

Methodological monism and methodological pluralism in economics

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Abstract

The aim of the paper is to give an outline of the relation between general epistemology and the epistemology of economics. The epistemology of economics can be treated starting from the ‘general epistemology of science’ and from the subject of the investigation, namely the problems of economics itself. Starting from the general or subject-independent epistemology one can make an attempt to adapt to economics various methodological approaches which were practically created to take only the subject of physics or mathematics into consideration. By way of illustration the Popperian falsificationism, research programmes by Lakatos, Kuhnian normal science or Hilbertian axiomatism can be mentioned. The characteristic feature of this mentality is often the methodological monism, a doctrine which implicitly or explicitly states the unity of epistemology in all disciplines. In methodological writings of economics, beside the supporters of some general epistemological viewpoints, there are serious critics of them on behalf of methodologists who start their researches based on economics.

The methodological pluralism does not reject the importation of methodological ideas from other branches of knowledge in an aprioristic way. However, the uncritical adoption of the methodology of physical sciences or the ‘general’ methodology leads to the realm of inadequacy and dogmatism. According to methodological pluralism, every research has to choose its methods and methodology conforming to the nature of its own problems.

The theoretical consequences of methodological monism are not always obvious. The questions concerning methodology are notwithstanding essential because these are the basis on which the theories are founded. Inappropriate methodology can lead to inappropriate theories and inappropriate practical decisions. The negative consequences of formalism will be illustrated by some spatial economic issues in the field of money and price theory, such as the empirical empty doctrine of purchasing power parity and the theory of optimal currency areas.

Keywords: methodology of economics, methodological monism, methodological pluralism

Introduction

The aim of my paper is to stress the simple but often overlooked postulate that epistemological and methodological problems cannot be treated without referring to the substantive issues of the branch of science concerned. The collective treatment of epistemological problems of all of the sciences is a highly unjustified practice. This especially common among ‘general’ epistemologists and secondly from their uncritical followers in various disciplines. This approach is frequently connected with methodological monism, which is often a tacit methodological viewpoint. According to methodological monism, in spite of the manifold subject of scientific cognition, there is an unified scientific method; only its fields of application are different. The monist view itself has many different and mutually incompatible variations. However, the typical advocate of methodological monism regards one particular methodology established in mathematized physics as the paragon of every empirical discipline, and therefore for economics also. The inverse of this view is virtually nonexistent, for example no academic would posit that the methodology of linguistics, biology, economics or human geography provides a paragon for every other discipline. The monists mainly use attributes as: rigour, analytical elegance, development, exactness and scientific as synonyms for the mathematized expression.

In methodological writings of economics there are not only the supporters of a general epistemological viewpoint, but heavy critics are present of those among the methodologists whose researches originate in the subject of economics. According to methodological pluralism, every research has to choose its own methods and methodology conforming to the nature of its own unique problems. As Mises, one of the steady supporters of methodological independence of economics, writes: ‘general epistemology can be studied only by those who are perfectly familiar with all branches of human knowledge. The special epistemological problems of the different fields of knowledge are accessible only those who have a perfect acquaintance with the respective field. There would not be any need to mention this point if it were not for the shocking ignorance of everything concerning the sciences of human action that characterizes the writings of almost all contemporary philosophers’ (Mises, 1962, p. VII).

Methodological pluralism has two intimately connected meanings. Firstly, it means that as any other discipline, economics also has its own subject-dependent methodology; secondly, the sub-disciplines of economics have their own methodology and the same subject can be approached with the help of many convenient methodology. Methodological pluralism does not reject in an aprioristic way the importation of methodological thoughts from other sources of knowledge. It only requires the reconcilability of the method and the matter of study. The choice between monism and pluralism could not be based on logics or on an arbitrary decision but on analysis and examination of economics. I will argue in such basis against monism by presenting several monist misconceptions and parallel with the critique of monism for the pluralism. In this short survey cannot dwell long on the enormous literature of this subject matter due to limitations of space.

The relation between general epistemology of science and epistemology of economics

The epistemological works of science can be divided according to their purposed domain of validity. This creates two large groups. Works of the first group deal with the whole or an unspecified field of science and the second group with a well-defined field of science. In works of the first group, sometimes a methodology-oriented definition of science is given. These definitions simply restrict the domain of scientific investigations according to method. The epistemology of each discipline can be treated by starting either from the ‘general epistemology of science’ or from the subject of the investigation, in the case of economics

from the problems of economics itself. Starting from the general or subject-independent epistemology one can make an attempt to adapt to economics various methodological approaches that were principally created to take only the subject of physics or mathematics into consideration. After this approach, which can be referred to as 'methodological monism', the various epistemological questions, such as the problem of the demarcation of science from non-science or unscience, the problem of induction and the causality, can be investigated in an unified way for the whole of science.

This view is based on a tacit assumption that a formal discipline exists, whose subject is the general epistemology of science. It is not to be questioned that the timeless, eternal and subject-independent questions of the rules of thought exists, and in this sense a formal discipline exists which examines these questions, namely the field of logic. However, the epistemology examines not formal and subject-independent questions but questions which are in connection with the factual events of empirical disciplines. When we examine the influential methodologists of science of twentieth century, Popper, Kuhn, Lakatos, Feyerabend, Quine and others, we can find them buttressing their views mostly from the field of natural sciences. In Popper's *Logic of scientific discovery*, examples are solely from physics except an short reference to biology in connection universal and singular propositions. However, this comment from Popper on the notion of mammal is be used for illustrating difference between physical and biological problems, since the study of the mammals of other hypothetical planets is not a relevant problem for biology. In Kuhn's *The Structure of Scientific Revolutions*, there are only three short references to the non-natural sciences and short passages in the thirteenth chapter. Kuhn practically asserts social sciences have no paradigm at all.¹ In Lakatos's and Feyerabend's works many astronomical references can be found (one of the popular example is the discovery of Uranus) but for instance there are no references to the fields of linguistics, economics, biology, ecology, psychology, sociology, geology or geography.

It is a different matter to ask whether their conclusions can ever be applied to physics or not. However, it appears that these works, which are often treated by uncritical adherents as holding general validity and a prescription in connection with the epistemology of science, are only relevant in the natural sciences. In best case scenario the misuse of Popper's, Kuhn's, Lakatos's thoughts led to incorrect innovation in terminology, the abuse of such words as corroboration, revolution, paradigm, hard core, research programmes and so on. These words were either used in the original sense and thereby incorrectly, or in a modified meaning. In the worst case scenario these monist views generated 'an artificially restricted study stifled by the unheeding blinkers of an epistemological concept' (Graves, 1981, p. 90). This unfortunate development of affairs can be attributed not to Popper and Kuhn because their aim was to deal only with the natural sciences.²

In my opinion this confusion could be resolved by the recognition of the epistemology of science is neither formal not empirical (real, factual) but a 'method science', which is in an interzonal position between formal and empirical sciences. This means that epistemological statements and prescriptions are not independent from the empirical sphere in which they were created.

¹ 'It remains an open question what parts of social science have yet acquired such paradigms at all' (Kuhn, 1970, p. 15).

² Popper's later works are engaged in biology, history and social sciences, limits of space make it impossible to discuss them here. About the misuse of Kuhn's concept in economics Baumberger (1977), Blaug (1976), Coats (1969), Loasby (1971); in geography see Graves (1981), Mair (1986); Johnston (1978), Pohl (1993); in pedagogy and behavioral sciences the brilliant work of Scharnberg (1984).

Some general monist misconceptions

It is a hard task to designate the predecessors and the countless promoters of the following monist misconceptions.³ However, their presence is so pervasive and widespread that the lack of an exact list of supporters is not such a great deficiency. The sources of misconceptions, of course, can be found in the many different approaches often in critical opposition each other. It is not possible to examine thoroughly the following problem; the key issue is the variety of misconceptions and their contact to methodological monism.

Monist misconceptions 1: Falsifiability and testability

Popperians assert that in the empirical sciences the scientific status of a statement must be falsifiable. A ‘statement (a theory, a conjecture) has the status of belonging to the empirical sciences if and only if it is falsifiable’ (Popper, 1983, p. XIX.). This view has its rational core because there are

- tautological statements which state nothing that is new about empirical reality,
- mystical and supernatural statements which cannot be examined in an objective way,
- subjective judgements of values which cannot be examined through an objective interpersonal standard.

However, the treatment of falsifiability, as a general demarcation criterion for science and unscience is clearly erroneous.⁴ Firstly, there are obviously false assumptions in economic model building which can be used, despite their evident falsity, of conceptual grasp of some aspect of economics. For example, in the basic model of Lösch’s spatial economy, there are many unreal assumptions relating to space (absolute homogeneity), but with these assumptions some aspect of the spatial organization of economy can also be well illustrated. Secondly, and this is a more important logical objection, in the field of human action there are propositions which are very elementary facts of general experience, evidently true for everybody and at the same time non-tautological. An example is the idea that free exchange leads to mutual benefit, is an assumption that is at the same time empirical and non-falsifiable (in the sense as the gravitation hypothesis would have been falsifiable), because its denial would lead to insoluble contradictions in deductive reasoning and it is incompatible with our fundamental knowledge concerning human action. These statements can be treated as laws of thought or laws of reality (Rothbard, 1957). However, the important thing is that such type of elementary statements ‘is not conceivably falsifiable, and yet empirically meaningful and true’ (Rothbard, 1957, p. 318).

In economics it would be difficult to find theories rejected on the basis of an empirical test. It is interesting how widely advocates of Popperianism in economics do not consider the real nature of economics and the everyday work of economists.⁵

Monist misconceptions 2: Econometrics as a tool for testing theories

It is often asserted that economics is at disadvantage compared to the natural sciences since in the economics controlled experiments cannot be performed. According to econometrics, economic theories can be tested and refined with the help of sophisticated statistical techniques. The supporters of this view maintain that ‘econometrics shares its

³ A thoroughly examination of the general monist claims can be found in Hayek’s excellent Classics, ‘The counter-revolution of science’ (1979) with some historical background.

⁴ Not only in the field of economics.

⁵ I must emphasize once more that this is not criticism of Popper as an epistemologist of physics, but a criticism of Popper’s economist followers.

logical foundations with psychometrics and biometrics and, for that matter, with meteorology and even experimental physics' (Marschak, 1984, p. 294).

In connection with econometrics the main question is whether 'there is any sound foundation for using probabilistic method in the case of economic data'. The answer, which is not based on logic, but on common sense, is generally not. The exceptions can be found in the field of microeconomics and business economics. The probability theory is applicable, if the examined events can be classified in a class of events. The events of macroeconomic have unique characteristics, they are not homogeneous members of an identifiable class with known parameters in the distribution of values. They are uncertain but not random in the sense of probability theory. Macroeconomic indices are aggregated (through time, space, individuals, quality and behaviour), weighted and, contrary to physics, biometrics and psychometrics, not results of designed experiments.⁶ Macroeconomic indices are not one actualisation of a repeatable 'random samples' derived from a larger population but a part of economic history, and they relate to unrepeatable organized complexity. Therefore econometrics could contribute to the grasp of concrete *ex post* relationships between economic indicators, although because its unjustified manipulation with probability distribution, it is hard to interpret any result gained through econometrics. Treatment of macroeconomic indices as a sample is a convention only, ungrounded by any theoretical basis.

In economics, mental experiments are really counterparts of controlled experiment of physics. 'Since the relevant variables of the social world cannot actually be held constant, the economist holds them constant in his imagination. Using the tool of verbal logic, he mentally investigates the causal inference of one variable on another' (Rothbard, 1973, p. 318).

Monist misconceptions 3: The misleading views about the role of assumptions

As Friedman writes in his famous essay, 'to be important, therefore, a hypothesis must be descriptively false in its assumptions; it takes account of, and accounts for, none of the many other attendant circumstances, since its very success shows them to be irrelevant for the phenomena to be explained' (Friedman, 1953, p. 15). In spite of the fact, that Friedman's statements were criticized exhaustively from many sides, building theories on unreal assumptions is an everyday practice in theoretical economics. As Musgrave in his seminal paper demonstrated 'both Friedman's and the subsequent discussion are marred by unclarity about the status of 'assumptions' in economic theories (and in physical theories, for that matter)' (Musgrave, 1981, p. 378). Musgrave isolates three assumptions. One type is the negligibility assumptions. 'Suppose a scientist is investigating some phenomenon and has the hypothesis that some factor F which might be expected to affect that phenomenon actually has no effect upon it, or at least no detectable effect' (Musgrave, 1981, p. 378). And one example from Musgrave: 'Now suppose an economist 'assumes that there is no government', meaning thereby to assert that the existence of the government has negligible effects on the phenomena he is investigating. It would be plain silly to object that this assumption is 'unreal' because there is, in fact, a government' (Musgrave, 1981, p. 379). The second type of assumption is the domain assumption, which posits that the theory can be expected to depict reality accurately as long as certain conditions exist. The third type of assumption is what Musgrave termed as 'heuristic assumption'. In this case, in the first stage the scientist takes 'no account of factor F, or 'assumes' that it is negligible; in the second stage he takes account of it and says what difference it makes to his results' (Musgrave, 1981, p. 383).

⁶ See Morgenstern (1963). 'In general, economic statistics are merely by products or results of business and government activities and have to be taken as these determine' (Morgenstern, 1963, p. 14).

The distinction between various assumptions is an elementary task for a clear treatment. It shows confusion when imaginary models, which are based on an obviously false assumption, are treated as help without any problem for explaining real life phenomena.

I must stress the existence a fourth type of assumption, which Musgrave does not mention, namely assumption, which are in accordance with reality and could be named 'reality assumptions'. For example the existence of transport costs is an empirical fact, which we can use in the explanation without restricting the domain of our models. Theories built on reality assumptions can be rejected two ways. First, one can assert that the assumption is not in accordance with reality. Second, flaws can be revealed in the chain of deductive reasoning. Abstract reasoning can also be based on real, but not precise assumptions, not only on unreal ones.⁷

Monist misconceptions 4: Axiomatization with false axioms

According to Debreu 'an axiomatised theory has a mathematical form that is completely separated from its economic content' (Debreu, 1986, p. 1265). It is an interesting question, why is it that such an axiomatised theory can be regarded as a part of empirical science? Mises used axiomatism and apriorism too in his deductive method. His methodology was criticized vehemently. For example, Blaug writes that: Mises's 'writings on the foundations of economic science are so cranky and idiosyncratic that we can only wonder that they have been seriously by anyone' (Blaug, 1980, p. 93). However, Debreu's axiomatism and Mises's axiomatism contrasted to each other very sharply. Mises's axiomatism, which is often misinterpreted, is founded not on arbitrarily chosen but self-evident, apodictically *true empirical* axioms, Debreu's axiomatism is grounded on *false empirical* or *unempirical* statements. Mises' system of statements are derived from reality extracted axioms, Debreu's system of statements is not derived from reality extracted axioms. Misesian statements are *a priori* to the theory but *a posteriori* to our very elementary general experiences about the world and the structure of human mind.

It is strange in the light of the content of the Misesian system how widely some methodologists misinterpret it. One might be correct to get the impression that Blaug's criticism is valid not to Mises, but to Debreu. Blaug discusses correctly the nineteenth-century English predecessors of Misesian method (Blaug, 1980, pp. 55-90). He quotes Cairnes: 'The economist starts with a knowledge of ultimate causes. He is already at the outset of his enterprise, in the position which the physicist only obtain after ages of laborious research' (Blaug cites Cairnes, Blaug (1980) p. 78). Blaug criticizes these and similar statements from the Popperian viewpoint (which is here inadequate): 'The question of whether there is any way of showing a theory to be false is never even contemplated' (Blaug, 1980, p. 81)⁸.

Monist misconceptions 5: The role and type of prediction

'I suggest that the most innocuous epistemological requirement on *any* science is that its theories can be improved in predictive precision' (Rosenberg, 1992, p. XIV; no emphasis in the original). This popular general epistemological viewpoint, which remains often categorical and unspecified, is constantly under criticism by countless practitioners of various

⁷ See Long (2004). 'Friedman's mistake lies in taking a theory that incorporates ancestry, eye colour, and so on to be the 'logical extreme' of realism. But realism does not demand that all these extraneous traits be specified; it merely demands that their *non-existence* not be specified either. Those who criticise neoclassical models for their lack of realism are not seeking a precise abstraction that *more closely* approximates reality; rather, they are seeking an abstraction that is not precise at all' (Long, 2004, p. 9).

⁸ Further questions on axiomatism see in Backhouse (1998), Chick (1998), Kumaraswamy (1996), Weintraub (1998).

disciplines. This postulate does not permit scientific status for example for linguistics, geography, economics, a large part of biology, part of quantum physics and astronomy.

Predictive success has at least two meanings in the epistemology of natural sciences, that are, predicting empirical facts in quantitative terms and predicting new theoretical statements for explaining empirical anomalies which can not be explained by current theories. Neither of these types of prediction has a reason for existence in economics, although they also have a distant analogy in economics. In economics, the trend of some economic indicators can be predicted more or less, but the prediction despite that its quantitative form remains qualitative. The data which seems paradoxical in the light of theory, compels us to rethinking the theory and all circumstances concerning to the data and theory. Predictions in economics have a fundamentally different epistemological character than in physics.⁹

Monist misconceptions 6: The false dichotomy between mathematical and literary economics

It has been argued many times by respected economists that the attempt to mathematise economics is healthy, because it demands a prior clarification of concepts and propositions about economic phenomena. 'It is sometimes forgotten that arguments against the most general types of mathematics are just arguments against science in general' (Tinbergen, 1954, p. 368). The alternative way of economics is the verbal one which suffers from ambiguity.¹⁰

There are many problems and doubts with both sides of this dualism. The capacity of normal human language is belittled and at the same time the primitive mathematical language is highly overvalued. Every natural human language is able to describe everything in the world, only the efficiency of description may be questioned. Every mathematical symbol has its equivalent in natural languages, but the opposite of this statement is not true, not every expression of natural languages has its equivalent mathematical one. The symbols of mathematical expressions need to be defined in natural languages (in Hungarian, in English etc.) for empirical economical status of theories expressed in mathematical form therefore the preciseness of mathematical expression is in this sense the same as the verbal one.

The mathematician Karl Menger presented with many examples, that mathematical presentation in economics is not more precise than use of natural language. For clarifying widespread mistakes it is worth to quote a larger part of Menger's paper:

'AUSTRIAN FORMULATION. For each good, the utility of a larger quantity is greater (or at any rate not less) than a smaller quantity, whereas the marginal utility of the larger quantity is less (or at any rate no greater) than that of the smaller.

MATHEMATICAL FORMULATION. If q denotes the quantity of a good, and u its utility, then

$$u=f(q), du/dq=f'(q)>0, \text{ and } d^2u/dq^2=f''(q)\leq 0.$$

To this day, some mathematical economist believe that these formulae express more than the simple words of the Austrians and, furthermore, that they describe the situation more precisely. But neither of these claims seems to be justified.

Far from saying more, those formulae actually say less than the Austrian formulation since they express the same assertion under an additional, if tacit,

⁹ See Mises (1966).

¹⁰ As Kumaraswamy comments Debreu's claim to mathematical formalism, 'however, we are not told whether the rigour sought by other schools of mathematics might not be based on alternative concepts of 'correct reasoning'. Are we being told that, say Wickseil or Simon, reasoned incorrectly and obtained false results simply because they were not followers of Hilbert or Bourbaki?' (Kumaraswamy, 1996, p. 253).

hypothesis, viz. the assumption that the function connecting utility with quantity admits a second derivative and its graph, therefore, has a curvature at each of its points – an additional hypothesis that clearly is not anchored in economical facts. The Austrian formulation of decreasing marginal utility is more general since it is valid even if there are places where the function does not admit a second derivative and its graph has no curvature, whereas at such places the mathematical formulation fails to assert anything. (...)

Many mathematical economists since Cournot have used tacit assumptions in the theories of return, supply, demand, and so on, by assuming continuity and differentiability of functions as though these properties were matters of course, whereas they are nothing but prerequisites for application of classical analysis and not based on facts. This point deserves being stressed since mathematical economists consider it as one of the advantages of their method that it brings tacit assumptions of verbal formulations to the surface. The Principle of marginal Utility is a case – and as we shall see, not the only one – illustrating the opposite situation’ (Menger, 1973, pp. 40-41).

This is a comprehensive survey. The reason for existence of continuity assumptions in economics is only exceptional. Marginality in economics refers not to an infinitely small unit, but to the relevant unit. It can also be added that examples for buttressing the popular statement, that the mathematical formulation brings tacit assumptions of verbal formulation to the surface are difficult to find. The alleged tacit assumptions can be discovered through thought, not through mathematics. It is groundless to oppose the clarity of mathematical expression to the vague verbal one. The right way is to draw a parallel between the mathematical and a sensible, intelligent verbal expression.

Mathematics can be used successfully in natural sciences. However, this fact does not legitimate such uncritical use in economics. The great difference is, that in physics not only the theory of mechanics, optics, vibrating strings and electrical attraction can be added but there also exist tools for measuring variables used in mathematical formulation of theories. In economics a theory of value, capital or business cycles can be presented in mathematical form but no instrument exists for an accurate and unambiguous measuring of the variables used in theories.

Monist misconceptions 7: Teleological explanation is unscientific

The abolition of teleological explanation from natural sciences after the Middle Ages was an inevitable and advanced step for the exile of the anthropomorphic elements from inhuman phenomena. The planets, stones, atoms have no aims and goals, the animal behaviour is conducted not consciously. However, the claim to eliminate teleology from human sciences is an unjustified monist view. People have goals, purposes, preferences, they choose among various courses of action, they learn and change their views. The abandonment of teleology would mean the abandonment of the very real human concepts, like learning, choosing, purposes.

Monist misconceptions 8: To refutation a theory it is necessary to propose a superior alternative

According to a methodological stereotype to refute a theory it is necessary to present a better one. This claim also stems from natural sciences also. As Kuhn writes, ‘once it has achieved the status of paradigm, a scientific theory is declared invalid only if an alternate candidate is available to take its place’ (Kuhn, 1970, p. 77). ‘To reject one paradigm without simultaneously substituting another is to reject science itself’ (Kuhn, 1970, p. 79). In the field

of physics it might be legitimate postulate. In economics and other sciences it is part of a defensive tactic by the supporters of criticized theories which are in dominant position among the different theories. In multi-paradigmatic sciences, as economics, pure criticism has a very important task, namely throwing light on the epistemological, methodological, logical, empirical weakness of competing theories (or 'paradigms') and the inadequate applications of some methods. The criticism without explicit positive alternative also has its justification. Every critique strengthens the competing theories in an automatic way, without permanent mentioning of the advantages of one of the competing theories.

Institutional side of monist misconceptions and the critique of critics of pluralism

Monist misconceptions are not based on a firm logical or empirical ground. After all each misconception stems from false parallels between economics and natural sciences. The misconceptions are legitimated through institutional elements and are strengthened by the power of inertia. Teaching in most universities suppresses systematically students' criticism of monist authorities. When someone has acquired and become accustomed to monist misconceptions, he or she does not abandon them easily. Econometrics, mathematical economics, unreal formalism have strong positions thanks to leading academic journals, associations, research foundations, conferences, government bureaucracy and universities in post-graduate and doctoral programs. It is supported by philosophers and sometimes also by natural scientists who propagate the idea of the unity of science without grasping the specific problems of various disciplines. However, monism is not an exclusive viewpoint, the above mentioned misconceptions have a share in heavy critics and they have sound alternatives, from which some Austrian example was mentioned briefly.

Every type of pluralism has an a priori advantages to monism, that is, a sound attitude to open ended intellectual quest and the diversity of research interest rather than artificial restriction of research through method. 'Since economics as practiced in the English-speaking world is strongly oriented toward mathematical models, any economic argument that has not been expressed in that form tends to remain invisible' (Krugman, 1990, p. 3). 'The fact that formalists have claimed the term 'economic theory' for their approach alone, and that the rest of us have let them get away with it, is a disgrace' (Chick, 1998, p. 1868). If formalists would stop insisting the unjustifiable belief that they were the representatives of the only true economics, the contrast would be decreased between non-formalists and formalists.

It must be admitted that rightful critique of some type of pluralism exist. Pluralism is in many respects more heterogeneous than monism, therefore it is not difficult to find works which have suffered from the weaknesses in argument, logical inconsistency and vague writing. However, monist critics concerning alternative views rely mostly on the misconceptions set out above. Most of all on the sixth misconception: the false dichotomy between mathematical and literary economics. Additional arguments frequently rest on simply psychological, political and institutional elements. For instance, one favourite 'counter-argument' to the critics of mathematical and statistical treatment of some issues is that the critics are not well trained in mathematics and statistics. It is also common practice to point at institutional academic competitions and at the incompetence of 'outsiders'. To mention only one typical example, according to Krugman, who is labelled on the backside of his own book 'one of the world's most thoughtful and innovative economist', 'many of those who reject the idea of economic models are ill-informed or even (perhaps unconsciously) intellectually dishonest' (Krugman, 1995, p. 79). Krugman understands 'economic models' to mean only those models which are expressed in algebraic form and are based on various unreal assumptions. As Krugman states in the following: 'The problem is that there is no alternative to models' (Krugman, 1995, p. 79). 'In fact, we are all builders and purveyors of unrealistic

simplifications' (Krugman, 1995, p. 80). Krugman's only argument is his a priori belief in absolute and only truth of his methodology which is based on misunderstandings and half-truth. He also leaves the many critics concerning his method unanswered. He admitted in an interview held with him and Fujita that one part of critics concerning his 'spatial economy' was generated by his 'insufficient attention of previously published works' (Fujita-Krugman, 2004, p. 149). Fujita's response to this assertion: 'In my opinion, such an apparent rejection by some traditional geographers arose not simply from an insufficient review of previous literature. Rather, it was based on emotional grounds' (Fujita-Krugman, 2004, p. 150). This typical reaction shows the lack of critical rationalist attitude, that is, openness to criticism, propagated by Popper.

Purchasing power parity theory and the theory of optimal currency area: examples for methodological confusionism

Illustrating some monist misconceptions, the two chosen theories have an other common characteristic, that is, they are based on an insufficient treatment of space.¹¹ Calculation of purchasing power parity has two entirely different applications. The first is as an economic indicator, a description of local economies. This is harmless and contributes interesting information to economic history. The second application is a theoretical one, in explanation of development and connections between national price level and exchange rates. This theory can be read in every elementary textbook of international economics. The theory is entirely fallacious for the following reasons. It is also without spatial and economic theoretical basis. After the tacit assumption of purchasing power parity theory, the national economies are spaceless, dimensionless points. Inside the countries the price level is constant everywhere. In reality the countries have spatial extension and the price level varied at different points within the countries and its temporal change is also different. As regards to general price level, it is only an abstraction. In fact only the individual prices exist, and one sort of general price level is extracted from the individual prices by the help of weighting, sampling and other auxiliary assumptions.

Purchasing power parity doctrine is examined by sophisticated statistical techniques.¹² These examinations suffer lack of support from the theory of statistics and probability. There is also an extra epistemological problem in this type of testing: the theory is based on unreal assumptions, which restrict the validity of the theory to an dimensionless imaginary world in which the transactions of goods are costless. In contrast to the theory, data employed in testing it originate from the real world, where the countries have extensions and the transport has cost. This situation makes the 'testing' worse and more unreasonable than one proving Pythagoras' theorem by measuring real triangles. The latter would also be absurd, but in this case measurements can be made and the assumptions on which the theorem based can be treated as intuitively true, because the connection between imaginary and real points, lines and circles can be created without a problem. In the case of PPP doctrine this is different, it is based on a false treatment of space and an unjustified aggregate view with immeasurable variables.¹³ This procedure is at the same time positivist (the test is grounded on observation

¹¹ I have dealt with spatial problems in an extended size in my book (A területi elemzések alapjai [The foundations of spatial analysis]).

¹² This 'testing' of purchasing power parity theory is very popular, almost exclusive treating PPP in mainstream journals. For example see Crownover-Pippenger-Steigerwald (1996), Feensrta-Kendall (1997), Frankel-Rose (1996), Frenkel (1978), Johansen-Juselius (1992) O'Connell (1998), Oh (1996), Ott (1996), Papell (1997), Pippenger (1993).

¹³ Mises had presented the sources of the mistakes of purchasing power parity theory as early as 1912 (Mises, 1980, pp. 95-102, pp. 195-203, pp. 215-223.). The theory was criticised by Ohlin also without any effect on mainstream theory (Ohlin, 1968). Similar counter-arguments can be found in Jetzer (1979).

statements) and strongly anti-positivist (the theory is grounded on unreal, unempirical assumptions) therefore it can be termed as methodological confusionism.

Turning now to the theory of optimal currency areas, this theory suffers from the same deficiency as purchasing power parity theory: false treatment of space and focusing on the connection between aggregated indicators without sound attention to original particles. On top of this it postulates the simple adjustment of monetary policy.¹⁴ The discussion about the theory after Mundell's article treats of matter of detail in first line and not of the conceptual mistakes. Labour and capital are not homogenous and labour is not mobile through space and through different branches of industry. Mundell's definition of region mixes the functional (factor mobility) and homogenous (uniform) elements (Mundell, 1961). The interregional flows belongs to functional elements. However, functional (nodal) regions, in the case of economics, do not have firm borders, the space divided to functional regions are continuous and it consists of overlapping regions. The modifiable areal unit problem is not mentioned, the very well-known fundamental problem in spatial data analysis namely, that all results of quantitative methods are potentially influenced by the method of spatial delimitation. Further problems are not discussed here as the above should be sufficient to serve as a clear illustration of the point.

Conclusions

The methodology of economics is an interesting area of study not merely for its own sake, but for the importance of sound practice of economic theory building and to serve sound policy prescriptions. The misconceptions about the nature of economic science lead to inadequate theories and they have had a negative influence on the development of economic theory. The misconceptions can induce the reduction of obtained knowledge in economics and the rebirth of old and refuted mistakes in a new form. It is easy to find theories, in which incompatible methodological prescriptions get haphazardly muddled and despite their inconsequence is being used to support political decisions. There are many theories which are 'elegant', 'rigorous' and 'great' but they have absolutely nothing to do with reality. While this type of reasoning in economics remains in a dominant position, a substantial part of the resources of research has to be used up in refuting them.

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¹⁴ See Block on Mundell (1999).

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