The Handbook of Economic Methodology

Edited by

John B. Davis

Department of Economics, Marquette University, USA

D. Wade Hands

Department of Economics, University of Puget Sound, USA

Uskali Mäki

Department of Philosophy, Erasmus University, Rotterdam, The Netherlands

pp. 307 - 310.

1998

Edward Elgar

Cheltenham, UK • Northampton, MA, USA

neo-Keynesian and new classical macroeconomics as one SRP, because they both adhere to methodological individualism as a research strategy, or should we classify them as separate SRPs because the former assumes fix-price and the second flex-price market clearing? And again, is monetarism Mark I *à la* Friedman really the same SRP as monetarism Mark II *à la* Lucas–Sargent since the former concedes that there is a short-run negatively inclined Phillips curve while the latter denies it (Backhouse, 1992; Hoover, 1991)? The point is not that we fail to find answers to these questions in Lakatos, but that the Lakatosian apparatus is so loosely described that we can fit it to any answer we care to find. Research programmes, at least in economics, frequently overlap, the implications of one programme feeding into another programme as a crucial input. Whether this is a serious criticism of Lakatos depends on one's point of view. Why should the role of overarching research programmes in one discipline perfectly match that of another?

Likewise, Lakatos' concept of SRPs has been criticized on the grounds that it may be difficult unambiguously to characterize the hard core of a programme. For example, is the hard core of neoclassical economics really that of individual optimizing behaviour, or is it that of rigorous mathematical modelling, or is it that of Pareto-efficient equilibrium outcomes? The arbitrariness of the answer suggests once more that SRPs in economics are not self-contained entities but also that the Lakatosian concept of a SRP is hopelessly imprecise. Against that it must be said that the Lakatosian separation of a 'hard core' from a 'protective belt' is partly a logical distinction. Once we agree that SRPs evolve, they must contain a flexible and an inflexible part if we are going to identify a changing research strategy as nevertheless the same essential strategy; the part that does not change may be labelled 'inflexible', 'unchanging' or 'hard core'. Perhaps that is all there is to Lakatos' categories.

That brings us to the second and more serious criticism of Lakatos, namely what Wade Hands (1993: 44–7) calls 'novel fact fetishism' as the criterion of 'progress of knowledge'. Lakatos' methodology of SRP enjoyed some esteem among economists in the late 1970s and 1980s as according with their view that sound economic theories are capable of accounting for out-of-sample data, thus demonstrating connections between events that had previously been thought unconnected. But a recent conference on economic methodology organized precisely to reassess that status of Lakatos' ideas in economics revealed a surprising wide-ranging hostility to it (de Marchi and Blaug, 1991: 500). Most of that hostility centred in fact on Lakatos' appraisal criterion, the dominant argument being that, while the history of economics frequently reveals 'analytical progress', it rarely demonstrates 'empirical progress'. By 'analytical progress' (or what Lakatos, 1978: 179 called 'heuristic progress') we mean the refinement of ideas and techniques, the clarification of terms, the honing of concepts and so forth. By 'empirical progress', we mean corroborated 'theoretical progress' *à la* Lakatos, that is, an improved grasp of the workings of the economic system as exemplified by more accurate predictions of the effects of changes in exogenous variables on the values of endogenous variables. There is no question that economics constantly exhibits analytical or heuristic progress but there is great doubt that it exhibits empirical progress, except intermittently. This being the case, the fear is that a Lakatosian appraisal of modern economics would leave little of it standing as a 'progressive' SRP. This may or may not be a fatal criticism, and it may be as much a criticism of modern economics as of Lakatos (de Marchi and Blaug, 1991: 504–10; Blaug, 1994), but the fact remains that the emphasis on novel facts as a criterion of appraisal remains one of the least developed aspects of MSRP.

That brings us to the third objection directed specifically against Lakatos' metamethodology for appraising methodologies. Lakatos proposed that historians of science should set down a 'rational reconstruction' of that history in the light of the methodology he or she is trying to appraise; he or she should then compare that reconstruction with the actual history because an acceptable methodology must be capable of endorsing the decisions made by practising scientists. In other words, a rational reconstruction must show that successful SRPs were 'progressive'; otherwise, the methodology that motivated that reconstruction is to be rejected as not capturing the 'rationality' of scientists.

Unfortunately, testing a methodology against the history of a discipline is almost as problematic as testing a theory against the empirical evidence for it. Moreover, the structure of incentives facing a scientist, the 'sociology' of the scientific profession, may be such as to distort the actual practice of scientists. There is no guarantee, therefore, that the history of a discipline will mirror an empirically oriented methodology. Be that as it may, the attraction of Lakatos' scheme is to clearly separate a positive and normative methodology, a study of what economists actually do as revealed by the history of economics, and the attempt to evaluate what economists do as enshrined in methodological precepts. No one in the philosophy of science has come closer than Lakatos in resolving the perennial tension that exists between these two separate but highly related activities.

Lakatos (1978) first defined his approach in 1970 in a paper entitled 'Falsification and the Methodology of Scientific Research Programmes' and further developed his ideas in 'History of Science and Its Rational Reconstruction', published in 1971. The first application of MSRP to economics came in 1972 and since then there have been some 25 case studies of Lakatos' methodology in economics (for a list, see de Marchi and Blaug, 1991: 29–30). In the final analysis, the utility of MSRP proves itself in these applications and the perusal of a few of these is more telling than all the general descriptions of MSRP or accounts of 'what Lakatos really meant'.

MARK BLAUG

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Microfoundations

When discussing the issue of microfoundations of macroeconomics it is important to know what kind of foundations one is discussing and for what object foundations are being discussed. In

other words, one has to know what the terms 'microeconomics' and 'macroeconomics' signify. Broadly speaking, two different sets of definitions can be distinguished in the literature (for some alternative definitions, see Machlup, 1963). Some economists (see, for example, Allen, 1967: 1; Henderson and Quandt, 1980: 2) regard microeconomics as the discipline investigating the behaviour of individual economic units, while they regard macroeconomics as the discipline studying relations between broad economic aggregates. Thus the quest for microfoundations can be understood as an attempt to base theorizing about aggregate relationships on the behaviour of individual agents. Some philosophers of economics, most notably Nelson (1984) and Rosenberg (1976), have taken this position when discussing some methodological issues concerning microfoundations.

The above division is, among other things, based upon the view that microeconomics confines itself to the explanation of the behaviour of individual units. However, it has also been argued (see, for example, Branson, 1979: 1; Nelson, 1989: 26) that microeconomics intends to provide explanations of aggregative phenomena such as relative prices and other market phenomena. Accordingly, both disciplines analyse *aggregate* phenomena. If this view is taken, the difference between the subjects can be understood along the following lines. Microeconomics is regarded as a method of doing aggregative economics in which (a) the actions of individual agents are regarded as the outcome of a constrained optimization problem and (b) the actions of individual agents form an equilibrium. Macroeconomics, on the other hand, is an interrelated set of concepts (like effective demand and involuntary unemployment) and theories that are not (yet) based on microeconomics. According to the second set of definitions, the quest for microfoundations of macroeconomics can best be understood as an investigation into the possibilities of rephrasing macroeconomic ideas in a microeconomic language or of making macroeconomics compatible with microeconomics.

The reasons for studying the possibilities of microfoundations also depend on which of the two sets of definitions are employed. If the first set is employed, the reason for studying microfoundations lies in a form of the reductionist credo of *methodological individualism*. Roughly speaking, the argument for this view runs as follows: society consists of individuals who are the only subjects that make economic decisions; in order to explain what is going on in the economy as a whole one has to understand the individual decisions from which a particular situation originates.

If the second set is employed, the reason for studying microfoundations is more compatible with a 'unity of science' argument. This argument roughly runs as follows. Two distinctively separate disciplines such as microeconomics and macroeconomics can only coexist in a fruitful way if they have different domains of application. However, as the two disciplines in question both study aggregative phenomena, it is not clear when to apply one (and not the other). It is thus natural to study the compatibility of the two disciplines and, as microeconomics has a better developed analytical structure, the reasons for investigating the possibilities for microfoundations become clear.

A careful investigation of contributions that are commonly regarded as hallmarks in the economic microfoundations literature reveals that the second set of definitions (with its associated interpretation of what microfoundations actually establish) is more appropriate than the first set. A few brief examples are given here. Bénassy (1975) and Drèze (1975) show that an equilibrium exists in fix-price models in which rationing schemes bring about an allocation of commodities over individual agents. Moreover, they demonstrated that these fix-price models capture quite a number of ideas associated with Keynesian macroeconomics:

involuntary unemployment could be regarded as an equilibrium phenomenon in which optimizing households face a quantity constraint on the amount of labour they can supply. Also the Keynesian notions of effective demand and the multiplier were reformulated in their models. These fix-price models have microfoundations in the sense that they are based on decision-making individuals and a well-defined notion of (quantity constrained) equilibrium.

New classical economics attempts to explain macroeconomic phenomena as fluctuations in unemployment by means of models that are as close as possible to general equilibrium models. A seminal paper in this respect is Lucas (1972) which regards incomplete information about the aggregate money supply as the major cause of the cycle. Lucas constructs a model in which prices are market clearing and agents behave optimally given their expectations, which are also formed 'rationally'. As there are two sources of disturbance, the agents cannot infer from the market-clearing prices whether a shock results from a relative demand shift or a shift in the money supply. This is why, according to Lucas, monetary shocks may have real (cyclical) effects. The more recent real business cycle models (for example, Kydland and Prescott, 1982) are even closer to general equilibrium models as they regard changes in technology and/or preferences, the basic elements of general equilibrium, as the main cause of the cycle.

Cooper and John (1988) point out a common element in many contributions to new Keynesian economics. They argue that most of this literature departs in one way or the other from the perfect competition assumption. Under imperfect competition, an individual agent's optimal action depends on the actions undertaken by other agents. Inefficient, or 'Keynesian', equilibria may arise as a consequence of the fact that individual agents are not able to improve upon their situation if all other agents stick to their actions. There may be multiple equilibria that can be Pareto-ranked. Agents might find themselves then in a 'bad' equilibrium, but individually they have no means of changing their situation. They call this a 'coordination failure'.

The above examples show that equilibrium notions and 'rational' expectations are widely adopted in the microfoundations literature. To complete the argument that it is the unity of science argument that underlies the microfoundations literature and not an argument about methodological individualism, it needs to be shown that equilibrium notions and the notion of rational expectations are not based on (rational) individual behaviour. Janssen (1993) shows that, although equilibrium notions are not inconsistent with notions of (rational) individual behaviour, they are (generally speaking) not derived from them either. An economic equilibrium is usually defined as a set of individual actions (and possibly a price vector) such that, given the actions of the others (and the price vector), no individual agent can improve upon his situation. Although an equilibrium is defined in terms of individual actions, it is usually not clear how individual actions bring about the equilibrium (Arrow, 1959). So the conclusion must be that equilibrium notions are *extra*rational and *not* based on individual behaviour. (An exception must be made for recent evolutionary models, but these models fall outside the scope of the micro-foundations literature as it is typically conceived.) To see that the concept of rational expectations is also an equilibrium concept (and not derived from an explicit optimization scheme) one has to notice that economic outcomes depend on the actions of individuals and these in turn depend on their expectations. Thus the expectation an individual has to have so that it coincides with actual outcomes depends on the expectations of other individuals. This means that the same critique applies here.

It seems that intuition tells many economists that the notion of equilibrium can ultimately be derived from individual optimizing behaviour. If this intuition proves to be correct, the quest

for microfoundations may be understood from the 'unity of science' perspective as well as from the methodological individualist perspective. The 'unity of science' that has been brought about by the microfoundations literature means that nowadays there is hardly any distinction between mainstream micro- and macroeconomic theory:

> The most interesting developments in macroeconomic theory seem to me describable as the reincorporation of aggregative problems such as inflation and the business cycle within the general framework of 'microeconomic' theory. If these developments succeed, the term 'macroeconomic' will simply disappear from use and the modifier 'micro' will be superfluous. We will simply speak, as did Smith, Ricardo, Marshall and Walras, of economic theory. (Lucas, 1987: 107–8)

A disappearance of the modifiers 'micro' and 'macro' will also imply that the term 'microfoundations' will stop being used.

It should be emphasized, however, that it is not the case that macroeconomics (or, more properly, economics) can be considered a branch of applied microeconomics as it existed at the beginning of the 1970s (Howitt, 1987). Important macroeconomic phenomena are inconsistent with the type of coordination that is assumed by the Walrasian equilibrium notion that prevailed in microeconomics at that time. As the above-mentioned literature suggests, the quest for microfoundations has also caused changes in the subject of microeconomics itself, in the sense that incomplete and asymmetric information, strategic behaviour, alternative equilibrium notions and adaptive behaviour play a dominant role nowadays. Paradoxically, the search for unification between micro- and macroeconomics has led to a situation in which some of the core assumptions are now shared by many economists at the expense of an enormous diversification of auxiliary assumptions.

MAARTEN C.W. JANSSEN

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