summarised by the phrase 'neoclassical economics', (of which one concrete form was the Marshallian cross, with all the puzzles created and solved by it; this is, of course, a simplification.) Three anomalies are referred to: the failure to integrate money and value theory, the problem of taking account of monetary factors in capital theory and in particular the theory of investment and the awkward position of business cycle theories which purported to explain phenomena that according to neoclassical theory did not or ought not to exist. These unsolved puzzles of the 1920s together with the mass unemployment of the period were sufficient to bring about the change. The agent of change was Keynes himself, who, in his General Theory (and, more particularly, through the IS – LM interpretation of it), gave the profession a new paradigm that, among other things, supported public deficit spending as an antidote to depression, as well as a whole lot of unsolved puzzles, meat for a generation of budding economists. The new position has been summarised by Moggridge,[14] although he steers clear of the word 'paradigm': "(The General Theory) . . . was an attempt to change the previous economics of tranquillity, or of confident foresight, into an economics of uncertainty. In the attempt, Keynes left economists with a series of concepts, which, although they often had precursors in the earlier literature, changed the way in which economists looked at the world, whether they agreed with Keynes or not."

(b) Blaug has criticised this picture on account of the sociological trappings that are associated with a revolutionary change: the dumbfoundedness of the rest of the profession and, in this case, consequent refusal to entertain public spending policies. This, he says, was evidently not the case, and hence the language and concepts of Kuhn should be replaced by those of Lakatos. Thus the Keynesian revolution should be seen instead as the replacement of a degenerating research programme with a progressive one.[15] The 'hard core' of the neoclassical research programme is the usual assumptions of competitive theory, while the " 'positive heuristic' . . . consists of such practical advice as (1) divide markets into buyers and sellers, or producers and consumers; (2) specify the market structure; (3) create 'ideal type' definitions of the behavioural assumptions so as to get sharp results; (4) set out the relevant ceteris paribus conditions; (5) translate the situation into an extremum problem and examine first (and second) order conditions; et cetera". The Keynesian research programme introduced a changed 'hard core' which no longer contained Smith's 'invisible hand' and brought in the importance of uncertainty; it produced also a 'protective belt' of auxiliary hypotheses – "the consumption function, the multiplier,

the concept of autonomous expenditure, and speculative demand for money . . ."

(c) It must be said, however, that what's wrong with the 'paradigm' version of the Keynesian revolution is not, as Blaug claims, that the sociological trappings do not quite fit the picture. Certainly some of these were present as economists did undergo theoretical 'conversions'. And the image of 'dumbfoundedness' prior to the revolution is hardly necessary to the analysis. Rather, the 'paradigm' version fails because it is not possible to produce a set of anomalies which are resolved by the Keynesian paradigm in conformity with the Kuhnian analysis. First, the main anomaly mentioned above – the failure to integrate money and value theory – can hardly be said to have been resolved, in the light of the manner in which most economists subsequently developed the subject – i.e., the IS – LM approach. Second, the other main reason quoted – persistent mass unemployment – may, if we like, be called an anomaly but it is hardly an anomaly which *would not have been noticed if there had been no neoclassical paradigm.* That mass unemployment will have an effect on economic theory is uncontentious, and is consistent with any number of interpretations of the history of the subject.

Moreover, it is not more appropriate to describe the Keynesian revolution in terms of MSRP. Just as it cannot be shown that in a specifically Kuhnian way the post-Keynesian developments did not remove the pre-Keynesian anomalies, so it cannot be shown that the Keynesian research programme was progressive (both theoretically and empirically) whereas the neoclassical programme was degenerate. In particular, it is not clear that the neoclassical programme was any more degenerate in the 1920s than, say, twenty years earlier. More importantly, the only way to explain the resurgence of the neoclassical programme, with the monetarist 'counter-revolution', is to argue that after some decades of degenerating it again became progressive. And such an argument which allows such a time period for degenerating reduces MSRP's prescriptive aspect virtually to the doctrine of 'anything goes'.

(d) Nonetheless, while the concepts of MSRP or of paradigm revolutions do not add to our understanding of the Keynesian revolution, their common emphasis on the unity of a programme of research do help us to understand their staying power. There was clearly something very attractive about the new economics of the General Theory, and while the pre-eminence of Keynes himself explains some of the force of this attraction, it is the 'paradigm' of the IS – LM curves, with its associated consumption function and demand for money function, which, by providing so many puzzles both theoretical and empirical, swung almost the entire resources of the profession in the Keynesian direction. (Here we have used 'paradigm' in the sense of a concrete device for creating puzzles.)

But to us it does not seem to matter a lot whether we call the activity which went on after acceptance of the General Theory the solving of paradigm-induced puzzles or the execution of the 'positive heuristic' of the Keynesian research programme. Both pictures describe aspects of post-war economics, but both fail to give an adequate – and sufficiently differentiated – version of how and why the Keynesian revolution took place.

3. Keynesian Research Programmes

In a recent paper Alan Coddington (1976) has sought to categorise those streams of thought which have their source in the General Theory. The categories he chooses are interesting not only in themselves but also in that the streams are seen as alternative research programmes. These are labelled 'Chapter 12', 'Reductionist' and 'Hydraulic' Keynesianism. Can the concepts of paradigms and puzzles or of MSRP shed any light on the development of these streams? 'Chapter 12' Keynesianism refers to the importance attached to uncertainty and the consequent instability associated with economic flows; its development is attributed primarily to the work of Shackle (see, e.g., his (1972)). 'Reductionist' Keynesianism refers to the work of Clower (1965), Leijonhufvud (1968) and others, which involves explaining economic phenomena in terms of a specified individual choice logic, yet in a disequilibrium context (in the sense that the dual-decision hypothesis implies disequilibrium). 'Hydraulic' Keynesianism is the standard Keynesian analysis of the text books, the assumed theoretical basis for macroeconomic policy making since the war.

In specifying the 'Reductionist' programme, Coddington also specifies the criterion of success – the *'tractability* of the scheme'. 'If the scheme cannot be made to yield definite, unavoidable implications, it is to be regarded as uninteresting, irrespective of the apparent merits of the specification on which the scheme rests.'[16] Programmes such as this should be both prescriptive *and* descriptive. That is to say, whereas Coddington delineated the programme by specifying what researchers ought to do according to its rules, it could also be delineated as a programme which describes what researchers actually do, so we can ask whether this criterion – which could be applied to both the other varieties with equal validity – can explain why it is that the hydraulic variety has been so dominant. For the criterion is clearly similar to the more precise definition of a 'progressive' research programme in MSRP. It seems to me that the answer must be partly in the affirmative, for hydraulic Keynesianism has several parts to it, each of which has at least one device for throwing up puzzles, making implications, suggesting new possibilities for investigation. This is not the whole story, since hydraulic Keynesianism has going for it also the plentiful supply of statistics. By contrast the reductionist method has a more general but more limited (in number) set of devices; its main effort so far has amounted to producing macro results known long before by hydraulic Keynesianism, after much complex argument, in order to

satisfy the reductionist requirements.[17] This testifies not to the inadequacy of the logic employed but to the inability of the framework to generate enough potentially soluble puzzles to compete with the hydraulic variety. This is unfortunate given that the reductionist variety arose as the hydraulic variety was gradually being overhauled by a neoclassical counter-revolution. This process has been traced by Moggridge (1976), who sees – in our view correctly – the beginnings of the demise of the theoretical innovations of Keynes as lying in the fact that "the change in the focus of analysis . . . found itself, from the onset, grafted on to earlier traditions".[18] From that point on, the road to Keynesian economics as the 'special case', is relatively clear. That is to say, it seems inevitable that once Keynesian macroeconomics was 'grafted' on to neoclassical microeconomics, the existence of an unemployment equilibrium could only be due to the special case of rigid wages. But despite the plausible claims to be representing the true Keynes, the reductionist disequilibrium view, shorn of so many devices (including the equilibrium concept) has proved unable to produce enough problems to keep itself functioning. And, moreover, similar remarks may be made concerning Chapter 12 Keynesianism.

4. Criteria for 'Progress' in Economics

We thus have something of a partial explanation as to why certain streams flourish while others do not. We should not overestimate the power of our explanation. For instance, the growing recognition that the hydraulic Keynesian system only produced unemployment under 'special case' assumptions must be seen in the context of the post-war full employment economies which served to re-establish faith in the capitalist system, and hence its accompanying theory, neoclassical economics. Indeed, the twists and turns of post-war economics are no more susceptible to interpretation in any one schema than the development of economics throughout the last two centuries.

In general, though, the unifying aspects involved in the concept of 'normal science' seem not unreasonable to apply in qualified manner to economics. On the other hand, we have rejected the obverse notion of 'extra-normal science', particularly in its supposed application to the Keynesian revolution. This raises the question as to how far it is possible to discern 'progress' in the development of economics, i.e., as economics develops has our economic knowledge increased, in particular in the period since the Keynesian revolution? Let us therefore consider the question of progress in Economics with respect to the methodologies discussed.

With Kuhn, as mentioned above in 1 (c), theory progresses in a certain direction like the growth of an evolutionary 'tree' of knowledge. Progress is expected to come about through scientific revolutions which is the point at which the genuine confrontation with nature occurs. We have argued that revolutions in economic thought – in particular the Keynesian one – do not fit the Kuhnian picture. There is a limited sense in which we can speak of the 'development' of economics as a normal science, while puzzles are being thrown up and solved. But since the treatment of anomalies and crisis in economics does not conform to the Kuhnian framework, we cannot speak of 'progress' in these terms.

According to a 'naive falsificationist' methodology (which is criticised above in section 1 (a)) progress occurs via crucial confrontations of theory with 'hard fact', whereby theories are either rejected or 'not falsified'. Whatever the demerits of this as a prescriptive or descriptive methodology for the natural sciences, the picture it conveys if applied to Economics is so far away from the reality that it clearly will not pass as a reasonable description of the methodology economists have actually adhered to.

According to MSRP, a research programme is progressive if it is generating *new* predictions, *and* is corroborating them empirically with the aid of the empirical basis contained in the hard core. We have already questioned (1 (c)) the usefulness of this definition of progress in so far as it is possible to adhere *rationally* to a degenerate programme for an unspecified, thence indefinite, period. If we ignore this problem temporarily, could we yet argue that economics has developed 'progressively' according to MSRP? Just as with Kuhn's framework, we find the answer no. While of course it is easier to assert a negative in that we do not therefore have to develop a history of economics in MSRP terms, we suggest that there is no way in which the main research programmes of post-war economics – 'hydraulic' Keynesianism and the neoclassical microeconomic paradigm – can be seen as continually throwing up new, empirically corroborated, facts about the economic world we live in. Rather, as Coats has observed,[19] economists have tended instead to place more value on such things as consistency, simplicity, generality, adaptability, than on the specifically empiricist ideals of congruence with reality, and testability. Thus, for example, we observe 'advances' in the analysis of capital which consists of isolating and eliminating errors such as those associated with 'capital reversing'.[20] Our 'knowledge' of capital thus advances in so far as there is improvement in generality and in consistency, but not according to empiricist standards. Another example is the advance of general equilibrium theory. And while these are only two examples we suggest that when economists speak of 'progress' in the subject it is advances of this sort that they have in mind.[21]

Our conclusion then is that the new 'growth of knowledge' theories of Kuhn and Lakatos do not allow us to identify progress in post-war economics. These are perhaps harsh standards by which to judge a subject as ambiguous, political and imprecise as Economics. Nonetheless, in so far as Economics is stated to be an empirically based subject, it is useful to know the extent to which it lives up to its own professed standards.

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