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CONSOLATIONS FOR THE ECONOMIST: THE FUTURE OF ECONOMIC ORTHODOXY[1]

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1. Introduction

There are thousands of economists around the world devoting a large quantity of human capital to an enterprise, about which many of them are developing serious doubts. Modern economics has a tightly defined mainstream 'orthodoxy' and a fragmented 'heterodoxy' (Sheila Dow explores the distinction between 'orthodoxy' and 'heterodoxy' in her paper 'Variety of Methodological Approach in Economics'). Many economists are concerned that the mainstream of their subject has become far too narrow: more a brackish puddle of techniques than a sparkling torrent of ideas. Several features of this orthodoxy can be identified.

- It involves a particular approach to theory, based on the idea of a 'mathematical model' (utilising a limited range of mathematical techniques).
- It entails empirical work based on a limited range of econometric techniques. This empirical material has little or no connection with economic theory.
- It only acknowledges a limited range of theoretical approaches and is antithetical to pluralism. Orthodox economists often extol the benefits of economic competition, but they much prefer unregulated monopoly in the market for ideas. Mathematical modelling, discussed in Section 2, provides a handy barrier to entry to this market.
- It is largely technique-driven rather than issue-driven: valuing difficult technique and formalism above work of applied or policy relevance.
- It has an 'imperialist' attitude to cognate disciplines such as politics, sociology and economic history, seeking to incorporate them safely into economics, rather than learning from them.
- It has a tightly defined professional structure which, for example, dictates a narrow range of publication outlets (the so-called core journals) which, exclusively, lead to career development. This is reinforced in the UK by

> the Research Assessment Exercise (RAE), which links financial reward with conformity to the mainstream approach.[2]

Many economists feel that, unless things change, their subject will end up in utter sterility, with rewards going to those who can count the number of Bayes–Nash equilibria that can dance on a locally compact pin-head. Apart from intellectual vacuity and irrelevance there is also the prospect that, in countries where academic research is state funded,[3] the tax-payer may object to her taxes being spent on sterile formalism and seek to withhold further contributions.

Mainstream economists are herd animals and are aware of the dangers of egregious (outside the herd) behaviour. They have therefore developed effective mechanisms of enforcement to ensure conformity to orthodoxy. So-called core journals and their role in appointments and career development have been mentioned above, as has the British RAE. Any economist specialising in the history of economic thought, methodology or radical economics would never get appointed or promoted to an academic job in the UK today. Development economists, applied labour or industrial economists would have serious difficulty.

Mainstream economists may well reply that they are simply following an obvious example, namely the natural sciences. Feyerabend (1970, p. 198) remarks:

> More than one social scientist has pointed out to me that now at last he has learned to turn his field into a 'science' – by which of course he meant that he had learned how to *improve* it. The recipe, according to these people, is to restrict criticism, to reduce the number of comprehensive theories to one, and to create a normal science with this one theory as its paradigm. Students must be prevented from speculating along different lines and the more restless colleagues must be made to conform and 'to do serious work'.

and later (p. 209):

> The sciences especially are surrounded by an aura of excellence which checks any inquiry into their beneficial effect.

It is surely better to emulate the white-coated miracle workers of physics and biology than bearded, woolly minded sociologists. Physicists and biologists get their hands on a far larger chunk of public money than economists and they are revered by the public as 'experts'. Alas this line of argument has some obvious flaws. First, the methodology of the natural sciences may simply not work in a social science such as economics (this point is explored in Thomas Boylan and Paschal O'Gorman's paper 'Axiomatization and Formalism in Economics' and in Sheila Dow's paper 'Variety of Methodological Approach in Economics'). Second, most economists are woefully ignorant of the natural sciences and how research in those subjects actually proceeds (it is far more pluralist and takes empirical testing far more seriously than most economists realise). Third, public approbation of the natural sciences is on the wane, largely because the public do not distinguish science from technology (various assorted public policy disasters such as BSE[4] are blamed, perhaps unfairly, on 'scientific experts').

2. Mathematical Models

The most important skill an economist can have (and this includes undergraduates and postgraduates) is a good command of a relatively narrow range of mathematical techniques, largely dating back to the eighteenth and nineteenth centuries. (The mathematisation of economics is discussed in Vela Velupillai's paper 'Variations on the Theme of Conning in Mathematical Economics'.) These techniques provide the means to construct those objects which are central to the professional life of every orthodox economist (and some heterodox ones), namely mathematical models. At the simplest, such models start from mathematical assumptions and, after several pages of tortured mathematics, arrive at some conclusions. The laws of logic dictate that these conclusions were embodied in the assumptions, so the precise purpose of such an exercise is not immediately obvious. It does, however, provide a good opportunity for economists to demonstrate how clever they are, and simultaneously to provide a useful barrier to entry to the market for ideas.

The orthodox economist will often argue that mathematics is essential to make their arguments 'rigorous' or 'deep'. But it is not clear what sort of rigour or depth they are talking about. If asked 'do you subscribe to the Zermelo–Frankel axioms or the Axiom of Choice?', the orthodox economist will typically supply no answer. But, as any mathematician will tell you, these questions require an answer if mathematical rigour is to have any chance of survival. The argument for 'depth' must not be taken too far.

Mainstream economists are clearly strongly committed to their mathematical models but it would be a mistake to assume that economics research is more likely to gain acceptance the more mathematical it is. It is quite possible for economists to produce research which is 'too mathematical' or, more accurately, uses the 'wrong kind' of mathematics. Mainstream economists have invested heavily in a very narrow and limited range of mathematical techniques (calculus, linear algebra, game theory). So, for example, research involving topological techniques such as catastrophe theory or chaos theory is often rejected by the mainstream, because those techniques are not part of the standard mainstream toolkit. There is a striking contrast here with the natural sciences where many new areas have been developed using such topological techniques.

3. Empirics

A genuinely useful role for mathematical models would be to present economic theory in a form suitable for empirical testing. If any implication of a theory can be empirically refuted then the theory is refuted, and can be rejected or modified. The mainstream economist often argues that, despite appearances to the contrary, economics is an empirical subject. Surely the elaborate mathematical models, beloved of the theorist, are continually subject to empirical testing and will be cast into the dustbin of history as soon as they are refuted by empirical evidence. The example to follow is, once again, the natural sciences where, it is claimed, experimenters confront theory with evidence and reject it if it is found wanting. Experiments in economics are usually impossible[5] (Jinkwon Lee's paper

'Repetition and Financial Incentives in Economics Experiments' discusses some of the difficulties with experimentation) so economists typically use statistical data and econometric methods. But, as with economic theory, techniques have come to dominate ideas. The orthodox econometrician is rewarded for developing new and demanding techniques but not for assembling useful data sets. Most statistical data used by economists has been collected for purposes quite different from theirs (e.g. tax collection) and is riddled with inaccuracy and bias. The refined and sophisticated techniques of the econometricians are useless without reliable data to apply them to.

It may be that the underlying assumptions of a theory are not themselves open to empirical test but that testable implications can be drawn from them. However, there immediately arises the problem of verisimilitude. The underlying assumptions of any theory are unlikely to be exactly true descriptions of the real world but, one hopes, are close approximations to it. Under such circumstances it is important that the implications of a scientific theory are robust with respect to small variations in the underlying assumptions. Such variations should only produce small variations in the theory's implications, not wild and dramatic ones. Without this property empirical testing of theories becomes impossible, because of random environmental perturbations in the conditions under which observations are made. Consider, for example, a chemical theory which predicts the outcome of a particular chemical reaction under conditions of constant ambient temperature. Whatever care the experimental chemist may take, she will not be able to hold the ambient temperature exactly constant; it is bound to fluctuate slightly during the course of the experiment. Suppose the outcome of the experiment is substantially different from what the theory predicted. Is the theory refuted? The theorist can always reply that the ambient temperature was not exactly constant, as his theory requires, and that the experiment does not therefore constitute a refutation. This would not be the case if the robustness property, discussed above, had been required of the theory *ab initio*. Had the theory satisfied this property, the experimenter could be sure that, according to the theory, small fluctuations in the ambient temperature could only generate small fluctuations in the outcome of the reaction. An experimental outcome substantially different from the theory's predictions would then constitute a genuine refutation of the theory. Non-robust theoretical predictions are, in practice, non-observable, and therefore of no scientific interest.

This kind of problem clearly arises in economics as well as chemistry. The theory under test is typically expressed as a model involving some parameters which are assumed to be constant. The marginal propensity to consume or the interest elasticity of the demand for money might fall into this category. Of course no one actually believes that parameters such as these are exactly constant over time: they are bound to vary slightly, just as the ambient temperature would in the chemical example discussed above. It is clear then that the robustness property should be required as a necessary (though not sufficient) property of any economic theory, if that theory is to be regarded as scientifically valid. There are many economic theories which fail this robustness test (for example, most models involving saddlepoint dynamics; see Oxley and George (2007) for an elaboration of this argument) and cannot, therefore, even be considered candidates for empirical refutation.

A further problem arises for those who wish to transplant empirical testing from the natural sciences to economics. Experiments are typically replicable, whereas the statistics used by economists are usually generated in particular, non-replicable, historical circumstances. They more closely resemble the data of the economic historian or sociologist than those of the physicist. Given these data restrictions, perhaps economists should drop their attempts at grandiose, all-encompassing theories and accept that they can only offer theories which are restricted to particular historical and political circumstances. The theory of gravity may hold as good on the moon as on the earth, but the theory of inflation does not.

It may be that economists will have to abandon their attachment to complex but irrelevant econometric techniques, and embrace less grandiose empirical methods. For example, case studies, usually dismissed as the preserve of business studies or sociology, provide a potentially useful alternative approach. Whether orthodox economists will forever find it beneath their dignity to acknowledge that remains to be seen. In fact most orthodox economic theorists have little or no interest in empirical testing, especially not empirical refutation. When asked 'what feasible observation would refute your theory?', the orthodox economist usually responds with blank incomprehension.

4. Pluralism

Clearly appeal to empirical evidence is not the primary means by which the 'good' theories are separated from the 'bad' ones: most mainstream economics is not susceptible to empirical refutation, even in principle. Nonetheless there appears to be remarkable agreement as to which theories shall be accepted and gain currency. Marxian economics, institutional economics and radical economics, for example, are all classified as 'heterodoxy' and ignored. But the mainstream rejection of pluralism operates at a much more detailed level than that. Most published articles in economics involve minute variations on already published material: adopting non-standard assumptions or criticising currently fashionable techniques is usually enough to get your paper rejected, especially by the so-called core journals.

The mainstream maintains its homogeneity, not by appeal to evidence but by appeal to authority. The discipline has a tightly defined professional structure, an 'invisible college' which controls publication, appointments and promotions. No other discipline maintains such a sharp discontinuity between its 'core' journals and all other forms of publication.[6] In major UK universities a tiny number of core journal publications can lead to appointment and promotion in economics, whereas extensive non-core publication and scholarship is treated with contempt. It is widely believed that the RAE uses the proportion of core journal publications as a key measure of research quality in economics, and this in turn impacts on the allocation of large sums of public money. Recent empirical research by Oswald (2006) casts serious doubt on the Core Journal Doctrine.[7] Oswald remarks:

> In universities all over the world, hiring and promotion committees regularly hear the argument: 'this is important work because it is about to appear in prestigious

journal X'. Moreover those who allocate levels of research funding, such as in the multi-billion pound Research Assessment Exercise in UK universities, often come under pressure to assess research quality in a mechanical way by using journal prestige ratings. The results in this paper suggest that such tendencies are dangerous.

Given the tendency of mainstream economists to ignore empirical evidence, Oswald's findings are unlikely to weaken the malign and anti-intellectual effects of the CJD. The difficult mathematical techniques required of modern economists provide a useful barrier to entry to the profession and help to keep alarming pluralism at bay. The mainstream economists place a huge premium on technique and abstract theory but nonetheless some applied or policy content may occasionally leak into the mainstream. It is even possible that rival disciplines such as politics, sociology or economic history may offer alternative accounts of topics which economists had hitherto considered their territory. (Alex Preda's paper 'The Sociological Approach to Financial Markets' provides an example.) As discussed above, these disciplines may also have a lot to teach economists. However, mainstream economists are wary of close involvement with cognate disciplines. There are essentially two approaches to these subjects. The first, and most popular, is to ignore them altogether. The second is to incorporate them safely into economics. This second approach involves flooding the rival discipline with formalism borrowed from economics: mainstream economists sneer at political science papers with no equations. Mainstream economics does not learn from the other social sciences, something it desperately needs to do, but rather attempts to colonise them.

But does not all this pre-suppose some kind of conspiracy theory? Is it not simply paranoia to imagine an invisible college controlling economics? First, conspiracies do happen: conspiring is not particularly costly and the development of electronic communication has made it even cheaper. Cronyism is widespread in economics and has a significant impact on publication, citation, appointments, promotions and access to research grants. Mainstream economists are herd animals and the herd offers many forms of security, not least financial security. In virtually all countries economics research is dependent on some form of state funding,[8] and if the tax-payer discovers that eminent professors of economics are spending her tax-pounds (or dollars) proving that a countably infinite number of equilibrium contracts exist in a convex, locally compact economy,[9] she may be disinclined to supply future contributions. In the United States, Senator William Proxmire founded in 1975 the Golden Fleece award, often given for ludicrous money-wasting research in the natural sciences. How long will it be before another enterprising politician does something similar for economics? This possibility provides a powerful incentive for mainstream economists to set up barriers to entry to their profession. They do not need to conspire in too much detail: all have a common interest in shading their subject from prying eyes.

At the level of teaching, the homogeneity of economics is often advanced as a positive feature of the subject: mainstream economists frequently explain with pride

that in an economics lecture, on any particular branch of the subject, the diagrams and equations will be the same all over the world, though the words may be in different languages. With the advent of space travel, no doubt the Martians will soon catch on too. Unfortunately, however, students are increasingly rejecting this sort of sterile formalism, turning instead (perhaps disappointingly) to subjects such as business studies. Since, in most countries, students generate the income necessary to pay for economics research, this presents a series of problems. For example there is an emerging dichotomy between the needs of teaching and the needs of research. Students like interesting courses which are related to real-world problems, whereas high prestige research involves lots of difficult maths. It is becoming increasingly difficult to hire academics who can supply both. For some UK universities a possible solution is to focus on postgraduate students who can be charged high fees and who are sufficiently acculturated to mainstream economics that they are willing to embrace its sterile formalism without too much complaint.

5. A Future for Economic Orthodoxy?

Economics should deal with topics of huge importance: economic growth, employment, production, world poverty, inequalities of wealth and power, crises and environmental degradation to name a few (aspects of this agenda are discussed in Brian Lin's paper, 'A New Vision of the Knowledge Economy', Fulvio Castellacci's paper, 'Evolutionary and New Growth Theories: Are They Converging?' and Donald A. R. George's paper 'Workers' Savings and the Right to Manage'). Young people studying the subject for the first time often believe that it will supply, if not the answers, then at least the intellectual tools to tackle these issues. They are usually shocked to discover that a large part of their studies involves mastering difficult and ultimately sterile mathematical formalism, which has virtually nothing to say about these matters. Rarely has so much human capital been so seriously misallocated. Should they go on to undertake academic research in economics, they will realise that this sterile formalism, published in core journals, is the only route to career success. Deviation from this route is ruthlessly punished. Mainstream economists are usually untroubled by their academic critics: after all, controlling publication, appointments, promotions and research grants provides much of the power[10] necessary to keep interlopers out. But, as mentioned above, most economics research depends on public funding. So far 'peer review' has kept the tax-payers' noses out of the thorny question of value for money in research funding. The implicit contract is that, provided the universities continue to provide the tax-payers' offspring with high status qualifications, there will be little scrutiny of what research funds are being spent on. This happy situation may not continue indefinitely and mainstream economists may lose their professional fig leaves sooner than they think.

If such an awful day of judgement were to arrive, could economists recover their professional dignity (not to mention their salaries)? There is an intellectually valid future for economics, and economists could contribute to analysing some of

the most pressing issues of today. But the path to this sunlit intellectual upland is strewn with obstructions and diversions. To reach it economists will have to:

- Abandon sterile formalism and start judging research quality from its content not its place of publication.
- Stop invoking notions of 'rigour' which they do not understand.
- Understand that theories are only interesting if they are in principle open to refutation: their purpose is to explain the world, not to show how clever their author is.
- Realise that techniques are a means to an end, not an end in themselves.
- Realise that their theories can only be limited to specific historical and political conditions.
- Value applied and policy work as highly as theory.
- Learn from cognate disciplines instead of trying to colonise them.
- Abandon futile 'ranking' exercises such as the RAE
- Adopt a pluralist attitude to theory and settle disputes, where possible, by appeal to evidence not appeal to authority.
- Learn some history of economic thought and stop re-inventing the wheel.

Notes

1. The title acknowledges Paul Feyerabend's (1970) paper 'Consolations for the Specialist' and his lectures at the University of Sussex, which provided me with great inspiration and an understanding of methodological anarchism. Feyerabend's ideas are developed in his (1975) book *Against Method*.
2. Orthodox British economists often argue that the UK needs an RAE to keep 'standards' closer to those of the United States. They miss the obvious fact that the United States does not have an RAE: no American academic would accept a committee of state bureaucrats telling them how good they are at research.
3. This category includes the United States, where large sums of public money enter the universities via research grants, etc.
4. Popularly known as 'mad cow disease' and the probable cause of the fatal human disease vCJD.
5. Though some economists have attempted to emulate psychologists and set up experiments with human subjects to 'test' certain propositions based on game theory. These experiments are never interpreted as rejections of the theory; any apparent rejection is typically explained away, for example by arguing that the rewards on offer were not large enough to induce the subjects to play the game 'properly'.
6. For the benefit of any non-economists who may inadvertently be reading this paper, it should be explained that mainstream economists subscribe to a proposition known as the 'Core Journal Doctrine' (CJD: no relation to the CJD of note 4, though possibly another form of madness). This asserts that there exists a small group (around 10) of journals ('core' journals) with the property that anything published in a core journal is of high quality and all other publications are of low quality.
7. See note 6.
8. See note 3.
9. I made that up, but you know what I mean.

10. Despite possessing so much of it, mainstream economists are surprisingly poor at understanding power. The usual formulation is to 'model' it as the exponent in a von Neumann/Morgenstern utility function, traditionally assigned the value 0.5, for 'ease of exposition'.

References

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