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Contemporary Economics

30.1 ECONOMICS AND ITS PAST

One of the main features to emerge from this account is the enormous degree of continuity involved in the development of economic analysis. The most substantial break is probably between the classical and Jevonian theories of value, yet even here there are important elements of continuity. In other branches of the subject (the theory of international trade, monetary economics, the theory of the cycle) it is hard to detect any discontinuity. The same is true of the break referred to as the Keynesian revolution: even in the field of macroeconomics there were important elements of continuity.

Despite this underlying continuity, however, change was sufficiently rapid, and sufficiently far reaching, around both the 1870s and the 1930s to justify the use of the word "revolutionary". It is important to note, however, that these revolutions involved much more than simply the emergence of a new theory, or theoretical framework. Referring to the Smithian, Jevonian and Keynesian revolutions, Hutchison has distinguished four, very different, types of change.

(1) New policy objectives are urged or given much greater priority than previously.
(2) There may be changes in interests, or research priorities.
(3) A new terminology, or conceptual framework, may be introduced.
(4) There may be changes in testable, refutable empirical content.[1]

These are, of course, interrelated. In addition to these types of change we could add changes in intellectual standards: the "marginalist" revolution occurred at roughly the same time that economics was becoming established as an academic subject, something which may have been a factor behind the increasing preference for more formal, if somewhat narrower, analysis.[2] Similarly, the Keynesian revolution is hard to separate from the quantitative revolution which took place at around the same time. There was, therefore, much more to the changes which occurred in both the 1870s and the 1930s than can be encompassed within either the Kuhnian or the Lakatosian framework.

Although they are clearly incapable of explaining all the developments which took place in the 1870s and the 1930s, the theories of Kuhn and Lakatos are nonetheless useful. Many aspects of these revolutions can be explained in terms of Kuhnian or Lakatosian terms. The Jevonian and Keynesian revolutions, and the rise of monetarism in the 1970s, for example, exhibit many features of Kuhnian revolutions: empirical anomalies which become too important to be neglected; increased interest, in times of perceived crisis, in methodological issues; losses as well as gains when economists move from one "paradigm" to another; and so on. In addition, it is easy to identify phases of "normal science", in which research is directed towards solving Kuhnian "puzzles". Similarly, the changes which took place in the 1870s and the 1930s exhibit many of the features of Lakatosian research programmes. Both the spread of marginalist economics in the 1870s, and the spread of Keynesian economics in the 1930s and 1940s, can be seen in terms of economists switching from a research programme thought to be degenerating into a progressive one.

30.2 THE STATE OF CONTEMPORARY ECONOMICS

Criticisms of contemporary economics

Economic theory has, especially since the 1970s, come in for a barrage of criticism, to such an extent that several commentators have written of a "crisis" in economic theory.[3] Economics, by which is usually meant mainstream, "neoclassical" economics, has been criticized for having no empirical content, for being too abstract, and for amounting to little more than intellectual game-playing.[4] In responding to such criticisms, however, it is important to separate two, distinct, issues. (1) Is the mainstream approach misguided, and in need of replacement with some other approach? (2) Is the emphasis (i.e. the allocation of intellectual resources within the research programme) wrong and in need of improvement? These questions are very different, and need considering separately.

Criteria for appraisal

To answer these questions we need a standard of judgement. It will therefore be assumed that the task facing economists is to produce empirical propositions: propositions which could be falsified, but which have survived attempts to falsify them. There may be severe limitations as to the extent to which this is possible, but it should nonetheless remain the underlying objective.

There is, however, as was pointed out in chapter 1, more to the question of methodology than this. Of particular importance is the fact that theoretical propositions are not isolated, but form part of a larger theoretical structure. The unit of appraisal has therefore to be something analogous to a Lakatosian scientific research programme: the set of assumptions and procedural rules which is used to generate empirical propositions. Given that the aim of economic analysis is to generate empirical propositions, a research programme should be judged in terms of whether it is progressive or degenerating.[5]

This Lakatosian extension to the falsificationist methodology is particularly important in economics, for a number of reasons. Falsification, although in principle problematic in *any* discipline, is particularly difficult in economics. Firstly, because controlled experiments are rarely possible, it is rarely possible to test the various components of a theory independently of each other. Thus in practice many hypotheses have to be tested "indirectly": as economists use certain concepts they find that they can neither understand economic phenomena, nor explain empirical data, without them.[6] Such "indirect" testing may be considered inadequate, but frequently nothing better is possible.

Secondly, the subject matter of economics is itself changing: we cannot even assume that people always respond in the same way to economic stimuli. For example, it is quite plausible that people today respond to inflation differently than they did even 20 years ago. This has two implications. (1) It imposes limitations on the ability of economists to build up data with which to test their theories in the way that, for example, astronomical data has been built up over centuries. Pre-war data, for example, may be useless for testing hypotheses about how people behave today. (2) More important than this, changes in the economy mean that economic theories are continually being applied under new circumstances, under which they have never been tested. This means that economists will, or perhaps should, never have confidence in any empirical generalization, unless it has been generated by a theory in which they have confidence. Economic theory has therefore to be more than merely a means of generating predictions.[7] For these reasons it is worth evaluating contemporary economics as a Lakatosian research programme.

The "neoclassical" research programme[8]

Using the Lakatosian criterion, a strong case can be made in defence of the neoclassical research programme. In any research programme it is inevitable that there will be a hard core of assumptions which are provisionally accepted as irrefutable. In economics this hard core will inevitably be more important than in many disciplines. The difficulties involved in testing assumptions have already been mentioned. In addition, the complexity of the societies with which economists are concerned means that economic theories must, of necessity, neglect certain phenomena. The question to be asked of many of the assumptions made in economics is, therefore, not whether they are true, but whether they provide a useful way of isolating the phenomena which are important for the problem in hand.[9] The existence of unfalsifiable, or even descriptively false, assumptions,[8] therefore, does not *necessarily* render a research programme unsatisfactory, provided that the research programme is progressive.[10]

In considering whether or not the neoclassical research programme is progressive, we will assume that it has succeeded in explaining much of what has happened to the economy (it should soon become clear that by assuming this we are not conceding very much). Two questions can be raised concerning the manner in which economic phenomena have been explained.

The first question is whether or not economic events have been predicted *in advance*, or merely explained after the event? Whilst the former is obviously desirable, it is not *quite* so important as it might at first sight appear to be. Consider the example of including inflationary expectations in the Phillips curve. When Phillips curves were first estimated, from around 1960, they appeared to work, even though they did not contain any allowance for inflationary expectations. More important, however, if economists had tried, in their econometric work, to allow for inflationary expectations they might well have turned out to be empirically unimportant, either because the public was not then aware of inflation in the same way as it is today, or simply because there were then far more important factors affecting inflation.

The second, and more important, question is whether new facts have been explained in a way which preserves the integrity of the research programme, or whether they have been explained simply by *ad hoc* modifications to the theory. Here the record of mainstream economics would seem very favourable, for anomalies have frequently been solved not through introducing *ad hoc* modifications, but through applying the underlying maximizing model of behaviour to more and more problems. *Ad hoc* assumptions have been *removed* rather than introduced. Consider some examples. (1) Kuznets' data on long run consumption patterns were incompatible with the Keynesian consumption function.[11] This anomaly was accommodated by replacing an *ad hoc* generalization about the propensity to consume with a theory in which consumers maximized utility: the permanent income and life-cycle theories.[12] In the 1970s further problems arose in that the savings ratio rose dramatically. Again this was explained, not by any *ad hoc* expedient, but through applying the theory more carefully and defining income correctly so as to allow for the effects of inflation on asset-values.[13] (2) When he introduced inflationary expectations into the Phillips curve, Friedman (1968) was not making an *ad hoc* modification, but was making the theory *more* consistent with basic price theory. Associated with this, the recent literature on the microeconomics of unemployment has been concerned with applying the theory of maximizing behaviour to new situations in order to explain the new fact of persistent unemployment.[14]

Though these examples are taken from macroeconomics, similar examples can be found in virtually any field of economics. It is thus possible to conclude that mainstream economics has been reasonably progressive.[15] This, however, is only one side of the coin, for if one research programme is to be abandoned, it is necessary to find a better one to replace it. For many economists, none of the alternatives seems any better. As an example, consider Weintraub's explanation of what would be required to cause him to support the post-Keynesian research programme:

> I could be convinced by two distinct but related lines of argument.
>
> The first would involve a demonstration that the neo-Walrasian program was degenerating, and the second would involve a demonstration that the (post) Keynesian program was progressing.
>
> Demonstrating the former would require, at best, arguments showing repeated ad hoc adjustments of the neo-Walrasian hard core in order to assimilate anomalies. It would further require a relative absence of new empirical facts which were explicable with theories derived from the hard core.

> Neither am I aware of reduced scope for the program. Rather than drawing in its extension over time, it has helped to explain, in recent years, new facts from female labor force participation rates to migration flows, from the relationship between race and earnings to the decline in US fertility.
>
> Demonstrating the progressivity of the (post) Keynesian program would require, at a minimum, an articulation of the hard core, and heuristics, of the (post) Keynesian program. Repeated announcements of what post Keynesians do not believe does not constitute an investigative logic. Further, what are the successes of the program? What extension has there been in its domain? What previously inexplicable, or unrecognised, features of economic life has it illuminated?[16]

Similarly, Blaug argues that, for all its failings, "it is only orthodox, timeless equilibrium theory – in short, the neoclassical SRP – that has shown itself willing to be judged in terms of its predictions".[17]

The direction of research

If the above arguments are accepted, then much of contemporary economics can be justified as constituting part of a progressive research programme. Despite this, however, it would be wrong to use this as a reason for dismissing all the criticisms which have been levelled against contemporary economics. Even if there are reasons for continuing with the same approach, it is possible to question whether research is being directed towards answering the most important questions.

One argument that has been used is that the structure of rewards in the economics profession is such as to lead to an inappropriate valuation of the different activities to which economics devote their attention. It has been argued that: (1) there is an incentive to go for research which yields novel results rather than genuine insights;[18] (2) there is an excessive emphasis on economic theory, and on formal econometrics, rather than on more mundane empirical work, such as gathering new data;[19] (3) that there is a tendency to seek confirmations of theory, not falsifications.[20]

Broadly, the argument is that academic career prospects depend on publications. It is thus all-important for academics to produce work which stands a high chance of being published. This, it is argued, has adverse consequences. The need to satisfy editors and referees, many of whom judge papers in terms of their compatibility with neoclassical economics, makes it unsafe to produce work that conflicts with this approach. In addition, it is safer to produce a paper which offers some small, but definitely novel, development of a well-established, or fashionable theory, than to work on genuinely new problems. With the latter, not only is there a greater risk of failure, but even if the work is successful it may not be appreciated.

The counter-argument is of course that such procedures do serve to maintain academic, or scientific, standards. However, even if rewards were to accrue in proportion to the value of an academic's research, risk-aversion would stop individuals from choosing the research strategy which maximized the expected value of their output. They would choose to forgo a fraction of their expected earnings in exchange for a reduction in the uncertainty they faced. Having said this, however, it has to be conceded that theoretical work is probably too highly regarded relative to empirical work. Whether this is of much importance, however, is a much more complicated question to answer.

Similar arguments have been put forward to explain why there is so little emphasis on falsifying theories, and so much emphasis on confirmations of theories. It is argued that journals require significant regression results, and that as a result negative results do not get published. Again, though there may be some truth in this, it is worth pointing out that it is important to get results consistent with a theory, for the task of economics is not only to test theories, but to discover empirical regularities.

Whilst it may be true that excessive resources are being devoted to abstract theoretical work, it is important to emphasize that even the most abstract theory is not necessarily valueless. Consider, for example, the work in the 1950s on proving the existence of a competitive equilibrium.[21] Such proofs are *of necessity* highly abstract, but if the competitive equilibrium model is to be used at all, such proofs are important. Their function, in the words of one contributor to this literature, "is to assure the theorist that his model is not vacuous. And *he has no business taking this for granted*".[22] It is thus inappropriate to criticize work on existence proofs on the grounds of excessive abstraction. They may be criticized on the grounds that the competitive equilibrium model is not worth taking seriously, but that is a very different type of argument.

It can, however, be argued that economic theories have been too abstract in the sense that economists have pursued excessive generality. Though this may be true, it is important to note that it is sometimes only *after* considerable research has been undertaken that it becomes clear whether or not general results are going to be available. In addition, if models are oversimplified, there is the danger that factors which ought to be analysed and made the subject of economic theory, will be left unanalysed, and that definite theoretical relationships may be implied where none exist.[23] Until more general models are investigated it may not be clear which simplifications matter and which do not.

30.3 CONCLUSIONS

Progress in economic analysis seems most likely to arise as the result of economists investigating specific problems, using whatever techniques seem most likely to result in the production of testable, and tested, hypotheses, rather than through attempts to replace mainstream economics with something radically different. There are two reasons for saying this. Firstly, even though it may have many failings, mainstream economics does not appear to be fundamentally flawed and in need of replacement: it would appear to be at least as progressive as any of the alternatives offered.

Secondly, though progress has been far from uniform, economic analysis would appear to have progressed as much through the gradual accretion of knowledge as through revolutionary transformations in the subject. This is not to say that a new "paradigm" may not emerge, but that if it does, this is at least as likely to arise out of attempts to solve specific empirical problems, as to arise out of methodological criticisms of contemporary economics.

Acceptance of these arguments, however, does not imply a complete rejection of the arguments put forward by the proponents of alternatives to mainstream economics. One reason for this is that, of all the successes of neoclassical economic analysis, there are nonetheless vital issues where its powers are severely limited, perhaps the most important example of this being its failure to explain why some countries have grown so much faster than others. Where issues can be related to simple functional relationships between variables, neoclassical theorizing has shown itself extremely powerful, but where this is not possible, it is very weak. Another reason is that economists critical of mainstream economics have raised a number of important issues which should not be neglected. The Austrian and Post-Keynesian arguments about the implications of uncertainty, the Institutionalist arguments about the welfare criteria used in mainstream economics, and Simon's arguments about satisficing would seem particularly important. It is quite possible to recognize the importance of these issues without accepting the claim that mainstream economics is fundamentally flawed. For all its limitations, and these are many, neoclassical economics has, over the past century, been successfully applied to an ever-wider range of problems, including ones which had previously been considered beyond the scope of formal economic analysis.