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I. ARTICLES

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FORMALISM AND MACROECONOMICS – A POST-KEYNESIAN PERSPECTIVE

Much of today's economic research is characterized by a uniform methodology: the use of formalism. Although it is unquestionable that economics as a social science has gained scientific strength through history by use of a formalistic approach, not all relevant economic information can be measured quantitatively and put on a mathematical mode of expression. Economics is not quite like the natural sciences. Much economic evidence is qualitative in nature. So one ought to discuss the use of formalism more critically than is common practise today. And the scientific community should accept that a more methodological pluralistic approach could further enhance the progressive status of economics.

INTRODUCTION

In Blanchard (2000) the development of macroeconomics from the pre-1940s until the end of the century is evaluated and highlighted. And the story that Blanchard tells is a happy one. From the very beginning we are told that "progress in macroeconomics may well be the success story of twentieth century economics" and that economic science should be characterised by the fact of "a surprisingly steady accumulation of knowledge", (Blanchard 2000 p. 1375).

Looking at modern macroeconomics today we are told that modern theory is solidly grounded in a general equilibrium structure. Modern models characterize the economy as being in temporary equilibrium, given the implications of the past, and the anticipations of the future. They provide an interpretation of fluctuations as the result of shocks working their way through propagation mechanisms. Much of the current work is focused on the role of imperfections (Blanchard 2000, p. 1402).

And much of the earlier debates within macroeconomics seem to have vanished almost completely. Perhaps as a consequence of, as Blanchard puts it, the a-ideological character of modern macroeconomic research. But can

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ideology really be put aside completely? Is economics no more a true social science? Or has economics developed to become too much similar to natural sciences?

However impressing the presentation by Blanchard is, one ought to discuss also perhaps in some detail the mainstream methodology of economics. In what follows, I present a personal, somewhat critical view, inspired by Keynes's attitude towards economics as a social science and the views held by the modern camp of Post-Keynesians, on the ruling methodology of today's macroeconomics which I find have gone too far in its excessive use of formalism. The main purpose of the article is primarily to put forward some important questions which modern economists should bear in mind, rather than to present some clear-cut and simple answers.

ON THE USE OF FORMALISM

Ever since the introduction of marginalism in economic theory and the victory of the neoclassical paradigm, macroeconomics has become more and more mathematical in its substance and in its mode of expression dealing with a formal-logical modelling of fundamental socio-economic phenomena. As a natural consequence of this nowadays there seems to be less room for the more qualitative aspects in macroeconomics. Our business has become more quantitative in its representation. Of course, we acknowledge that real life is not all about phenomena that can be accurately measured. When we address the public and between ourselves discuss economic problems of the day we also take, one must hope, the relevant more qualitative aspects into consideration. But looking upon the academic life on campus we hardly get any credit and certainly no merit for our efforts within what often is called the soft side of economics.

Perhaps the majority of professional economists find this very good, to be sure most of us have learnt to live under these conditions but still some (Post-Keynesians in particular), although not many, would argue that economics is and has always been a social science that has also to address and discuss matters of a more qualitative character. According to Blaug (2001) some even leave mainstream economics and go to more heterodox fields for research as a consequence of this. Or as Blaug (2001, p. 147) expressed it:

> If you are philosophically inclined – an intellectual rather than a technocrat – but are attracted to economics because of its policy relevance or the belief that society rests essentially on economic foundations, you may well find yourself drifting towards history of economic thought ... history of economic thought is a haven for heterodoxy,

a heterodoxy which no doubt has many sources but at its foundation takes its departure ... from a certain type of mind, a certain congenial style of thinking.

The degree to which economics is or should be more or less qualitative is not a new discussion among economists. Looking upon history John Maynard Keynes expressed himself very clearly in his correspondence with Roy Harrod in 1938 where he discussed the role of econometrics as a new field of economic research. Keynes was working on a review of the early works of Jan Tinbergen, which appeared in The Economic Journal in 1939. Although sceptical in his views on econometrics, Keynes recommended Tinbergen nevertheless to continue his efforts and to follow up his early work. In his review of Tinbergen he pointed out that: "the main **prima facie** objection to the application of the method of multiple correlation to complex economic problems lies in the apparent lack of any adequate degree of uniformity in the environment", (Keynes 1939, p. 316).

Or as he expressed himself in the correspondence with Harrod:

In chemistry and physics and other natural sciences the object of experiment is to fill in the actual values of the various quantities and factors appearing in an equation or formula; and the work when done is once, and for all. In economics that is not the case, and to convert a model into a quantitative formula is to destroy its usefulness as an instrument of thought ... economics being a moral science ... it deals with introspection and with values ... it deals with motives, expectations, psychological uncertainties ... economics is a science of thinking in terms of models ... which are relevant to the contemporary world ... because, unlike the natural science, the material to which it is applied is ... not homogeneous through time (Keynes 1938).

But this is not to say that micro- as well as macroeconomics does not deal with more measurable empirical matters. Quantitative phenomena can and should be measured in the best way possible. But not all economic phenomena are measurable. You have to allow methodology to be pluralistic, which is exactly Keynes's point. In some respect, the scientific approach of the natural sciences can be used effectively. But it cannot be used successfully in handling all kinds of economic problems. Or in the words of Joan Robinson (Robinson 1976, p. 26): "Without the possibility of controlled experiment, we have to rely on interpretation of evidence, and interpretation involves judgement; we can never get a knock-down answer". Later on Robinson becomes even more sceptical about the economic science as she writes not without irony:

... lacking the experimental method, economists are not strictly enough compelled to reduce metaphysical concepts to falsifiable terms and cannot compel each other to agree as to what has been falsified. So economists limps along with one foot in untested

hypotheses and the other in untestable slogans. Here our task is to sort out as best we may this mixture of ideology and science (Robinson 1976, p. 28).

So Robinson too is sceptical if the use of mathematics in economic theory becomes too extensive and according to Davidson (1991, p. 23) she once should have said about her own research "I never learned to use mathematics to develop theory; therefore I had to learn how to **think** about problems".

Of cause, there are similarities as well as differences between different kinds of sciences. Perhaps scientific breakthroughs in the natural sciences has more of "a once and for all" character than those of the social sciences. The natural sciences is at least in some respect a-historical because a given experiment can be made over again and again right until one is certain that the relationship one has found actually is a correct and a stable one. The scientific achievements within the natural sciences are in this respect independent of the economic, the social, and the psychological environment in a way that is not the case with economic research. Ours is the case of interdependency. Economic problems have to be seen in a given historical context:

...economic ideas are always and intimately a product of their time and place; they cannot be seen apart from the world they interpret. And that world changes – is, indeed, in a constant process of transformation – so economic ideas, if they are to retain relevance, must also change (Galbraith 1987, pp. 1–2).

With inspiration from the natural sciences we might have invented Homo Economicus, but as Thaler (2000) has pointed out, this is not a very good representation of the living modern Homo Sapiens.

Also Payson (1997) is critical about the scientific status of modern economics. Has economics the right scientific status as it ought to have when judged as a social science discipline? Although much modern macroeconomics, for instance by the acceptance of the rational expectation hypothesis, is characterized by the heavy use of mathematics and thereby have become properly scientific, as many economists would argue, is such an approach really always helpful in understanding the true nature of economic phenomena? Perhaps formalism is quite convenient to use when you make a theoretical analysis but one should be aware as Payson points out that:

... mathematics does not underlie economic phenomena – human and institutional behavior do, and that involves psychology and sociology ... the only explanation for the fact that science is not used very much in economics is that most economists today are simply carrying out the only functions they have ever learned to perform: high-powered mathematics, neoclassical synthesis, and "scholarship" in the game of getting published (Payson 1997, p. 260 & p. 273).

A similar line of critical argumentation can be found in Boulding (1971). In this, (Boulding 1971, p. 233), he argues that the job of many mainstream economists is in danger of becoming more and more: "an endless modification of variables and equations in regions of strongly diminishing returns in the knowledge function, and still sharper diminishing returns in the significance function". These views are in good accordance with the attitude presented in Patinkin (1976). Like Payson, he is also worried that economics is in danger of losing scientific power if, as Patinkin fears, it should manifest itself as a discipline where:

> ... symbolic pseudo-mathematical methods of formalizing a system of economic analysis ... which allow the author to lose sight of the complexities and interdependencies of the real world in a maze of pretentious and unhelpful symbols (Patinkin 1976, p. 512).

And scepticism concerning the use of formalism in economics is far from a new phenomenon. Even Alfred Marshall – one of the fathers of the neoclassical paradigm – advocated the case of thoughtfulness when using mathematics in economic theory as is shown in his correspondence with his friend A. L. Bowley. In 1906 Marshall wrote what later has become a very famous quotation:

> ... I had a growing feeling in the later years of my work at the subject that a good mathematical theorem dealing with economic hypotheses was very unlikely to be good economics: and I went more and more on the rules – 1) Use mathematics as a shorthand language, rather than as an engine of inquiry. 2) Keep to them until you have done. 3) Translate into English. 4) Then illustrate by examples that are important in real life. 5) Burn the mathematics. 6) If you can't succeed in 4), burn 3). This last I did often (quoted from Landreth & Colander 1994, pp. 290–91).

In Keynes's biography on Marshall, he quotes Marshall's views on the need to study political economy and what he found out about economics throughout his long life (as he himself wrote in retrospect about the year 1917):

> I ... regarded myself as a wanderer in the land of dry facts; looking forward to a speedy return to the luxuriance of pure thought. But the more I studied economic science, the smaller appeared the knowledge which I had of it, in proportion to the knowledge that I needed; and now, at the end of nearly half a century of almost exclusive study of it, I am conscious of more ignorance of it than I was at the beginning of the study (Keynes 1924, p. 171).

Therefore, according to Marshall theory could never stand alone. It always had to go hand in hand with empirical evidence. If one does not work along these lines one runs the risk of getting a theory constructed that is too far away

from real life economic phenomena. As Beed & Beed (2000) have pointed out one of the dangers of giving to much way to pure formalism is that:

'Theory' is no longer seen as propositions purporting to describe, explain, or predict the real world. It is becoming schemata describing how the real world **might** look if people behaved in the way the theory suggested. Economic theories describing how people **actually** behave are less common.

But such an approach has not always been easy to pursue in economics as the history of economic thought tells us. On the contrary, it has often been seen by many as a long defeated research strategy. As Keynes stated in his biographic essay on Thomas Malthus, the blame for this ought perhaps at least partly to be ascribed to David Ricardo, as he - in clear opposition to Malthus, who in this respect is the hero of Keynes - came to dominate completely the methodology of economic theory in its early years: "... that ... the complete domination of Ricardo for a period of a hundred years has been a disaster to the progress of economics", where Malthus, in a much better way than Ricardo, blended theory with empirical evidence as he worked to give:

... formal thinking to the complex confusion of the world of daily events ... so as to penetrate these events with understanding by a mixture of intuitive selection and formal principle and thus to interpret the problem and propose the remedy (Keynes 1935, p. 98 & p. 107).

Of course, Keynes's sympathy towards Malthus's approach reflects the way he himself tried to work scientifically. Economics is concerned with problem solving and some of these problems are empirical in nature. So why not try to blend inductive information about the way the economy is supposed to work with deductive logic? The development of society and the development of macroeconomic theory cannot and should not be seen apart from one another (this approach of Keynes's has been termed a realytic approach by Landreth & Colander (1994, p. 463) as they state: "a realytic theory is contextual"). Although perhaps it is easy to understand the relevance of this approach it is not always a simple task to follow such a research strategy for the economist. It can indeed be very difficult to change ones preconceived opinions about the accepted state of affairs as Keynes himself very elegantly pointed out in the preface to **The General Theory**:

The ideas which are here expressed so laboriously are extremely simple and should be obvious. The difficulty lies not in the new ideas, but in escaping from the old ones, which ramify, for those brought up as most of us have been, into every corner of our minds (Keynes 1936, p. xxiii).

Another problem related with too much use of formalism in macroeconomics is the problem of how to incorporate in the right way historical time in economic models. As John Hicks has argued, one has to acknowledge that: "Economics is in time, in a way that the natural sciences are not. All economic data are dated ... time is a device which prevents everything from happening at once" (Hicks 1979, p. 41). Economists ought to remember that economic activity, be it production or consumption, consists of very time consuming processes. And if you accept this, then what about the concept of equilibrium and the universal use of general equilibrium models and rational expectations which only allow economic agents to make stochastic errors in modern macroeconomics? Can GE-models really handle the problems of incorporating historical time successfully? What if economic behaviour is not characterized to a significant part by simultaneous action, perfect competitiveness, rationality or perhaps more importantly institutional stability? If: "economic theory cannot be static when its object of investigation is fundamentally historical" (Davis 1989, p. 436), should a non-contextual approach then be accepted as the predominantly right one? According to Davis one has to take account of these aspects and very critically discuss the advantages and disadvantages of general equilibrium models before using them without hardly any methodological concern at all:

> Axiomatic General Equilibrium Theory, however, embodies a theory of concept development that is inescapably ahistorical. That the full elaboration of its key notions proceeds a priori through a conceptual analysis tied to formal demonstration of market-clearing and optimality means that these notions cannot accommodate any development of the economic process which fails to reflect these results. The Theory itself, that is, is a closed logical structure out of time, which must accordingly be representationally inadequate ... Axiomatic General Equilibrium Theory is then ... simply unrealistic (Davis 1989, pp. 436–37).

The argument of Kaldor (1972, p. 1237) runs along similar lines as he sums up his views on the use of the concept of equilibrium economics. According to Kaldor this

> has become a major obstacle to the development of economics as a **science** – meaning by the term science a body of theorems based on assumptions that are **empirically** derived (from observations) and which embody hypotheses that are capable of verification both in regard to the assumptions and the predictions.

This very critical assessment of the relevance of using GE-models was immediately questioned by others as being too hard and too narrow-minded, e.g. by Hahn (1973).

And yet, although Davis's concern is both understandable and important, do not all micro- as well as macroeconomic theories lack realism at least to some degree? To get a theory operational and working do we not have to give up the ideal of perfect realism? But of course, the trade-off between realism and operationalism is a very crucial one. As Keynes warned us in 1936, one ought not to assume all difficulties away just to get a handy and smooth running theory. "It may well be that the classical theory represents the way in which we should like our economy to behave. But to assume that it actually does so, is to assume our difficulties away" (Keynes 1936, p. 34).

But although the use of formalism in economics should give reason to critical and methodologically inclined assessment formalism also brings about many advantages to the economist. For instance, it can give us a more structured presentation and a better general view of a perhaps very complicated theory than just qualitative arguments alone. And you can build a model, collect data, and make estimations and simulations upon which you should try to falsify or to corroborate the proposed theoretical hypotheses and statements. And perhaps the econometric efforts can result in not only better prognoses but also give room for a more qualified debate on economic affairs and even help the politicians to conduct economic policy in a more accurate way. Although one should be careful not to overestimate the blessing of econometrics as Keynes warned us already in the 1930s. If Hutchison (1994) is right in his conclusion arguing that the economies of today are more dynamic and volatile in behaviour and institutional structure than in the past, then the assumed stability that lies behind much econometric work breaks down. Then Rodrik (2000, p. 177) may still be right in claming that "economists rank second only to astrologers in their predictive abilities". Aside from this, formalism can be a very handy pedagogical tool when one generation of economists tries to pass on the scientific knowledge gained through history to the next generation. Used with care and insight, formalism can make science grow progressively to use a Lakatosian term.

Some economists even argue to the point that it is only through the use of more formalism in economic theory that economics as a science has gained strength. For instance, Lazear (2000) points out that economics is more scientific than all other of the social sciences exactly because of a more cogent reasoning. In economics we assume that agents behave rationally and effectively in an environment of equilibrium, and we can formulate these actions of the rational economic man in precise mathematical terms. And in doing this, we can make the best of both worlds because according to Lazaer (2000, p. 102): "economics is scientific; it follows the scientific method of stating a formal refutable theory, testing the theory, and revising the theory

based on the evidence". But is this really a true picture of modern macroeconomics? To some it might seem a little bit too prosperous to be true. Perhaps Niehans (1981, p. 174) is not all that mistaken in saying that:

> Hardly any economic theory is ever empirically falsified. It rather falls into disuse and is forgotten, perhaps to be rediscovered decades later ... economic doctrines are usually tested not by systematic methods, but by a Darwinian struggle for survival in the arena of history.

And economics is not like the natural sciences because in many important respects economics is "a science of unique events" (Niehans 1981, p. 175). If that is to be the case then we are bound to have a problem with falsification in economics. It does not come in as handy in economics as is the case with the natural sciences. But the problem of the economist is not only to make tests and try to falsify hypotheses. He must also give way to try

> to "explain" the past and, by so doing, learn something that might be useful in the future. In the chaos that reality really is, he tries to create little islands of intellectual order, in the ever-changing flow of history he tries to create durable and predictable patterns (Niehans 1981, p. 167).

Then to Niehans and Blaug (2001) and many others, economic history and the development of economic thought matter. And the study of these two disciplines need not be very formalistic in their approach.

CONCLUDING REMARKS

Above all science is about explanation and the solving of problems. In performing our task we have to apply the right methodology. Although mainstream macroeconomics might agree upon that the methodology should be one of formalism, not all relevant economic evidence can be found or put in quantitative terms. Economics is a social science. And economic actions performed by bounded rational people in imperfect markets, in constant changing societies happen in historical time. So economic phenomena and their actual development are path dependent in a very crucial way. Or as Davidson (1991, p. 35) states:

> Our knowledge about economic events occurring through time is, however asymmetric: although we may know the past, we cannot be sure that we have any reliable knowledge about the economic future. The future remains to be created by human actions and is not merely determinated by some immutable economic law. In other words, for many important economic activities – especially long-duration ones –

information about future current outcomes does **not** currently exist. The economic future is yet to be created and is not predetermined by existing rules or economic laws.

And the expectations people form may be rational in the way that they always try to make the best expectations possible but that is not to say that they only make stochastic errors when their planned economic activity is realized. Human behaviour is not rational in the way the rational expectation hypothesis argue.

So macroeconomic theory is bound to be contextual if it wants to be successful. And in gaining scientific success formalism has its place. But formalism cannot do the job alone. You have to accept that not all economic relevant information is quantitative in its nature. You have to give way to qualitative evidence as well. And you have to realize that most scientific economic knowledge is not easily bought. Not even with advanced econometric tools and as Johnston (1991, p. 52) has warned us, we should be aware of the possible dangers of modern techniques:

> It is thus all too possible for someone to activate an econometric software package, of which he has only a dim understanding, to apply it to data of whose nature and provenance he is ignorant, and then to draw conclusions about an economic situation, whose historical and institutional realities he has, perhaps, not studied in any depth.

Therefore, there should also be room for at least a minimum of knowledge of how economic thought and society developed in the past as argued by Blaug (2001).

If you accept that economics is a true social science and give way to more methodological pluralism than just the enhanced chase after the virtues of formalism alone and overcome the uniformities of many of today's academic circles within economics, then the future for economic science looks very prosperous indeed. With a slight rewriting of a well known economic statement we could then have it that macroeconomics does matter and so do perhaps also economists in the future to come as was the case in the past.

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