

## **The Nature of Heterodox Economics**

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Recent years have seen the emergence of numerous activities in economics identified first and foremost as heterodox. For example 1999 witnessed the formation of the *Association for Heterodox Economics* (AHE), an organisation that now sponsors an annual conference, postgraduate training workshops and more<sup>1</sup>. 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 *Review of Radical Political Economics* has recently announced that a forthcoming edition will comprise a special issue on the History of Radical 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” and providing significant relevant resources<sup>2</sup>, including a heterodox newsletter<sup>3</sup>.

So it seems that something called heterodox economics is alive and flourishing. My question here is what (sort of thing) is it? In asking this question I do not 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. But I do take the view that things in process can still be known, if only as historical (and geographical and cultural) products. And I believe, and hope to show, that there can be gains to critical self-reflection upon the nature of that with which we are dealing or involved at any point in time.

Amongst the very few that have questioned the nature of heterodox economics it is recognised that heterodoxy as an umbrella term to cover the coming together of, sometimes long-standing, *separate* heterodox projects or traditions. The latter include post Keynesianism, (old) institutionalism, feminist, social, Marxian and Austrian economics, among others.

With this in mind my initial question can be reformulated as an enquiry into what, if anything, these traditions hold in common. For if there is a (set of) trait (s) in virtue of which any tradition qualifies as heterodox it is presumably included amongst the features, if any, that the traditions share. I think too that we can regard any non-shared features as contingent with respect to the constitution of heterodoxy, and likely serving to distinguish the heterodox traditions from each other.

It is on this presumption that I shall proceed. The interpretation I defend is indeed one of unity in difference. Of course, to conceptualise heterodox economics is also to distinguish the mainstream against which it stands opposed. And to thus determine both is to distinguish economics from other disciplines, and so on. A process is thus set in train that stretches far beyond my original question. But I here explicitly step

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<sup>1</sup> For an account of how the Association for Heterodox Economics was formed see Lee, 200x

<sup>2</sup> See for example <http://www.orgs.bucknell.edu/afee/hetecon.htm> which currently lists various heterodox economics associations, heterodox economics journals, heterodox publications (news, commentary & analysis), and heterodox discussion groups

<sup>3</sup> See <http://l.web.umkc.edu/leefs/htn.htm>

beyond a discussion of the heterodox economics only to the extent that it is necessary to do so to get a reasonable initial assessment of its specific determinations.

### **The separate heterodox traditions**

When we turn to the separate heterodox traditions we find that the task of identifying the nature of any one of them is not straightforward. In fact there is a good deal of debate within most, if not all, of the various traditions as to whether they constitute constructive programmes at all (see Peukart, 2001) or even coherent individual projects (see e.g. Hamouda and Harcourt, 1988). However, there do appear to be some prominent common features of all these separate traditions, even if some work is required in interpreting the implications. These prominent commonalities are, or include, the following:

1. a set of recurring fairly abstract tradition-specific themes and emphases;
2. a multiplicity of attempts within each tradition to theorize around its tradition-specific themes and to form policy stances, or else to determine tradition-specific main units of analysis or other methodological principles based on them. The results are often presented as the theory/policy stances, basic units of analysis, or methodological principles that constitute the relevant tradition's alternatives to those of the mainstream;
3. an *a posteriori* recognition that it is usually impossible to generate very large agreement within any given heterodox tradition on specific "alternative" theories and policies or specific methodological stances, a recognition typically resulting in an (often begrudging) inference that, even within any one tradition, the only definite common ground in terms of achieved position, is an opposition to the mainstream or 'neoclassical' orthodoxy.

Consider, as an illustration, the case of post Keynesianism. Few would doubt that various themes or emphases are prominent. I refer, for example, to the concern with fundamental uncertainty in the analysis of decision making, the rejection of the idea that macro outcomes can be provided with micro-foundations, a significant emphasis on methodological analysis, a recognition of the importance of time, institutions and history, a frequent drawing on the writing of Keynes, and so forth (see for example Arestis, 1990; Davidson, 1980; Dow, 1992; Sawyer, 1988).

However, attempts to produce substantive theories, policies or methodological stances have usually led to such a degree of variation or competition that post Keynesians, and their observers, have tended to conclude that indeed the only definite point of agreement among post Keynesians is that they stand opposed to the mainstream or 'neoclassical' contributions. Consider the following assessments:

"Post-Keynesian economics can be seen as covering a considerable assortment of approaches. It has sometimes been said that the unifying feature of post-Keynesians is the dislike of neoclassical economics" (Sawyer, 1988, p. 1)

"...post-Keynesian economics is often portrayed as being distinguished more by its dislike of neoclassical theory, than by any coherence or agreement on fundamentals by its contributors" (Hodgson, 1989, p. 96)

"It is less controversial to say what post-Keynesian theory is not than to say what it is. Post-Keynesian theory is not neoclassical theory" (Eichner, 1985, p. 51)

"...post-Keynesians tend to define their program in a negative way as a reaction to neo-classical economics" (Arestis, 1990, p. 222)

"Some have argued that what unites post-Keynesians is a negative factor: the rejection of neoclassical economics" (Dow, 1992, p. 176)

"What seems to be striking to outsiders of post-Keynesianism and neo-Ricardianism is that these two schools of thought and their major proponents only seem to have one cementing theme, their rejection of the dominant neoclassical paradigm" (Lavoie, 1992, p. 45).

I think heterodox economists will recognise that the sorts of commonalities listed above, and illustrated for the case of Post Keynesianism, hold to a degree for all the heterodox traditions<sup>4</sup>. David Colander, Richard Holt and J. Barkley Rosser Jr are not alone in concluding that "In economics, at least, beyond this rejection of the orthodoxy there is no single unifying element that we can discern that characterises heterodox economics" (Colander, et al , 2004, p. 492)

In short, we appear to reach an apparently widely shared assessment of heterodox economics only in terms of what it is not, or rather in terms of that to which it stands opposed; the one widely recognised and accepted feature of all the heterodox traditions is a rejection of the modern mainstream project.

Of course, such an oppositional stance should not surprise us. For this is precisely what use of the term heterodox entails. According to the *Shorter Oxford English Dictionary*, for example, the qualification heterodox just means "Not in accordance with established doctrines or opinions, or those generally recognised as orthodox".

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<sup>4</sup> Briefly consider, for example, the project of old institutionalism, or anyway the manner in which it is commonly perceived. To speed up matters let me rely on the commentary of the institutionalist historian Malcolm Rutherford. In his view:

"All attempts to define American institutionalism, whether in terms of a set of key methodological or theoretical principles or in terms of the contributions of [major contributors]... have run into problems with apparent disparities within the movement" Rutherford, 2000, p. 277).

Rutherford notes the "dramatic differences" in the methodological principles, theoretical positions and definitions of major contributors, and recognises the impression this has given:

"Institutionalism easily appears as incoherent, as little more than a set of individual research programs with nothing in common other than a questioning of orthodox theory and method. Thus Mark Blaug has stated that institutionalism 'was never more than a tenuous inclination to dissent from orthodox economics' (Blaug, 1978, p. 712), and George Stigler has claimed that institutionalism had 'no positive agenda of research,' 'no set of problems or new methods,' nothing, but 'a stance of hostility to the standard theoretical tradition' [...] This view still finds wide currency – for example Oliver Williamson has recently argued that [in the light of its failures elsewhere] 'the older institutional economics was given over to methodological objections of the orthodoxy' (Williamson 1998, p. 24; see also 1998a) " (Rutherford, 2000, pp. 277-8)

However, this recognition need not imply the heterodoxy is purely reactive. Nor does it follow that there is little more to be said. Indeed an explicit rejection of orthodoxy in any sphere is presumably undertaken for certain reasons. And a sustained opposition, such as we find in modern economics, presumably follows for deep ones. Further, in addition to explicitly formalised reasons for an opposition to any orthodoxy there are often other less-than-clearly-unrecognised presuppositions. I think this is so with heterodox economics, as we shall see.

It is clear, though, that if we are to progress in our quest to understand the nature of heterodox economics we need first to determine something of the nature of that to which the heterodox traditions stand opposed. Only with this achieved are we likely to be successful in identifying the basis of the heterodox opposition. That is, before we can elaborate the presuppositions of heterodox economics, we need some insight into the nature of the project to which heterodoxy stands so seemingly implacably opposed.

### **What is modern mainstream or orthodox economics?**

Perhaps at this point the argument begins to get somewhat (more) contentious. For although most observers of modern economics do recognise that the discipline is dominated by a mainstream tradition, and is so to a degree that is rather striking, there is remarkably little sustained discussion or analysis of (as opposed to a few quick assertions about) the nature of that mainstream project (even though practising economists usually suggest that they know it when they see it). Amongst the conceptions of the mainstream that are to be found the following two are perhaps the more prominent, though each, I believe, is ultimately unsustainable.

The first such conception of mainstream economics is as a project concerned primarily with defending the workings of the current economic system, a conception often systematised under the heading of ‘mainstream economics as ideology’. A recent example is provided by Bernard Guerrien (2004). Although the term ideology is rarely defined it carries the connotation of a theory adhered to irrespective of its method or level of justification (or lack of justification). It is maintained, rather, because of some purpose it serves. Guerrien (2004) writes in this context of a mainstream ungrounded insistence that “‘market mechanisms’ produce ‘efficient’ results if you abstract from ‘frictions’, ‘failures’ etc..”(Guerrien, 2004, p. 15)

Rajani Kanth (1999) provides an excellent contribution that seems to interpret mainstream economics in a similar fashion. According to Kanth, mainstream economics (which he sometimes refers to as neoclassical economics) is deliberately ‘rigged’ so as to generate results that support the *status quo*:

“To state the moral: *the entire enterprise of neo-classical economics is rigged to show that laissez-faire produces optimal outcomes*, but for the disruptive operation of the odd externality (a belated correction) here and there” (Kanth, 1999, pp. 191-2, emphasis in the original).

How is this rigging said to be achieved? One component of the most common strategy is everywhere to stipulate that human beings are rational (meaning optimising)

atomistic individuals. A second is the construction of theoretical set-ups or models specified to ensure that (typically unique) optimal outcomes are attainable.

This is not yet enough to ‘show’ that the overall economic system is itself optimal in any way. If the claim is that mainstream economists seek to defend the economic system *per se*, something more is required to guarantee this result. This, it is usually supposed, is achieved by the commonplace construction of an equilibrium framework, the latter being so specified that the actions of isolated optimising individuals somehow (tend to) work to bring an equilibrium position about. Thus Kanth, for example, refers to the “economic science of capitalism” as

“{...} simply *irrelevant* for being a fantasy world of an ideal rational, capitalism where all motions are mutually equilibrating, in a Newtonian co-ordination of the elements” (Kanth, 1999, p. 194)

There is little doubt that some mainstream economists approach their subject in the manner that Guerrian and others suggest. But most do not. And I do worry that portraying mainstream economics as driven by the goal of achieving results in these terms is overly conspiratorial.

Nor even is the presumption that mainstream results are consistent with an efficient or optimal social order correct as a generalisation.. Even those who have spent their careers studying models of equilibrium typically do not draw the sorts of inferences that can be used to justify the economic system.

Consider the conclusions of Frank Hahn a major contributor to general equilibrium theory who has also been concerned to comment continually on the nature of the enterprise. In both his Jevons memorial lecture entitled *In Praise of Economic Theory* and the introduction to his collection of essays entitled *Macroeconomics and Equilibrium*, Hahn explicitly acknowledges that he everywhere adopts (1) an individualistic perspective, a requirement that explanations be couched solely in terms of individuals; and (2) some rationality axiom. But in referencing questions of economic order or equilibrium Hahn further accepts *at most* (3) a commitment merely to the *study* of equilibrium states. Poignantly, Hahn believes equilibrium outcomes or states are rarely if ever manifest:

“[...] it cannot be denied that there is something scandalous in the spectacle of so many people refining the analyses of economic [equilibrium] states which they give no reason to suppose will ever, or have ever, come about. It probably is also dangerous. Equilibrium economics ..is easily convertible into an apologia for existing economic arrangements and it is frequently so converted” (1970, pp. 88-9).

Further, there are groups of economists, seemingly acceptable to the mainstream, who, though adopting the individualist-rationalistic framework, seem determined from the outset to demonstrate the *weaknesses* of the current economic system. Those economists who are often described as ‘rational-choice Marxists’ seem to be so inclined.

Equally to the point if not more so, most economists who accept the individualist and rationalistic framework do not actually concern themselves with questions of equilibrium at all, or, more generally, do not focus on the workings of the economic system as a whole. Most such economists, rather, concern themselves with highly specific or partial analyses of some highly restricted sectors or forms of behaviour. Moreover, to the extent that it is meaningful for the various results or theorems of these economists to be considered as a whole, or in total, the only clear conclusion to be drawn from them is that they are mostly wildly inconsistent with each other.

Notoriously, even econometricians using identical, or almost identical, data sets are found to produce quite contrasting conclusions. The systematic result here, as the respected econometrician Edward Leamer (1983) observes, is that: "hardly anyone takes anyone else's data analysis seriously" (p. 37).

If we turn away from econometrics to the mostly non-empirical 'economic theory' project, and look beyond its general equilibrium programme (which in any case has been in decline for some time now), there seems not even to be any agreement as to the project's purpose or direction. As one of its leading practitioners Ariel Rubinstein admits:

"The issue of interpreting economic theory is... the most serious problem now facing economic theorists. [...] Economic theory lacks a consensus as to its purpose and interpretation. Again and again, we find ourselves asking the question 'where does it lead?' (Rubinstein, 1995, p. 12).

### **Mainstream economics as the study of optimising individual behaviour**

So what are we to make of this situation? How are we to understand the project of mainstream economics in a manner that make sense of this more complex situation? An obvious alternative hypothesis to examine in the light of the discussion so far, perhaps, is that if there is anything essential to the mainstream tradition of modern economics it is merely a commitment to individualism, coupled with the axiom that individuals are everywhere rational (optimising) in their behaviour. Perhaps the mainstream just is so committed, but without any overall common purpose in terms of the sorts of substantive results that 'should' be generated?

This is the second reasonably widespread interpretation of the mainstream endeavour, and the chief alternative to the view that the mainstream project is one concerned to defend the workings of the economic system. It constitutes an assessment, in particular, that is likely the more dominant amongst modern historians of economic thought.

Perhaps this characterisation of the mainstream programme is closer to the mark. But in the end it is not sustainable. There are numerous game theory contributions where rationality is no longer invoked, and seemingly not even meaningful. Mainstream economists are sometimes even prepared to assume that people everywhere follow fixed highly simple rules whatever the context (see references in Lawson 1997, chapter 8). Moreover, some mainstream economists are prepared to abandon the individualist framework entirely if this will help make the 'economic

theory' framework more productive in some way. As the 'economic theorist' Alan Kirman writes:

“The problem [of mainstream theorising to date] seems to be embodied in what is an essential feature of a centuries-long tradition in economics, that of treating individuals as acting independently of each other” (Kirman, 1989, p. 137)

Kirman adds:

“If we are to progress further we may well be forced to theorise in terms of groups who have collectively coherent behaviour” (Kirman, 1989, p. 138)

So it is not obvious that even assumptions of individualism and rationality are ultimately essential to the mainstream position. Indeed many (e.g., Davis, 2005) find with Colander et al (2004) that “economics is moving away from strict adherence to the holy trinity – rationality, selfishness, and equilibrium – to a more eclectic position of purposeful behaviour, enlightened self-interest and sustainability” (Colander et al., 2004, p. 485)

Do we, then, give up on our search for the essence of the current mainstream project? Some commentators believe so, with many concluding that the current mainstream is just too slippery a project to pin down. But I do not think that this is the case. Further, I think that an essential core of the mainstream project of the last fifty years or more is identifiable and remaining in place even through the sort of (ongoing) changes recorded by Colander et al, Davis and others, a matter to which I return below. What then do I take the mainstream to be?

### **The Mathematising inclination**

As I say, I believe there is a feature of modern mainstream economics that is essential to it. And it is an aspect so taken for granted in that it goes largely unquestioned. This is just the formalistic-deductive framework that mainstream economists everywhere adopt, and indeed insist upon.

I am not suggesting that the mathematical framework goes unrecognised. This could hardly be the case. But the mathematical framework is usually only briefly noted at best; it is considered so essential that worries about its usefulness, or dispensability, if they are raised at all, tend to be summarily dismissed rather than seriously addressed. It is because mathematisation is understood as being so obviously desirable, indeed, that the project is rarely defined in such terms. Serious work, it seems to be supposed, could never be otherwise.

Consider just the (mainstream) economists already mentioned. Rubinstein notes in passing that (mainstream) economic theory “utilises mathematical tools” without questioning the legitimacy of this. Kirman (1989) though acknowledging that “the mathematical frameworks that we have used made the task of changing or at least modifying our paradigm hard, is undeniable” insists that “it is difficult to believe that had a clear well-formulated new approach been suggested then we would not have adopted the appropriate mathematical tools” (Kirman, 1989, p. 137). Leamer on noting a continuing “wide gap between econometric theory and econometric practice”

laments not being able to perceive “developments on the horizon that will make any mathematical theory of inference fully applicable” (Leamer, 1978, p. vi -- the idea that there may be relevant non-mathematical theories of inference is seemingly never contemplated). And Frank Hahn probably most epitomises widespread sentiment when he declares of any suggestion that the typical emphasis on mathematics may be misplaced that it is "a view surely not worth discussing" (Hahn, 1985, p. 18). In fact, Hahn later counsels that we "avoid discussions of ‘mathematics in economics’ like the plague" (Hahn, 1992a; see also Hahn 1992b)<sup>5</sup>.

The truth is modern mainstream economics just is the reliance on certain forms of mathematical (deductivist) method. This is an enduring feature of that project, and seemingly the only one; for the mainstream tradition it is its unquestioned, and seemingly unquestionable, essential core.

Consider some more observations. The worry of non-economist observers is often that descriptions or overviews by critics of modern mainstream economics are likely to be uncharitable caricatures. So I focus on more impressions of mainstream economists themselves. Richard Lipsey, an author of a best selling mainstream economic texts book, acknowledges:

“[...]to get an article published in most of today's top rank economic journals, you must provide a mathematical model, even if it adds nothing to your verbal analysis. I have been at seminars where the presenter was asked after a few minutes, ‘Where is your model?’. When he answered ‘I have not got one as I do not need one, or cannot yet develop one, to consider my problem’ the response was to turn off and figuratively, if not literally, to walk out” (Lipsey, 2001, p. 184).

Just as tellingly, when William Thomson was recently invited by a leading mainstream journal to provide a piece entitled “The Young Person’s Guide to Writing Economic Theory” the taken-for-granted meaning of “writing economic theory” is clear in the opening three sentences:

“Here are my recommendations for writing economic theory (and, to some extent, giving seminar presentations). My intended audience is young economists working on their dissertations or preparing first papers for submission to a professional journal. Although I discuss general issues of presentation, this essay is mainly concerned in its details with formal models.” (Thomson, 1999, p. 157)

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<sup>5</sup> Even Kanth notes the emphasis on mathematics but without quite appreciating its essentiality:

“The apparent rigour of mathematics was recruited avidly by neoclassicism to justify and defend its truistic, axiomatic, and almost infantile, theorems that deeply investigated but the surface gloss of economic life. Indeed, for the longest time, Marxists (in the U.S.) had to live in the academic dog-house for not being familiar with matrix algebra, until keen (if not always scrupulous) Marxist minds, with academic tenures at stake, realised the enormous (and inexpensive) potential of this tool for restating Marxian ideas in formalised language and instantly acquiring the gloss of high science, the latter-day pundits of repute here being Roemer in the U.S. and Morishima in England, who were of course soon emulated by a host of lesser lights to whom this switch in language alone promised hours of (well funded) computerised fun and games.

Of course, all the formalisms did not advance a critical understanding of the organon of Marxian system, and its many difficulties, one iota; but it did succeed in generating grudging respect for the Marxist by the even more facile and shallow savants of neo-classicism” (Kanth, 1999, p. 189)

Or consider assessments of some Nobel Memorial Prize winners in economics. Wassily Leontief observes critically how “Page after page of professional economic journals are filled with mathematical formulas [...] Year after year economic theorists continue to produce scores of mathematical models and to explore in great detail their formal properties; and the econometricians fit algebraic functions of all possible shapes to essentially the same sets of data” (Leontief, 1982, p. 104). Friedman concludes that “economics has become increasingly an arcane branch of mathematics rather than dealing with real economic problems” (Friedman, 1999, p. 137). And Coase finds that “Existing economics is a theoretical [meaning mathematical] system which floats in the air and which bears little relation to what happens in the real world” (Coase, 1999, p. 2). And there are many other observers of this situation too<sup>6</sup>.

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<sup>6</sup> William Baumol focuses on hurdles facing students in particular:

“These days few specialised students are allowed to proceed without devoting a very considerable proportion of their time to the acquisition of mathematical tools, and they often come away feeling that any piece of writing they produce will automatically be rejected as unworthy if is not liberally sprinkled with an array of algebraic symbols” (Baumol, 1992, p. 2).

Roger Guesnerie focuses on research:

“Mathematics now plays a controversial but decisive role in economic research. This is demonstrated, for example, by the recourse to formalisation in the discussion of economic theory, and increasingly, regardless of the field. Anyone with doubts has only to skim the latest issues of the journals that are considered, for better or worse, the most prestigious and are in any case the most influential in the academic world” (Guesnerie, 1997, p. 88).

And Robert Solow admits that:

“Today if you ask a mainstream economist a question about almost any aspect of economic life, the response will be: suppose we model that situation and see what happens... modern mainstream economics consists of little else but examples of this process” (Solow, 1996, p. 43)

Of course, heterodox economists do often capture the situation best. Consider the very apt assessment of Diana Strassmann, the editor of *Feminist Economics*. Like other heterodox economists Strassmann certainly does not reduce economics to mathematical formalism but notices that this is an essential feature of the mainstream:

“To a mainstream economist, theory means model, and model means ideas expressed in mathematical form. In learning how to “think like an economist,” students learn certain critical concepts and models, ideas which typically are taught initially through simple mathematical analyses. These models, students learn, are theory. In more advanced courses, economic theories are presented in more mathematically elaborate models. Mainstream economists believe proper models - good models - take a recognizable form: presentation in equations, with mathematically expressed definitions, assumptions, and theoretical developments clearly laid out. Students also learn how economists argue. They learn that the legitimate way to argue is with models and econometrically constructed forms of evidence. While students are also presented with verbal and geometric masterpieces produced in bygone eras, they quickly learn that novices who want jobs should emulate their current teachers rather than deceased luminaries.

Because all models are incomplete, students also learn that no model is perfect. Indeed, students learn that it is bad manners to engage in excessive questioning of simplifying assumptions. Claiming that a model is deficient is a minor feat - presumably anyone can do that. What is really valued is coming up with a better model, a better theory. And so, goes the accumulated wisdom of properly taught economists, those who criticize without coming up with better models are only pedestrian snipers. Major scientific triumphs call for a better theory with a better model in recognizable form. In this way economists learn their trade; it is how I learned mine.

## The changing face of the mainstream

Perhaps it will be thought that changes in the mainstream project that are currently underway serve to undermine the assessment sustained above? Colander et al (2004) (in their paper explicitly titled “The changing face of mainstream economics”) argue that the mainstream is currently being transformed quite significantly, and criticise heterodox economists for failing to notice such ongoing developments. Specifically these authors criticise heterodox contributors for adopting an overly “static view of the profession” (p. 486); for referring to the current mainstream as neoclassical; and for missing the “diversity that exists within the profession, and the many new ideas that are being tried out” (p. 487). In contrast these authors insist that “Mainstream economics is a complex system of evolving ideas” (p. 489), and refer to the “multiple dimensionalities that we see in the mainstream profession” (p. 489).

Now as it happens I agree with each of these assessments. In my view it has always been unhelpful to make reference to a ‘neoclassical economics’, a category rarely clearly defined, and always misleading. And diversity within the dominant tradition has never been absent. Further, as always in the past, changes in the dominant tradition (as elsewhere) are certainly currently underway. For example, I can acknowledge (with Colander *et al*) that evolutionary game theory is redefining how institutions are integrated into analysis; that ecological economics is redefining how rationality is treated; that econometric work dealing with the limitations of classical statistics is defining how economists think of empirical proof; that complexity theory is providing a new way to conceptualise equilibrium states; that computer simulations offer approach to analysis; that experimental economics is changing the way economists think about empirical work, and so on.

But it remains the case that these and all other widely sanctioned examples of ongoing change, diversity, novelty, complexity, evolution and multi-dimensionality, etc., are occurring within the framework of formalistic modelling. The insistence on mathematical-deductive modelling prevails in all cases; the essential feature of the recent and current mainstream remains intact.

In fact, Colander *et al* have noticed this aspect of ‘the changing face of mainstream economics’ themselves. I am not sure they fully appreciate the significance of their observation (they give it little emphasis) but in any case they acknowledge that:

“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:

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Therefore, imagine my reaction when I heard feminists from other disciplines apply the term theory to ideas presented in verbal form, ideas not containing even the remotest potential for mathematical expression. "This is theory?" I asked. "Where's the math?" (1994, p. 154).

“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)

And they add, with reason: “Specifically, it is because of their method, not their ideas, that most heterodox find themselves defined outside the field by the elite” (Colander et al, 2004, p. 492).

Even ongoing developments, then, ultimately provide support for my assessment that the mainstream project of modern economics must be characterised in terms neither of substantive results (such as demonstrating the desirability of the current economic order) nor of basic units of analysis (rationalistic or optimising individuals), but of its *orientation* to method. This is my first major contention: the mainstream project of modern economics just is an insistence, as a discipline-wide principle, that economic phenomena be investigated using only certain mathematical-deductive forms of reasoning. This is the mainstream conception of proper economics. It is the one feature or presupposition that remains common to (if not always explicitly formulated in) all contributions regarded as mainstream, remaining intact throughout all the changes in the projects flits in fads and fashions both previously as well as those currently under way.

### **The nature of heterodox economics**

What follows for our understanding of heterodox economics? If the latter is first and foremost a rejection of modern mainstream economics, and the latter consists in the insistence that forms of mathematical-deductive method should everywhere be utilised, then heterodox economics, in the first instance, just is a rejection of this emphasis.

Notice that this does not amount to a rejection of all mathematical-deductive modelling. But it is a rejection of the insistence that we all always and everywhere use it.

In other words, heterodox economics, in the first instance, is a rejection of a very specific form of methodological reductionism. It is a rejection of the view that formalistic methods are everywhere and always appropriate.

To say more about the nature of the heterodox traditions of modern economics, I think it is clear that we need to explain this opposition. And, as noted, we are concerned here with explaining an opposition that it is a sustained and enduring.

One conceivable explanation, I suppose, is that heterodox economists believe that methodological pluralism is desirable *per se* and no more needs to be said. But is that really all there is to it? After all in some fields of physics, such as super string theory, mathematical methods seem actually to be universally applied, but without any sign of a heterodox opposition. In economics, by contrast, there clearly is a heterodox opposition to the mainstream. And the phenomenon to explain is not just that a heterodox opposition exists, but that it is, as noted, relatively widespread, firm, often highly vocal and enduring.

## **Accounting for the nature of the heterodox opposition**

To make sense of the fact of a sustained and widespread opposition to the mainstream I think we must acknowledge that it is at least in some part based on an assessment by heterodox economists and others that the mainstream approach is actually very rarely up to the task at hand. And, indeed, the mainstream project is perceived by many as being more or less systematically irrelevant (see especially Fullbrook, 2003, 2004; Howell, 2000).

Even mainstream contributors seem increasingly to be accepting the assessment that their project (albeit typically conceived just as economics) is not doing too well in a general sense, and may actually be not very appropriate in the way it is done. Considering only those already noted, Rubinstein (1995, p. 12) notes the explanatory and predictive weaknesses of the mainstream project, whilst Leamer draws attention to a disparity of mainstream theory and practice (1983, p. 37). Coase as we have seen concludes that "Existing economics is a theoretical system which floats in the air and which bears little relation to what happens in the real world", whilst according to Leontief the mathematical formulae with which economists fill economic journals lead "the reader from sets of more or less plausible but entirely arbitrary assumptions to precisely stated but irrelevant theoretical conclusions" while econometricians fail "to advance, in any perceptible way, a systematic understanding of the structure and the operations of a real economic system" (Leontief, 1982, p. 104).

## **Ontology**

Now how could a project in modern economics turn out to be as systematically deficient as these commentators and others appear to find it? I want to suggest that a compelling (and perhaps the only plausible) explanation of it is that the sorts of methods on which the mainstream put so much emphasis are just not appropriate for dealing with social material, given the latter's nature. This is my second central thesis.

Here we get to the topic of ontology. Ontology is the study of, or a theory about, the basic nature and structure of (a domain) of reality. We all adopt ontological stances. And the acceptance of any method of analysis carries with it certain ontological preconceptions. As Marx says somewhere microscopes and chemical reagents are not appropriate to the analysis of economic forms. I suspect most of us would agree with this assessment. But the point is a general one. All methods of analysis are appropriate to some sorts of material but not others. This is as true of mathematical methods as others. My claim here is that the explanation of the poor showing of much of modern economics is that mathematical methods are being imposed in situations for which they are largely inappropriate.

In due course I shall further argue that it is an appraisal that mathematical methods are mostly inappropriate to social analysis that ultimately underpins the heterodox opposition. In short, I am contending that *the essence of the heterodox opposition is ontological in nature*. This, indeed, will be my third central thesis.

Although I will address this third contention further on, let me immediately emphasise that if it does indeed constitute a correct assessment I do not claim that the

ontological orientation of the heterodox opposition has always been, or is always, recognised. To the contrary I believe that one reason that the heterodox traditions have been less effective than their case appears to warrant is precisely that the ontological nature of their opposition has rarely been made sufficiently clear. Let me elaborate.

### **The preconceptions of modern mainstream economics**

I start by rehearsing the implicit ontology of the modern mainstream project, thereby indicating the worldview that I believe is, in effect, being opposed by heterodox contributors.

We can note, first, that the sorts of formalistic methods that economists wield mostly require, for their application, the existence (or positing) of closed systems, i.e., those in which (deterministic or stochastic) event regularities occur. Mainstream economics is thus a form of *deductivism*. By deductivism I just mean any form of explanatory endeavour that relies upon (which seeks or posits) closed systems.

Actually mainstream economics is slightly more specific than this: it is a version of deductivism that posits functional relations presupposing *closures of causal sequence*. The latter are closed systems in which the events correlated are such that one set (conceptualised as ‘independent variables’) are considered to stand in the causal history of the remaining events (the ‘dependent variable’). Thus a standard formulation of, say, a ‘consumption function’, which typically involves the correlation of household expenditure with its disposable income, posits the latter as a factor standing in the causal history of the former.

Of course, the fact that formalistic modelling methods require the identification or construction of event regularities of some sort is well recognised by mainstream economists. Allais (1992), taking the association of deductivist modelling and science for granted, expresses the conventional situation well:

"The essential condition of any science is the existence of regularities which can be analysed and forecast. This is the case in celestial mechanics. But it is also true of many economic phenomena. Indeed, their thorough analysis displays the existence of regularities which are just as striking as those found in the physical sciences. This is why economics is a science, and why this science rests on the same general principles and methods of physics" (Allais, 1992, p. 25).

But if Allais correctly points to the need for modern mainstream economists to identify or formulate social event regularities, his description of the situation of modern economics is actually quite wrong in two of its aspects. Econometricians repeatedly find that correlations of the sort formulated are no sooner reported than found to break down; social event regularities of the requisite kind are hard to come by (see Lawson, 1997a, chapter 7). And, it is just not the case that ‘striking’ event regularities of the sort Allais appears to reference, and which modern mainstream economists pursue, are essential to science. Their prevalence is a precondition for the mathematical-deductivist methods that economists emphasise having relevance, but the application of these methods cannot be equated to science (see Lawson 2003, chapter 1). Here, though, I merely note that any presumption of the universal

relevance of mathematical-modelling methods in economics ultimately presupposes a ubiquity of (strict) event regularities.

### **Atomism and isolationism**

But this is not the end to the ontological preconditions of methods of mathematical-deductivist modelling as employed in modern economics. A further important feature, which is less often recognised (or at least rarely explicitly acknowledged), is that the dependency of mathematical-deductivist methods on closed systems in turn more or less necessitates, and certainly encourages, formulations couched in terms of (i) isolated (ii) atoms. The metaphorical reference to atoms here is not intended to convey anything about size. Rather the reference is to items which exercise their own separate, independent and invariable (and so predictable) effects (relative to, or as a function of, initial conditions).

Deductivist theorising of the sort pursued in modern economics ultimately has to be couched in terms of such 'atoms' just to ensure that under given conditions  $x$  the same (predictable or deducible) outcome  $y$  always follows. If any agent in the theory could do other than some given  $y$  in specific conditions  $x$  -- either because the agent is intrinsically structured and can just act differently each time  $x$  occurs, or because the agent's action possibilities are affected by whatever else is going on -- the individuals of the analysis could not be said to be atomic and deductive inference could never be guaranteed.

Notice this assessment is not novel, at least with regard to econometrics. It is pretty much the one advanced by Keynes over 60 years ago, albeit using a slightly different terminology. Thus in response to an invitation from the League of Nations to review Tinbergen's early work on Business cycles, Keynes writes:

"There is first of all the central question of methodology, - the logic of applying the method of multiple correlation to unanalysed economic material, which we know to be non-homogeneous through time. If we are dealing with the action of numerically measurable, independent forces, adequately analysed so that we were dealing with independent atomic factors and between them completely comprehensive, acting with fluctuating relative strength on material constant and homogeneous through time, we might be able to use the method of multiple correlation with some confidence for disentangling the laws of their action....

In fact we know that every one of these conditions is far from being satisfied by the economic material under investigation.....

To proceed to some more detailed comments. The coefficients arrived at are apparently assumed to be constant for 10 years or for a larger period. Yet, surely we know that they are not constant. There is no reason at all why they should not be different every year (1973, 285-6).

The point then, however unoriginal, is that the ontological presuppositions of the insistence on mathematical modelling include the restriction that the social domain is everywhere constituted by sets of isolated atoms<sup>7</sup>.

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<sup>7</sup> Most typically such deductivist modelling endeavour encourages a specific conception of atomistic human agents (social atomism) where these are the sole explanatory units of social analysis

Now it is immediately clear, I think, that this latter restriction *need* not characterise the social realm. I want to suggest that the noted conditions for closure (a world of isolated atoms) may actually be rather rare in the social realm. I draw this conclusion on the basis of the (*a posteriori* derived) theory of social ontology, a conception of the nature of the material of social reality defended elsewhere (especially in Lawson, 1997, 2003). I shall not provide a *defence* of this ontology here but merely give a brief overview of some the central components of it that are relevant to the purpose at hand.

### **A theory of social ontology**

By *social reality* or the social realm I mean that domain of all phenomena whose existence depends at least in part on us. Thus, it includes items like social relations which depend on us entirely, but also others like technological objects, where I take technology to be that domain of phenomena with a material content but social form.

Now if social reality depends on transformative human agency, its state of being must be intrinsically dynamic or *processual*. Think of a language system. Its existence is a condition of our communicating via speech acts, etc. And through the sum total of these speech acts the language system is continuously being reproduced and, under some of its aspects at least, transformed. A language system, then, is intrinsically dynamic, its mode of being is a process of transformation. It exists in a continual process of becoming. But this is ultimately true of all aspects of social reality, including many aspects of ourselves including our personal and social identities. The social world turns on human practice.

The social realm is also highly *interconnected and organic*. Fundamental here is the prevalence of internal social relations. Relations are said to be internal when the relata are what they are and/or can do what they do, just in virtue of the relation to each other in which they stand. Obvious examples are relations holding between employer and employee, teacher and student, landlord/lady and tenant or parent and offspring. In each case you cannot have the one without the other; each is constituted through its relation to the other.

In fact, in the social realm it is found that it is social positions that are significantly internally related. It is the position I hold as a university lecturer that is internally related to the positions of students. Each year different individuals slot into the position of students and accept the obligations, privileges and tasks determined by the relation. Ultimately we all slot into a very large number of different and changing positions, each making a difference to what we can do.

The social realm is also found to be *structured*. By this I mean that it does not consist just in one ontological level. In particular, it does not reduce to human practices and other actualities but includes underlying social structures and processes of the sort just noted and [their] powers and tendencies.

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(methodological individualism).

A further fundamental category of the ontological conception I am laying out is that of *emergence*. A stratum of reality can be said to be emergent, or as possessing emergent powers, if there is a sense in which it has arisen out of a lower stratum, being formed by principles operative at the lower level; and remains dependent on the lower strata for its existence; but contains causal powers of its own which are both irreducible to those operating at the lower level and (perhaps) capable of acting back on the lower level.

Thus organic material emerged from inorganic material. And, according to the conception I am defending, the social realm is emergent from human (inter-) action, though with properties irreducible to, yet capable of causally affecting, the latter.

Finally, the stuff of the social realm is found, in addition, to include *value* and *meaning* and to be *polyvalent* (for example absences are real).

This broad perspective, as I say, is elaborated and defended elsewhere. But I doubt that, once reflected upon, the conception is especially contentious. Nor in its basic emphasis on dynamism and organicism or internal-relationality is it especially novel. However, it should be clear that if the perspective defended is at all correct, it is *prima facie* quite conceivable that the atomistic and isolationist preconceptions of mainstream economics may hold not very often at all.

That said, I emphasise that the possibility of closures of the causal sequence kind, i.e., of the sort pursued by modern mainstream economists, cannot be ruled out *a priori*. Certainly, there is nothing in the ontological conception sketched above which rules out entirely the possibility of regularities of events standing in causal sequence in the social realm. But the conception sustained does render the practice of universalising *a priori* the sorts of mathematical-deductivist methods economists wield somewhat risky if not foolhardy, requiring or presupposing, as it does, that social event regularities of the relevant sort are ubiquitous.

Equally to the point, in discussing the nature of modern mainstream economics above we saw in passing that it is not in an entirely healthy state; indeed I think it is fair to say that intellectually it is in a state of disarray. In particular, it performs badly according to its own explanatory and predictive criteria, and is plagued by theory practice inconsistencies. In the foregoing discussion we have an explanation. For if the conception of social ontology sketched above does not altogether rule out the possibility of social event regularities of the sort in question occurring here and there, it does provide a rather compelling explanation of the *a posteriori* rather generalised lack of (or at best limited) successes with mathematical-deductivist or closed-systems explanatory methods to date

I do not doubt that mathematical-deductive methods have many desirable features. But 'fit' with reality matters too. The problem with the mainstream stance is that the ontological preconditions of its formalistic methods appear to be not only *not* ubiquitous in the social realm, but actually rather special occurrences. If we knew both that social life was everywhere atomistic, and also that for any type of outcome we could effectively isolate a fixed set of causes (treating all other causal processes as a kind of stable, non-intervening or homogeneous backdrop), we would have grounds for feeling confident in the emphasis that mainstream economists place on the sorts of

deductivist methods they use. However, our best ontological analysis suggests that closures are a special case of social ontology, whilst our *a posteriori* experience is that this special case seems not to come about very often at all.

### **Implications for heterodox economics**

So what? How does this help us understand heterodox economics? My claim is that something like the alternative ontology described above (and defended elsewhere see Lawson, 1997, 2003) forms the implicit preconceptions of the various heterodox traditions, and ultimately explains their enduring opposition to the mainstream. This was my third basic thesis already noted.

I repeat that I do not claim that such an ontological orientation is always explicit in heterodox contributions<sup>8</sup>. Indeed the term ontology itself is rarely mentioned. At least this has been so until recently<sup>9</sup>. But the sorts of emphases that are prominent clearly do presuppose something like the ontological position I have described above.

Thus, briefly, the Post Keynesian emphasis on fundamental uncertainty is easily explained if openness is a presupposition, just like the institutionalist emphasis on evolutionary method and on technology as a dynamic force (and institutions as a relatively enduring feature of social life – see Lawson 2003) are explained if it is presupposed that the social system is a process, and the feminist emphasis on caring and interdependence presupposes an ontology of internal relationality, amongst other things. The dominant emphases of the separate heterodox traditions, in other words, just are manifestations of categories of social reality that conflict with the assumption that social life is everywhere composed of isolated atoms; as I say they are categories best explained by an implicit attachment to something like the social ontology outlined above.

As I have also already noted, the heterodox ontological presuppositions are rarely rendered explicit. Part of what I want to say is that they should be. Mainstream economists have found it all too easy to find closed-system substitutes of heterodox claims or emphasises, once it is admitted that heterodox economists have made a point. Thus, uncertainty is mapped onto risk; evolutionary concepts are shorn of their Darwinianism and reinterpreted in terms of the requirements of non-linear or game theory modelling; care for others becomes a variable in a utility function; and so on.

The fact that heterodox economists resist the mainstream reformulation of their concepts of uncertainty, evolutionary developments, care, institutions and history, etc., reveal that heterodoxy is not so much committed to the latter categories *per se*, as insisting on their possessing the ontological properties of openness, processuality, and internal-relationality, etc. that I have elaborated above. Once the heterodox groups make their attachment to this ontology explicit in the mainstream's transformative manoeuvres are pre-empted. The heterodox challenge becomes at once more powerful and less easily by-passed or seemingly accommodated.

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<sup>8</sup> Though there are exceptions, such as Paul Davidson's concern with (non) ergodic systems (see Davidson, 1991, 1994, and 1996).

<sup>9</sup> Of course where critics of the mainstream see the latter as substantive ideology the emphasis on ontology promoted here can be viewed as a distraction (see for example Guerrien, 2004).

I return to these sorts of considerations in due course. But for the time being I want to re-emphasise the point that the feature that drives the heterodox opposition to an insistence on mathematical formalism is an implicit worldview at odds with that which the formalistic methods presuppose. Thus I am arguing that, collectively, heterodox economists are primarily motivated, in their opposition to the mainstream, by ontological (not epistemological) considerations. Specifically, I believe we can explain the heterodox resistance to the mainstream incorporation of their key categories (uncertainty, evolutionary change, caring relations, etc.,) only by recognising that the latter are really defended as manifestations of, and that heterodox economists carry commitments to, an underlying ontology of openness, process and internal-relationality. The latter is an ontology which mainstream economists simply cannot accommodate as long as they insist on employing only mathematical-deductivist methods.

### **The nature of heterodoxy**

I have suggested, then, that the various heterodox traditions can be identified as heterodox through a recognition of the fact that they advance claims or practices or orientations which are either concrete manifestations of, or presuppose for their legitimacy, a social ontology of the (seemingly coherent) sort set out above. *In short, the set of projects currently collected together and systematised as heterodox economics is, in the first instance, an orientation in ontology.*

But, of course, the heterodox projects on which I am focussing present themselves as first and foremost traditions in economics; they would resist being reduced to the orientation in ontology that, I am suggesting, distinguishes them as heterodox. Indeed this latter orientation has rarely been explicitly acknowledged anyway, at least until recently, as I have pointed out<sup>10</sup>.

Interestingly, however, once we start looking at the more substantive orientations of the heterodox groups we focus on the issues that begin to distinguish the individual heterodox groups from each other.

At this point a new question of interest arises. If an implicit commitment to the ontological conception described above renders heterodoxy coherent as a collective project, is it the case that the separate traditions can each claim individual coherency with the latter being achieved at a more substantive level than ontology and in a

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<sup>10</sup> I do not wish to imply that individuals working mostly within heterodox traditions in economics could not themselves make a contribution to philosophical ontology. On philosophical matters the flow of insights can be both ways between projects in ontology and the heterodox traditions in economics. Indeed, currently there is real blossoming of insightful output by heterodox economists and others critically interacting with and seeking to shape (at the least the application of) the sort of ontological perspective described above, a perspective often systematised as critical realism in economics. See in particular Arestis, Brown and Sawyer, 2003; Beaulier and Boettke, 2004; Davis, 2004; Dow, 1999, 2003; Downward, Finch and Ramsey, 2003; Downward and Mearman, 2003a, 2003b; Dunn, 2004; Finch and McMaster (2003); Graça Moura, Mario da, 2004; Hands, 2004; Hargreaves Heap 2004; Kuiper, 2004; Lee, 2003; Lewis, 2004a, 2004b; McKenna and Zannoni, 1999; Nell, 2004; Olsen, 2003; Pagano, 2004; Pinkstone, 2003; Rotheim, 1999; Setterfield, 2003; Smithin, 2004.

manner that renders each heterodox tradition distinct from the others<sup>11</sup>? It is this question that I now address.

### **Distinguishing the heterodox traditions.**

It will be remembered that I earlier noted of each separate heterodox project that except for its basic guiding emphasis there is much internal debate and disagreement over substantive theories and policy stances, as well as appropriate basic units of analysis and other methodological principles. This situation, indeed, has led some to question whether projects like post Keynesianism (e.g., Hamouda and Harcourt, 1988) or old institutionalism (see Rutherford, 2000) can actually be regarded as coherent. I now want to propose a conception or interpretation of these heterodox traditions that can make sense of, and indeed ground, the fact of competing conceptions or theories and methodological claims within in any one separate heterodox tradition, whilst simultaneously rendering the separate heterodox traditions coherent.

The coherence of each separate project *as a form of heterodoxy* is achieved, I have been arguing, just by recognising each project as being committed broadly to the sort of ontological conception earlier discussed. With regard to distinctions, I contend that the heterodox traditions are can be coherently identified and distinguished from each other, but *not* according to any specific theories or policy proposals favoured and defended, nor in terms of any features of the economy held to constitute the most basic units of analysis, nor according to any other specific substantive or methodological claim.

Rather I suggest that the most, and perhaps only, tenable basis for drawing distinctions between the various heterodox projects is *according to substantive questions raised or problems or aspects of the socio-economic world thought sufficiently important or interesting or of concern as to warrant sustained and systematic examination*. That is, I suggest that the separate projects be characterised according to the features of socio-economic life upon which they find reason continually to focus their study.

In other words, if ontology can account for the distinctions between the heterodox traditions and the modern mainstream, i.e., if *ontological commitments identify post Keynesians, institutionalists, feminist economists and others as heterodox*, it is their particular *substantive orientations and concerns and emphases, not answers or principles, that distinguishes the heterodox traditions from each other*. The latter is my fourth and final basic thesis or contention.

### **The place of economics in social science**

It may be useful to set this contention against the conception of economics I defend in *Reorienting Economics* (Lawson, 2003). Mainstream economics, of course, has implicitly defined the discipline in terms of method. The obvious alternative

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<sup>11</sup> I might note at this stage that there is surprisingly little comparative work in the literature. Some recent exceptions do exist (for example Danby, 2004.) But there is a need for far more. Some insights can be gained though by considering those who have compared and likened one heterodox tradition (e.g., Beaulier and Boettke, 2004; Dunn, 2004; Kuiper, 2004;) or more (e.g., Austen and Jefferson, 2004) with the ontological conception described above.

approach is identify the different sciences not according to methods they employ but according to the nature of the material(s) or principles with which they are concerned. Thus physicists study certain physical principles, biologists study life processes, and so on.

Now it is a claim mentioned above and defended elsewhere (Lawson, 1997, 2003) that the materials and principles of social reality are the same across economics, sociology, politics, anthropology, human geography, all other disciplines concerned with the study of social life.

Perhaps this is why economists have sought to distinguish themselves (arbitrarily) through an adherence to a specific set of methods. But the reasonable response, I suggest, is just to accept that there is no legitimate basis for distinguishing a *separate* science of economics. Rather economics is best viewed as at most a division of labour within a single social science.

What is that division of labour? I think this question can only be answered historically, though guided by ethical considerations such as inclusivity. In *Reorienting Economics* (Lawson, 2003) my strategy is to seek to synthesise the main accounts (of Mill, Marshall and Robbins) traditionally regarded as competing contenders. In doing so the conclusion I reach is that economics is best characterised as the division of social theory or science primarily concerned with studying all social structures and processes bearing upon the material conditions of well-being<sup>12</sup>. This view is easy to sustain, but in its detail it does not concern the present discussion. For whether or not the specifics of the latter suggestion are accepted the broader point of relevance here is that if economics is to be distinguished as a strand of social research it cannot be according to its own ontology, methodological principles, or substantive claims, but in terms only of its particular focus of interest (I elaborate on all this in Lawson, 2003, chapter 6).

### **The place of the heterodox traditions within economics**

It is in a similar fashion that I am proposing that the various heterodox traditions might also be considered as divisions of labour, albeit as divisions now within economics. It is the mainstream project that has purported to provide general theories at a substantive level, as well as setting down supposedly universal methodological principles. Heterodox economics can, and I believe should, avoid adopting the mainstream criteria of success uncritically. My suggestion is that just as economics (like all other disciplines) is appropriately conceived as a programme of research, not a set of answers or principles, the same holds for the heterodox traditions within economics.

Specifically I have at several points noted that each separate heterodox tradition has tended to emphasise various features of social reality regarded as fundamental or of significant interest (even if attempts to theorise around these features is highly

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<sup>12</sup> More specifically I suggest that economics is: “the identification and study of the factors, and in particular social relations, governing those aspects of human action most closely connected to the production, distribution and use of the material conditions of well-being, along with the assessment of alternative really possible scenarios.”

variable and competitive). I am now suggesting that each individual heterodox tradition be identified precisely with these sorts of general features and emphases, conceived of as constituting topics for research.

Thus, post Keynesians, given their previous emphases, might be identified according to their concern with the fact of fundamental uncertainty stemming from the openness of social reality. Such a focus could take in 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 the 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 the latter in turn impact back on individual acts and pressures for structural transformation, etc.

By similar reasoning I contend that it is best to distinguish institutionalism, *not* according to claims along the lines that institutions or evolutionary processes constitute main units of analysis or some such but, given that project's traditional concern with evolutionary issues, in terms of its interest in *examining how* social items change and/or endure over time. From such a perspective those aspects of social life that are most enduring, such as institutions and habits, are particularly significant. So too are the interactions of factors like institutions and technology in the process of reproduction and change. And a yet further likely related focus of interest is the manner in which the institutional shaping and reshaping of specific individual identities can be explanatorily consequential in given contexts.

Austrians may perhaps be best identified according to their emphasis on studying the market process and entrepreneurship in particular, or perhaps in line with the attention given by this project to the role of inter-subjective meaning in social life, and so on.

Such suggestions are consistent with recent reflections of some heterodox thinkers concerned with the nature of the particular project with which they are associated (see some of the contributors to Lewis, 2004). For example, they do tie in with the way that feminist economists have tended to identify their own project as one that (in addition to proceeding very differently to the mainstream) concerns itself with women as subjects (which may include, for example, giving attention to differences amongst 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 economy.

The latter focus 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 power and strategy, and so forth. This orientation has inevitably meant a significant attention, within feminist economics, to issues that historically have been gender-related, such as caring, especially for children, and indeed the nature of family structures in specific locations. But in principle there is no area of social life that is excluded. It is the sorts of questions

pursued that seems most to distinguish an approach *within* the heterodox traditions as feminist, not specific substantive claims or methodological principles<sup>13</sup>.

I might emphasise, finally, that in suggesting that heterodox economic traditions be identified and distinguished in the manner described, i.e., as divisions of labour within economics, I do not suppose any separation of subject-matter. There can be no strict partitioning of topics. The social world seems to be a highly internally related (emergent) totality. 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. Similarly, the various heterodox projects can be seen as approaching the same totality from different perspectives within economics, each with their own set of questions, immediate motivating interests, emphases, and so forth, and each achieving results warranting of synthesis<sup>14</sup> with all others.

## Conclusion

In questioning the nature of heterodox economics, I have advanced and defended four basic theses or contentions. These can be summarised as follows:

- i) The nature of the enduring modern mainstream project which the heterodox traditions continue to oppose, and against which they must ultimately identify themselves as heterodox, is set not in terms of its substantive results or basic units of analysis, but according to its *orientation* to method. The mainstream project of modern economics just is an insistence, as a discipline-wide principle, that economic phenomena be investigated using only (or almost only) certain mathematical-deductive forms of reasoning.
- ii) The often noted intellectual failings and limitations of this mainstream project arise just because its *emphasis* on mathematical deductivist reasoning is inappropriate given the nature of social material. In other words, the ontological presuppositions of these methods do not everywhere match the nature of social reality.
- iii) The heterodox opposition is based on a (albeit often no more than implicit) grasping of the fact that the situation expressed in the just noted second contention is the case. In other words, modern heterodoxy is, *qua* heterodoxy, first and foremost an orientation in ontology.
- iv) The individual heterodox traditions are rendered distinct from each other by their particular substantive orientations, concerns and emphases, not by theoretical claims or results, empirical findings, methodological principles or policy stances.

The perspective sustained will surely be contested, not least by those economists who prefer to view themselves as heterodox but who believe that mathematical-deductivist reasoning is desirable in itself. But in the absence of any more coherent or

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<sup>13</sup> Of course, the question 'what is feminist theory?' is highly discussed amongst feminist writers and generates various often very different responses. For a critical survey of the question that reaches a not dissimilar assessment to the very general suggestion advanced here, see Beasley, 1999.

<sup>14</sup> I use the word loosely. If findings are inconsistent clearly forms of critical resolution are required.

empirically adequate thesis on the nature of modern heterodoxy, the broad thesis advanced here does have something to commend it. In particular, the set of contentions defended allows, without any obvious tension, a way of distinguishing the various heterodox traditions collectively from the mainstream and individually from each other, in a manner that does not compromise their coherence as fruitful traditions in economics.

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