

Appraising Economic Theories

Studies in the Methodology
of Research Programs

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light on the emergence of the new classical macroeconomics? Does the new classical macroeconomics constitute a scientific research program?

Using economics, and more particularly the development of the new classical macroeconomics, as a test case, I shall argue that Lakatos's approach does not capture the essential features of scientific progress. In the process, I shall isolate – following hints provided by Kuhn – a better description of scientific progress. And, finally, I shall apply this new approach to the case of the new classical macroeconomics.

1 A NEW CLASSICAL RESEARCH PROGRAM

As is well known, for Lakatos the unit of description and the unit of appraisal of scientific endeavour is the scientific research program, comprising a hard core, a positive heuristic and a negative heuristic.³

A scientific research program is theoretically progressive if the positive heuristic generates models with increasing content: that is, models that account for the success of earlier models, while adding novel implications. If these implications are *intermittently* confirmed by empirical observation, or even if apparent anomalies can be reinterpreted retrospectively to support the model, the program is empirically progressive. Anomalies are counted against the program only when it fails to progress theoretically and empirically in these senses.

The methodology of scientific research programs suggests several questions about the new classical macroeconomics: is it a distinct program? If so, what is the program? How did it develop? Is it progressing? To answer these questions in the spirit of Lakatos, one must offer a programmatic interpretation.

The hard core of the new classical macroeconomics is encapsulated in the following propositions:⁴

- HC1 The economy consists fundamentally of self-interested individuals (methodological individualism).
- HC2 The economy is characterized by complete interdependence (general equilibrium).
- HC3 Real economic decisions depend only on real magnitudes and not on nominal or monetary magnitudes (no money illusion).⁵
- HC4 The economy is in continuous equilibrium (that is, every agent successfully optimizes up to the limits of his information).
- HC5 Agents' expectations are the same as forecasts generated by the true economic model of the economy (rational expectations).⁶

11. Scientific Research Program or Tribe? A Joint Appraisal of Lakatos and the New Classical Macroeconomics

Kevin D. Hoover¹

Our craving for generality has another main source, our preoccupation with the method of science . . . Instead of 'craving for generality' I could also have said 'the contemptuous attitude towards the particular case'. (Ludwig Wittgenstein, *The Blue Book*)

The new classical macroeconomics, sometimes known as the rational-expectations school or, more accurately, as market-clearing macroeconomics, dominates macroeconomic analysis today. This is not to say that most macroeconomists have enlisted in its ranks; rather its troops occupy the high ground.² Robert Lucas, Thomas Sargent, Robert Barro and its other leaders set the agenda for modern macroeconomics. With rare exceptions, other macroeconomists are either partisans in a guerrilla war against new classicism or collaborators (not committed, but going along, especially on matters of method) or they are raising an opposing army (so far without success) that will emulate the organization and discipline of the new classicals while aiming to achieve opposing ends.

The new classical macroeconomics has been the dominant school for the past decade and has existed in a recognizable form for a decade before that. It is not too early to ask how it is related to alternative approaches, how it achieved its ascendancy and where it is going. When new classicism was on the rise, two views of the history and methodology of science – Thomas Kuhn's (1970) scientific revolutions and Imre Lakatos's (1970) scientific research programs – dominated discussions in philosophical circles. Lakatos's approach largely supplanted Kuhn's and subsequently fell out of favour itself. The fashions of the philosophy of science reach economics only after a long and variable lag: Lakatos retains some currency among economists. My approach will be to ask, should it? Does Lakatos's theory of the history of science shed much

The negative heuristic of the new classical program could be summed up in the simple injunction, 'do not violate any hard core propositions'. The new classicals, however, border on the puritanical in their lust for prohibitions; so it is well to be more specific.

- NH1 Avoid underived supply and demand curves (no ad hoc models).
- NH2 Avoid irrational expectations.
- NH3 Avoid partial equilibrium.
- NH4 Avoid non-neutrality.
- NH5 Avoid disequilibrium.
- NH6 Avoid aggregation.
- NH7 Avoid unidentified reduced forms.

The characteristic practices of the new classical macroeconomics can be summarized in the positive heuristic, which includes propositions something like the following:

- PH1 Construct general equilibrium, dynamic optimization models.
- PH2 Use representative agent models.⁷
- PH3 Test over-identifying restrictions.
- PH4 Test the ability of models to mimic the variances and covariances of the actual economy.⁸
- PH5 Use state-of-the-art mathematical techniques.

2 A TRIBAL VIEW

The new classical program just laid out is typical – neither better nor worse than similar exercises constructed for other areas in economics.⁹ The reader may nonetheless get the impression that it is essentially arbitrary. The propositions convey a real sense of the nature of the new classical macroeconomics, but it is difficult to say that there are not other sets of propositions and injunctions that differ in some points of substance and form that would serve equally well as a description of the new classical program. The difficulty arises, I believe, because the Lakatosian framework implies too sharp a distinction between what is in the program and what is outside it. Its borders are too definite and too well patrolled, its customs post too heavily manned. The plausible program that I have set out does not inspire the necessary conviction because, in some loose sense, *the new classical macroeconomics is a research program*; however, in Lakatos's particular sense, *it is not a 'scientific research program'*. What is more, I doubt that anything is a

scientific research program. To progress beyond vague doubts, we must now consider in greater detail how Lakatos's scientific research programs are too restrictive as descriptions of the scientific endeavour. We shall then consider an alternative description.

The Problem of Individuation

How many research programs are there in economics? Economists give varied answers. Some see each sub-discipline as representing its own research program. Others believe there are but a few competing programs. Remenyi (1979) regards neoclassical economics as the one successful program, alternative programs such as institutionalism and Keynesianism having been driven off the field. Weintraub (1985, ch. 8 and p. 141) adopts a similar position, identifying Walrasian general equilibrium theory as the hard core. Mirowski (1988, ch. 1, esp. pp. 16, 24) likewise finds neoclassical economics to be the dominant (although he is a partisan for a competing program), but he finds the hard core in 'energetics', the foundational metaphor borrowed from physics.

Two points arise from these varied answers. First, there is real disagreement about what programs are currently active and what their nature is. Second, some commentators take a broad view, others a narrow one: neoclassical economics is one big program or a group of specialized programs not in direct competition.¹⁰ Lakatos (1970, p. 132) gives warrant to switching back and forth between the broad and the narrow view: 'Even science as a whole can be regarded as a huge research programme with Popper's supreme heuristic rule: "devise conjectures which have more empirical content than their predecessors".'

Is the new classical macroeconomics a scientific research program, or is it nested in a larger program? Such questions arise with respect to other sub-disciplines with such regularity that Remenyi (1979) has developed a theory of core/demi-core interaction. In Remenyi's scheme, neoclassical economics is a single program in which the various sub-disciplines are nested. Each sub-discipline has its own core, the *demi-core*, which is hard relative to its own protective belt and the other nested programs, although not hard with respect to the hard core of neoclassical economics. Weintraub (1985, ch. 8) uses the core/demi-core model to place general equilibrium analysis in the centre of neoclassical economics while at the same time recognizing the mutual relative autonomy of its sub-disciplines. A glance at the hard core propositions identified for the new classical macroeconomics above is evidence enough that it could be cast as a sub-program of Walrasian

general equilibrium theory with HC1, 2 and 4 overlapping with the Walrasian hard core and HC3 and 5 differentiating the demi-core (HC1-5) from the hard core.

The core/demi-core analysis, however, strikes me as a fairly desperate response to the proliferation of apparently separate programs that nonetheless have common features. It subverts the force of the metaphor of the hard core and the protective belt: the fortress – an impenetrable citadel surrounded by elaborate defences. Instead the protective belts of sub-programs overlap and, although their demi-cores may be directly distinct, they share common elements mediated through the hard core. For the hard core to be truly common it is of necessity denuded of substantial content and is reduced to formal and meta-physical features.

So far we have assumed, as most commentators have, that programs are differentiated by their hard cores. Lakatos (1970, p. 133) warrants this assumption. But Lakatos is not consistent on this point, arguing that different programs may have a common hard core and be differentiated only by their positive heuristics.¹¹

If programs are differentiated by their positive heuristics, the project of Lakatosian interpretation of economics is in even deeper water. Compared with most presentations of the positive heuristics of putative programs, propositions PH1-5 above are precise. Still I have intentionally used the phrase that the positive heuristic is 'something like' PH1-5 in order to capture the almost universal vagueness on the details.¹² Lakatos himself expects that the positive heuristic should not only be partially articulated, but also set out in advance of the development of progressively more complex models. The positive heuristics are not even partially articulated in the detail that Lakatos requires because neither the scientist nor the commentator can see how models will develop. There are, of course, counter-examples. Lakatos is impressed by Newton's theory of gravitation:

Most, if not all, Newtonian 'puzzles', leading to a series of new variants superseding each other, were foreseeable at the time of Newton's first naive model and no doubt Newton and his colleagues did foresee them: Newton must have been fully aware of the blatant falsity of his first variants. Nothing shows the existence of a positive heuristic of a research programme clearer than this fact: this is why one speaks of 'models' in research programmes. (Lakatos, 1970, p. 136)

The closest parallel in the new classical macroeconomics is the sequence of real business cycle models following Kydland and Prescott (1982).¹³ Taken as a separate program or sub-program, the hard core

of the real-business-cycle program is no different from that of other sub-programs in the new classical macroeconomics. And once Kydland and Prescott's model had been subjected to the data, there emerged a fairly clear blueprint of the type of modifications that might make it mimic the data more accurately. The technical difficulties in executing these plans were formidable and have occupied a generation of Minnesota's graduate students.

What is striking, however, is that it is not easy to point to any similar examples of a sub-program within the new classical economics, one which knew exactly where they were going. Such programs or sub-programs exist, but are rare – and not only in economics. Newton-Smith writes:

One cannot discern in the work of Newton, Einstein or others who have launched successful [Scientific Research Programs] anything like even 'partial articulation' of the protective responses that are to be made in the case of anomalies. (Newton-Smith, 1981, p. 84)

If distinct hard cores or positive heuristics do not provide a basis for effectively discriminating between programs, perhaps the negative heuristic does. Is there any element of the program that is truly beyond all thought of change? It is hard to imagine in 1989 any new model being called new classical or being acceptable to new classicals that violated any of the hard core propositions noted above. Still, when one considers history, adherence to the five propositions is not universal among new classical models.

I once wrote that Lucas and Rapping's (1969) essay on the Phillips curve is 'surely the first paper to deserve to be called "new classical"' (Hoover, 1988, p. 27). Yet Lucas and Rapping employed adaptive expectations, violating NH2 and contradicting HC5. Lucas says that, although he knew about rational expectations at the time the paper was written, he had not yet appreciated the far-reaching implications that they had for modelling nor how essential they were to the project of modelling a world of optimizing agents (see Klamer, 1984, p. 38). What makes Lucas and Rapping (1969) a 'new classical' paper is its then startling insistence on modelling the labour market as in continuous equilibrium without money illusion and with regard to the micro-economic behaviour of individual workers (that is, its strong allegiance to HC2-4).

The models that brought the new classical macroeconomics to the forefront in the profession were the policy-ineffectiveness models of Sargent and Wallace (1975, 1976). These models incorporated rational expectations but otherwise were standard IS/LM models – aggregate,

non-optimizing models, violating NH1 and NH6 and contradicting HC1. Sargent and Wallace understood at the time that these models did not conform to the implicit standards of the program.

Sargent and Wallace's (1982) model of the monetary economy advocates a real bills regime in which nominal money balances affect real consumption choices, apparently violating NH4 and contradicting HC3. There is, however, a range of propositions identified as the 'neutrality of money'.¹⁴ Early new classical models had a fairly strong neutrality proposition: namely, anticipated changes in the stock of money have no real effects. This form is violated in Sargent and Wallace's real bills regime and also in the business cycle models advocated by Lucas (1987, p. 83). Still, Lucas (1987, p. 81) says that every serious monetary model must have a neutrality proposition of a weaker form: namely, real allocations are unaffected by currency reforms.

It might be argued that what is happening here is a 'hardening of the hard core'. Lakatos writes: 'The actual hard core of a programme does not actually emerge fully armed like Athene from the head of Zeus. It develops slowly, by a long, preliminary process of trial and error' (Lakatos, 1970, p. 133, fn. 4).

Lakatos unfortunately never gives a thorough discussion of this process. Weintraub (1985, ch. 7) gives a brilliant account of the hardening of the hard core of the Walrasian general equilibrium program. For the Walrasian program, at least, Weintraub believes: 'The propositions of the core remain fixed but the interpretations of the terms of the core change as the core hardens' (Weintraub, 1985, p. 141, emphasis in original). While this situation is a possible one, it is not generally true. It works in the new classical macroeconomics for the neutrality proposition already discussed: HC3 is maintained for all new classical models so far; but the interpretation of neutrality shifts over time until only the weak sense of a currency reform is defended against all-comers. Equally the rational expectations hypothesis itself may be subject to this process. Early interpretations stressed the actual behaviour of people. One interpretation was that rational expectations imply that people do the best they can with the information they have. Another was that rational expectations imply that people actually know (implicitly) the structure of the model which truly describes the world and use it to form their expectations. Lucas (1987, p. 14, fn. 4) calls the first interpretation 'vacuous' and the second 'silly'. Instead he interprets the hypothesis as 'a consistency axiom for economic models, [which] can be given precise meaning only in the context of specific models' (Lucas, 1987, p. 14, fn. 4). The tune changes, but the words remain the same. On the other

hand, one cannot treat the adoption of rational expectations after Lucas and Rapping (1969) or the use and subsequent banishment of the IS/LM framework from new classical models as simply a reinterpretation of a fixed hard core. Rather, the hard core has changed or, perhaps more to the point, has not been fully articulated.

One might be willing to concede that the process of hardening involves substantive change to the hard core, yet believe that once the hard core has set it cannot change further: the hard core is something like epoxy. But perhaps not; perhaps the hard core is more like Elmer's glue, which is hard enough until it gets wet. It is hard to imagine the new classical macroeconomics giving up the rational expectations hypothesis. It is not, however, inconceivable. The inventor of rational expectations, John Muth, has recently worked on 'implicit expectations'.¹⁵ *Implicit expectations* are unbiased expectations in which the prediction error is uncorrelated with the actual realization. In a parallel form, *rational expectations* are unbiased expectations in which the prediction error is uncorrelated with the predictions. Although implicit expectations and rational expectations are mutually exclusive, Lovell (1986, 112-14) points out that both are unbiased and can be defended as the product of rational choice. Lovell also presents evidence of cases in which implicit expectations dominate rational expectations empirically.¹⁶ It is not inconceivable that models which satisfy HC1-4 might nevertheless employ implicit expectations and still be regarded as new classical.

To take another case, the assumption of continuous equilibrium has been interpreted as involving a Walrasian equilibrium of price-takers. One could easily imagine this being extended to an equilibrium of price-setters. This would be an example of reinterpretation of an unchanged proposition of the hard core. The discussion of policy rules in the new classical macroeconomics after Kydland and Prescott (1977) has, however, become dominated by game-theoretic analysis.¹⁷ A principal characteristic of game theory is the proliferation of equilibrium concepts to the point where 'equilibrium' may have no substantive content at all. It is conceivable that new classical macroeconomics may some day so fully incorporate game theory that HC4 is no longer considered essential.

Tribe and Nation

I have so far tried to undermine the plausibility of Lakatos's methodology of scientific research programs: to suggest that it is difficult to identify the hard core of a program conclusively or to maintain that it

is in any sense immutable, that the negative heuristic is after all just a heuristic, not a set of unbreachable injunctions and that the positive heuristic is rarely well-enough defined in practice to be fitted to the theoretical task to which Lakatos sets it. Still I go this far with Lakatos: no program, even a methodological program, falls under its own weight alone; it must be pushed by a more attractive program.

So what is the alternative? Let us recall Kuhn's competing theory of the development of science. Perhaps Kuhn's (1970) most lasting contribution was to elevate 'paradigm' from a relatively obscure grammatical term to a key word in the lexicon of general intellectual discourse.¹⁸

Kuhn was famously imprecise in the first edition of *The Structure of Scientific Revolutions* (1962) on the meaning of 'paradigm'. Masterman (1970) finds 21 senses of paradigm in Kuhn's book. These, she believes, can be encapsulated in three primary meanings. Kuhn concedes Masterman's point, although he sees but two principal meanings:

On the one hand, [paradigm] stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science.

Philosophically, at least, this second sense of 'paradigm' is the deeper of the two . . . (Kuhn, 1970, p. 175)

Most commentators have not followed Kuhn in preferring this second sense, but continue to prefer the first, sociological or Gestalt sense. Appreciating that common usage had hijacked his preferred sense, Kuhn adopts the term 'exemplar' for his second sense (Kuhn, 1970, p. 187).¹⁹ I would like to take Kuhn at his word and concentrate on the second, philosophically deeper, sense of paradigms as concrete exemplars of puzzle-solving.

Sartre's dictum, 'existence precedes essence', has an analogue in scientific practice: *models (or theories) precede programs*. To take one example of many, Lucas (1972b) is an attempt to resolve a particular puzzle: what constitutes a decisive test of the natural rate hypothesis? The natural rate hypothesis predated the new classical macroeconomics; and Lucas introduces rational expectations into the model not to establish a new world-view or to further a nascent program but to make Friedman's original presentation of the natural rate hypothesis more consistent with the general presumption that economic agents optimize. It is only later, only after one of the many messages of this paper has

been generalized into the 'Lucas critique', only after the principal authors of new classical models began to write interpretive tracts, that the programmatic force of this paper became clear.²⁰

One might conclude from this example that programs can only be written retrospectively. But, in fact, it is more complicated than that. Concrete models are a mixture of the programmatic and the adventitious with no firm boundary between the two. The characteristic feature of the concrete exemplar is its richness. Its power over the imagination arises from the virtually inexhaustible lode of special features. When the model is used as an exemplar for another model, usually by another modeller, not every feature of the exemplar is transferred. Key features or methods or assumptions are transferred, while others are neglected. In choosing which features to borrow, however, the modeller already provides an interpretation of what is important – an interpretation which in turn may be adopted by still other modellers. Or it may be rejected by them, and the mine of the original model reworked once more. More explicitly interpretive works serve the same function. They isolate features from a model or a group of models in order to sell them as an appropriate way forward. Interpretive works, then, serve a prospective as well as a retrospective role in the development of a program or school.

The image of scientific practice that I wish to convey is one of specific models serving as concrete exemplars. Other models may develop merely as elaborations or variants of them. Others again may be quite different in detail or purpose, but nonetheless take over key assumptions or borrow techniques. Still others may borrow only the 'spirit' of the original exemplar. Scientific practice, therefore, consists of groups of related and partly overlapping models and practices.

The picture here differs from Kuhn's (1970) account of scientific revolutions. No model is primary. One model may have preceded the others. Others may have borrowed different elements from it. The model may continue to have life or it may become moribund with later models supplanting it as an exemplar. When many variants on a model exist, any one may, for many purposes at least, serve as the exemplar. Kuhn's account of revolution producing a dominant paradigm, which becomes the exemplar for a period of normal science is then too simple, too linear, to capture the multifariousness of scientific practice.

If this account is correct, however, one might ask in what sense there could be a new classical school or, even in a loose sense, a new classical research program. Where is the *essence* of new classicism in the sea of overlapping but not universal features or models? My point is that there is no such essence, yet there still is a new classical macroeconomics. The

models that form new classicism bear a *family resemblance* one with another. The philosopher Ludwig Wittgenstein writes:

The tendency to look for something common to all the entities which we commonly subsume under a general term. – We are inclined to think that there must be something in common to all games, say, and that this common property is the justification for applying the general term 'game' to the various games; whereas games form a *family* the members of which have family likenesses. Some of them have the same nose, others the same eyebrows and others again the same way of walking; and these likenesses overlap. The idea of a general concept being a common property of its particular instances connects up with other primitive, too simple, ideas of the structure of languages. (Wittgenstein, 1975, p. 17)²¹

In a similar vein, he writes:

We find what connects all the cases of comparing is a vast number of overlapping similarities, and as soon as we can see this, we feel no longer compelled to say that there must be some one feature common to them all. What ties the ship to the wharf is a rope, and the rope consists of fibres, but it does not get its strength from any fibre which runs through it from one end to the other, but from the fact that there is a vast number of fibres overlapping. (Wittgenstein, 1975, p. 87)

There are two points here. One is that the concrete cases or entities are primary and that they do not 'participate' in some sort of pre-existing essence but constitute the multifarious essences that interpretation finds in them. The second is that, despite this lack of essence, these cases or entities nevertheless form families: ropes have no fibres that run their entire lengths, but there really are ropes and they really do tie up ships; the models of the new classical microeconomics do not all have an identical hard core or protective belt, but there really is a family of models that deserves to be called 'new classical'.

Lakatos is not unaware of nor completely unsympathetic to the view I advocate here. He writes:

Some eminent philosophers, however, ridicule the idea of statute law, the possibility of any valid demarcation [between science and non-science]. According to Oakeshott and Polanyi there must be – and can be – no statute law at all: only case law. They may also argue that even if one mistakenly allowed for statute law, statute law too would need authoritative interpreters. I think that Oakeshott's and Polanyi's position has a great deal of truth in it. After all, one must admit (*pace* Popper) that until now all the 'laws' proposed by the apriorist philosophers of science have turned out to be wrong in the light of the verdicts of the best scientists. (Lakatos, 1978, pp. 136, 137)

To give up statute law is not to give up general principles; rather it is to acknowledge that one must discover the general principles in the particular cases. The complexities and the highly particular features of individual cases do not rule out a rational approach to the law. Nor do they rule out the individuation of tort law from contract law, contract law from criminal law and so forth: the borders are not strict, but there are distinct subjects. The same is true of alternative programs in economics.

Lakatos seems aware of some of this. Still there is far more of Popper than of Oakeshott, Polanyi or Wittgenstein in Lakatos. Lakatos's methodology of scientific research programs sees science as a collection of nation-states. Each program is defined by its hard core and protective belt – a constitution, and police and military. The negative heuristic defines its borders with a surveyor's precision, while the positive heuristic stands guard ready to protect the nation from unwanted anomalies or adversarial nation-states.

In contrast, the view I advocate sees science as a collection of tribes. Models and theories are united by ties of kinship and consanguinity. Each program or school stakes out its ground. Occasionally boundaries are sharp, as when tribes are divided by a river or mountain range, yet one country usually imperceptibly blends into the other. Even a tribe's sense of self-identity may show the same blending. Economists on the borders of new classical economics may sometimes identify themselves as new classical and sometimes as something else. Still, for all the blurring of the edges, when one is in the heart of Cherokee country or the heart of Chicasaw country, one knows it. Where the Lakatosian map of the scientific world delineates the borders of nations, occasional border disputes to the contrary notwithstanding, my map shows the names of the various scientific tribes scattered indefinitely (but not inaccurately) across the known world and with the *terra incognita* and the uncharted seas marked clearly with the cartographer's warning: 'There be monsters and dragons'.

3 THE ETHNOGRAPHY OF THE NEW CLASSICAL MACROECONOMICS

A Table of Exemplars

My suggestion that an anthropological metaphor captures the scientific endeavour – and economics in particular – has been buttressed with essentially anecdotal evidence. It should be possible, however, to

investigate systematically the relations of kinship and consanguinity among the concrete exemplars of the new classical economics. Figure 11.1 is a tentative step towards such an investigation. It is a sort of family tree of the new classical economics. For the most part its entries are particular papers that were important in the development of new classical doctrine. I did not intend to list every new classical paper; rather, following the logic of my earlier argument, the papers serve as exemplars of the main lines of new classical thinking. Just as many Latin nouns once declined serve equally well as grammatical paradigms, so too many of the papers on the list could be replaced with a related paper.

The table was constructed backwards from current research to its antecedents. My aim was to indicate which concrete exemplars influenced which other ones. Arrows indicate direction of influence. Sometimes this influence is direct, sometimes indirect.²² I have not attempted to reproduce the full and complex interrelationships between the exemplars listed.²³ Papers or topics in solid rectangles indicate influences from outside the new classical macroeconomics. Papers in broken rectangles indicate cases in which new classical influences have moved outside the new classical macroeconomics.

The table is divided into a grid for ease of reference. The bottom cells of the column indicate seven families of currently active research within the tribe of new classical macroeconomics: monetary models of business cycles, finance-based models of money, real models of business cycles, the analysis of policy rules, structural estimation of market-clearing rational expectations models, vector autoregression for policy analysis and overlapping-generations models of money.²⁴ Even when the papers listed are several years old, there is other derivative work currently being done in these areas. I have intentionally omitted some topics. Recent work on growth theory with increasing returns is omitted because of my own ignorance of the literature, and work on the consumption function under rational expectations (following Hall, 1978) is omitted because of its equivocal place in new classical macroeconomics. Other possibly eligible areas may also have been omitted. Including the omitted topics would not alter the general conclusions I shall draw about methodology.

The rows of the table indicate roughly the generations of new classical models. The story told by the table goes roughly as follows. The two principal influences on the earliest distinctively new classical papers are Friedman's and Phelps's natural rate hypothesis (IA) which influenced Lucas and Rapping (1969) and which, together with Muth's now famous paper on rational expectations, form the core of Lucas's papers on the

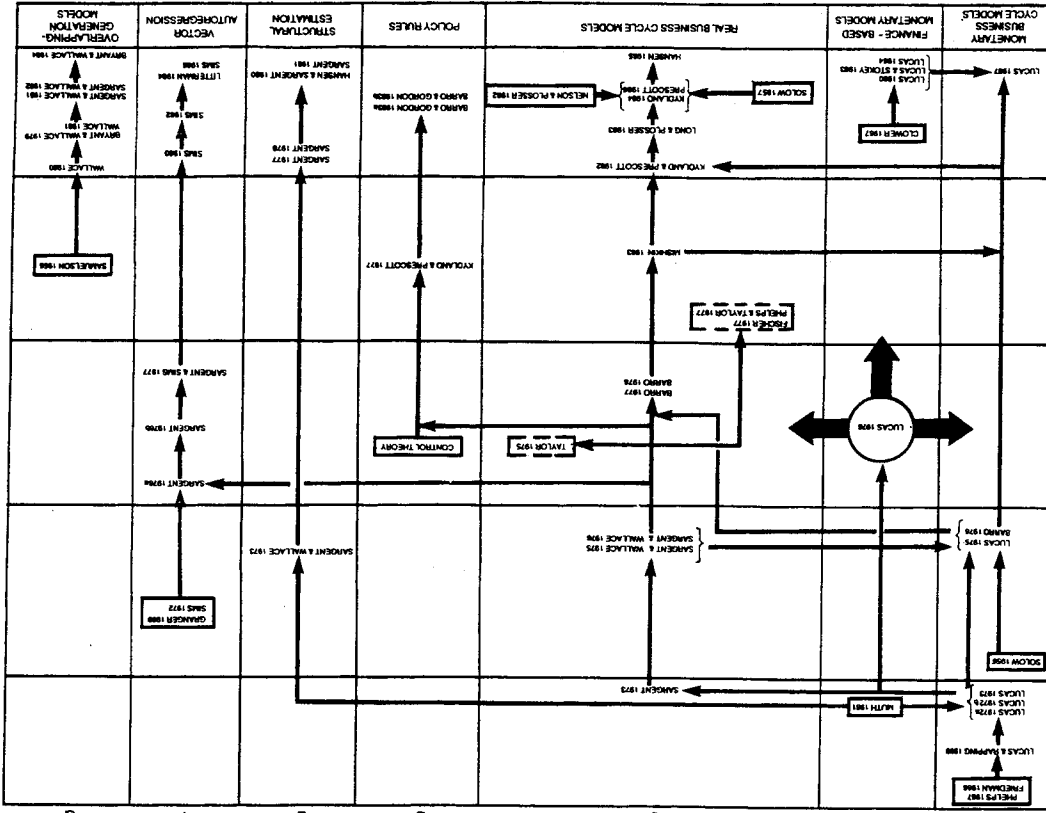


Figure 11.1 A table of kinship and consanguinity for the new classical macroeconomics

natural rate hypothesis and the Phillips curve in the early 1970s. Lucas's surprise-only aggregate-supply function suggested to him an explanation for business cycles, so that these papers form the direct ancestors of all monetary equilibrium theories of the business cycle.

Lucas's early papers are tremendously important for their other influences as well. Sargent's extension of Lucas's work to the consideration of interest rates (IC) became the basis for his work with Wallace on the ineffectiveness of monetary policy (IIC). This is part of the second generation of new classical models. The first generation aimed largely at discovering whether the insistence on general equilibrium and rational expectations made much difference to theoretical models and to the interpretation of pre-existing debates. It had little influence on the larger profession. The second generation began staking out novel and controversial ground. The policy-ineffectiveness papers sought to undercut the very possibility of policy, which had animated macroeconomics since its inception. The early business cycle papers (IIA) similarly sought to overturn the general presumption that business cycles must be disequilibria.

The third and fourth generations worked at exploring and systematizing the implications of the second-generation models. In this generation, we find the attempts to isolate which assumption are critical to the policy-ineffectiveness conclusions. One result is that contract or sticky-price models are developed which, while not new classical, nonetheless use the rational expectations hypothesis (for example, Fischer, 1977 and Phelps and Taylor, 1977: IVC). Similarly this debate produced models of the process of learning and convergence to rational expectations (for example, Taylor, 1975: IIIC). The first real empirical investigations of new classical macroeconomics occur in these generations (for example, Barro, 1977, 1978: IIIC) and Sargent's classical macromodel (IIIF).

What distinguishes generation III from generation IV in the table is Lucas's (1976) policy invariance paper – the famous 'Lucas critique' (IIIB). Lucas's paper is the most important synthesis of new classical ideas ever written. In the table it is circled to indicate its special place. Bold arrows are drawn pointing generally to exemplars in its own and later generations. To draw in any detail all of the necessary arrows would clutter the table too much. Lucas's paper is the Ibn Saud of the new classical macroeconomics. Ibn Saud, the first king of a united Saudi Arabia, married the daughters of important and often warring tribes, gave them sons and then divorced them, leaving them with a generous pension and much goodwill. As a consequence Saudi Arabia has an enormous number of royal princes, and formerly fractious tribes are

united by ties of kinship and loyalty to the royal family. Lucas's paper serves a similar function, inseminating every new classical model in generation IV and ~~often~~ ^{often} at once with concrete examples of how ignoring new classical principles can lead modellers astray and with concrete suggestions about how to proceed (for example, beware of free parameters; reduce everything to the bedrock of optimization with only tastes and technology given).²⁵ Even the distant family of the overlapping-generations models of money (G), which has no other direct connection with the other six families, is clearly part of the new classical tribe because of the virile influence of Lucas (1976).²⁶

Some Anthropological Questions

Having set out a table of kinship for the new classical tribe, it is – with sincere apologies to any serious anthropologists – time to pursue some further anthropological investigations.

Foundation myths

Every culture has some account of its own beginnings – some are folk tales, some are literary fabrications. The ancient Greeks are an example of the former, believing themselves to be autochthonous. The ancient Romans, however, looked to the poet Virgil to teach them that they had descended from Aeneas, who had fled from the ruins of his native Troy. In economics, neoclassical economics follows the Greek example. Economics is the consequence of the inexorable logic of choice, a product of the human condition rather than of history. New classical macroeconomics is more like the Romans. The new classical look back to an earlier age, to the classics (as Keynes defined them). When the classical citadels crumbled after Keynes slipped in his Trojan horse, the *General Theory*, a few weary survivors (Friedman *inter alia*) made their way to Chicago to refound a Republic of Virtue on virgin soil. Lucas, who from our table of kinship, is clearly Octavius, the perfected product of Roman virtue, is also Virgil himself, the fabricator of the myth (see especially Lucas, 1977, 1980b).

Like most myths, this one has a germ of truth. We have already seen the critical role played by Milton Friedman in the formation of the new classical tribe, and there is no doubt that Friedman provides a link to pre-Keynesian traditions. Still, the new classical myth is not real history.²⁷ It nevertheless has its uses. Lucas's interpretive papers serve to underscore certain new classical values with enough rhetorical force for the younger generation of new classical economists to know what is

expected of them, how to behave, how to model – not in technical detail (for that they have other exemplars) but in broad spirit.²⁸

Scope and range

Any tribe occupies some particular places for some particular times. The table of kinship reveals chronological development of the new classical macroeconomics, vertical development from generation to generation, as well as horizontal development, a range of topics from theoretical monetary models to policy issues to technical empirical applications. One might ask why I have drawn these particular papers together as ‘new classical’ rather than a much narrower or much broader collection.

The case for the much broader grouping is indicated by the important exemplars that I have chosen to box in as outside the tribe: should not all the relatives be included? Unfortunately the end of that road is complete lack of discrimination. After all, in some sense, every person is related to every other person.²⁹ Yet, even where I come from, in Franklin County, Virginia, a place overrun with Perdues, Hodges’s and Scruggs’s, no Perdue, Hodges or Scruggs ‘claims kin’ with all of the other Perdues, Hodges’s or Scruggs’s.

Gordon (1989) takes the new classical macroeconomics to be much narrower: the close descendants of Lucas’s original work on the natural rate hypothesis and the Phillips curve – including IA, IIA, IC, IIC, IIC, IVC and IIIB on Figure 11.1, but excluding just about everything else. New classical macroeconomics died for Gordon at a conference in 1979 when Barro presented an empirical paper that failed to support the Lucas surprise-only aggregate-supply function. Gordon has apparently identified new classical macroeconomics with its most prominent policy conclusion: the policy-ineffectiveness propositions after Sargent and Wallace (1975, 1976).

Now something like Gordon’s death of new classicism occurred. It is represented in Figure 11.1 by the exemplar of Mishkin (1983) – an empirical investigation critical of the surprise-only proposition. This was a key event in the life of the new classical tribe – but not its death. Lucas’s recent monetary model of the business cycle (VA) as well as the entire real business cycle literature (VC) is a reaction to the empirical research in IIC and IVC. Gordon recognizes the real business cycle models as somehow being new classical, but dismisses them as arid, technical exercises. What really tells against Gordon’s narrow identification of the new classical economics is the strength of the influences from those papers that he admits are centrally new classical to the papers in columns D, E, F and G that he ignores.

I have argued that it is family resemblance that links the exemplars of research programs together. Resemblance is clearly in the eyes of the beholder, but the point of tables of kinship is that some of the links of family are objective: there is little room for reasonable people to disagree on the existence of the influence. The reason to reject the narrow view of what is new classical is twofold: genetic linkages exist between exemplars and the resemblances among exemplars are strong. Although it is hard to be sure where to draw the borders, which wayward daughters to exclude or which prodigal sons to include, there is no real doubt about the members in the centre of the family.

Anthropologists and linguists distinguish between *diachronic* analysis, which concentrates on a particular time, and *synchronic* analysis, which concentrates on change over time. Looked at synchronically, it is easier to see the argument for a narrow conception of what constitutes new classical macroeconomics – although not so narrow a conception as Gordon’s. The current research in VA–E on Table 11.1 is central to most conceptions of the new classical macroeconomics. Financed-based models of money (VB) are direct competitors with overlapping-generations models. Similarly the strategies of structural estimation (VE) and vector autoregression are usually seen as incompatible.³⁰ One might then simply say that the program of vector autoregressions and overlapping-generations models should be excluded from new classicism altogether. But that would be a superficial judgement. Financed-based models employing cash-in-advance constraints and overlapping-generations models both enthusiastically endorse the principle of Lucas (1976) that everything should be derived from tastes and technology, and they both aim at providing a secure microfoundation for policy analysis. The differ over the substantive point – what is ad hoc. To Sargent and Wallace the cash-in-advance constraint has no deep justification, whereas to Lucas, who places it in a search framework, it arises from the fact that there are 24 hours in the day and the earth goes around the sun. Similarly the advocates of vector autoregressions agree with the advocates of structural estimation that well-founded theoretical restrictions should be used to identify econometric models; they disagree on what is well founded.³¹

Such disagreements are the sort that Lakatos might see as arising from different hard cores and hence as defining separate programs. Looked at diachronically, however, we can appreciate the common genesis of these alternative approaches. We can see them as cousins under the skin – what they share in roots outweighing the vastly differing circumstances in which they live. It is possible for cousins to ‘claim kin’, and still not care for each other very much. It also helps

explain what would otherwise be paradoxical: that Sargent and Wallace, who are key players in the central new classical story, are also key players in the overlapping-generations program; that Sargent, who is the principal advocate of structural estimation, played an important part in the establishment of the program of vector autoregressions, and has not renounced it even though he no longer works within it.³²

Social structure

The table of kinship alone does not tell us all that we might wish to know about the social structure of the new classical tribe. I would like to concentrate on one aspect of social structure often stressed by anthropologists and economists alike – the division of labour. One might think of each of the current families of research (VA–F) as Hindu castes with specialized occupations. Or, if we take a bolder division of occupations into just two – theory and empirics – we might think of these families as like the 12 tribes of Israel, which were divided into priests and everyone else.

It is evident in considering the papers that make up each family that they are not as radically separated as the table might suggest. In particular there is a close interplay between theory and empirics. The econometric techniques developed in the structural estimation program, for instance, are almost wholly a response to the theoretical implications of the Lucas critique.³³ This relationship would not surprise Lakatos: new theory leads to appropriate new empirical methods. Lakatos (1970, p. 137), however, believes that the methodology of scientific research programs accounts for the fact that theoretical science is relatively autonomous. He writes:

If a scientist (or mathematician) has a positive heuristic, he refuses to be drawn into observation. He will 'lie down on his couch, shut his eyes and forget about data'. . . . Occasionally, of course, he will ask Nature a shrewd question: he will then be encouraged by Nature's YES, but not discouraged by its NO. (Lakatos, 1970, p. 135, fn. 1)

The case of the new classical macroeconomics suggests that this is not quite correct. First, two of the new classical families we have identified are families of empirical methods (VE and F). Although these families reveal their own programmatic tendencies, their provenance is the difficulties inherent in the correct empirical implementation of new classical theory. New classical have not been content to lie on their couches, but in some degree have felt compelled to confront data.

Second, although our diachronic analysis of the new classical macroeconomics does not reveal any 'crucial experiments', it does provide

examples in which empirical results fundamentally re-directed theoretical research. The most obvious case is the series of tests of the surprise-only aggregate-supply function (IIC and IVC). The theoretician in the thrall of a positive heuristic asked Nature a shrewd question and Nature answered, MAYBE NOT.³⁴ Contrary to Lakatos, the advocates of monetary business cycle models were discouraged. They did not renounce their models, but interest shifted almost entirely to real-business-cycle models. And, even when interest in monetary models has been revived, as in Lucas (1987), the monetary source of aggregate fluctuations is no longer shocks to monetary policy.

Less obviously, the program of real-business-cycle models within that program is supported by empirical results. Nature's less-than-decisive suggestion that GNP is non-stationary (Nelson and Plosser, 1982: VC) is thought by new classicals to favour real models of the business cycle over monetary models. Monetary cycles, they argue, must be cycles about a trend, whereas the time-series evidence suggests that movements in the trend itself dominates cyclical behaviour.³⁵ Similar evidence from the program of vector autoregressions suggested that monetary policy had little effect on output. Again this was taken to favour real-business-cycle models.

Particular developments within the program of real-business-cycle models also responded to empirical results. Proponents of real-business-cycle models have taken the appropriate test of their models to be the ability of the model to reproduce the variances/covariances of the actual post-war data for the US. Kydland and Prescott's (1982) model did not do this very well. The precise manner of its failure helped determine the details of the modifications represented by the four exemplars listed below in Figure 11.1 (VC).

At a cruder level, the observation that money is dominated in rate of return by other financial assets explains not only the progressive development of the overlapping-generations models (G) from simple store of value models such as Wallace (1980) to models with legal restrictions such as Sargent and Wallace (1982) and Bryant and Wallace (1984), but also the introduction of the cash-in-advance constraint, borrowed from Clower (1967), into financed-based models of money (VB).

There is a division of labour in scientific research between theory and empirics with influence running in both directions. And, as any economist would expect, there are gains from trade.

Insiders and outsiders

I have argued that what makes a particular model new classical is partly

its genesis – where it fits in the family tree – and partly its family resemblance to exemplars that are clearly part of the new classical tribe. Anthropologists, however, understand that such relationships are not enough to forge and maintain tribal identity. The borders of a tribe need to be at least loosely patrolled if its identity is not to be submerged into surrounding tribes. The ancient Israelites had their shibboleth. ‘Shibboleth’ is the ancient Hebrew word for an ear of corn. Non-Hebrews could be distinguished from Hebrews by their difficulty in pronouncing this word correctly. Likewise the dietary laws of many religions and cultures function in part to distinguish them from surrounding groups.³⁶ Those propositions and injunctions identified as the hard core (HC1–5) and negative heuristic (NH1–7) in the tentative Lakatosian new classical program in section 1 above serve less as rigid guides to research than as shibboleths and dietary laws. I have myself had the experience of people at certain conferences inching a little bit further away from me at dinner simply for having expressed healthy scepticism about one or more of these propositions and injunctions.

On the other hand, tribes are not autarkic; they trade and communicate with other tribes or link up with them through marriage. Such intercourse is often vital. The table of kinship imperfectly, but strikingly, reveals the importance of influences from outside the new classical economics. There is probably not a single technique or basic model that originated within the new classical tribe. Many were adapted and modified, but their connections with outside sources remain clear.

Similarly the new classical tribe has itself influenced outsiders. The cases indicated on Figure 11.1 are cases of largely negative influence. The papers in the boxes in IIC and IVC are reactions to the unpalatable conclusions of Sargent and Wallace (1975, 1976). The new classical macroeconomics has, however, had positive influences as well. The drive for microfoundations and for technical rigour, which is characteristic of the new classical tribe, has infused much of the macroeconomic profession.³⁷ This is seen very clearly in Blanchard and Fischer’s (1989) graduate textbook, which is Keynesian in ‘spirit’ but stresses microfoundations and aspires to technical rigour no less than that found in Sargent (1979, 1987) – the key new classical textbooks.³⁸

Although trade outside the tribe is crucial, we are not really somehow one big family. The role of the shibboleth is important. This is most strikingly illustrated by the overlapping-generations models of money (G). As I observed above, this family is less tightly linked through direct bloodlines to the other families in the new classical tribe. What is more, there is a large literature on overlapping-generations models that could not be considered to be remotely new classical. In the final analysis,

what makes the exemplars listed in G ‘new classical’ is the motivations that their authors have in formulating them – their ritual incantation of new classical principles, their obedience to the dietary laws, as it were.³⁹ Wallace (1980, p. 51), for example, justifies his interest in overlapping-generations models by his hope that they will provide a structural explanation for time-series correlations (that is, that they will offer a way around the Lucas critique).

Even though the new classical macroeconomics has, and must have, its shibboleths and dietary laws, its commerce with other tribes demonstrates that fruitful communication is possible. Contrary to Lakatos, who rules out discussion of hard core propositions, and contrary to Kuhn, at his most radical, or to Feyerabend, both of whom find competing approaches to be actually incommensurable, communication is both possible and useful. It is true that different schools of economic thought often fail to understand one another. The new classicals often do not understand the Keynesians and do not usually even attempt to understand the post-Keynesians, but this lack of communication cannot be blamed on incommensurability, and a conventional decision to opt for ignorance should not be endorsed.⁴⁰ Semantic disputes, equivocations and confusions are commonplace; they nonetheless should not be raised into methodological principles.

4 CONCLUSION

Lakatos writes:

All methodologies, all rational reconstructions can be historiographically ‘falsified’: science is rational, but its rationality cannot be subsumed under the general laws of any methodology. (Lakatos, 1978, p. 130)

The historiography of the new classical macroeconomics shows that Lakatos’s own methodology is falsified like all the others. Lakatos, of course, was not claiming any special dispensation. He would argue that the best methodology is not that which is truest, but that which rationalizes science the most thoroughly.

My principal criticism of Lakatos’s method is that its unit of appraisal, the scientific research program, cannot be individuated in a commonly acceptable way. Because it cannot be individuated it cannot have normative force for working scientists and economists. What does have normative force is the concrete model, the exemplar of how to solve a particular sort of problem. I do not, however, wish to raise the exemplar to the status of a Lakatosian ‘unit of appraisal’. Exemplars resonate on

many levels: theory, technique, world-view, metaphysics. The development of the new classical economics, and no doubt other areas of economics and other sciences, shows that science develops on all of these levels. Scientific practice cannot be split into well-defined atoms to be judged on formal criteria as Lakatos's negative heuristic attempts to do. Rather alternative scientific models must be judged according to which gives the best explanation, and they must be allowed to communicate and inform one another.⁴¹ The only iron-clad methodological rule is C.S. Peirce's (1931, para. 135): 'Do not block the way of inquiry'.

For Lakatos, the history of science is important because one cannot assess a research program unless one knows its past: is it progressing or degenerating? But if scientific practice is to be judged according to how good an explanation it provides relative to the alternatives, it would seem that only the present counts. Still the tribal view of science presented in section 3 above suggests that history is important.

The problem of judging scientific achievement is that we have no bird's-eye view. Modern mariners or adventurers have the benefit of maps and devices to locate themselves on the maps. In contrast, the scientist, like the early explorers, has no place to stand to look down upon his own progress. The scientist must construct his maps as he goes. Before the eighteenth century, when the sextant and chronometer permitted sailors to determine their longitudes precisely, navigators relied on *rutters*, log books that recorded the directions of currents, the changing colours of the sea, the characteristic features of important bits of land, and so forth. The more experienced the navigator on a route, the better his rutter. The rutter represented accumulated knowledge to be passed on to other navigators, a record of the past. It could be useful in only a general way in truly untravelled waters. But if, in sailing such waters, the navigator reached an impasse, the rutter not only told the way back, it also provided the best hints about which alternative routes might be worth a try. The history of economics, like the history of other sciences, is a rutter: a record of the roads taken and also a record of the forks, of the roads not taken that lead who knows where.

In Hoover (1988), I attempt to assess the successes and failures of the new classical macroeconomics. Starting with strong exemplars, it has been a programmatic success. Lucas, Sargent and Wallace in effect forged a trail that was easy for their followers to walk along. No other approach seemed to move ahead so far or so fast. Still, on balance, I feel that the new classical macroeconomics has built a fine, paved road into a valley surrounded on three sides by impassable mountains. The message of my methodological ruminations is partly that there is no reason for other schools of thought not to learn from new classical

efforts. If nothing else, we know a lot more about road-building. But now I think we must back-track if we are to find a way forward. The traditional concerns of Keynesians with disequilibrium, especially with involuntary unemployment, uncertainty and macroeconomic policy, are not to be dismissed using the clean, simple devices of perfect competition, Walrasian general equilibrium, representative agents and (nearly) perfect knowledge.

NOTES

1. I am grateful to Thomas Mayer, Steven Sheffrin, Julie Nelson, Nancy Wulwick, Wing Woo and the participants in the Conference on Alternative Research Programmes in Recent Economics, Capri, Italy, 16-19 October 1989, for comments on earlier drafts.
2. Gordon (1989) goes to great lengths to document that its following is highly limited.
3. Lakatos (1970). For an excellent account of Lakatos's methodology, see Newton-Smith (1981) ch. 4.
4. It would be possible, but tedious, to present detailed citations from new classical literature justifying the hard-core propositions listed here as well as the negative and positive heuristics listed below. Instead, the reader is referred to Hoover (1988), especially chs 1, 9 and 10, to the interpretive literature of the new classical economists themselves - particularly, Lucas (1977), (1980b), Lucas and Sargent (1979) and Sargent (1982, 1986) - and to Klammer (1984) especially chs 1-4.
5. This proposition is not to be confused with the usual assumption of rationality in microeconomics, which merely requires complete, transitive preferences. This proposition goes further and places restrictions on the *content* of utility functions (for example, that nominal money balances may not be a source of utility) rather than on their *form*.
6. This might be seen as an implication of HCl-4; but for a contrary view see Hahn (1986) p. 281.
7. A tragic flaw in the new classical macroeconomics is the inconsistency between PH2 and NH6; see Hoover (1988) pp. 242-4.
8. PH3 and PH4 are not really compatible. Yet both are found among new classical economists and sometimes in the same economist.
9. See, for example, Latsis (1976) p. 22 on the program of situational determinism; Coats (1976) pp. 53, 54 on demand analysis; Weintraub (1985) p. 109 on general equilibrium theory and Remenyi (1979) pp. 55-61 on neoclassical economics.
10. The point was made splendidly at the Capri conference in the papers presented in the same session as this one (and published in this volume). Rodney Maddock interprets the new classical macroeconomics as a narrow Lakatosian program, the hard core of which is (1) the Lucas supply function, (2) rational expectations and (3) policy characterized by fixed rules. In contrast, Roger Backhouse characterizes the new classical macroeconomics as a sub-program of a fully specified neo-Walrasian program. My characterization in section 1 above falls neatly between these extremes.
11. 'But one should not forget that two specific theories, while being mathematically (and observationally) equivalent, may still be embedded into different rival research programmes, and the power of the positive heuristic of these programmes may well be different. The point has been overlooked by proposers of equivalence proofs (a good example is the equivalence proof between Schrödinger's and Heisenberg's approach to quantum physics)' (Lakatos, 1970, p. 164, fn. 3, italics in the original).

12. Such vagueness is by no means unique to applications of Lakatos to economics; cf. Newton-Smith (1981) ch. 2, section 2.
13. See Hoover (1988) ch. 3.
14. Fisher and Seater (1988) gives an exhaustive theoretical and empirical account of the alternatives.
15. See the citation in Lovell (1986).
16. Lovell (1986) pp. 121, 122, observes that Muth's recent empirical work uses implicit not rational expectations.
17. See Hoover (1988) ch. 4, section 4.3 for a discussion and references.
18. Having even made the list, 'What Literate Americans Know', in Hirsch's (1987) *Cultural Literacy*.
19. Most commentators are so impressed by the proliferation of senses identified by Masterman that they neglect the degree to which she and Kuhn pare them down.
20. See in particular Lucas (1976), Lucas and Sargent (1979), Sargent (1982) and, above all perhaps, Sargent's (1979) textbook, which literally made a school out of new classical models.
21. See also Wittgenstein (1953) pp. 31 ff.
22. Sometimes influence shown as mediated through various papers was actually more direct than shown. I have not always indicated the direct influences when it would have overtly cluttered the diagram.
23. A careful examination of the patterns of citations of the various papers would provide a more complete analysis of patterns of influence. This task, however, awaits a more dedicated historiographer than I profess to be.
24. These topics are critically surveyed in Hoover (1988).
25. Maddock, who tries to place the new macroeconomics into a narrow Lakatosian framework (see n. 10 above) is puzzled by the role of Lucas (1976). He sees it finally as a paper 'written to detract from the credibility of alternative research programs'. The straitjacket of Lakatos's categories forces this view and makes Lucas's most cited paper peripheral. Yet to those inside the new classical macroeconomics and to most outsiders it is crucially important. Maddock is left with a puzzle, while the view expounded here explains the importance of Lucas's paper naturally.
26. Of course one should also note that Sargent and Wallace are the principal authors in this literature. That alone would no doubt forge the link.
27. Hoover (1988) chs 9 and 10 seeks to correct two aspects of the new classical foundation myth.
28. Although I have not tried to map them onto Figure 11.1, there of course exist *interpretive* exemplars as well as substantive ones.
29. Traditionally, of course, we all descended from Adam and Eve. Recent analysis of chromosomes from people all over the world indicates that we probably all have at least one common human female ancestor. And of course logical analysis suggests that this must be so: each person has two parents, four grandparents, eight great-grandparents, etc. Carrying this back, say, 35 generations, implies that, if they were all distinct, each of us would have 34 billion ancestors. This exceeds the number of people who have ever lived. We must share a large number of ancestors with others; and, taken far enough back, it is highly improbable that any two of us do not have a common ancestor. This insight forms the basis for Bernheim and Bagwell's (1988) criticism of proofs of Ricardian equivalence that typically assume that families are unrelated dynasties.
30. Cooley and LeRoy (1985); but cf. Hoover (1988) ch. 8, sec. 8.4.
31. Sims (1980) and esp. (1986b) p. 15.
32. Pure time-series econometrics is important in Sargent's (1979) textbook. Also see his comments on the 'atheoretical' approach in Klammer (1984) p. 75.
33. See Hoover (1989).
34. Gordon (1989) regards Barro and Rush (1980) as nature saying, NO!, emphatically - as a decisive crucial experiment. Gordon, however, seems to have been the only one who heard nature quite so clearly.

35. This reasoning has been questioned, see Hoover (1988) pp. 42-8.
36. See Levi-Strauss (1978).
37. I am sympathetic with the view that rigour is often bought at the price of relevance and sound analysis.
38. Lucas tends to see a revolution of rigour rather than a competition between schools; see Lucas (1980b) p. 286 and (1987) pp. 35, 46, 47, 107, 108. This picks up on a genuine similarity between neo-Keynesians and new classicals, but badly underplays the differences.
39. In this, they are like the Falashas, the black Jews of Ethiopia - isolated, but faithful.
40. In interviews with Klammer (1984) both Lucas (p. 35) and Sargent (p. 76) admit ignorance of and lack of serious concern for the work of Marxists, post-Keynesians and other non-conventional economists.
41. An extended discussion and defence of this moderate realist view of science is found in Newton-Smith (1981).

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