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**Featuring:** *Tony Lawson*

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## Chapter 6

# Heterodox Economics and Pluralism

## Response to John Davis

Recent years have seen the emergence of numerous activities in economics identified first and foremost as heterodox<sup>1</sup>. In the modern academy, it seems, heterodox economics is alive and flourishing. But what (sort of thing) is it?

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<sup>1</sup> For example 1999 witnessed the formation of the UK based *Association for Heterodox Economics* (AHE), an organisation that now sponsors an annual conference, and has run numerous postgraduate training workshops and more. In October 2002 the University of Missouri at Kansas City hosted a conference on “The history of heterodox economics in the 20<sup>th</sup> century”. December 2002 saw the inaugural conference of the Australian *Society of Heterodox Economists* (SHE) at the University of New South Wales. Six months later, in June 2003, back at the University of Missouri at Kansas City, ICAPE (the International Confederation of Associations for Pluralism in Economics) celebrated its ten-year birthday with its “first world conference on the future of heterodox economics.” The July 2004 edition of the *Review of Radical Political Economics* comprised a special issue on the History of Radical

John Davis and I are both concerned to understand the nature of the contemporary heterodoxy in economics. We are so, I believe, not because either of us is especially fond of taxonomy, certainly not for its own sake. Nor, I believe, do either of us wish to reify or fix the project. There is no reason at all to suppose that heterodox economics, any less than any other social phenomenon, is other than intrinsically dynamic and indeed ultimately transient. However, things in process can still be known, if only as historical (and geographical and cultural) products. And there are always likely to be gains to critical self-reflection upon the nature of that with which we are dealing or involved at any point in time. For my own part at least, I also think a critically self-aware heterodoxy is of vital strategic importance at this moment, given the degree of dominance and exclusionary orientation of the contemporary mainstream.

If, Davis and I, then, seek, for whatever reason, to gain insight into the nature of the contemporary economic heterodoxy, our assessments of what it is are seemingly not the same, with Davis using his present contribution to set out an alternative to the conception I elaborate in *Reorienting Economics*. In the main part of my response, I indicate why I prefer to stick with my conception in the face of Davis' arguments for an alternative.

Discussions of the nature of heterodox economics are not overly common in the economics literature, but those that do emerge seem increasingly to be linked to discussions of pluralism, or being pluralistic, in economics. At the end of his piece, Davis hints that there may be problems with my own professed pluralistic orientation stemming from my very conception of the nature of heterodoxy. One or two other commentators have been more explicit in expressing concerns of this sort. I thus treat this current essay as an opportunity not only to clarify my conception of the nature of heterodoxy but also to indicate why I believe the expressed concerns (that my position may carry undesirably anti-pluralistic features) are in the end mistaken<sup>2</sup>.

I start by briefly summarising my account of the nature of contemporary economic heterodoxy before turning to assess why Davis rejects it for some alternative. I deal with the topic of pluralism (or being pluralistic) in due course.

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Heterodox Economics. The University of Utah sports a *Heterodox Economics Student Association (HESA)*. And on the Internet it is possible to find a large number of sites dedicated to promoting specifically "heterodox economics". As I write, for example, it is possible to find at <http://www.orgs.bucknell.edu/afee/hetecon.htm> a 'heterodox economics web' listing: heterodox economics associations; heterodox economics journals (scholarly); heterodox publications (news, commentary and analysis); heterodox teaching resources, heterodox discussion groups, and so on.

<sup>2</sup> In doing so I am also responding to Edward Fullbrook's editorial request to 'discuss pluralism somewhere in the book'.

### A conception of contemporary heterodoxy in economics

I take it to be analytic to the notion of heterodoxy that it involves the rejection of some doctrine held to be true by a prevailing orthodoxy. That is simply what it means to be heterodox. And it is clear that the self-identifying heterodox traditions in modern economics not only all ardently oppose the mainstream output currently, but also have done so persistently over a lengthy period of time, even through changes in the mainstream forms. Thus, it seems reasonable to conclude that the heterodox opposition stands against some feature that is enduring and central to the modern mainstream; certainly it is opposed to something common to, or presupposed by, all its contributions.

In order to distinguish the modern economic heterodoxy *qua* heterodoxy I thus start by identifying the (set of) feature(s) of the modern orthodoxy or mainstream that is common to all its contributions. The assessment I defend in *Reorienting Economics* and elsewhere is the following. The project that has dominated the discipline of economics for the last forty years or so is one that, although highly heterogeneous in detail, and fluid in revising its manifest form, is united and stable in, *but only in*, adhering to the following single doctrine or edict. This is an insistence that mathematical methods be more or less always employed in the study of economic phenomena. This insistence often runs over to claiming that any contribution that does not take the form of a mathematical model is not proper economics (see Lawson, 2003a, chapter 1).

This is not to say that there is not an elite within the mainstream who feel their privileged positions allow them sometimes to set out some less-than-overly formalistic pieces, especially in presidential addresses and such like. But it is only in virtue of their previous, and other, formalistic contributions that such deviations are rendered legitimate. Such individuals may even introduce their favourite non-mathematical associates into the mainstream scene. But whilst the latter chosen few are few indeed, they are not really part of the mainstream as such, and are seemingly mostly included/tolerated only because of their associations with powerful others who are. Even here, though, the mathematical contributions of their patrons (or matrons) constitute the essential condition.

If an oppositional stance to the noted orthodox doctrine (that formalism is normally compulsory) is the nominal essence of the current heterodoxy, what is its real essence, the explanation of this opposition?

It is the recognition (albeit one that is often no more than implicit) that the universal application of the sorts of mathematical methods that mainstream economists formulate presupposes an untenable account of social reality as

everywhere composed of systems of isolated atoms. In *Reorienting Economics* I argue that underpinning this heterodox oppositional stance is an implicit (and sometimes more explicit) commitment to the alternative sort of social ontology I defend in that book, namely of emergence, structure, process, internal relationality, and so forth.

So in short, if the only common and so distinguishing feature of the current mainstream is its continuing insistence upon forms of mathematical deductivist reasoning, the real essence of the heterodox opposition (*qua* heterodox opposition) is an accepted (but rarely explicitly acknowledged) ontological conception. It is a conception that is at odds with the implicit (closed-system and atomistic) ontology of mainstream deductivist reasoning, and so ultimately accounting for the heterodox oppositional stance.

Notice, though, that I do *not* distinguish the individual heterodox traditions from each other according to ontological commitments; indeed I suggest that ontological presuppositions are something they broadly hold in common. Nor, incidentally, do I believe it is possible for the separate traditions to be identified or distinguished according to their own results or methodologies principles and such like. Rather my assessment is that old institutionalism, post Keynesianism, feminist economics, Austrianism, Marxian economics, etc., are each best conceived in terms of *questions and issues traditionally addressed* within their own programme.

Thus old institutionalism, I argue, is a project concerned first and foremost with questions of stability and change in economics. Hence, its traditional and ongoing concerns are especially with technology (perceived as an important source of change), habits and institutions (seen as important sources of stability), evolutionary science, and so forth (see *Reorienting Economics* chapter 8, and also Lawson, 2002, 2003b). I return to this issue of characterising the different heterodox traditions below.

With this heterodox emphasis on questions and interests rather than answers, there is scope both for different members of any given heterodox tradition to produce competing conceptions, as well as for the best-substantiated contributions to be continually improved upon. So, the conception I defend is quite consistent with the sort of (shifting) variety of contributions we find within any heterodox tradition.

Notice, too, that I do not suggest that heterodox contributors do not, or should not, experiment with mathematical deductivist techniques and the like. Social conditions may occasionally arise that are locally of a sort presupposed by methods of formalistic modelling. If I characterise the mainstream in terms of its usual *insistence* that (for a contribution to count as economics) various sorts of mathematical deductivist methods be everywhere and always employed, I conceive heterodoxy as an (implicitly) ontologically motivated rejection of *the universalising and dogmatic*

*aspects of this stance*, not as a refusal ever to experiment with formalistic methods, or to employ them where conditions indicate their relevance.

The mainstream itself is pluralistic within its constituting constraint, of course. Despite the best advice of those economists associated with the Bourbaki school, it is impossible to pursue a mathematical economics purely in the abstract. There has to be content, and this is found to be highly variable. Indeed, both the substantive programmes pursued by the mainstream, and the sorts of mathematical deductivist methods employed (along with their interpretation) are highly variable (Lawson, 2005b). There are those who argue that *within* orthodoxy there exists a dominant and relatively enduring (though by no means fixed) ‘neoclassical core’ or some such. But assessments of what this entails vary quite significantly (see for example, Ben Fine 2006, Geoffrey Hodgson 2006, Christian Arnsperger and Yanis Varoufakis 2004, or Edward Fullbrook, 2005). For the purposes at hand, I do not need to consider these matters further at this point, though they do eventually bear on some of the contentions made by Davis. So, I return to this topic as necessary below.

Similarly, I will have more to say on this overall conception of the broad structure of modern economic projects or groupings when I turn, in due course, to consider the issue of pluralism. But for now, this outline is sufficient to allow a contrast to be made with the alternative conception of economic heterodoxy that Davis is seeking to promote.

### **Davis’ Alternative Conception**

How does Davis’ assessment differ? After summarising my conclusions concerning the respective natures of mainstream and heterodox economics, Davis writes:

“Broadly, I agree with these conclusions. My concern is that they may be truer of economics circa 1980, and neither fully capture the state of economics since then, nor provide us a sufficient understanding of the current direction of development of economics”.

Now on the face of things this is an unpromising response, certainly a surprising one. For since 1980 the mainstream has become increasingly formalistic in orientation, not less so, certainly in my neck of the woods, but seemingly in most other places too. The mainstream output is still basically deductivist in nature presupposing the same closed and atomistic scenarios. In consequence, if the formalistic emphasis, given the nature of social reality, was inappropriate around 1980 it remains at least as inappropriate today. And any heterodox tradition worthy of

being so identified presumably continues to recognise the inappropriate nature of mainstream formalistic contributions. Thus, if the heterodox opposition around 1980 was motivated, if implicitly, by the acceptance of an ontological orientation that was significantly different from, opposed to, and indeed far more sustainable than, the presuppositions of these formalistic methods, I see little obvious reason for heterodox economists to reorient the nature of their opposition since that time.

So how or why does Davis argue that things have changed? He has little to say about the mainstream project *per se*. His main concern is instead with “the changing nature of heterodox economics and its changing relation to orthodox economics”. Specifically he writes:

“I will argue a view I believe is largely contrary to Lawson’s, namely:

- (i) that heterodox economics is more heterogeneous than [Lawson] and many others believe and moreover heterogeneous in ways generally not recognized by many who see themselves as heterodox,
- (ii) that the reference of the term ‘heterodox economics’ is quite different from what most economists, heterodox and orthodox, believe it to be, and
- (iii) that understanding this heterogeneity is important for understanding the direction of development of current economics”.

Let me consider each contention in turn.

### **The heterogeneity of heterodox economics**

Now whether or not heterodox economics is more heterogeneous than I and many others believe, and in unrecognised ways, this constitutes a challenge to my conception of the nature of heterodoxy only if the latter conception is somehow found to be at odds with, or unable to sustain, the forms of heterogeneity that Davis has in mind. Our conception of apples may remain stable despite the introduction/discovery of new varieties or heterogeneity in terms of size, shape, colour, texture, and so forth.

So what sort of heterogeneity does Davis have in mind? Davis focuses on features to which he refers as the ‘origins’ of these projects, on the processes that certain projects pass through (or have passed through) in becoming heterodox (or orthodox). Specifically, he distinguishes those that fail to become orthodox following a period of pluralism; those that lose the status of orthodox when a new orthodoxy emerges; those that fail to redirect orthodoxy from outside orthodoxy; and others that fail to redirect orthodoxy from inside orthodoxy.

Davis recognises that his interpretation of specific heterodox strands from this perspective is contentious, but adds:

“Nonetheless, the history of economics seems to tell us that there are different types of origin stories for different research approaches in economics, and thus that what most people take to be heterodox economics exhibits considerably more heterogeneity when seen from this dynamic perspective than appears to be the case when point-in-time comparisons between different approaches are made”

Now contra Davis’ suggestion, I have no problem with any of this. I cannot see, though, how it bears on the issue before us. If I am right that the only coherent way to identify the mainstream is according to its insistence on a particular form of method, and that modern heterodoxy is appropriately distinguished by its ontological grounds for opposing this insistence, it is not obvious that the origins of forms of either mainstream method or heterodox opposition have any necessary bearing (any more than our conception of apples is necessarily threatened by recognising that competing varieties may originate in different countries or regions). Certainly Davis gives no reason to conclude to the contrary.

Instead of explaining why he supposes the traits he observes somehow undermines the conceptions of the nature of the current heterodoxy that I defend (a conception that Davis seemingly accepts as characterising the situation circa 1980) Davis proceeds by distinguishing a further form of difference or heterogeneity within the heterodoxy, one that he interprets as a matter of ‘orientation’. This relates to whether heterodox criticism of the mainstream or ‘core’ is internal/immanent or external. Or, as Davis prefers, it relates to whether the approaches are ‘inward’ or ‘outward’ in orientation, or oriented towards or away from the ‘orthodox core’.

The former orientation is that of engaging the mainstream by “challenging the core’s own principles from the vantage point of those *same* principles”. If this seems clear enough (though as I argue in response to Geoffrey Hodgson below, I believe it unlikely to be successful), the outward orientation, for Davis, seems to involve drawing on principles found in different disciplines:

“Orientation towards the field’s periphery is a matter of placing emphasis on principles closer to other sciences beyond the field’s boundaries, principles moreover which appear at any given point in time to be clearly not part of the field’s core”.

Again I feel bound to say “so what?” I cannot see how these considerations, certainly in the form presented so far, bear on the issue before us. Moreover, some



heterodox groups clearly lean both ways, with some of their members emphasising internal critiques, others looking away from the core, and some doing both.

Davis summarises his position as follows:

“Heterodox economics is heterogeneous, then, because different approaches differ in the ways that they combine different origin stories and different orientations. Their origin stories and orientations, that is, distinguish their different dynamics, and accordingly serve to place their point-in-time shared commitments in historical context as temporary and transient states of affairs”.

Now no one is denying that most things in this universe, and not least social phenomena, including the various traditions in economics, are transient. Everything becomes and begoes in time, and often in complex ways. And the heterodox traditions are clearly heterogeneous in numerous ways, including their origins and intellectual strategies or orientations. But it is not yet clear that differences in origins or even of orientations *of the sort* that concerns Davis, bear significantly on the issue of the relative transience/persistence of ‘shared commitments’ at this point in time.

My assessment, as I say, is that the feature that unites the various contemporary heterodox projects *qua* heterodox projects is recognition that the mainstream mathematical-deductivist emphasis presupposes an ontology that is at odds with (or at best a very special case of) our most sustainable account of the nature of social reality. If this is so, then so long as an insistence on such formalism remains orthodox doctrine, and to this point support for this doctrine is certainly proving resilient, the heterodox traditions have a rationale for actively uniting through any differences, including differences in origins and debating strategies. Though Davis’ comments on certain forms of heterogeneity are interesting, they do not yet give any reason for rejecting the conceptions I have been defending, or provide a sustainable basis for some alternative.

### **Heterodoxy is not what we think it is**

In the second section of his paper Davis observes that after around 1980 a number of new research programs begin to be emerge:

“These include game theory, behavioral economics, experimental economics, evolutionary economics, neuroeconomics, and complexity economics. Other new approaches and combinations of these have emerged in their wake”.

Davis’ primary goal in this section of the paper seems to be to establish that these new programmes must be regarded as heterodox. Now this is something on which we

do disagree quite significantly. As this section appears to contain the heart of Davis argument, I will spend some time on it. I start by very briefly sketching my own interpretation of the status of the ‘new programmes’ before examining whether Davis gives any compelling reason to reject it.

My own assessment is that it is an error to suppose that any new method or programme of the sort in question (whether or not it figures on Davis’ list) is inherently either mainstream or heterodox (any more than, say, mathematics is). Notice that the sorts of programmes on Davis’ list are not constituted in terms of substantive (economic) results or questions (whether about how the individual or economy or society works). Rather they are essentially methodological programmes concerned with ways of doing economics, including ways of incorporating ‘observations’ or results from other (experimental, animal studies, neuroscience, etc.) disciplines.

The feature that makes the difference in terms of whether any ‘programme’ becomes in practice associated with the orthodoxy or heterodoxy is how it is used. I have already argued that what distinguishes the mainstream is its *insistence* on formalism. But, as already noted, I do not suggest that heterodox economists should not experiment with formalistic methods. The mainstream abuses formalism by repeatedly using it in situations in which it is inappropriate to do so, failing to seek to understand its scope and limitations. But use of formalism is not in and of itself essentially mainstream. And nor need be any new method or any (aspects of) the new methodological programmes. As I say, it all depends on how things are done.

Of course, the form that any new programme does take (or the manner in which it is presented) is likely to influence its reception in the academic institutions of modern economics. Specifically, if a programme is not (or cannot easily be) formulated mathematically, the existing mainstream is unlikely to be receptive at all (though there can be no guarantee that each particular approach will be accepted anyway). But then again a formalistic programme may in principle be ignored by the mainstream, and yet used by heterodox economists, albeit typically only under conditions in which it seems *prima facie* appropriate; or for purposes of trial and error experimentation. As I say, much depends on how things are done.

Davis’ position or interpretation, though, is rather different. According to him, as I say, these new programmes are simply to be characterised as heterodox. Let me trace through his reasoning as to why.

Davis starts out by suggesting that with “the possible exception of game theory [...] none of these new approaches has yet come to be regarded as orthodox”. The

nearest that Davis comes to saying what he means by orthodox at this stage is via the following passage:

“Orthodoxy typically requires moving from being purely a research program to being a well-established teaching program, where changes in teaching work their way from top downward through the social hierarchy of universities and top programs. Neoclassicism, of course, still dominates economics teaching (though this dominance has become more uneven as courses in experimental economics and non-linear simulation techniques are added to more and more department curricula)”.

This sort of process, if it is indeed more than a contingent feature of how a new (feature of) orthodoxy may be established, seems to be precisely what Davis (even in the parenthesis of the noted passage) is arguing is happening with regard to the ‘new programmes’.

However, believing himself to have established with the above remarks that “these new research programs in economics are not orthodox”, Davis asks: “what are they?” Suggesting that they accept in varying degrees all three of the commitments of ‘heterodox economics’ that, Davis supposes, “constitute the dividing line between orthodoxy and heterodoxy circa 1980”, Davis concludes that “these new research programs taken as a group must thus be by definition heterodox”.

#### **Davis’ (initial) conception of heterodoxy**

What precisely are these three commitments that constitute the dividing line between orthodoxy and heterodoxy circa 1980? In the opening paragraph of his paper Davis writes:

“Tony Lawson’s critique of mainstream economics is that it is everywhere formalistic and deductive, that this leads it to a closed systems approach based on identifying social event regularities, and that this is inappropriate strategy for dealing with the subject matter with which economics is concerned (Lawson, 1997, 2003a). Heterodox economics is then distinguished by its rejection of all this and by its commitment to an ontological analysis that takes social reality to be intrinsically dynamic or processual, interconnected and organic, structured, exhibits emergence, and includes value and meaning and is polyvalent (Lawson, 2006a, pp. 495-6). Broadly, I agree with these conclusions”.

However, on the next page he sets out his own conception of “what most people identify as heterodox economic approaches”. Here he lists the ‘three commitments of heterodoxy’ to which he refers in the passage noted above (where he questions what these new research programs are). According to Davis, the “shared three specific commitments circa 1980” are:

- “(1) rejection of the atomistic individual conception in favour of a socially embedded individual conception
- (2) emphasis on time as an irreversible historical process
- (3) reasoning in terms of mutual influences between individuals and social structures”

Despite Davis’ declaration of broad agreement with my own position, it is not clear that these commitments, as formulated, express precisely the same ontology as I defend. In fact, I worry that, as formulated, they may be too vague to do the job.

For example, the idea of an individual being embedded, expressed in commitment (1), is not inherently inconsistent with it being an atomistic individual. To embed is firmly to enclose; there is no connotation of the entity in question being continually transformed or re-constituted associated with the notion of embeddedness. When we employ the metaphor of an atomistic individual I presume we do not mean a small one, or even a randomly moving one, but (like, for example, Keynes, 1973, pp. 276, 277) an entity that has its “own separate, independent, and invariable effect” whatever the context. In modern engineering, for example, the category of *embedded system* tends to refer to a special-purpose system in which a computer is completely encapsulated by the device it controls and (unlike general-purpose computers) performs but one (or a few) pre-defined tasks only.

The point is that human beings are not so much socially embedded as in large part socially constituted; human beings are social beings. From birth (and perhaps earlier) onwards, our personal identities are moulded in relation to others as well as to existing socio-cultural structures. Eventually we also take on various positional or social identities, with all such identities determined in relation to others and continually reproduced/transformed through practice.

Nor, turning to commitment (3), is it just that there are “mutual influences between individuals and social structures”. This is too weak or contingent a claim. Rather each is in part constituted in relation to the other. Teachers presuppose students (and vice versa) as well as the connecting relationships between the two, relations to the educational authorities, to funding agencies and so forth.

The notion of time irreversible historical processes, expressed in commitment (2), is fine if by this category we really mean ‘processes’ and ‘historical’ and indeed real time. Everything that happens in the social world is reproduced and/or transformed through human (transformative) practices, practices that are situated in space-time, in geography and history, and constitute an open-ended continuous process of becoming.

I am not merely suggesting that my reworking of Davis' three heterodox commitments is the more sustainable interpretation of the ontology in question. I believe, too, that it is the conception implicit in heterodox thinking. We find it (whether implicit or explicit) in thinkers like Marx, Veblen, Keynes, and Hayek around whom heterodoxy was organised in the 1980s (see Lawson, 2003a, part III). But, more to the point, an acceptance of this ontology explains heterodoxy's loud and persistent opposition to the mainstream. For it is only some such ontology (or Davis' commitments interpreted in some such fashion), that resists being expressed as the presuppositions of the sorts of mathematical deductivist-methods that mainstream economists continually wield.

Remember there is no heterodox opposition to the use of mathematical methods in natural science even where (as in super-string theory) such methods dominate; an opposition exists in economics just because of a conviction that the sorts of mathematical deductivist methods continually employed are not especially appropriate to social analysis.

Now it may well be that, in setting out his three principles or 'commitments of heterodoxy', Davis intends precisely the interpretation that I have just elaborated, where both human beings and structure are repeatedly reproduced/transformed through practice. I suspect he does intend this understanding. If not, then I think he is mischaracterizing the traditional heterodox opposition. But if Davis does indeed intend my interpretation/elaboration of these 'commitments' then I think there are problems with his assessments of recent developments. Let me briefly elaborate.

### **The nature of the new programmes**

The "new research programs" Davis mentions as emerging post 1980, let me recall, "include game theory, behavioral economics, experimental economics, evolutionary economics, neuroeconomics, and complexity economics". According to Davis these programmes must be heterodox just because they *collectively* (but only collectively, not individually) adhere to Davis' three commitments of heterodox economics listed above:

"[...] these new research programs taken as a group – though not individually – accept in varying degrees all three of the commitments of heterodox economics listed above. As these three commitments in my view and that of many others also constitute the dividing line between orthodoxy and heterodoxy circa 1980, these new research programs taken as a group must thus be by definition heterodox"

I do confess to finding this criterion, as expressed, not only unconvincing, but also rather puzzling. It seems to allow that we can render any individual programme heterodox just by virtue of grouping it with others that collectively span the three Davis commitments. If so, we could, of course, add in the mainstream itself and thereby render it heterodox.

We might suppose that we can rule out the latter incongruous possibility by stipulating that for a new programme to qualify as heterodox in the (collectivist) manner suggested, it must satisfy at least one of Davis' three commitments. Perhaps this indeed is what Davis has in mind. But as a criterion this too is hardly satisfactory. For, consider a programme thought to satisfy a (proper) subset of Davis' three heterodox commitments. It seems to follow that the claims to heterodoxy of any such programme depends on how it is grouped with others, and in particular on its being lined it up with a package of programmes that collectively satisfies the commitment(s) it does not itself satisfy. Whether or not the programme in question is included in such a package (one that thereby spans the three Davis commitments) is something that can seemingly be changed at will, thus rendering the (heterodox/orthodox) status of any such programme largely arbitrary, certainly open to manipulation (and of course any two contributors can reach opposed conclusions just by selecting the members of their package of programmes differently).

Davis does anticipate the objection (from "those who traditionally regard themselves as heterodox") that, because no single one of the new programmes exhibits all of the 'three principles of heterodoxy' (they only do so as a group), then no single program ought to be regarded as heterodox. But without examining the nature of this objection, and I suspect without appreciating the tensions or apparent incoherencies that arise, Davis dismisses the objection for reasons I come to below.

Before looking at Davis' response to that anticipated objection, however, it is not without interest to question whether the programmes listed by Davis, *in the form in which they are in fact entering the discipline*, do, even collectively, meet his revised criterion of heterodoxy. And I must admit that it is not clear to me that (individually or collectively) these 'new programmes' inevitably meet *any* of the three commitments (let alone all of them) *in practice*. At least this is so if we are to interpret the latter commitments according to the ontological sketch (or elaboration of Davis' commitments) that I have given the preceding section.

Consider complexity theory, which Davis suggests in a footnote is the most promising candidate for meeting all three of his 'heterodox commitments'. As this has entered economics, this is basically a form of non-linear modelling. As such it is nothing more than an additional form of deductivist method. Its implicit ontology is still one of closed systems of isolated atoms. Perhaps complexity theory will be

thought to be at least consistent with Davis' second 'heterodox principle' of "time as an irreversible historical process". Certainly the path mapped out in any simulation will be irreversible. But there is no history, no time, and no real process. There is merely a one-way relationship. If we consider the functional relationship  $y = x^2$ , then we find that for any given value of  $x$  we can determine a unique value of  $y$ , but we cannot take the reverse path, a given value of  $y$  does not lead us 'back' to a unique  $x$ . Complexity economics, as it stands, is merely a complex version of such a functional relationship. As I say, the ontology is clearly still one of isolated atoms. All that has changed is the degree of complexity of the system formulations.

I recognise that the concept of complexity (like 'equilibrium' and some other system concepts, see Lawson, 2005a, 2005b, 2007) can be interpreted in a multitude of ways (see especially Perona, 2004). But the interpretation I am giving here is fairly characteristic, not a peculiar deviation advanced merely by, say, a few members of the mainstream. Consider an enthusiastic defence of complexity economics found in a recent edition of (the hardly mainstream) *Post-Autistic Economics Review*. Here we are told that in complexity economics the:

"dynamics of a complex system are best described by non-linear as opposed to linear relationships, but as yet it is not possible to accurately model the former. The closest one can come are simulations based on cellular automata" (Smith, 2004).

I am also not sure how game theory, a further programme on Davis' list, as it figures in modern economics, even begins to qualify according to Davis' heterodox commitments. As far as I can see it is a formal modelling approach focussing on typically isolated worlds of optimising atoms 'making' decisions in contexts where the anticipated decisions of other optimising atoms are considered. No societal constitution of individuals, and no real time or history.

Behavioural economics is a programme that claims to combine psychology and economics in investigating what happens in markets where people display (what economists seem to perceive as) 'non-rational' motivations or behaviours (such as fairness, envy, present-bias, and so forth<sup>3</sup>). So conceived, the programme need not be formalistic at all. However, as it is being taken up in economics it seems still to be mostly a deductivist modelling endeavour, thus presupposing the usual systems of isolated atoms. In most cases, indeed, atomistic agents continue to maximize a preference relation over some space of consequences where any solution typically involves standard equilibrium concepts.

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<sup>3</sup> In essence the typical paper starts with some description of phenomenon uncritically accepted from research in psychology, or frequently even studies of animal behaviour.

Neuroeconomics is a form of analysis that makes use of data on brain processes to suggest new underpinnings for economic theories concerned with issues like how much people save, why there are strikes, why the stock market fluctuates, the nature of consumer confidence and its effect on the economy, and so forth. As such, it too need not be tied to formalistic modelling. Moreover, there does indeed seem to be a (relatively non-formalistic) strand of neuroeconomics, mostly content with reporting (albeit typically rather uncritically) the research findings of neuroscience and suggesting how these findings might be tied to ‘non-rational’ motivations. But it seems to me that the increasingly more dominant strand is a further form of mathematical-deductivist modelling closely allied to behavioural economic modelling. Thus whilst behavioural economics concerned with formalistic modelling has previously relied heavily (if again rather uncritically) on the findings of experiments and animal behaviour studies, this second strand of neuroeconomics seems to be developing largely as a version of this form of behavioural economics that seeks also to draw (uncritically yet once more) on the reported findings of neuroscience<sup>4</sup>.

I could go on. But my concern is surely clear. If we interpret Davis’ three heterodox commitments in a manner that actually fits with the ontological conception I defend (to which Davis signals agreement), and if we examine the form in which the new programmes are mostly being taken up, it is not even clear that these new programmes (collectively or otherwise) conform in practice to *any* of Davis’ commitments, let alone all of them.

I emphasise, that from my own perspective none of this matters anyway (at least for the question of deciding how the new programmes are best to be classified). For on my own interpretation, the feature that characterises heterodoxy *qua* heterodoxy is opposition to the mainstream insistence that formalism only be used. From this perspective, as I say, the new programmes are not inherently heterodox or mainstream; it all depends on how they are used.

### **A new criterion of heterodoxy?**

As we have seen, though, Davis demurs, insisting that the new programmes, seemingly whatever their form or mode of application, are necessarily a part of the contemporary heterodoxy, just because *collectively* they adhere to Davis’ three commitments. As I say, I am not sure that even the latter is actually so. However, at this point I want to turn, as promised, to Davis’ response to the objection he anticipates, namely that some economists might resist characterising the new

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<sup>4</sup> For a useful critical overview of these developments see Ariel Rubinstein, 2006.



programmes as heterodox just because any single programme on its own might not meet all of three of Davis 'heterodox commitments'.

What is Davis' response? It is to suggest that this criticism is 'fair' only "if the standards of heterodox economics circa 1980 are to be thought good for all time". He continues suggesting that "if we accept that what counts as orthodox and heterodox is historically changing, then [...] it seems we should also be open to the reconstitution of what counts as heterodox".

Well yes, we can accept that everything is changing, so clearly it would be wrong to reify what counts as heterodox. So let us be open to the possibility that what counts as heterodox will shift. By similar reasoning, we should be, and are, open to the idea that what counts as being human, copper, organic, a tree or an apple may shift. But still we need an argument or something to persuade us that the defining criteria for any of these, including what counts as orthodox or heterodox, has shifted. Instead, all we really get from Davis is the statement that because everything is historically changing "we should also be open to the reconstitution of what counts as heterodox".

It seems to me that, at this stage of his discussion at least, instead of seeking somehow to ground his case, Davis' argument for interpreting the new programmes as heterodox has become rather self-serving: *Here is a criterion of what currently counts as heterodox. Here are some new programmes. They do not fit our criterion for counting something as heterodox. To infer thereby that they are not heterodox is only a fair criticism if we stick to our criterion of what it is to count as heterodox. Everything changes eventually, so let us change our criterion of what is to count as heterodox, and in such a manner as to admit the new programmes into the heterodox.*

In short, Davis seems not to provide any good reason for adopting his revised criterion for heterodoxy; but rather is driven more by an interest in finding a criterion that, if accepted, will allow heterodoxy to accommodate the new programmes. Moreover, it is not obvious that his revised formulation is especially coherent (the status of an individual programme depends on the package of others in which it is included) or even that, appropriately interpreted, any of these three commitments (let alone all of them) are satisfied by the new programmes in the form they are being taken up. In consequence, to this point at least, I do not really find compelling reason to abandon our seemingly previously shared (Davis' circa 1980) conception of heterodoxy.

#### **A second new criterion of heterodoxy?**

Instead of moving to provide the missing argument for his case that, because collectively satisfying the three commitments, “these new research programs taken as a group must thus be by definition heterodox”, Davis seems next to confuse matters further by suggesting the existence of a yet additional new criterion of heterodoxy, one that again is said to render the new programmes heterodox. For Davis next writes:

“Indeed, *prima facie* the new research programs in economics are heterodox in virtue of their origins *outside* of economics in other sciences. It would be a mistake, I think, to claim that the understanding of science in other fields is the essentially same as it is in economics, since why otherwise are there different sciences? Thus the emergence of the new research programs in economics presumably imports new science principles into economics. Examples include behavioral and neuroscience foundations for choice, graph-based network analysis, experimental techniques, and non-linear, non-equilibrium simulation methods. Identifying these developments as heterodox is not to say, of course, that they represent those heterodox principles most valued by those individuals who currently self-identify themselves as heterodox. Nor is it to say that these are all necessarily valuable principles for economics. The main point, as emphasized in Davis (2006), is that what has come into economics from other sciences cannot be orthodox, at least at the outset”

It is feasible that Davis is maintaining here his first transformed criterion of heterodoxy (namely that the programs in question collectively satisfy his identified heterodox commitments), and arguing that because the ‘new programmes’ originate outside economics they must (collectively) qualify. But if, as we have seen, it is not clear that *in practice* these new programmes Davis lists collectively do satisfy his three commitments, it is even less obvious that *any* set of new programmes must do so merely in virtue of originating outside economics.

So Davis, as I say, is presumably here presupposing here some second new criterion of orthodoxy. It is one that can presumably make sense of both the contention of the first sentence of the noted passage, namely that a program originating outside economics must be heterodox, and also the contention of the last sentence, namely that “what has come into economics from other sciences cannot be orthodox, at least at the outset”. Why should these contentions be accepted? Is originating outside economics a criterion of heterodoxy in itself? Here Davis seems to be returning to his earlier focus on origins and orientation.

Davis seeks to motivate his latest contentions by suggesting that it is a “mistake [...] to claim that the understanding of science in other fields is essentially the same as it is in economics, since why otherwise are there different sciences?” Actually this

does not follow. The fact of different fields of science does not mean that we must understand science differently in the various fields, only that the different fields treat different sorts of materials (possessing different sorts of properties), and likely do so using different methods (see *Economics and Reality*, chapter 1-4).

But even overlooking this, if approaches originating outside of economics could not be immediately regarded as orthodox because they are different, we are left wondering why, by this reason alone, they should they be regarded as necessarily heterodox. Davis seems to regard this as self-evident. So here we seem to uncover Davis' second implicit new criterion of heterodoxy, namely: openness to, or encompassing of, anything that the orthodoxy does not (yet) do.

If this is indeed Davis' revised criterion of heterodoxy, it, too, seems an unhelpful way to proceed. For, to adopt such a criterion is to trivialise the heterodox traditions, to reduce them to little more than a concern for difference for the sake of difference alone. In contrast, any heterodoxy worthy of the name, like the traditional heterodox traditions in economics, takes a principled and reasoned stance for opposing some specific orthodox doctrine and indeed for putting something else in its place.

Heterodoxy, I repeat, simply means opposed to orthodox doctrine. As far as I know there is no orthodox doctrine to the effect that 'new programmes in economics should all originate within the discipline', or that 'programmes new to economics cannot be (immediately) accepted'. And if we can define heterodoxy without reference to it's being opposed to orthodox doctrine, why not according to any arbitrary criterion?

The traditional heterodox opposition in economics has arisen, and is sustained, in response to the failings and inappropriateness of the formalistic emphasis in economics. That is, there already is a huge self-identifying heterodoxy, opposing the mainstream defining doctrine, including post Keynesians, Old Institutionalism, Austrians, Marxian economists and so forth. Many of these were pushed into being heterodox only in virtue of the rise to dominance of a project that takes as its central doctrine an insistence that mathematical methods be everywhere used. A criterion for heterodoxy of merely being different to the mainstream would significantly blunt the heterodox oppositional message (and of course if the relevant criterion of heterodoxy is just 'originating outside economics', this would seem to exclude these traditional projects from heterodoxy entirely).

Or to view the same tension from a different perspective, suppose furthermore that a group of mainstream economists were to devise, or import, some new methods of a mathematical-deductivist nature, and integrate them into a research programme. Let us suppose too that the existing heterodox projects saw little reason to incorporate such methods, viewing them as not especially appropriate to social analysis. On his

second new criterion, Davis would still have to accept these methods, or the project by mainstream economists incorporating them, as heterodox, at least initially; which does seem rather unhelpful, not to mention counter-intuitive.

Davis' response is to distinguish the "new heterodoxy" and the "old heterodoxy", with the former designating the new programmes and the latter the traditional projects. But I think as it stands this response merely signals the incoherence of Davis' position rather than resolves it (though I think there may be a resolution to this problem, a matter I return to shortly).

I might also note, somewhat parenthetically, that Davis' conception, if accepted, would not only serve to water down the heterodox opposition to the mainstream, it would also serve to distract attention from what is wrong in economics. The insistence that mathematical methods should be everywhere used is not only mainstream doctrine; it is the cause of the discipline's continuing ills (see *Reorienting Economics*, chapter 1). As I say, in other disciplines where there is an exclusive reliance on formalism, there is no heterodox opposition, and there is none just because the formalism is perceived to be doing the job. I repeat that the traditional heterodox opposition in economics is precisely a constructive response to the failings and inappropriateness of the formalistic emphasis in economics.

We can see, looking back over the passage by Davis extracted above (at the start of the current section), that Davis himself seems to recognise some of this:

"Identifying these [new] developments as heterodox is not to say, of course, that they represent those heterodox principles most valued by those individuals who currently self-identify themselves as heterodox. Nor is it to say that these are all necessarily valuable principles for economics".

So the new programmes may not be of value, as currently formulated they appear not to fit with the orientation of the existing heterodoxy, and, in the manner by which they are constituted as heterodox, they are not opposed to any particular orthodox doctrine. I thus remain as cautious about embracing this second new criterion of heterodoxy as I am in adopting Davis' previous one.

### **Developments within the mainstream**

To this point, I have merely indicated problems that arise from Davis' attempts to devise criteria that render the new programmes heterodox. If we add to these difficulties the claims of David Colander, Richard Holt and J. Barkley Rosser Jr. (2004) that new programmes of the sort Davis considers are in any case being taken

up by the mainstream (and not just entering economics as formalistic systems) then the picture Davis gives seems troublesome indeed. For, focusing explicitly on the mainstream, Colander et al report how evolutionary game theory is redefining the manner in which (notions of) institutions are integrated into mainstream analysis; how ecological economics is redefining the manner in which rationality is treated; how econometric work dealing with the limitations of classical statistics is defining the manner in which economists think of empirical proof; how complexity theory is providing a new way to conceptualise equilibrium states; how computer simulations offer a new approach to analysis; how experimental economics is changing the way economists think about empirical work, and so on.

Unsurprisingly, although the authors in question focus on the changing features of the mainstream, they find that the latter's insistence on mathematical-deductive modelling prevails in all cases; the essential feature of the recent and current mainstream remains intact:

“modern mainstream economics is open to new approaches, as long as they are done with a careful understanding of the strengths of the recent orthodox approach and with a modelling methodology acceptable to the mainstream” (Colander et al, 2004, p. 492).

Perceiving an ‘elite’ within the mainstream that determines which new ideas are acceptable Colander et al. also write:

“Our view is that the current elite are relatively open minded when it comes to new ideas, but quite closed minded when it comes to alternative methodologies. If it isn't modelled, it isn't economics, no matter how insightful” (Colander et al., 2004, p. 492).

So where does Davis' desire to render the new programs heterodox leave us? It seems that we acquire two new and not wholly compatible conceptions of heterodoxy. Neither seems to be well motivated or justified or especially successful in facilitating the desired outcome; and both create their own problems of coherence. Moreover, if the first conception (being heterodox in virtue of accepting at least one of Davis' early assessments of heterodox commitments and/or in virtue of being part of a package that covers them all) seems especially arbitrary, the second criterion (of including anything orthodoxy does cover) seems unhelpfully dismissive of the traditional heterodox critical opposition, being not even connected to the explicit opposition to some specific orthodox doctrine.

Central, to such problems as are created is Davis' conviction that it is necessary to interpret the new programmes as necessarily mainstream or heterodox independently of how they are applied; and in particular his desire to see them as necessarily heterodox. If instead we accept that methodological approaches (and with the new programmes are indeed primarily methodological in orientation) are not inherently heterodox or mainstream, then problems of the noted sort just do not arise. The mainstream just is constituted by its insistence that formalism is compulsory; the heterodoxy is a principled resistance to this

doctrine. The principle behind this resistance is that social reality is of such a nature that the presuppositions of formalistic method cannot have universal relevance. Where or when local conditions render formalistic methods relevant is always a matter of trial and error experimentation or empirical investigation; not something for prior dogmatic assertion.

It is conceivable that in seeking to claim the new programmes for heterodoxy Davis is primarily motivated by a concern not to abandon them entirely to the orthodoxy. If this is so, then the conception I am defending not only avoids all the tensions that Davis' alternatives introduce, but manages to meet Davis' motivating concern anyway; for it follows from the framework I am defending that these programmes, as any others, need not be confined to orthodox or heterodox take up. What matters, as I say, is the manner in which they are applied.

In any case, whatever may be Davis' motivation for seeking to revise his criterion of heterodoxy, I see no compelling reason in any of this to abandon or even transform my own conception in favour of Davis' revised formulation. To the contrary, given the noted ease with which the original conception can avoid the sorts of tensions and potential incoherencies introduced by Davis' proposed transformations, my commitment to it is probably strengthened.

### **The future of economics**

Davis opens his third and final section noting there is little communication between the (traditional) heterodox groups and the new programmes (which as I say he refers to as a 'new heterodoxy' to distinguish it from the former traditional or 'old heterodoxy'), whilst acknowledging that there is considerable cross-communication *within* the separate groups (for example between post Keynesians and old Institutionalists, etc.).

If the new programmes, at least in the manner they are mostly taken up, are being rendered largely formalistic in nature, *irrespective* of whether this is the route to insight, then this should not surprise us.

This is not to say that the new methods will not (and I certainly do not suggest that they should not) also be explored by (some) heterodox economists. Nor is it to suggest that there should not be any inter-group communication; exploration and engagement is always desirable, where feasible, whatever the groups involved. But to the extent that the 'new programmes', as introduced, are essentially based on forms of mathematical deductive reasoning, they will carry the atomistic presuppositions that, when advanced as universal claims, the heterodoxy continues to reject.

So, to the extent that proponents of the new programmes seek to apply the latter without regard to conditions of relevance, and this does seem to be happening, engagement may be limited.

Indeed, all things considered, it is likely that the new programmes will enter the discipline mostly through a revision in (or perhaps more likely through additions to) the concrete (formalistic) practices of the current mainstream, not by transforming its (formalistic) nature; a conjecture that, as we have seen, is assessed by Colander et al (2004) as already being carried through. Behavioural economics is an obvious example of this.

From the perspective of Davis' analysis, however, this lack of cross-communication between the traditional heterodoxy and the new programmes is viewed as 'odd' and in need of an explanation. The reason Davis finds this odd is that "both share a number of critiques of mainstream neoclassicism".

Here, as in several other places in Davis' commentary, we come across an expressed association between mainstream economics and neoclassicism. I confess that I have never considered the latter to be a useful or an appropriate label. I assume that by using it here, Davis is referring to one form of formalised system, perhaps one especially associated with the introduction of marginalism in economics. It is possible that Davis understands it as Ben Fine (2006) does, as "as the technical apparatus established [...] from the marginalist revolution onwards [...] [fundamentally involving] the use of utility functions and production functions, with accompanying assumptions to allow the theory to proceed". Or perhaps it is understood as the triptych of rationality, equilibrium, and (something like) perfect knowledge (see for example Hodgson, 2006). Or perhaps instead it is understood as a formalism turning on the triptych of methodological individualism (explanations always couched in terms of individuals), methodological instrumentalism (preference driven behaviour), and methodological equilibration (equilibrium as an organising concept), as argued by Christian Arnsperger and Yanis Varoufakis (2006). For a more complex overview, see Fullbrook (2005).

Whichever the interpretation, we can see that Davis' surprise at the lack of communication between the groups appears to stem from his supposing that my opponent's opponent must be like me, that any two approaches that oppose 'neoclassicism', must be of similar nature. This clearly need not follow.

**Orthodoxy's heterodoxy?**

Still, to the extent that Davis is correct in characterising the ‘new programmes’ as rejecting a set of doctrines that are accepted by a majority of mainstream economists, albeit whilst nevertheless refusing to challenge the mainstreams’ most basic and universal doctrine, there may yet be coherence in thinking of the new programmes (even if only applied in a formalistic manner, and by mainstream practitioners) as a form of heterodoxy, *albeit as a heterodoxy within the mainstream*. After all heterodoxies are often acknowledged within various religions, as for example in the Roman Catholic Church, designating (typically relatively small) groups whose members do not accept all the doctrines but retain sufficient faith in the most fundamental doctrine(s) to avoid heresy (or we might say to avoid a more fundamental heterodoxy). So, to the extent that proponents of the new programmes accept the doctrine that formalism be compulsory in economics, but stand in some way opposed to certain specific formalisms practiced by a majority (or perhaps even a dominating large minority), the pursuit of these programmes might be referred to as a ‘mathematical economics heterodoxy’, or as a heterodoxy within the mathematical deductivist church, or some such.

For this strategy to be sustainable it is beholden on Davis to establish that something called neoclassicism continues to be a majority or dominant project within the mainstream, and that the new programmes both are accepted by (some) mainstream economists and also are ranged against any neoclassical (or other) core. I am not actually convinced this is the case. But, if Davis can establish something like this, then, as I say, such an interpretation of a ‘heterodoxy within’ carries some coherence. And it may capture some of Davis’ thinking. In any case, I shall not refer to any ‘mathematical economics heterodoxy’ in what follows, but leave it to Davis or others to further ground the case for interpreting matters in this way.

The relevant point in all this, though, is that whilst it is quite feasible that certain proponents of the new programmes do indeed advance a critique of one form of mathematical deductive reasoning in order (and by way of seeking) to replace it with a different form of such formalistic reasoning (one that still retains the atomistic ontology of the methods it seeks to challenge), the (traditional) heterodoxy challenges the mainstream as a whole, and does so in order (and by way of seeking) to achieve a space for methods that presuppose a more realistic social ontology.

Davis’ explanation of the lack of cross communication between the two groupings supports this interpretation, although I am not sure he fully appreciates the significance of what he writes. Basically, he points to differences of ‘origins’ and more especially ‘orientations’ of the two groupings. He writes:

“[...] the new research programs in economics – the ‘new heterodoxy’ [...] drawing on principles originally developed *outside* economics [...] display an



inward orientation; that is, they largely aim to change the principles reigning in orthodoxy, and do not aim to substitute new ones altogether”.

In contrast the majority of traditional heterodox programmes, whatever their perceived origin are what Davis describes as outward oriented, meaning not wanting to adjust the mainstream core, but to do things entirely differently:

“[Focussing on the majority of the] traditional heterodox research programs, the few that share periphery origin stories are all oriented toward the periphery of economics [...] Other traditional heterodox approaches that have different origin stories are all also outward-oriented. It is not revision and adjustment of the core that drives them, but its wholesale abandonment”.

And Davis sums up the resulting situation as follows:

“Thus the state of affairs in economics as a whole is that orthodoxy has come into question – here I agree with Lawson – but that the two heterodox groupings both interested in changing the field have almost entirely different views about how this should happen, and accordingly have little to say to each other”.

As I say, the obvious inference to draw here is that the two groups have little to say to each other just because, in practice, they are committed to projects that are not only entirely different but perhaps fundamentally oppositional. The one, the traditional heterodoxy, is concerned to break away from the *insistence* on formalism in order to produce a more realistic or relevant economics; the other, *in the form that it is actually taking*, appears to be more concerned with introducing new formalistic programmes. Given the mathematical emphasis of the mainstream, the former group truly is heterodox in rejecting the formalistic emphasis, the latter group is not; in effect the latter group (in practice if not in principle) is a set of potential mainstream (or even mainstream core) activities in waiting.

### **The way forward**

Instead of treating his explanation (of the limited communication across the two groups) as a further reason to abandon his revised (two pronged) conception of the heterodoxy (or at least to see one as a heterodoxy within the formalist camp), Davis responds by asking a quite different question: “Which is the correct scenario?” By this, he means which is the best way to transform the mainstream. Is it by adopting the orientation of the new programmes or by following the path of the traditional heterodoxy?

Davis presents this option as a choice between “chipping away at the core on a gradualist schedule” or “betting on a big scientific revolution”. I believe this presentation is misleading, on two accounts.

First, new forms of formalism that still carry all the existing problems of the old are not a solution to anything. If “chipping away at the core” is motivated simply by a desire to replace one form of largely irrelevant formalism with another, then it is not obvious that a great deal of progress is involved. Game theory has largely replaced general equilibrium theory in academic faculties like my own, but I do not see how this can be interpreted as a move forward, as a transformation worth having worked for. However, if ‘chipping away’ is motivated by a desire to undermine the formalistic emphasis *per se* then, whilst no one wishes to discourage any move that might improve matters, it is difficult to see *how* introducing new formalisms is expected to help.

Second, a comparison between ‘chipping’ and ‘betting’ makes the former seem active, and the latter passive (or even irresponsibly speculative or indulgent). Heterodox economists and others, who under-labour for a more relevant economics, are just as concerned to work to transform the situation (though perhaps a real difference, over and above strategic ones, lies in the visions of the sort of outcome that is worth working for).

Davis, in any case, persists with his notion of a choice, and presents it as one facing the traditional heterodoxy:

“[...] traditional heterodox economists have two choices. They can maintain their outward-orientation, so that if change occurs in economics it will likely be on the terms determined by behavioural economists, experimentalists, and others in the new approaches. The risk here is that these movements may become more conservative as their success at influencing the core improves. Alternatively they can reverse their orientation, and turn to trying to shift what exists in the core, looking for allies in the ‘new heterodoxy’ along the way, so as to improve the chances of successful change for both”.

This statement begs various questions. What does it mean to say the new programmes are likely to become more ‘conservative’ as they influence the core? Why are the new programmes likely to become more ‘conservative’ as/if they influence the core? Is there any reason to suppose that if the traditional heterodoxy oriented itself more towards the core it could thereby achieve changes worth having?

I presume that by ‘more conservative’, Davis means something like becoming more sympathetic to, and having minimal or reduced impact on, the current

mainstream insistence that formalism be everywhere used<sup>5</sup>. If so, Davis is surely correct that it is only through adopting such an inward orientation that proponents of the new programmes will be accepted by the mainstream. But this is the case whatever the orientation of the traditional heterodoxy. Putting forward a formalistic programme is the only basis on which the current mainstream has been found to accept change. Furthermore, the traditional heterodoxy is, by its nature, opposed to the insistence that formalism be everywhere involved. The only way it could thus orientate itself to the mainstream in the manner Davis suggests is to drop this opposition, and relinquish its generalist heterodox status. But then it is unclear why any changes subsequently wrought by such a transformed project, should any occur, would be any less 'conservative' than any brought about by the new approaches acting alone.

### **Pluralism: some concerns**

Davis finishes his piece by indicating his support for pluralism in economics. Although he does not actually say it, the reader could certainly interpret him as suggesting that my own position, whether wittingly or otherwise, is somehow less pluralistic in orientation than Davis' position, and perhaps even inconsistent with pluralism.

Thus, before mentioning pluralism, but after suggesting that the (traditional) heterodoxy have the choice of adopting either an outward orientation or an inward one, Davis offers the opinion that "Lawson's view of heterodoxy [...] does not allow this choice to emerge".

Davis further adds that I counsel an outward orientation anyway:

"Moreover, by asserting, 'there is a set of characteristics by virtue of which any tradition qualifies as heterodox' (Lawson, 2006a, p. 484), and by associating these shared characteristics with the rejection of the core of economics, [Lawson] counsels an outward orientation".

And in his final paragraph Davis asserts:

"For many [an expression of pluralism] seems to mean an open stance toward the different heterodox research programs associated with ICAPE [the International Confederation for Pluralism in Economics] that seeks to promote a unity within

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<sup>5</sup> Thus I assume Davis does not mean politically conservative. Davis explicit rejects the idea that we should relate distinctions in the sorts of economic programmes pursued (and in particular any differentiations as to whether they are orthodox or heterodox) to political differentiations/allegiances.

difference. This stance seems to me to be shortsighted and anti-pluralist in important respects”.

Davis ends by hoping that “ICAPE will become an increasingly pluralist organisation in strategy as well as membership”. Davis does not actually associate me explicitly with the “many” to whom he refers in the above passage, but I am, as he notes, concerned with promoting “unity within difference”.

Whether or not Davis is indeed suggesting that my position is somehow less pluralist than is reasonable, I will treat this as an opportunity to allay the fears of others who wonder more explicitly whether such is not the case.

An example is provided by Robert Garnett (2006), the current ICAPE Secretary and Conference Organizer. In an important and wide-ranging (and I believe overall a very fair) contribution, Garnett does actually first express the view that “To date, the most rigorous justifications for [...] heterodox pluralism have been offered by ‘open-systems realists’”, a grouping in which he explicitly places myself (see also Garnett, 2005). But later in his piece, he sounds a note of caution. Criticising those heterodox ‘paradigmist economists’ who seek to replace a mainstream paradigm with their own hopefully superior one, Garnett worries that my own approach carries residual traces of such a vision:

“Even the open system pluralisms of [...] Lawson carry residual traces of this paradigmist vision, insisting that heterodox economics define itself as the Other of orthodox economics. This is Cold War paradigmism in a different guise but still the same oppositional project, with the same truncated pluralism: offering intellectual openness and respect to persons and arguments within our own paradigm communities but not to outsiders. To define heterodox economics in this way is to warrant the charge that heterodox economics has no positive identity, that it defines itself only in terms of what it is not, rather than in terms of what it is. (Colander et al., 2004, p.491). This keeps us in the reactive position of ‘permitting the mainstream to set the heterodox agenda for heterodox economics....to define its structure and content’ (King, 2004). It also demonstrates that our professed commitments to pluralism are fundamentally ill-conceived, insincere, or both” (Garnett, 2006, pp. 531-2).

Somewhat more trenchant in his criticism is Jeroen Van Bouwel (2005). After distinguishing five different motivations for declaring oneself a pluralist (the ontological, the cognitive limitations, the historical and geographical, the pragmatic and the strategic motivations) Van Bouwel worries about the motivation for my support for pluralism:

“[...] Lawson’s quest for heterodox economics is not so much focusing on elaborating compatibility and complementarity with *mainstream* (or neo-classical)

economics, but rather creating his own alternative, that would be the new (monist) standard.

If we call Lawson's contribution pluralist, as he does, we can distinguish two different forms or conceptions of pluralism. Firstly, Lawson's work is pluralist in the sense that it provides us with an alternative to the mainstream, and as such we have more than one alternative (hence we have plurality). Secondly, we can understand pluralism as engaging in a conversation, as exchanging ideas, and not merely developing different isolated (and essentially monist) alternatives.

Lawson's account does not defend this second kind of pluralism. He does not develop a form of pluralism that shows how the different schools or alternatives can be used for different occasions. He rejects the *mainstream* completely, without considering possible positive contributions. He does not elaborate a form of pluralism that might show the complementarity of the schools or make us understand the origin of the differences between and the plurality of schools [...]" (Van Bouwel, 2005, article 3).

And in his conclusion Van Bouwel adds:

"I claim that a *really* pluralistic approach should engage in a conversation, in spelling out compatibilities and complementarities between the mainstream and the heterodox approaches (both sides should be engaged). The pluralism of Lawson risks leading us to an isolated diversity, to a lack of exchange of ideas." (Van Bouwel, 2005, article 3).

### **A pluralistic orientation maintained**

I take it that by pluralism is meant something like the affirmation, acceptance and encouragement of diversity. Clearly, such a notion itself has a plurality of meanings or inflections, of which two in particular are worth distinguishing.

One such is the notion of pluralism as description, as a claim about the way (some domain of) reality is.

Accepting this notion, then, for example, *ontological pluralism*, on one conception, designates the claim that multiple non-overlapping worlds exist (see Bruce Erlich, 1986, p.527). A second notion of *ontological pluralism* has it that our *one* reality contains an (at least synchronically) irreducible multiplicity of constituents<sup>6</sup>.

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<sup>6</sup> Typically, it is also held that each constituent or entity can be known only fallibly and partially, in various ways, under various competing descriptions, with all ways of knowing reflecting the situatedness and specific capacities of the 'knower', etc.

To believe that at the base of everything is one substance, say energy, or vibrations, is to be a monist rather than pluralist in some metaphysical sense. Many Eastern religions support a monistic rather than a pluralistic philosophy. It seems to me that either of the two conceptions of ontological pluralism just described may (or may not) be consistent with such a monistic metaphysics.

Clearly, this is not the place to attempt to elaborate an account of all the various kinds of descriptive pluralisms imaginable, and to indicate where I might stand with respect to them. I mention the foregoing merely to indicate the complexities of the topic, and ambiguities of any personal declaration to be a pluralist.

A second inflection on the term pluralism interprets it as a (normative) orientation, one of inclusiveness, of supporting and encouraging the acceptance of all interested parties, whatever their differences, within some process. The latter could be a society, or an academic conversation, a sports club, or whatever.

It is possible that because this second notion expresses an orientation rather than a state of affairs it is best captured by the adjective pluralistic. In any case, the two conceptions appear distinct. For it seems to me that no matter how pluralistic an individual might be in the second sense, they could still be led to the view that some domain of reality is, as a matter of fact, monistic in nature.

In any case, it is this second inflection of pluralism, or 'being pluralistic', that seems most relevant here. For each of the commentaries just discussed seems to be motivated by the worry that, by virtue of my conceiving heterodoxy in oppositional terms, my position is necessarily insufficiently pluralistic in the sense of somehow excluding, or showing insufficient respect or tolerance for, or engagement with (the views of) certain others in the academic conversation.

It is this particular set of worries, then, and specifically a concern that I am advancing a conception in which heterodoxy is somehow discouraged from engaging others, is necessarily oriented to replacing the mainstream with an undesirably monolithic paradigm, and/or is encouraging of isolationism, that I seek to allay in what follows. For I am indeed convinced that a pluralistic orientation in the sense in question is desirable, not least because it seems essential both to human flourishing in general and knowledge advance in particular. But I also believe that such an orientation is quite consistent with the position I am defending. Let me briefly elaborate

#### **A positive orientation for heterodoxy**

Actually, before addressing these specific concerns, there is a further worry, expressed in the middle of the quoted passage by Garnett, which really has little to do with pluralism, or being pluralistic. Here, drawing on the fears of Colander et al. (2004) and King (2004), Garnett suggests that to conceive heterodox economics in oppositional terms is to “warrant the charge” that “heterodox economics has no positive identity”. Rather “it defines itself only in terms of what it is not, rather than in terms of what it is”. As a result, the mainstream sets the agenda for heterodox economics.

If the situation were as Garnett describes, that is if heterodoxy had no positive identity (and this does at one point appear to be Davis’ conception – but it is not mine), this, it seems to me, would be a weakness of heterodoxy, a consequence of a supposed non-pluralistic orientation, rather than a further example of it.

In any case, and considering things first in general terms, it does not follow for any heterodoxy characterised by its rejection of specific orthodox doctrine(s), that it must thereby be a purely reactive programme, lacking identity and defining itself purely in terms of the orthodoxy. It does mean that heterodoxy can be *identified as heterodox* in virtue of the opposition (its nominal essence). But if the opposition is to a specific set of doctrines, rather than opposition for opposition’s sake, there will typically be a determinate cause, or set of causes, of this opposition rooted in the nature of the opposed doctrine(s), revealing something more fundamental about the heterodoxy *qua* heterodoxy (its real essence).

And over and above any rejection of specific orthodox doctrine, including the reasons for this rejection, any heterodoxy or heterodoxies can be as complex and heterogeneous as you like. As projects in their own right, each separate heterodox grouping can have its own identity, set its own agenda, and be continually evolving. Moreover, this can be so even if, throughout this variety and evolution, a rejection of fundamental orthodox doctrine is sustained. Indeed, this is precisely my conception of the situation of modern heterodox economics.

As I say, only if it were the case that any opposition to orthodox doctrine was caused solely by a desire to be oppositional for opposition’s sake irrespective of doctrine would it follow that heterodoxy is purely reactive. I repeat that if this does, at moments, seem to be Davis’ conception of the heterodoxy of modern economics, it is certainly not mine. Rather, on my understanding, contemporary economic heterodoxy possesses deep-seated and valid reasons for its enduring and widespread opposition to specific orthodox doctrine. If the relevant orthodox doctrine were to be abandoned, this would be reason for the traditional heterodoxy to abandon the heterodox ascription, not for its seeking some other doctrine to oppose, nor for its abandoning

the constructive endeavour by virtue of which each division of this heterodoxy constitutes one particular heterodox group rather than another.

I turn, then, to the concerns that more clearly bear upon the issue of whether the conception I defend is consistent with being sufficiently pluralistic.

## Engagement

First, just because heterodox traditions are constituted as heterodox traditions through their rejection of some orthodox doctrine, it does not follow that engagement with orthodox practitioners is thereby rendered necessarily infeasible or undesirable. Nor need communication be other than open and respectful. The possibilities for exchange will depend on context and the nature of the differences. But this will be so however heterodoxy is constituted. I myself have never wished to discourage respectful engagement with others.

Being more concrete, it is clear that a rejection of the defining doctrine of contemporary orthodoxy does not involve a rejection of all endeavours to explore the usefulness of formalistic methods. Heterodoxy *qua* heterodoxy, as I conceive it, involves a necessary opposition *not* to the use of formalism but only to *the dogmatic insistence that only these sorts of methods be used, irrespective of their ability to illuminate*. I do not see how a pluralist can accept this insistence, this orthodox doctrine, in the circumstances. Indeed in rejecting this one enduring orthodox doctrine, heterodoxy, *qua* heterodoxy is inherently pluralistic in its very constitutive orientation (whether or not specific heterodox contributions remains pluralistic in all other respects). If, however, individuals within or outside the traditional heterodox groups wish to explore new formalisms, or methods of any kind, who is going to object?

To date, formalistic methods that presuppose an atomistic ontology have met with very little success, and from the perspective of the ontological framework I defend, this is none too surprising. But even if the ontology I defend is roughly right, there may yet be pockets of social reality that provide the appropriate conditions for successes with formalistic modelling, as I regularly acknowledge. In addition, of course, I recognise that the ontological conception I defend may yet turn out to be significantly mistaken in various ways; all knowledge claims are fallible. So no one wants to inhibit any serious methodological experimentation, whether involving formal techniques or otherwise. All that is being rejected by heterodoxy is the orthodox constraint on a pluralistic approach to economics analysis. This takes on a special significance just because the mainstream is constituted through this constraint. But if that is the nature of the beast, we just have to accept that opposing the



mainstream (rejecting its constitutive doctrine) is a pro-, not an anti-, pluralistic stance.

It warrants emphasis, though, that any desire to engage does not mean heterodox economists must resort to constructing formalistic models (although of course there is no reason not to try that route if there is thought to be some promise of success). In particular, meta-theoretical discussion is at least as valid, where feasible. This can take the form of engagement via publications. Other forms depend on context.

I well understand the problems. As Richard Lipsey reminds us, if anyone presents an economics seminar without formulating a mathematical model it is not unknown for the mainstream economists “to turn off and figuratively, if not literally, to walk out” (Lipsey, 2001, p. 184).

But not all mainstream contributors are like this, especially the more thoughtful ones, despite appearances. While I was originally formulating my critique of the mainstream, Frank Hahn was head of the Cambridge economics faculty in which I am located. Hahn’s commitment to the mainstream is clear enough from his retirement speech to the *Royal Economic Society*, where he famously gave advice to students to “avoid discussions of ‘mathematics in economics’ like the plague” (Hahn 1992a, see also Hahn, 1992b), adding that we should “give no thought to methodology”. Elsewhere, as I have often observed, Hahn writes of any suggestion that the emphasis on mathematics may be a problem that this is “a view surely not worth discussing” (Hahn, 1985, p. 18). But appearances or rhetoric can mislead. This set of beliefs did not prevent Hahn himself, on various occasions, accepting invitations to talk at the *Cambridge Realist Workshop*<sup>7</sup> that I co-ordinate. In that forum, a genuine exchange of ideas took place on the sorts of issues here in contention, with large audiences of mostly PhD students listening (and indeed joining) in. I mention this just to reinforce the idea that possibilities for engagement depend very much on people and context. In particular, there should be no presumption that we should always hide our real critique, or perpetuate approaches we actually think are very unlikely to reveal insight, just in order to be able to engage.

### **Paradigmism**

Second, it also does not follow that, just because heterodoxy is characterised by its rejection of some orthodox doctrine, heterodox conceptions need be monolithic, monist, paradigmist or whatever. In principle, such heterodox projects can be as small, partial, open, multifaceted, fragmented, transitory, and inclusive as you like.

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<sup>7</sup> For a listing of programme for the last ten years or so, including several presentations by Hahn, go to: [http://www.econ.cam.ac.uk/seminars/realist/previous\\_workshops.htm](http://www.econ.cam.ac.uk/seminars/realist/previous_workshops.htm).

Having said that I see nothing inherently anti-pluralistic about specific individuals exploring the possibility of creating a successful substantive paradigm of any sort.

Perhaps, though, it will be said that I am being less than pluralistic in supporting one specific social ontological conception above others. I hope it is clear that the conception I defend is consistent with many modes of explanation and forms of substantive theorising (see, for example, my response to Bjørn-Ivar Davidsen in this volume). Indeed, I would describe my position as one that is, if ontologically bold, then epistemologically and substantively very cautious. But still some might worry that my defence of a specific ontology, and my resting my arguments for inter-, or across-, group collaboration upon it, constitutes an undesirably anti-pluralistic stance in itself.

I do not think it does though. No one is saying that alternative ontological conceptions are not possible. Clearly they are. And to the extent that competing conceptions are produced, the point, once more, is to do whatever it takes to encourage all parties to constructively engage. But if one ontological conception can be shown to be better grounded than available alternatives, is that not a reason for drawing on it? Would anyone counsel a different approach in any other walk of life? Yes, let us leave options open. Let us also (repeatedly) try out alternatives, where appropriate. Certainly, let us include everyone in the conversation, whether it is oriented to the nature of ontology, substantive work, the nature of pluralism or being pluralistic, or whatever, and seek to do so with respect for, and encouragement of, each other. But *if*, when the time comes to act, to make use of an ontological conception, and one such conception (whatever the focus) seems to be significantly more appropriate than others, not least because it is found to be far more explanatorily grounded, then it seems reasonable (for at least those that believe in it) to make use of the latter. This applies to our theories of the nature(s) of pluralism(s), of how we ought to be pluralistic, as well as to everything else.

### **Differences in economic traditions as divisions of labour**

Third, there is nothing inherently isolationist about the heterodox stance I set out. Van Bouwel complains that I do not “develop a form of pluralism that shows how the different schools or alternatives can be used for different occasions”, that I do “not elaborate a form of pluralism that might show the complementarity of the schools or make us understand the origin of the differences between and the plurality of schools”. I have actually had much to say on this not only in *Reorienting Economics* (Lawson, 2003a), but also in Lawson (2004, 2006a). Let me briefly outline my position.

The basic thesis I advance concerning the (traditional) heterodox projects is that they are best conceived as divisions of labour in one overall project. Remember I do not think the heterodox projects can be distinguished by the answers given (within any given tradition these are far too variable, both at any point in time and over time). Rather I argue that the individual heterodox traditions, like, I think, research endeavour in almost all other disciplines, are identifiable more by the sorts of questions asked (see Lawson, 2003a; Lawson 2006a). It is with this understanding of heterodoxy in mind that we can view the separate traditions as divisions of labour.

Central to this interpretation is the ontological conception that I defend, a conception that I also believe these heterodox traditions mostly implicitly presuppose. This conception has many facets. Social phenomena are, for example, viewed as bearing emergent powers, being structured, open, processual, highly internally related, comprising value, carrying meaning, and so forth. The various heterodox traditions I believe are best viewed as exploring, if implicitly, specific aspects of this ontology (whilst maintaining a commitment to the whole).

Post Keynesians, for example, make fundamental uncertainty a central category. This clearly presupposes an ontology of openness as many post Keynesians have in recent years come increasingly to acknowledge. Such a focus has involved examining the implications of uncertainty or openness for the development of certain sorts of institutions, including money, for processes of decision-making, and so forth. At the level of policy, the concern may well include the analysis of contingencies that recognise the fact of pervasive uncertainty, given the openness of social reality in the present and to the future, etc. For those influenced by Keynes, especially, a likely focus is how these matters give rise to collective or macro outcomes, and how they in turn impact back on individual acts and pressures for structural transformation, etc. (see Lawson, 1994; 2003a, chapter 7).

By similar reasoning, and as already noted earlier in this essay, I believe that it is best to distinguish (old) Institutionalism, following Veblen especially, as concerned with the processual nature of social reality, and so as focussing especially on those forces working for stability and on others working for change. This orientation has taken the manifest form of a traditional concern with evolutionary issues, and with studying those aspects of social life that are most enduring, such as institutions and habits, along with those that most inducing of continuous change, such as technology (see Lawson, 2002, 2003a, chapter 8; 2003b, 2006b).

Feminist economics, I believe, is best distinguished in terms of a focus on social relationality. Relations of care are of course a central issue. But relationality in itself seems central to most feminist concerns. Very often feminist economists have identified their own project as one that first of all concerns itself with women as

subjects (which may include, for example, giving attention to differences among women, as well as between genders) and takes a particular orientation or focus, namely on the position of women (and other marginalised groups) within society and the economy. In practice this project includes an attention to the social causes at work in the oppression of, or in discrimination against, women (and others), the opportunities for progressive transformation or emancipation, questions of (relations of) power and strategy, and so forth.

Austrians may perhaps be best identified in some part according to their emphasis on the role of inter-subjective meaning in social life (see Lawson, 1997, chapter 10), and so on.

I suggest, then, that at least some heterodox traditions are most easily viewed as primarily (though not exclusively) concerned with different aspects of the properties of social phenomena (openness, processuality, internal relationality) uncovered through philosophical ontology.

Others traditions, though, seem to be more interested in elaborating the nature of specific social categories, and in particular how the features uncovered through philosophical ontology (openness, relationality, process, etc) coalesce in certain social items of interest within that particular tradition. An obvious example is Marxian economics, a project primarily concerned to understand the nature of the relational totality in motion that is capitalism. But we also find a significant Austrian interest in the nature of 'the market process' and entrepreneurship in particular. And as already noted there is significant post Keynesian interest in the nature of money, institutionalist interest in institutions and technology, feminist interest in care, and so forth.

How does the current mainstream join the party? Clearly its *insistence* that mathematical deductivist methods be more or less always and everywhere used and by all of us, is ill fitted to this pluralistic picture. Of course, the argument can be heard, but there can be no compulsion for anyone to follow. But those who experiment with formalistic methods, without insisting that others always and everywhere do so, certainly have a place. Formalistic endeavour will likely be most fruitful where social conditions most approximate the atomistic ontology that such endeavour presupposes. In *Reorienting Economics* (chapter 1), I sketch the sorts of scenarios under which the emergence of such conditions appears most feasible and wherein, indeed, some successes seem occasionally to have been achieved.

I hope it is clear, then, that there is a place for more or less all types of research practice on the conception I defend; I am not at all advancing a vision of (or seeking

to encourage) isolated practices<sup>8</sup>. To the contrary, according to the conception I am advancing it is actually vital that the various divisions perpetually keep in touch with each other's contributions and developments. For all are working on aspects of the same whole, and each tradition requires some understanding of the whole (and so of each other's contributions) in order to carry out its own division of labour competently (an issue I consider further in my response to David Ruccio in the current volume).

In short, the very fact that (or my defending the contention that) heterodoxy is identified through its opposition to a specific orthodox doctrine does not in or of itself undermine the possibility of maintaining pluralistic orientations of the sort that most seem to concern Davis, Garnett, Van Bouwel and others<sup>9</sup>. In fact, the realising of such pluralistic orientations, I hope it is clear, is also something that I, like very many others, continue to support.

## **Conclusion**

Davis' paper along with others he has recently produced provide a valuable assessment of ongoing developments in the modern economics; in these papers we find many insights into the dynamics, including various transient aspects, of much that is going on. In this work Davis is raising fundamental questions and providing stimulating answers.

Clearly, though, I am yet to be convinced of all of Davis' conclusions. In his current piece, Davis advances a picture of heterodoxy in the context of modern

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<sup>8</sup> This indeed is something I have endeavoured to emphasise over and over again. Thus, for example, in Lawson 2006a I wrote:

"I should finally perhaps emphasise (though it is hopefully apparent throughout) that, although I am arguing that each heterodox tradition be distinguished according to a traditional set of concerns and emphases (rather than answers or methodological principles), I do not want to suggest that each somehow works with isolated components of society or economy. The object or subject matter of social theory/science, no less than economics, is an interrelated whole (in process). To focus competently on specific aspects requires an understanding of the totality (just as the investigation of any specific aspect of the human body presupposes some prior understanding of its functioning within the whole). There is no part of the social realm that does not have an economic aspect (although social reality does not reduce to its economic aspects). And, similarly, there is no part of social life that cannot be viewed under the aspect of its degree of openness, or its processuality/fixity, or the nature of its social relationality, etc. In other words, on the conception laid out each of the various heterodox traditions is viewed as approaching the same totality but with a distinguishing set of concerns, emphases, motivating interests and (so) questions. And, ideally, each will be achieving results that warrant synthesising with the findings of others (again see Lawson, 2003a, especially Part III)".

<sup>9</sup> This is not, of course, to imply that things couldn't be improved (for an argument that heterodoxy could be more pluralistic see Randall Holcombe, 2006).

economics (that challenges my own interpretation). In truth, though, his revised notion of heterodoxy is either arbitrary, or not consistently oriented to any accepted doctrine of the existing orthodoxy. Moreover, the result of following Davis' lead, however interpreted, is seemingly two different forms of heterodoxy (the old and the new) that appear to have little in common with each other, with each grouping, as Davis notes, being rather cautious about the other's contributions. Furthermore, Davis does not really give us any good reason for taking his path.

Having said that, there is a point at which Davis suggests that the 'new programmes', like the traditional heterodoxy, oppose (what Davis labels) neoclassical economics. Davis does not seem to characterise the new programmes by this opposition. But it is an opposition, nevertheless, one focussed on a doctrine accepted by at least a significant portion of the modern mainstream (albeit not the mainstreams' most basic and universal tenet: that mathematical-deductivist methods should be employed). As such, if the designation heterodoxy is really so important for the new programmes, it may be the case that, given the mathematical form they are taking in practice, there is coherence to thinking of them as coming to constitute a heterodoxy within the mainstream, perhaps being designated a 'mathematical economics heterodoxy' for clarity.

In the end, though, the primary concern here, Davis would probably agree, is not with characterising either the mainstream or heterodoxy *per se*. I think we both start with an assessment that all is not well with modern economics, and are both first and foremost concerned that something is done to repair the situation.

From there on in, though, our paths seem to diverge. We may even differ in our assessments of the nature of the problem. For my part, as I have often repeated, I am opposed to the mainstream insistence that only mathematical deductivist methods be used. Not only is this an undesirably anti-pluralist stance but also, in my assessment, it accounts for the continuing poor intellectual state of modern economics (see especially Lawson, 2003, chapter 1). Perhaps, though, this orthodox insistence bothers Davis less. It may be that Davis' primary worry is more with the sort of formalism that prevails, and that Davis is opposed in particular to the sort of formalism to which he refers as neoclassicism. Certainly, this would explain our different emphases and strategies.

But whatever Davis' assessment of the primary nature of the problem, if the accepted goal is ultimately to introduce new forms of formalism into the mainstream, then Davis' preferred strategy of 'looking inwards' is surely the superior. If, though, the goal is a more genuinely pluralistic economics, one that does not support an unreasoned insistence on mathematics only and everywhere, and this of course is my own preference, then Davis' strategy, I believe, is almost as surely the inferior.

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