Journal of Economic Methodology 11:3, 329–340 September 2004

Roundtable: Tony Lawson's *Reorienting Economics*

Reorienting Economics: On heterodox economics, themata and the use of mathematics in economics

Tony Lawson

It is always a pleasure to have a piece of research reviewed. It is especially so when, as here, the reviewers offer their assessments, including criticisms, in a constructive and generous spirit. I provide brief reactions to the reviews, addressing them in the order in which they appear above. There is in each review much with which I agree, as well as critical suggestions on which I want to reflect more. Although all the reviewers express some agreement with *Reorienting Economics*, each concentrates her or his focus mostly (though not wholly) where differences (or likely differences) between us seem to remain. In responding I will do the same.

1 DOW

Sheila Dow starts with an accurate summary of my purpose with *Reorienting Economics*. Central to the latter is my arguing for a reorientation of social theory in general, and of economics in particular, towards an explicit, systematic and sustained concern with ontology. Dow seems to accept the need for this. But she believes there is a problem with an aspect of my argument, or, more specifically, with an implication I draw from my own ontological analyses, concerning the possibilities for modern economics. The issue in contention is, or follows from, the manner in which I characterize the various heterodox traditions.

An essential component of my argument is that mainstream economists, by restricting themselves to methods of mathematical-deductivist modelling, are forced to theorise worlds of isolated atoms. Heterodox economists, on the other hand, attempt to theorise social reality as they (fallibly) find it to be, unconstrained by a prior need to consider only closed atomistic scenarios.

I go further. In *Reorienting Economics* I defend a conception of social reality as possessing emergent causal powers, as structured, intrinsically dynamic, highly internally related, and so forth. I argue that this alternative ontology, whether or not it is explicitly acknowledged, is presupposed

Journal of Economic Methodology ISSN 1350-178X print/ISSN 1469-9427 online
http://www.tandf.co.uk/journals
DOI: 10.1080/1350178042000258536

by the sorts of conceptions defended in the more prominent strands of heterodoxy.

I do not argue that it is ontological differences that distinguish the various heterodox traditions from each other. Nor even is it competing theoretical or methodological claims. Rather I suggest the various heterodox traditions are best distinguished by their particular substantive *concerns and emphases*, where these in turn rest implicitly on different aspects of the shared ontology described in *Reorienting Economics* (thus the post Keynesian emphasis on uncertainty reflects the a presupposition of openness; the institutionalist concern with evolutionary method rests on a presupposition that society and economy are processual in nature, and so on).

I also suggest that to the extent I am correct in identifying a shared (if typically unelaborated) ontological conception underpinning the various heterodox approaches there are likely to be numerous advantages to a joining of the relevant strands of heterodoxy in a programme of linked or co-development, with each tradition viewed as a division of labour within this broader programme.

In short, I argue that heterodoxy is distinguished from the mainstream along ontological lines, but that the separate traditions within heterodoxy are best seen as in effect divisions of labour, with each group motivated by different substantive and political concerns and questions.

Dow seems reluctant to accept this assessment, mostly, I think, because she believes I am being overly optimistic in interpreting the various heterodox groupings as potentially cooperating divisions within one coherent overall project. Let me comment on Dow's line of reasoning to see if my (admitted) optimism is necessarily misplaced.

I am not sure that Dow rejects my contention that an ontological conception such as I defend is commonly presupposed by the heterodox projects. Certainly, she does not provide, or point to, counter-evidence to the critical case studies I provide to support it. Rather, although she considers herself to be concerned with ontology, and wonders whether 'it is reasonable to expect agreement at the ontological level', Dow seems to bracket off features of the conception I defend as 'pure ontology', and to point instead to the difficulties that arise 'as soon as we start to conceptualise the economic system', mentioning features such as identifiable individuals, products, firms and institutions.

Now the reason for Dow regarding my vision of inter-heterodox compatibility and co-operation as overly optimistic seems to be a presumption that we all inevitably invoke closures in our more concrete analyses, and it appears that each heterodox group necessarily invokes its own distinct closures.

Unfortunately I have to admit, at this point, to being somewhat unclear as to the precise meaning that Dow gives to the term closure. It does seem to me that Dow is employing the term in a different, or anyway a less restricted, manner than I am. In *Reorienting Economics* (and elsewhere) I use the term simply to denote a system in which regularities of actual outcomes (events or states of affairs) occur. Dow though may mean something more. For she writes that

> As soon as we start conceptualising the economic system, we inevitably invoke closures . . . . Epistemology cannot be conceived as an open system in the same pure sense as social ontology.
>
> The very words we use involve closures . . .

And further on she adds

> it is the closures which are … invoked to structure perception of experience and to allow analysis to proceed, and the way these closures are regarded (provisional or fixed, partial or complete), which characterise different schools of thought. This illustrates the significance of downplaying differences between heterodox schools of thought, distracting attention from the fact that each rests on a set of (provisional, partial) closures

Now however Dow uses the term closure – and she may (on occasion) mean by 'invoking closures' little more than 'theorizing' – it seems to me that any case for concluding that the conceptions of heterodox groups are very likely to be significantly incompatible presupposes at a minimum both: i) that in invoking Dow's inevitable closures we all necessarily distort (or lose touch with) reality and do so to such an extent that groups working in isolation are unlikely to produce conceptions that are commensurate, and ii) that members of the competing heterodox groups are indeed sufficiently isolated from each other as to prevent the development of shared constructions and meanings (whilst those within any given heterodox group presumably interact closely and regularly enough to ensure that within that tradition the same, or sufficiently similar, constructions are employed or are imbued with the same meaning and so forth).

As far as I can tell condition ii), specifically the isolation of heterodox economists of different persuasions, has never held in recent times. And this is surely a good thing. Nor am I yet convinced that condition i) holds. I accept, of course, that all knowledge is dependent on human capacities for, and ways of, knowing. I realise too that society depends on us, our practices and conceptions, and so can be transformed with changes to the latter. And I recognize further that knowledge is always practically conditioned, situated and partial, and also in a sense a (transient) construction. But it does not follow from any of this that our human constructions cannot express aspects of our reality, and be true (or contain truth) in their claims about it. Certainly, it does not yet follow that we must *knowingly* distort (as the mainstream likely must, for example, as a result of its insisting on universalizing certain very specific methods that appear somewhat inappropriate to most social situations).

I admit that I am not completely sure that Dow holds to i), not least because of my uncertainty as to Dow's precise meaning with the phrase 'invoking closures'. However, Dow at one point also remarks that 'Any form of abstraction from reality involves some form of inconsistency between theory and reality'. Now if we accept, as I think we must, that all thinking involves abstraction, it does seem safe to suppose that Dow indeed accepts that distorting reality is unavoidable.

Though space is limited let me at least respond to the contention that abstraction involves some necessary inconsistency between theory and reality. Although Dow does not seek to defend this contention, the usual justification for it rests on a conflation of abstraction and the method of theoretical isolation, where the latter mostly does depend on closed-system reasoning (as I understand the terminology). But to abstract does not mean to treat something as though it exists in isolation. Rather it is to concentrate on an aspect of something, momentarily leaving other aspects out of view; abstraction is always from (the rest of) something (usually referred to as the concrete). However, there is no necessary reason for distortion to be involved, no reason to suppose that what is not under focus does not exist or must be described in any way other than it is. And there is certainly no need to treat an open system as one that is closed (in my terminology).

For the past few moments I have been focussing on my computer monitor, and abstracting from (amongst very many other things) the chair on which I am sitting and the gravitational forces preventing me from floating off into space. But this does not mean I assumed in this period that these factors no longer existed, or that I was inevitably treating them as other than they are.

To abstract is in some ways like watching a sports game unfold on a TV screen, in the knowledge that players and movements not currently in view are nevertheless in existence and making a difference to the aspects we currently can see. In any case, I am cautious about accepting that abstraction necessitates some form of inconsistency between theory and reality. Certainly an argument to the contrary remains to be made.

Perhaps the conception that I have in mind can usefully be compared with the study of the human body. The latter, like society, is a highly interconnected whole in process. The skin, blood, bones, eyes, ears, mouth, heart, liver, etc., work the ways in which they do in virtue of their relations to other parts of the body; the various parts and the whole are necessary to, and depend upon, each other.

The mainstream in economics, as I interpret it, is like an approach to medical research that uses only one rather narrow method determined in advance of the study, and seemingly in neglect of available insights into the nature of the object of study. We should not therefore be too surprised if it is found to be highly limited in its scope of relevance and successes in advancing understanding.

The heterodox traditions instead start from insights into the (open, structured, highly internally related, intrinsically dynamic) nature of the human body, but each specific tradition specializes in the study of its own separate aspect of interest (the skin, blood, bones, ears or the eyes or the nose or the heart, etc.). Each branch of investigation must employ abstraction, but without treating its object of analysis as somehow functioning in isolation from the rest of the body or in any known way as other than it is. And each branch is constrained to fallible and always restricted human ways of knowing and communicating. Medical researches sometimes get things drastically wrong, are in a perpetual state of transformation and critical reflection, and are very often in possession of competing theories of how things work, all these theories always constructions. Certainly I do not wish to understate the complexities involved. Yet the various branches of medical research concerned with different aspects of the human body are also very often able knowledgably to communicate and cooperate successfully nonetheless, that is, to function successfully as divisions of labour within a reasonably coherent overall project.

If I must not understate the complexities of modern medical research here nor should I overdo the analogy being drawn. I well understand that society is in very many ways a different type of thing to the human body. But I am not convinced that the issues in contention are affected by these differences; specifically, I do not see that these differences render meaningful inter-group communication and enrichment about the nature of a common and knowable object less feasible in the case of society than of the human body.

So, at this point, and awaiting further argument, I remain optimistic about the prospects of compatibility and cooperation amongst the heterodox traditions.

2 PEACOCK

Mark Peacock's piece interestingly reconstructs aspects of *Reorienting Economics* as a form of thematic analysis. Peacock focuses on three themata in particular:

$\mathbf{T}_{\mathbf{Ep}}$ – 'theory' in economics must be mathematico-deductive in nature
$\mathbf{T}_{\mathbf{Ex}}$ – 'explanation' consists in showing that individuals optimise some variable
$\mathbf{T}_{\mathbf{Eq}}$ – explanation of economic phenomena is held to involve the identification of equilibrium (or movements towards one if the system is not in equilibrium).

Peacock observes that I give

> less attention to $\mathbf{T}_{\mathbf{Ex}}$ and $\mathbf{T}_{\mathbf{Eq}}$ than to $\mathbf{T}_{\mathbf{Ep}}$. Yet it is more often the former pair which is mentioned in thematic statements by economists. More submerged still are the ontological implications of $\mathbf{T}_{\mathbf{Ep}}$

This is so. I do emphasise the mainstream attachment to formalistic method over and above any proclivity either for assuming that individuals always optimise or for investigating equilibrium possibilities. And I recognise that this differentiates my account not only from the few mainstream self-assessments that can be found but also from most assessments of heterodox critics. Let me explain my reasoning.

First of all $\mathbf{T}_{\mathbf{Ep}}$ is the one 'thema' accepted consistently by the mainstream, despite the fact that, as Peacock rightly emphasizes, it receives very little explicit defence; throughout all the (numerous) flits in fads and fashions of the mainstream project, a commitment to $\mathbf{T}_{\mathbf{Ep}}$ is one feature that remains constant. Although $\mathbf{T}_{\mathbf{Ex}}$ and $\mathbf{T}_{\mathbf{Eq}}$ are often found, they are not universally so. It is the mainstream commitment to $\mathbf{T}_{\mathbf{Ep},}$ one not shared by the heterodox groupings, that actually identifies the project as mainstream.

Second, where $\mathbf{T}_{\mathbf{Ex}}$ and $\mathbf{T}_{\mathbf{Eq}}$ are maintained, their adoption is most plausibly explained by a prior commitment to $\mathbf{T}_{\mathbf{Ep.}}$ This follows once we realize that reliance on methods of mathematical deductive modelling more or less necessitates a focus on conceptions of atomistic individuals and closure. The point here is that if social reality is indeed open, highly internally related and processual in nature, then substantive specifications thrown up by the modelling project will very often need to be fictitious in numerous respects.

Why does this encourage $\mathbf{T}_{\mathbf{Ex}}$ and $\mathbf{T}_{\mathbf{Eq}}$? Consider the former first. In explanatory endeavour there are usually numerous ways of 'proceeding'. Specifically, numerous theories can be constructed. So a basis of selection between them is required. Now if, in virtue of the mainstream prior *insistence* on deductivist modelling (in an open system), criteria of 'realisticness', such as explanatory power, are mostly rendered problematic, some alternative basis for discriminating is required. In the circumstances, the challenge of interpreting phenomena of interest as resulting from the optimizing decisions of agents is an obvious alternative way of narrowing the options, and one that sits well with closed and atomistic set-ups.

Ultimately, though, the presumption of optimization is not necessary if achieving a deductivist model is the dominant consideration. Any assumptions that guarantee 'whenever *x* then *y*' can be utilised. And in the mainstream literature various alternative (atomistic) conceptions can be found (see Lawson 1997, Chapter 8). Hence if $\mathbf{T}_{\mathbf{Ex}}$ is encouraged by the prior insistence on $\mathbf{T}_{\mathbf{Ep}}$ the former is far less pervasive than the latter.

How about $\mathbf{T}_{\mathbf{Eq}}$? If the assumption that individuals optimise is in some ways the most satisfying to make at the level of substantive theory the obvious question of interest or relevance to pursue at a system level (using formulations found individually to have little or no explanatory power) is whether the set of equations of the system are at least in some sense mutually consistent. In other words, determinacy, as a property of a formalistic system, is an obvious (and perhaps almost the only) question of interest. It used to be the case that this notion was widely referred to as an equilibrium, and often surrounded, if misleadingly, with comments about real world states of affairs, along with references to the contributions of Adam Smith. But more recently such endeavour has waned, and mainstream contributors seem more open about their concern being merely with solution concepts of formalistic systems.

But if it is now more fully recognised that an 'equilibrium' is basically little more than a solution to a set of equations, it is equally clear that interpretations, other than equilibrium, can be put on solution concepts. In addition of course, a single equation model is often all that an individual contributor is interested in. So for several reasons $\mathbf{T}_{\mathbf{Eq}}$ is less pervasive than $\mathbf{T}_{\mathbf{Ep}}$. And indeed it is the latter that ultimately encourages the attention to equilibrium theorizing (as it does the focus on optimization).

Finally, it is because $\mathbf{T}_{\mathbf{Ep}}$ is taken as so fundamental that most of its proponents fail to appreciate a need to defend it or even mention it. To repeat Whitehead (1926: 61) yet again (also restated by Peacock):

> 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. . . .

Peacock goes on to suggest that economists do not make explicit ontological arguments, or even recognize their implicit ontological positions. The former suggestion is certainly true, and I suspect that the latter is often so as well. Peacock concludes that this reduces the persuasive value of my own project, involving, as it does, a critique of the mainstream presuppositions as untenable. I perfectly understand the difficulty here. But even if mainstream economists do take little note, there is still good reason for seeking to identify the causes of the problems and suggesting ways of resolving them. Fortunately the mainstream does not yet encompass all economists, and, as Dow reminds us, younger people do tend to be open to a wider range of ideas. And although I recognize that change always requires much more than ideas and argument, it remains the case that argument can count as well. I offer my assessment just because I find it to be as explanatory powerful of current developments as contending alternatives.

However, Peacock further asks, and with good reason, why it is that heterodox economists should be interested in my argument, even if it is correct. After all, if all I am doing is identifying what they already do, what difference can I actually make?

The point, of course, as Peacock himself realises, is that if it is indeed their ontological preconceptions that distinguish the heterodox traditions as heterodox, it does not follow that this is fully appreciated. Yet it needs to be, especially if the heterodox goal of a more pluralistic economics is to be realised. For so long as heterodox economists advance substantive claims, without emphasising the ontological presuppositions of these claims, the force of their argument is weakened. Most obviously, there remains the possibility that mainstream economists can take over the categories considered most important within the heterodox contributions, but without abandoning their insistence on methods of mathematical modelling. This is mostly how it has worked to date. Thus the post Keynesian emphasis has been on uncertainty, rather than the implicit presupposition of openness, and this has translated within the mainstream into mathematical risk; in similar fashion the institutionalist emphasis on evolutionary method, with its presuppositions of process as cumulative causation, has been translated into mainstream as a non-static version of game theory; and so on. Putting an explicit emphasis on the ontological presuppositions *per se* can help prevent the heterodox insights being deformed into questionable, typically unrealistic modelling categories.

Alternatively put, whilst the mainstream project can respond to substantive claims in ways that leave the state of the discipline much as it is, it can only accommodate insights from ontology though de-emphasizing the role of mathematical-deductivist modelling and allowing for a more pluralistic economics academy.

3 REISS

Julien Reiss starts with a good summary of my critique of the mainstream reliance on methods of mathematical modelling. However, his helpful assessment, if largely accurate, does contain interpretations of my position that, if understandable, are ultimately not always quite right. I thus appreciate the chance to clarify my views on certain matters.

There are two basic features of Reiss's description of my position that I want to qualify. The first is a suggestion that I hold that economists, in their explanatory endeavour, necessarily have no interest in, or conception of, reality, over and above a concern with event regularities necessary for deductivist explanation. At least I think this is what is being suggested when Reiss indicates his view that Lawson 'sets up a straw person called mainstream economics caught in a positivistic trap, where scientific explanations are no more than deductions from law-like statements, and the latter represent event regularities'. The interpretation I am taking is also consistent with Reiss's argumentative ploy of first observing my own support for a conception of social reality as, amongst other things, consisting in underlying, and changing, causal structures, and then pointing to two sets of authors, identified as mainstream, that 'agree significantly with

Lawson's precepts' – as if this undermines my position. Let me reply to this interpretation of my position before turning to the second misunderstanding in due course

I really do not hold, and have never defended, the position that mainstream economists are necessarily concerned only with events and their putative correlations, or that the latter features need be all that is involved in mainstream explanation. What I do argue is that the mainstream *a priori* commitment to methods of mathematical deductivist modelling restricts the manner in which substantive (including causal) claims can be theorized.

There is no presumption here that the broader visions of economists could not, or do not, coincide with a causalist ontology such as I accept. Indeed, after outlining my conception in *Reorienting Economics*, I comment that I doubt that it is especially contentious. My argument is just that the prior attachment to certain sorts of mathematical methods imposes an (often unnoticed) ontology mostly inconsistent with those visions. Indeed, and along with others working on social ontology, I have often argued that it is a largely unrecognized mismatch between the implicit ontology of methods adopted and the broader vision espoused, that accounts for many tensions in the history of our discipline, including Schumpeter's widely noted enduring inconsistencies (see Graça Moura 1997, 2002), as well as Marshall's inability to produce a second edition of his Principles incorporating insights from evolutionary biology (see Pratten 1998)

Notice, too, that I do not suggest that the methods of mathematical-deductive modelling which mainstream economists wield can never have an appropriate application. To the contrary, I suggest in *Reorienting Economics* that the perspective I adopt can identify the sorts of conditions in which success is likely and so cast light upon such successes as occur (see e.g. p. 20). And nor do I suppose that mainstream economists do nothing but mainstream economics; indeed I expressly deny that this is so (see e.g. p. xxi).

So it is not obvious that Reiss's references to the studies by Card and Krugar (1994, 1994) and by Hoover (2001), serve as a challenge to the position I hold. The former two contributors seek out conditions in which their method, an example of an approach I systematize as contrast explanation, is entirely appropriate, and examine the effects of the introduction of minimum wage legislation on unemployment. Hoover's book is an interesting largely philosophical account of the nature of causality and a questioning of its relevance to macroeconomics.

I think both works are extremely admirable in various ways. I do not have the space to give detailed comment here. And nor do I want to debate whether the sort of contributions these authors typically produce should render them sufficiently acceptable to the mainstream. But I do note that once Card and Kruger have achieved their main insights they turn to a regression analysis, which, though forming a very small fraction of their (excellent) book, constitutes a major portion of that part of their study also published in a mainstream journal (Card and Krugar 1995). And it is noticeable, too, that Hoover's empirical case studies involve only regression analyses. In other words, even studies as original and insightful as these still tie-in with the standard mathematical-deductivist approach.

This brings me to the second interpretation of my position that I think Reiss holds, but which is somewhat mistaken. This is the notion that I take a narrow and inherently negative orientation towards mathematics *per se*. Reiss does not put things so starkly. But he does emphasise, as if opposing me, that he thinks 'there is no necessary link between mathematics and bad practice'; that it 'is neither true that expressing a claim in mathematical terms implies that explanations (that somehow use this claim) must be deductivist, nor does a deductivist mode of explanation presuppose event-regularities'; that 'there is no necessary connection between mathematical methods and an ontology of event-regularities'; and that 'An association between mathematical methods and a causalist ontology is possible'.

Let me first respond by stressing that I fully agree that 'there is no necessary link between mathematics and bad practice'. Indeed, I cannot emphasize my agreement with this statement too strongly. *My argument is not at all an anti-mathematics one; and it never has been*. I have only ever criticised the way (certain) mathematical methods tend to be used in modern economics. Indeed it is precisely the belief that mathematics ought not to be applied without due care and consideration, coupled with a conviction that in modern economics it too often is so, that explains the direction of much of my writing. If you like, my concern is that much of economic modelling appears somewhat analogous to a violin being used as a drumstick. To suggest that this may be 'bad practice' is in no way to devalue the violin, or to deny it a place in the orchestra.

Nor do I limit mathematics to deductivist modelling. Actually, if what economists do is mathematics, it is a form of applied mathematics; it is the application of mostly already worked out mathematical systems. And if it is ever appropriate to 'associate' a method with an ontology I see no reason to suggest that some mathematical methods could not be associated with a causalist ontology.

If I do *not*, then, limit mathematics to methods of deductivist modelling I *do* contend that the *sorts of modelling methods mostly utilised by modern economists* are deductivist in nature. My one difference with the position Reiss raises against mine is that I do suppose that the deductivist mode of explanation presupposes event-regularities. But actually our disagreement here is merely semantic. For (like many others) I actually define deductivism as an explanatory approach for which event regularities (whether real or imagined) are an essential component (for my own discussion of Mill and others on tendencies, which relates to this, see Lawson 1998).

In short, I do not denigrate the use of mathematics. I do, though, believe that the sorts of mathematical-deductivist methods mainstream economists mostly use presuppose an implicit worldview that is not especially typical of social reality. I also suggest that many of the widely acknowledged failures of the discipline arise just because these methods are being applied in conditions for which they are not especially appropriate. In consequence, in *Reorienting Economics* I argue for a more pluralistic orientation to economic theorising, and spend time demonstrating that alternatives methods of relevance do exist. In this I do not suggest that formalistic methods be excluded for the methodological options on offer. But I do insist that methods of mathematical-deductivist reasoning (like any other tools) have limits to their usefulness, and that this be recognized and respected. However, I see this as a pro-, rather than an anti-, mathematics position.

In concluding, let me thank the three reviewers once more for their critical input. Although it is possible that we all agree far more than we disagree, it is sensible that we concentrate on potential differences. Of course, this is not the place we can expect to resolve the latter, and I do need to reflect more on some of the issues raised. But I welcome the opportunity provided here to discuss and reconsider some parts of the overall thesis I have been putting forward.

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ACKNOWLEDGEMENTS

My thanks to Steve Pratten for helpful comments as well as for organising the round table on my book at the September 2003 Leeds INEM conference, where these reviews, and my response, were first presented

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