

# **WHAT IS WRONG WITH MODERN ECONOMICS, AND WHY DOES IT STAY WRONG?**

**Tony Lawson**

What is wrong with modern economics? The clear answer is that it is mostly simply irrelevant. It has been becoming increasingly so for about seventy to eighty years now. Its formulations, in the main, are patently and repeatedly unrealistic, and so able to provide little or no explanatory insight or understanding of the world in which we live. Human beings, in the formulations of modern economists, are regularly endowed with perfect foresight, rational expectations, omniscience, amazing powers of calculation or 'rationality', rendered homogeneous, placed in scenarios where just two commodities exist etc., etc. It is all really quite ludicrous if the goal is social illumination.

Indeed the situation is so bad that failings of the discipline are openly acknowledged not just by those that identify as heterodox, but also, at least occasionally, by mainstream practitioners too, even including some Nobel Memorial Prize winners in economics. Nor is this an especially recent development. Thirty five years ago prize winner Wassily Leontief lamented that the discipline had reached a point where 'Page after page of professional economic journals are filled with [...] entirely arbitrary assumptions [leading] to precisely stated but irrelevant theoretical conclusions....' (Leontief 1982:104), whilst 17 years later even Milton Friedman criticised the discipline for no longer 'dealing with real economic problems' (Friedman 1999:137), and Ronald Coase reported that 'Existing economics is a theoretical system which floats in the air and which bears little relation to what happens in the real world' (Coase 1999:2).

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So what is the explanation? It is simply that modern economists persist in insisting that a set of tools be everywhere adopted that are mostly inadequate to social analysis, given the nature of social phenomena. Specifically, modern economists rely on certain deductivist (defined below) methods of mathematical modelling: this is more or less compulsory for, and indeed defining of, the modern mainstream project that dominates the discipline, but many heterodox economists are also clearly enamoured. However, social reality is of a nature that the sorts of mathematical tools regularly employed by economists (mainstream and heterodox alike) are simply not up to the task of successful social analysis.

To put the matter bluntly (the pun may be useful), it is like attempting to cut the grass with a hammer or a piece of paper. The latter objects have their uses, but mowing the lawn is not one of them. Methods of applied mathematics of the sort economists wield have their uses, but illuminating social reality is not one of them, or at best, is so only in exceptional circumstances. I hope that it is clear that this explanation, whether correct or not, reflects a stance that is *not* anti-mathematics but *anti* a mismatch of tool and object -- and so, given the circumstances, anti the *abuse* of mathematics.

Mathematical modelling methods of the deductivist sort that economists use, to be relevant, presuppose the existence of closed systems, those in which event regularities or correlations (whether actual, imagined, deterministic or stochastic, simple or complex) occur. Deductivism just is any form of explanation that relies upon such closed systems. For these to be covered by theories, the entities posited in the latter must in effect take the form of isolated atoms. By an atom I mean a causal factor that has the same independent and invariable effect whatever the context. The factors have to be isolated to prevent external factors affecting the outcome and undermining and presumed correlation.

So the modern dominant emphasis on various methods of mathematical modelling presupposes a ubiquity of closed systems of isolated atoms. It is easy enough to show that social reality is in general not at all like this (see, for example, Lawson 1997, 2003, 2015a). In brief, social reality is not only open (explaining the failure of econometrics and other modelling projects over the last 80 years), but highly internally related, meaning that everything social, including our social identities and ways of going on, are constituted *in relation to* everything else social

(undermining the isolationist assumption of economic modelling). Additionally, the phenomena of the social world are everywhere continually undergoing *processes of transformation* (undermining the assumption of atomism). Further still, social reality is also characterised by meaning and value; and the whole system revolves around processes of social positioning whereby existing phenomena are incorporated as components of emergent social totalities (see, for example, Lawson 2012, 2015b, 2016).

If the mathematical methods used presuppose a world of a nature other than that which generally obtains, then the continuous reliance upon such methods regardless necessitates that social reality must be regularly distorted in theorising. Thus, assumptions like rationality, perfect foresight, two commodity worlds and so on prevail not because anyone thinks they are realistic. Rather, they adopted simply because they (or other claims like them) conform to the specifications of closed systems composed of isolated atoms. The result is more or less worthless if social understanding is the goal, however well-meaning and skilful the modeller<sup>1</sup>.

Notice too that the account advanced here is extraordinarily powerful in terms of phenomena that it can explain. It can render intelligible the fact that the discipline was explanatorily successful before the introduction of mathematical methods but not after; the continuing failings of the discipline both before and since the recent crisis (despite the numerous [modelling] changes introduced as a response); the uniform failings of the discipline across the board including within macro, micro and econometrics, as well as 'novel' developments like neuro- and

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<sup>1</sup> Let X be a set of conclusions we suppose are true, or desirable to reach, or simply the properties of a data set. We can easily generate, and that sense deductively 'explain', X just by assuming 'A', and 'A implies X'. Where modelling is involved the assumptions required, as we have seen, are necessarily false. That is, if 'A', and 'A implies X' comprise the set of mathematical model specifications, either or both will typically be false. In which case what is the point? Certainly we cannot say that X is explained, or indeed that X receives any support at all. After all, if we can use one set of unrealistic assumptions why not any other? If, for example, X is a policy conclusion considered desirable by the modeller, an opponent can just assume 'B', and 'B implies not X', (deriving not X). All such lines of reasoning are on par in being worthless, adding nothing at all to understanding. This can change only if we agree to employ claims that we believe (have grounds for supposing) to be true. But then the claims employed will be about phenomena of the open social system in which we live, and so methods of mathematical modelling will/must be mostly cast aside.

behavioural economics; the use of traditional atomising assumptions (such as claims of rationality) as well as the more recent (equally atomising) importations from neuroscience, psychology and the like; and so on.

So why does the dire situation of modern economics prevail? Why do we not just change our methods? Specifically why do we not just make use of ontology to tailor our methods to the nature of the stuff being analysed, as they do in other disciplines (particle colliders constructed according to conceptions of Higgs boson particles to test theories of mass; telescopes designed to assess theories about distance phenomena, and so on), and as indeed was common practice in economics itself prior to about 70 or 80 years ago<sup>2</sup>. In particular, why do we not take the fact of an open complex social reality seriously in method design? Achieving the latter is certainly feasible<sup>3</sup>, and indeed we all do it successfully in our day to day non-academic interactions.

This is where matters get more complex. *Prima facie* the possible explanations could be any of various kinds including: (1) psychological;

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<sup>2</sup> As with any other successful discipline, it was once fairly standard to put ontological reasoning up front. Keynes wrote his *A Treatise on Probability* to question if social reality was of a *nature* that probability judgements (of various forms) were relevant to its analysis. He concluded in the main that it was not, and later applied this ontological analysis to demonstrating the inappropriateness of econometric methods. Arguably Karl Marx's *Capital* is a book that almost entirely on ontology. It is common place to recall that Marx opens with a chapter on the commodity. But more than that he questions its *nature*. In finding it to be characterised by both use value and exchange value, Marx moves to studying the nature of value itself, and thereafter the nature of labour, labour-power and money. Using the term metaphysics as a substitute for ontology Marx sums his initial findings in section IV of the opening chapter on commodities as follows:

A commodity appears, at first sight, a very trivial thing and easily understood.

Its analysis shows that it is, in reality, a very strange thing, abounding in metaphysical subtleties and theological niceties (Marx 1974:76).

Further, Veblen's whole argument about evolutionary science and the place of institutions is ontological, as is Hayek's account of social order. Those mentioned, like most other contributors of the time, drew out implications of ontological reasoning for social analysis. It was with the turn to a heavy mathematical emphasis that this all changed. For, with the onset of mathematical-modelling reductionism, there was no longer a perceived need for questioning the nature of phenomena. Instead of using ontological insight to fashion appropriate methods of analysis, the latter were determined *a priori*. This unhelpful way of proceeding is the dominant practice of the discipline today.

<sup>3</sup> For applications (and discussions) of methods (in particular of dialectical methods of contrast explanation) that are appropriate for dealing with phenomena generated in open systems see, for example, Lawson (2009); Morgan (2013); Morgan and Patomäki (2017).

(2) political, including conspiratorial; (3) plain ignorance, error, or lack of criticality; or (4) institutional.

No doubt all four types here identified have some bearing, and indeed interconnect and mutually reinforce. Most critics seem to suppose that the problems of the discipline are wholly political, and so focus almost entirely on the second kind of explanation. This assessment and response, I will suggest, is not only misguided but serves mostly to reinforce the emphasis on mathematical modelling, by deflecting attention from more relevant criticism. But let me say at least something about each type of explanation, in the order they are listed.

### **Psychological explanations**

It is often noted that many of us, especially those brought up gendered as men, have a psychological disposition to overemphasise the possibility of predictability and control. It seems that in many cases the recognising and embracing of openness and contingency is scary. Julie Nelson (2003), for example, defends a specifically 'feminist critique of economic methodology', arguing along these lines:

The idea that the universe may be open, in some ways fundamentally unpredictable, and intrinsically purposive – in contrast to being a closed system, ultimately distillable into formulae, controllable, and fundamentally indifferent – is not simply a reasonable alternative ontology that can be carefully weighed for its logical implications and neutrally evaluated for its relative merit. [...] The idea of an open universe feels fundamentally *scary* for those who sense that not only their status as scientists set above the objects they study, but also their safety vis-à-vis chaos, their 'manhood' (whether actual, or, in the case of female scientists, symbolic), and their very own distinct selfhood are threatened unless they can keep the living, novel, relational aspects of nature safely at bay (Nelson 2003:111).

In similar fashion, Vinca Bigo (2008) writes of fantasies of supremacy and prediction as gendered coping mechanisms in the face of, and for dealing with, an open social system<sup>4</sup>, mechanisms traceable to infant development, with experiences that vary according to gender assigned<sup>5</sup>.

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<sup>4</sup> Bigo writes: 'Both coping mechanisms basically work by allowing the individual to feel 'in control'. They serve to compensate for the loss of control that derives from the recognition of, first, different others and, second, mortality. In the former case, the

But still the openness of social reality is always with us. If mainstream economists, or economic modellers more generally, were to behave as they do in the academy in everyday life (when crossing roads, planning events, indeed making any decisions) it seems unlikely they would long survive, let alone get by at all adequately with anything. However important in society at large may be the mechanisms and processes that Nelson and Bigo identify, the practices they bear upon and contribute to explaining seem to intensify and become rather more bizarre as individuals become positioned participants in the economics academy. Something more must be going on as well.

### **Political, including conspiratorial, explanations**

Amongst heterodox economists the main explanation of the irrelevant and indeed often incredible assumptions of modern economics, or rather of those deemed mainstream or 'neoclassical', is, as noted, that it is mostly down to politics and, specifically, political conspiracy founded on economic/political ideology. It is the assumptions that come first, or matter the most, leading to poor models; the role of modelling itself overlooked.

As the French heterodox economist Bernard Guerrien asks of the 'totally irrelevant' mainstream: 'how [is it that] such intelligent people can propose – and endlessly study – such *stupid* models?' (2004[2009]:160-1). After describing the sorts of assumptions typically made in mainstream modelling exercises (such as 'perfect competition and an 'omniscient' 'representative agent'), Guerrien asks 'How can a normal

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emergent fantasy entails demeaning different others, thereby rendering the differences somehow less threatening. In the latter case, the fantasy entails treating the future as open to manipulation, thereby rendering our mortality somehow less real. The puzzle of modern economics with which I started, as I say, can clearly be seen as a particular manifestation of the fantasies in question. First, the emphasis of mainstream practitioners on the unquestionable superiority of their methods, and (so) output, over any heterodox alternative is a particular example of the fantasy of supremacy. Second, the emphasis on formalistic economic modelling, is a playing out of the fantasy of prediction' (Bigo 2008:543).

<sup>5</sup> Bigo adds: 'In summary, [...] I show that certain puzzling features of the practices of modern mainstream economists are forms of fantasies encountered in certain practices in society at large [being...] manifestations of mechanisms of defence against (real or perceived) separation anxiety [traceable ...] back to infant development and identity formation, in so far as they constitute a 'blueprint' for dealing with differences and uncertainty later on in life' (Bigo 2008:550).

person make any sense of this?' (*ibid*:161). Guerrien plumps for the following answer: 'I only see one reason for that: *ideology* (intuitive beliefs which render them blind)'.

But economic or political ideology is not the explanation. Most mainstream economists, in my experience, do not think about the way they proceed: they are more sheep than conspirators. Few understand terms like neo-liberal, or neo-classical or care what they mean. And the small band amongst them that do think at all critically are well represented by the likes of Frank Hahn who warns against a blind focus on conceptions like equilibrium states just because such a focus 'is easily convertible into an apologia for existing economic arrangements' (1970:88-9). Indeed, despite being an equilibrium theorist himself, Hahn worries that 'there is something scandalous in the spectacle of so many people refining the analyses of economic [equilibrium] states which they give no reason to suppose will ever, or have ever, come about' (1970:88-9).<sup>6</sup>.

In effect, rather than knowingly *leading the way* (say, in promoting a market based society oriented to accumulation) mainstream economists are better described as *lost*, as not even knowing where any paths they are on in fact *will lead*; they really do not know what they are doing. As Ariel Rubinstein, himself a mainstream 'theorist' put it in a speech to honour the award of the Noble Memorial Prize to the game theorist John Nash:

The issue of interpreting economic theory is [...] the most serious problem now facing economic theorists. The feeling among many of us can be summarized as follows. Economic theory should deal with the real world. It is not a branch of abstract mathematics even though it utilises mathematical tools. Since it is about the real world, people expect the theory to prove useful in achieving practical goals. But economic theory has not delivered the goods. Predictions from economic theory are not nearly as accurate as those by the natural sciences, and the link between economic theory and practical problems [...] is tenuous at best. Economic theory lacks a consensus as to its purpose and interpretation. Again and again, we find ourselves asking the question 'where does it lead?' (Rubinstein 1995:12).

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<sup>6</sup> Elsewhere, Hahn reveals in rather dramatic fashion what he feels should happen if people contemplate using such models for policy: 'When policy conclusions are drawn from such models, it is time to reach for one's gun' (Hahn 1982: 29).

Numerous heterodox economists, however, suppose the situation to be otherwise. Many even distinguish themselves as heterodox *not* by reducing their emphasis on mathematical modelling, but according to the sorts of policy conclusions (for example anti-austerity) they profess to support with their modelling. In so doing, of course, they are most of the time simply reproducing the typical mistakes made by most other modellers; their results, if left-leaning, or 'alternative', are mostly just as irrelevant because of the manner in which they are produced.

By here suggesting that mainstream economics are not in the main politically motivated, that economic/political ideology does not explain their choice of modelling activities, I do not deny that the mathematical deductivist project has received political support *qua* a mathematical project. But this has rarely been achieved as a result of any intentional design by modellers themselves; usually it is but a fortuitous contingent happening akin to the evolutionary environmental selection of some population trait that is in no way laudable, but merely well suited to developments in context. Essentially, the project receives support (where it does) because it is irrelevant.

An example is the impact on the discipline of economics of the McCarthyite witch-hunts in the US the face of the Cold War, following the Second World War. The group most feared and mistrusted by the McCarthyites were the intellectuals (Reinert 2000). Under the conditions of the witch-hunts, the nature of the output of economics faculties - traditionally a form of political economy, a field that attracted those who sought a more humane system than capitalism - became a particularly sensitive matter. In such a scenario, administrators of economic research found the project of mathematising the discipline to be especially attractive in that it carried scientific pretensions whilst being significantly devoid of any necessary empirical content or basis for critical reflection. These features rendered acts of supporting the project extremely convenient, not just to insecure or fearful university officers but also to the funding agencies of US social scientific research (who were especially important in this period - see for example, Coats 1992; Goodwin 1998; Yonay 1998). Clearly, by allocating funding to the mathematising economics project there was little risk to these bodies of being accused of supporting those who wanted to transform the economic system; for by everyone's account the mathematising project had little obvious bearing on social reality (on all this see especially Lawson 2003, chapter 10; also Lawson 2015a).



Such cases, however, if interesting, do not allow us to understand either why so many were already pursuing mathematical economics – allowing others the opportunity to financially support it – or why the dominance of the project still persists despite around 70 years of fairly unmitigated failure at providing insight. Even less do they account for the current popularity of these practices amongst many of those who identify as heterodox. If such developments allow us to understand how economics arrived at its current state (for a fuller account see Lawson 2003: ch. 10) they do not explain why it survives, and does so almost unchallenged.

### **Lack of philosophical nous and criticality**

The more immediate explanation of this ongoing situation, I suggest, is that the sort of factors just summarised have conspired to bring about a scenario or culture wherein modern economists, including those who identify as heterodox, are, with some notable exceptions, just unable, or find it difficult and overly laborious, to think outside the modelling box. They are methodologically blinkered, and unable or unwilling to question the presuppositions of the dominant generation. Behind it all, perhaps, is a widespread and understandable, if ultimately erroneous (see Lawson 1997, 2003 2015a) notion that mathematics of some form is essential to science, coupled with the desire to be scientific. In any case, the practices in question go largely uncriticised. Mostly, as noted, the heterodox label for modelling is ‘justified’ by the sorts of results reached being interpreted as radical or progressive. As a result, most of the critical focus taken within heterodoxy is on the sorts of conclusions or assumptions that mainstream modellers choose to defend. This critical focus is again understandable. Nevertheless, making this the only or the primary one is a major obstacle to going forward. Nearly 100 years ago, Alfred North Whitehead warned of the dangers of directing attention primarily to the most apparent and contested, rather than seeking to uncover the taken-for-granted:

When you are criticising the philosophy of an epoch do not chiefly direct your attention to those intellectual positions which its exponents feel it necessary explicitly to defend. There will be some fundamental assumptions which adherents of all the variant systems within the epoch unconsciously presuppose. Such assumptions appear so obvious that people do not know what they are assuming because no other way of putting things has ever occurred to them. With these

assumptions a certain limited number of types of philosophic systems are possible, and this group of systems constitutes the philosophy of the epoch (Alfred North Whitehead 1926: 61).

More than a quarter of a century even before Whitehead's observations, Thorstein Veblen coined the term *neo-classical* precisely to capture those economists who were making this sort of mistake (see Lawson 2013). More precisely, Veblen used the term for those that were being somewhat astute in recognising, at least in a very general manner, significant advances at the level of ontology, but who failed to question adequately the (ontological) presuppositions of their own methods, and in so failing in this way, persevered with methods inconsistent with explicit ontological assessments.

In Veblen's terms, the sort of social ontology that I have defended as realistic is referred to under the head of (evolutionary) processes of cumulative causation (grounding an evolutionary science of economics), whilst the emphasis on correlation analysis or pattern prediction is termed *taxonomic*. As I say, it is for the inconsistent attempt to maintain both that Veblen introduced the label *neo-classical* (use of the latter methods being regarded as *classical* and adherence to the former more realistic ontology warranting the suffix *neo*).<sup>7</sup>

Interestingly, Veblen sought to illustrate the sort of inconsistent practices he had in mind using the examples of Alfred Marshall and John Neville Keynes (Maynard Keynes' father). Neither were philosophical slouches, and such examples indicate that methodological presuppositions can linger even in the best of contributors. The central taken-for-granted presupposition which these two failed fully to challenge was that results should take the form of correlations or uniformities or event associations, *i.e.*, that analysis is taxonomic:

There is a curious reminiscence of the perfect taxonomic day in Mr. Keynes's characterisation of political economy as a 'positive science,' the sole province of which is to establish economic uniformities; and in this resort to the associationist expedient of defining a natural law as a 'uniformity,' Mr. Keynes is also borne out by Professor Marshall (Veblen 1900: 265).

The evolutionary-ontological view of cumulative causation that Veblen defends was perceived as relatively new at the time, and (so) Veblen was relatively restrained in his criticism. Instead of accusing the likes of

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<sup>7</sup> On all this see Lawson (2013).

Marshall of being hopelessly incoherent, Veblen allows that as yet such contributors had just not found a way to render method consistent with social ontological insights which, at a general level, they embraced. Specifically neoclassical economists had yet to develop methods of analysis appropriate to evolutionary preconceptions. As Veblen puts it:

All this gives an air of evolutionism to the work. Indeed, the work of the neo-classical economics might be compared, probably without offending any of its adepts, with that of the early generation of Darwinians, though such a comparison might somewhat shrewdly have to avoid any but superficial features. Economists of the present day are commonly evolutionists, in a general way. They commonly accept, as other men do, the general results of the evolutionary speculation in those directions in which the evolutionary method has made its way. But the habit of handling by evolutionist methods the facts with which their own science is concerned has made its way among the economists to but a very uncertain degree. The prime postulate of evolutionary science, the preconception constantly underlying the inquiry, is the notion of a cumulative causal sequence; and writers on economics are in the habit of recognising that the phenomena with which they are occupied are subject to such a law of development. Expressions of assent to this proposition abound. But the economists have not worked out or hit upon a method by which the inquiry in economics may consistently be conducted under the guidance of this postulate (Veblen 1900: 265-66) .

There is clearly a good deal of irony here for modern heterodoxy, especially for some of those that style themselves institutionalist in the Veblenian mould. For many openly (and without apparent restraint) use the term 'neoclassical economics' as a term of abuse or derision for others. Yet they themselves express realistic visions of an open social system of cumulative causation in a general fairly loose fashion, whilst seeking to provide insight into it using (mathematical) methods that presuppose it is closed. This is precisely the combination for which Veblen coined the term neoclassical.

More than a century has passed since Veblen wrote, allowing sufficient opportunity for the methodological and other implications of this ontology to be fully recognised, and the nature of his neoclassical critique of Marshall and others appreciated. Moreover, in the intervening period, many others, including Keynes, have been explicit in criticising (mathematical deductivist) methods that rely upon event correlations, on

similar ontological grounds. Thus, on various occasions, Keynes warns that:

If we are dealing with the action of numerically measurable, independent forces, adequately analysed so that we were dealing with independent atomic factors and between them completely comprehensive, acting with fluctuating relative strength on material constant and homogeneous through time, we might be able to use the method of multiple correlation with some confidence for disentangling the laws of their action . . . . In fact we know that every one of these conditions is far from being satisfied by the economic material under investigation (Keynes 1973: 285-6).

And yet many post-Keynesian modellers, like original (Veblenian) institutionalist counterparts, continue in a way that is not merely internally inconsistent in the manner Veblen termed neo-classical and explicitly rejected by Keynes, but is sometimes even presented as advancing in the spirit of their figureheads.

The ‘justification’ offered for their practices, to repeat, is merely the claim that results achieved can be interpreted as somehow alternative or radical. Clearly, this is inadequate. The reason for it can only be that these modellers do not fully grasp the nature of their errors, that they fail to consider the deeper issues involved in a sufficiently serious or critical fashion. After all, many explicitly identify as heterodox and set themselves up as opposing the mainstream. So they are presumably not seeking mainstream accolades. And yet, in their modelling endeavour, they are very often no more relevant than the mainstream or ‘neoclassicals’ that they criticise.

### **Institutional explanation**

Why do the criticisms made not run deeper? As already touched upon in passing, I can only think it is a failure of the system, a result of institutional conditioning. Most modern economists, whether mainstream or heterodox, are educated in departments where philosophy is no longer on the agenda. Criticism everywhere is couched in terms only of contrasting substantive theories developed and policy implications drawn, not in terms of methods or orientations that can be justified. Ontology of an explicit and systematic sort, in particular, is mostly absent. Although the discipline has been an explanatory failure over the last sixty years or so, it has successfully fostered a culture wherein the

necessity of using methods of mathematical modelling in most instances has become part of the background common sense, accepted by critics as much as proponents of substantives positions developed. A *methodological* ideology thus prevails in much of the modern economics academy whereupon a reliance upon mathematical methods is the proper way to proceed. In fact, some heterodox economists, just like their mainstream counterparts, have come to accept, as an unexamined presupposition, that contributions that are not mathematical are not serious, scientific or other than woolly. The result, then, is a falling away of criticality precisely where currently it matters most.

At the same time, the inevitable failure of the project to illuminate entails that a good deal of dissent and advocacy of change of some sort regularly emerges. Currently, in the wake of the recent economic crisis, this is significant. Much of this is led by the students, especially through the *rethinking economics* movement. And they are being heard. But even here there is insufficient analysis of where the problems lie. In particular, sympathetic academic economists coming to aid this movement are mostly, if unwittingly, offering more of the same.

The *Institute for New Economic Thinking* (INET) sponsored by George Soros also supports the rethinking economics students, especially financially. But although INET no doubt sponsors a few projects that do avoid the noted problems, in the main, and despite Soros' own best intentions (see Lawson 2015a, chapter 9), the enterprise mostly seeks to promote endeavour that focuses on revising model assumptions, applying novel types of mathematical (deductivist) models, or developing alternative approaches to model estimation, etc., and mostly it fails to address the discipline's more fundamental problems. It risks constituting an enormous waste of resources and opportunity.

In the light of all this, it is perhaps unsurprising that a glance at the students' own programmes for 'rethinking economics' conferences and workshops reveals that a similar imbalance tends to be reproduced. If philosophical contributions appear at all, they are usually marginalised, being placed in sessions within multiple 'parallel streams', while the plenaries are mostly reserved for supporters of 'alternative' economic theories and policies, almost always supported by the results of economic modelling.

The outcome, then, is that, despite widespread dissatisfaction with the state of the modern academic discipline, the real source of its major

problems remains almost unchallenged, as any critical attention is mostly given to presenting theories and policies, derived by modellers, that are interpreted as more radical in nature. And so the irrelevance of modern economics continues.

### **The way forward**

So what is to be done? Clearly we need to go beyond an unreflective claiming or attribution of labels like heterodox or neoclassical. In particular, is not sufficient merely to establish departments of economics where courses taught are labelled heterodox (or anti-neoclassical) but which focus merely on substantive theories and policies. Despite the best of intentions of those involved, the latter can all too easily degenerate into courses where mathematical modelling exercises remain overly emphasised, if given radical-sounding interpretations. It is not uncommon to find teachers even arguing that it is a duty to ensure that modelling methods dominate curricula, or assessing 'stronger' students in terms of modelling abilities. This is a particular concern if such courses end up diverting those students attracted because brave enough to seek to prioritise relevance as much as career.

The solution can come only through the inclusion, and indeed prioritisation, of courses that are overtly philosophical in nature and encouraging of critical thinking. I do not see how ontology can be reasonably excluded. This assessment may sound dogmatic. But it is no more than a recognition that researchers cannot hope to get anywhere worthwhile without explicit consideration of the nature of the subject-matter with which they intend to work. Although I hold to a particular set of ontological conceptions, defending the latter is not my primary goal here. All claims, including ontological ones, are fallible. The essential point is simply that a return to critical, philosophically, including ontologically, informed thinking, as a systematic and sustained programme, is vital if economics is to regain relevance. Ontology, *as a form of study*, needs to be reclaimed.

Heterodox economists ought not to be resisting this assessment. Most and perhaps all the figureheads of the modern heterodox traditions, for example Marx, Veblen, Keynes, and Hayek, engaged explicitly and in a sustained fashion in philosophy and specifically ontology (albeit if often calling it metaphysics).

Further, most heterodox economists seemingly embrace the idea of pluralism. The need for pluralism, however, applies not just at the level of substantive theorising and policy formulation, but also at the level of method, with informed choices necessitating philosophical reflection and analysis. As I have often repeated, there is no need to exclude methods of mathematical modelling from the tool box; but there are many other methods and approaches that can be fruitfully (and with greater reason) included. A reliance upon any warrants explanation.

No doubt it is the case that those who teach/research social ontology explicitly are currently thin on the ground. But in truth we all are ontologists in our daily practices; we all regularly successfully navigate the social world.<sup>8</sup> All that is required is an allocation of intellectual space within the academy to open critical thinking about the nature of phenomena that we deal with on a daily basis; with time allocated to determining how to make our academic practices relevant to them. What specifically is the nature of money, the corporation, care, technology, gender, the market, value, capital, capitalism, the economy, human nature, social community, social relations, power, rights, obligations, norms, trust, and so on? We deal with these sorts of phenomena all the time, so we are already familiar with our subject-matter under some description. Addressing them in an explicit, systematic and non-superficial fashion, allows for methods appropriate to their analysis to be easily recognised.

Ontology, explicitly conceived, is equally relevant to projects of progressive change. Only if we include the systematic study of human nature and the possibilities for human flourishing (along with the flourishing of other beings), as well as the nature of social reality and the possibilities for its competent social (emancipatory) transformation, will the discipline be appropriately placed to contribute to making the world a better place (on all this, see Lawson 2015c).

There is a good deal wrong with modern economics. There is much to be done to remedy matters at all levels of analysis. But little can improve at any level until we discard the widely-worn methodological blinkers which encourage the view that mathematical modelling is everywhere

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<sup>8</sup> Ontology is ever present; the only issue of choice is whether to leave our presuppositions about the nature of social reality implicit and unexamined, or to do ontology in an explicit, systematic, sustained, and thereby more coherent, fashion.

automatically relevant, even essential, so that paying explicit attention to matters of ontology is unnecessary.

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